Studies

in the History of Statistics and Probability

Vol. 6. Collected Translations

F. W. Bessel, C. F. Gauss
and about them

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Contents

Introduction by the Compiler

The works of Gauss are mentioned throughout, and I list them here.

Werke, Bde 1 – 12. Göttingen, 1863 – 1930. Reprint: Hildesheim, 1973 – 1981.

Werke, Ergänzungsreihe, Bde 1 – 5. Hildesheim, 1973 – 1981.

These volumes are reprints of the previously published correspondence of Gauss with Bessel (Bd. 1); Bolyai (Bd. 2); Gerling (Bd. 3); Olbers (Bd. 4, No. 1 – 2); and Schumacher (Bd. 5, No. 1 – 3).

Notation: *W*-i = *Werke*, Bd. i.

W/Erg-i = *Werke*, Ergänzungsreihe, Bd. i.

Bessel's *Abhandlungen*, Bde 1 – 3. Leipzig, 1875 – 1876 are his selected works (editor, R. Engelmann). A list of Bessel's works is in his *Abhandlungen*, Bd. 3, pp. 490 – 504. These contributions are there numbered; two numbers are provided for those that are included in the *Abhandlungen*.

Notation: [No. i] = Bessel's contribution i included in the list, but not in the *Abhandlungen*

[No. i/j] = Bessel's contribution i included both in the list and in the *Abhandlungen* and accompanied there by number j

Many letters exchanged by Bessel. Gauss, Olbers and Schumacher are quoted.

Notation: B – S = letter from Bessel to Schumacher;

G – O = letter from Gauss to Olbers; etc

Special notation: **S, G,** i means see Document i on my website www.sheynin.de or on its copy at Google, Oscar Sheynin, Home. The document is in my translation from Russian or German or in its original English if barely available.

I mention three representatives of the Repsold family, all of them manufacturers of optical instruments; the last-mentioned was also the author of [viii]:

Johann Georg, 1770 – 1830; Adolf, 1806 – 1871; Johann Adolf, 1838 – 1919

Finally, I was unable to understand the description of some astronomical instruments, especially since the appropriate German terms are too difficult to find in English translations.

General Comments to Some Items

[i] This tiny contribution does not include any discoveries. On the contrary, it contains serious errors (see Notes 3 and 9) and, in general, Gauss had not thought it out properly (Notes 5 and 14). His own later statement, which Biermann quoted at the very beginning, is valid here also: Gauss has *neither time nor inclination for* [prior] *literary studies*. The title of Biermann's paper is thus too generous for Gauss. Biermann, for his part, vainly tried to enhance the significance of Gauss' efforts and his own comments are not at all sufficient.

There are two other instances in which Gauss made wrong statements about his predecessors, both in his *Theory of Motion* (1809). First, in § 177 he stated that Laplace (actually, Euler) first calculated the integral of the exponential function of a negative square. However, he later noted his mistake (Sheynin 2013, § 10A1.4). Second, in § 186 Gauss attributed to Laplace one of the two conditions of the Boscovich method of adjusting observations.

[ii] This writing is certainly little known. It clearly shows Gauss' attitude to investigation of instruments and to observations proper, and it proves once again (see [i, Note 16]) that not only Bessel, but he and Gauss had originated the attitude just mentioned. Thus, Gauss was apparently the first to investigate the errors of graduation (§ 3.8). He also attempted to consider the effect of gravity on the bending of telescopes (§ 3.14). Bessel studied the same effect much later (1844; 1846). Another interesting noveltiy was Gauss' reasoning belonging, to the later physical geodesy (Note 27).

The value of this writing is, however, lowered due to some unexplained conclusions and, worse, strange statements, see for example Notes 7, 16, 18, 22. Strange is also the inclusion of 16 unnamed stars in the provided tables, for example, in § 1. And, after studying the determination of a certain coefficient θ (§§ 3.9 – 3.10) it turned out that in another context θ was not needed at all (§ 4). An explanation would have been quite proper.

[vi] Bessel′s talent as well as his diligence are clearly seen in these *Recollections*. He could have, but happily did not become a prominent businessman. In the beginning of § 4 he tells us that he feared of losing his job, but the following events refuted his opinion, and his yearly bonuses gradually became rather impressive. In his childhood Bessel *detested the rudiments of Latin* (§ 1), but in 1818 he published an important contribution (*Fundamenta Astronomiae*) in that language (and even earlier had reviewed a few works in Latin), so when did he learn the language?

The included text of Olbers' letter to Bessel of 1804 (§ 9) deserves special attention.

[viii] It will be difficult or even impossible to find elsewhere much of the unearthed information. The description is not, however, always coherent and many statements (some of them formulated by Repsold himself) are not sufficiently explained. Especially disturbing is the obviously wrong remark at the end of § 28 that Bessel, apparently in 1840 or earlier, had already visited Oxford. His journey to England occurred in 1842 (§ 30).

At the end of his paper the author makes known that Bessel and his wife were buried near his observatory. It is desirable to find out whether their graves and/or the ruins of his observatory are still in existence in present Kaliningrad.

[ix] This brief essay provides a general impression about the life and work of Olbers and it is the more valuable since the author (Bessel) had been intimately connected with his hero.

[x] I point out an unexpected aspect of Bessel's work, and I note here that there exists a general tendency for concealing the negative sides of the work of our betters.

I

Kurt-R. Biermann

C. F. Gauss as a historian of mathematics and astronomy

K.-R. Biermann, C. F. Gauss als Mathematik- und Astronomiehistoriker.
Hist. Math., vol. 10, 1983, pp. 422 – 434

Dedicated to Prof. Dr. Kurt Vogel
on the occasion of his 95th birthday

Abstract (in Author's Translation)

In 1810, Gauss published two essays on the history of mathematics and astronomy in Germany between 1700 and 1800. Today, these papers are unnoticed. Here these papers are reproduced and discussed.

Preliminary Remark

It can be presumed that experts in the works of Gauss will shake their heads since they know about the *Princeps mathematicorum's* dislike of historical and literary research and popular expositions. Thus (G – S, 6 July 1840), Gauss wrote to his intimate friend:

I reluctantly express myself in detail about the achievements attained by others working in the same field as I did, if only not being entirely convinced in that I really may mention them approvingly.

And, again,

Nevertheless, I recognize […] *that I did not at all study critically* [the history of the theory of magnetism]. *As a rule, I am unable to decide just like that who should be favourably mentioned and thus to unconditionally reinforce myself. And, when desiring to provide authoritative connections, it would have been necessary to conduct prior literary studies for which I have neither time nor inclination. Indeed, such investigations are not exactly to my taste*.

And when Gauss was about to propose prize mathematical problems to [his] Göttingen students, he (G – S, 25 Jan. 1842) stated:

I do not like to propose historical problems and prefer to occupy myself with my own work.

The practised dislike of searching for, and quoting his predecessors earned him C. G. J. Jacobi's reproach:

For Gauss, it is not de mortuis nil nisi bene [nothing but good should be spoken of the dead] *but de mortuis et de vivis nil* [nothing about either the dead or the living], see his letter to Bessel of 3 April 1835 (Biermann 1963, p. 222), see also [   ].

However, Gauss felt himself prepared to compile a popular paper (1836) for Schumacher's *Astron. Jahrbücher* (Tübingen, 1836 – 1844), and thus to please his intimate friend. It occurred, however, that Humboldt misunderstood some of it, and Gauss disappointedly reported to his friend (G – S, 15 April 1836):

After all, it is apparently my own fault. In spite of my efforts, I have not properly achieved necessary clarity.

On 10 May 1853, he wrote to Humboldt (Biermann 1977, p. 112):
For a long time now, I am not setting high store on attaining a taste of haute culture or the so-called high position by reading popular literature or attending popular lectures. I rather believe that in science a tried and tested insight can only be attained by our own efforts and by treating of that, which was proposed by others.

In 1977, an unnoticed exception (1813) to the Gauss rule of refusing to compile popular historical surveys came to light, and I am reprinting it below. I was unable to find it in any bibliographies of Gauss' works[1].

Gauss was indeed the author of that paper as follows from a footnote by the compiler of the monograph, *History of Literature*, which contains it. Johann Gottfried Eichhorn (1752 – 1827), a Göttingen Professor of oriental languages and a historian. There is also another similar footnote in the text of the Gauss paper[2]. Gauss provided §§ 102 and 103 in vol. 3, No. 1 of that monograph which appeared in 1805 – 1813 in six volumes.

Understandably, Eichhorn thought himself incapable of dealing with these sections and asked his competent colleague to help him. For his part, Gauss understood that Eichhorn's request was not unreasonable. He overcame his dislike and began working. Now, according to the spirit of the time, Eichhorn considered his monograph as a single entity and included both literary and scientific items.

When appraising the Gauss paper we ought to take into account that he had to guide himself by Eichhorn's ideas who saw his work as a *Reference Book on the History of Literature*, as a *survey which describes the destiny of science during the previous thousand years* (vol. 1, p. III). In the first place, his survey should have guided *future scientists*, i. e., students (p. IV), whereas *experience will determine its significance for those who have already concluded their learning* (p. V).

The names and the dates of birth and death of mathematicians and astronomers mentioned by Gauss are included in a special list which follows after his text.

The Gauss Text

102. During this period, sufficiently known names have also appeared in Germany so that in mathematics its comparison with other countries can be honourably withstood. Leibniz, that man of genius, whose many diverse occupations had been too dispersed, participated in the further extension of the vast field to which he had paved the way by scattering fruitful embryos rather than by a methodical and coherent treatment of the whole.

However, at the hands of the two no less remarkable scholars, the brothers Jakob and Johann Bernoulli, the edifice of the analysis of infinitesimals rapidly soared up to an admirable height. For these great geometers the doctrine of series, the curves, and a large number of most difficult problems in mechanics became an inexhaustible source of interesting discoveries. Even their mutual jealousy engendered the initial cause for various delicate studies[3]. Daniel Bernoulli, the son of Johann, followed in his father's footsteps. His numerous contributions to the doctrine of series, calculus of probability, mechanics and hydrodynamics earned him an honourable place alongside the best geometers of his time[4].

However, the immortal Leonhard Euler shone like a star of the fist magnitude. His most difficult researches carried out with a barely understandable speed, his inexhaustible and fruitful torrent of new ideas and methods cannot also be attributed to any other mathematician either of olden times or recent. Euler revised all branches of mathematics and in his hands most of them acquired a completely new form. Unforgettable are his merits in higher arithmetic, in dealing with cylinder functions, applications of analysis to curves, the doctrine of series, the theory of algebraic equations, the differential and the integral calculus, mechanics and optics.

Because of his numerous applications of mathematical theories to events in everyday life, Johann Heinrich Lambert also belongs to the most meritorious mathematicians of the century. His *Photometria* marked an epoch in that new branch of optics also revised by Bouguer[5].

Only a few names of those many, who had furthered some branch of mathematics, can be mentioned here. The calculus of probability which Jakob Bernoulli had already applied to games of chance, had been successfully used in problems of civil life connected with the laws of longevity of life. Valuable work was here due to Süssmilch[6], Florencourt and especially Tetens.

Just the same, Jakob Bernoulli had splendidly treated the doctrine of combinations and Hindenburg attempted to systematize the adjoining operations. Pfaff provided excellent contributions to the theory of series.

By following a new approach, Gauss had successfully studied the theory of algebraic equations whose essential difficulties had not been satisfactorily overcome. In the same lengthy contribution of 1801 on the number theory or higher arithmetic this important part of mathematics, only studied until now by a few geometers, acquired a new form and even an interesting and unexpected explanation and generalization by connecting it with a branch of analysis, namely, with the theory of regular polygons.

A grateful remembrance also deserve Wolf [Wolff, K.-R. B.], Hansen, Segner, Karsten, Kästner, Klügel, Vega, Pasquich and others. By useful textbooks they endeavoured to alleviate the entrance to the science which has grown so widely.

103. During that period, Germany had played the main role in achieving great progress of Astronomy. In addition, a large number of new public observatories had been erected, especially in Gotha, Mannheim and Göttingen, which benefited science[7]. Very many private people took to observation which was greatly useful for geography [for the geographical distribution of observations?] and separate parts of astronomy, and it also resulted in remarkable new discoveries.

In Vienna and Berlin, following the example of other nations, [the moments of] celestial events, and especially ephemerides have been calculated in advance, and the work with ephemerides is continuing.

Yearly surveys of the most important astronomical events coupled with the still richer journal edited by von Zach provided a valuable addition to the above are as though a platform for uniting the astronomers not only of Germany, but of whole Europe.

There never was a period more fruitful in discoveries in the heaven than the last quarter of the past, and the beginning of this century. For the discovery of five main (!) planets of our Solar system made during that time we are thankful to four Germans, Herschel, Olbers and Harding[8] as well as to Gauss, von Zach and Olbers for the rediscovery of the fifth one of them, Ceres, which foreign astronomers vainly searched for and partly gave up on their efforts.

The power of his telescope which he himself brought up to a previously unknown measure of perfection allowed Herschel to discover six satellites of Uranus and two new satellites of Saturn. Not less remarkable are his observations of double stars, nebulae, the surface of the Sun and planets and of their rotation around the Sun[9].

Schröter's observations of the surface and other remarkable physical features of the planets, of the Saturn ring, Jupiter's satellites, comets, and, quite exceptionally, of our Moon, made in a laudable competition with Herschel, show what can achieve an excellent talent for observation together with untiring diligence.

Great is the glory of the Germans in observing unknown facts and phenomena in the heaven, and no less is their merit in an ever more precise determination of the places and movements of celestial bodies by observation and theory. In richness and precision Tobias Mayer's list of zodiacal stars surpassed everything that existed previously. At the observatories in Gotha and Mannheim von Zach and Barry compiled similar new lists based on observations. Their precision is of the highest measure which can be achieved by the new perfected art of observation. Until the mid-18th century there had only been tables of the most considerable inequalities in the lunar motion and those tables essentially diverged from observation.

Euler was the first to show the form which the lesser inequalities ought to have, and, starting from his theory, Tobias Mayer compiled his remarkable lunar tables after carefully and successfully combining observations over many years. Their errors are restricted by very narrow bounds.

A tireless diligence of another German, Tobias Bürg, brought these lunar tables to a still higher measure of precision, and for a long time nothing more will be achieved here. Tobias Mayer, von Zach and Triesnecker deserved similar merit for the theory of the motion of the Sun and Mars.

Subtle astronomical calculations attained by an accurate unification of analysis and astronomy acquired a new form and the calculation of parabolic cometary orbits very much profited from the work of Euler and Lambert as well as quite superbly from Olbers. In convenience and brevity his method of solving [the appropriate problem] left all the other methods far behind.

The discovery of the new planets and the necessity of as an early, and as precise as possible determination of their orbits to ensure their future rediscovery have prompted Gauss to similar and more extensive studies of the motion of celestial bodies in conic sections of each kind.

When applying astronomical observations, namely eclipses of the Sun and occultations of stars, for geographical determination of longitudes, Triesnecker and Wurm succeeded more than all the previous astronomers taken together. And geodetic measurements [triangulations] partly carried out with most possible precision in Austria, Swabia [a historic region], Westphalia, Bavaria, and Thuringia essentially glorified Germany. Triangulation in Thuringia together with an arc measurement will be second to none of similar work done abroad if only circumstances of time will not hinder its completion.

The achievements of Tobias Mayer, Lambert and Kramp in studying astronomical refraction belong to the best results in that branch of astronomy.

Explanation

At first sight, the honourable mention of Leibniz in § 102 is seen to be qualified by a reservation. This exactly accords with what Sartorius von Waltershausen (1856, pp. 84 – 85) had told us about Gauss' oral remark:

The two geniuses of the 17th century had often been compared with each other, and also by Gauss. He recognized Leibniz' great talent and did not deny his merit in the discovery of the differential calculus, but quite bitterly reprimanded him for studying all possible things, regrettably at the expense of mathematics. For this reason we are certainly unable to compare the merits of Leibniz and Newton.

It is quite possible to add that Gauss judged without considering Leibniz' economical constraints or taking into account his mathematical estate[10].

After an appropriate mention of the most eminent representatives of the Bernoulli dynasty, there followed Gauss' likely pithiest known opinion about the significance of Leonhard Euler.

Gauss' contribution was restricted to Germany, i. e. to the German-speaking region so that he only meritoriously mentioned one Frenchman (Bouguer) and only in connection with the work of Lambert who had indeed earned due attention. Among the writers on combinatorics Gauss mentioned Hindenburg, and it ought to be remarked that the papers which Gauss had sent in 1796 and 1799 to his *Archiv der reinen und angewandten Mathematik* were rejected, see Gauss' letters to Hindenburg of 8 Oct. 1799, *W*-10/1, pp. 429 – 431, and Stargardt (1971, pp. 114 and 115). This incident had not apparently affected Gauss. Pfaff, his former [and mostly formal] scientific mentor, who also worked in combinatorics, did not forget to mention Gauss and to implicitly refer to his dissertation (1799) or *Disquisitionen* (1801)[11].

Some of the other more or less summarily treated mathematicians had since been forgotten, at least if judging by their absence in the *Dictionary of Scientific Biography* (DSB). These are Süssmilch, Florencourt, Tetens, Hausen, Karsten, Vega and Pasquich. We assume that the merits of Wolff, Segner, Gauss' former instructor Kästner and Klügel[12] are known or at least readily brought to memory by the DSB, and we therefore restrict our account to a few words about those forgotten, mentioned by Gauss and absent in the DSB.

Süssmilch (*Allg. Deutsche Biogr.*, ADB, vol. 37): a Berlin theologian, up to our time considered as the most eminent German representative of the school of *political arithmetic*, so called from William Petty onward. By elementary arithmetical methods *een goddelijke orde in de vaste getelverhoudingen der sociale statistieken meende te ontdekken* (Freudenthal 1966, p. 5). The fourth edition of his main work (1741) was published in 1775. The historiographer of the Berlin Academy Adolf Harnack (1900, pp. 458 – 461) compiled a list of Süssmilch's academic reports which shows the problems dealt with by him, by the learned *Ober-consistorial councillor and Probst.*

At the time when the ADB had been prepared, Florencourt, on the contrary, was quite forgotten and we can only find him in vol. 1 of Poggendorff's reference book (Pogg). He came from a French family which moved to Braunschweig, Gauss' birthplace, and became extraordinary professor at Göttingen and edited the *Abhandlungen* (1781). That Gauss considered him worthy of mention can be understood as loyalty towards a fellow countryman.

Tetens (ADB, vol. 37; Pogg, vol. 2) ,whom Gauss had placed above Süssmilch and Florencourt, is now probably known to no one. A Professor of mathematics and philosophy at Kiel since 1776, he was included in 1789 in the supreme Danish financial administration and in 1803 became one of the directors of the Danish state bank and director of the Danish general widow fund. Gauss could have mentioned him primarily owing to his book (1785 – 1786).

The most eminent of those textbook authors mentioned by Gauss and not included in the DSB was certainly Hausen, Professor of mathematics in Leipzig (ADB, vol. 15; Pogg, vol. 1), and author of the book (1734).

Among the writings of Karsten, Professor of mathematics in Rostock, then Bützow, and, from 1779, in Halle (DSB, vol. 15; Pogg, vol. 1), the most influential was probably his eight-volume work (1767 – 1777).

For a long time Vega (ADB, vol, 39; Pogg., vol. 2) remained important for mathematicians, astronomers and geodesists owing to his tables (1783; 1793; 1794). Until the mid-20th century they had run into more than a hundred editions. Surprisingly, he was not includcd in the DSB. Vega's career as a mathematician in the Austrian military service finally brought him, a son of a Slovenian small peasant, a baronial title. It, the career, was as unusual as his death at the hands of a robber whose crime was only revealed nine years later (Depman 1953; Allmer 1977). Gauss certainly included him among textbook writers not because of his tables, but owing to his *Vorlesungen* (1786 – 1802) which ran into many editions.

Finally, Pasquich. He is forgotten in the history of mathematics but plays a certain role in the history of astronomy. Pasquich was an astronomer in Pest, then in Ofen (Buda) and was accused of falsifying his observations (Küssner 1981, pp. 19 and 29). Gauss defended him and wrote an *Ehrenrettung* (Honourable rescue), but only in 1825. When, in 1810, he named Pasquich as the author of mathematical textbooks, it was certainly due to his works (1790 – 1791) and (1799)[13].

We turn now to the lengthier § 103. When compiling his note, Gauss was at the peak of his so-called *astronomical creative period*. He never gave up his mathematical ambitions, but exactly then, after the appearance of his *Theory of Motion*, which *will be studied even after* [some] *centuries*, see his letter to his publisher Perthes (Salié 1957, p. 24), his hopes for the continuation of the construction of the Göttingen observatory had germinated anew, and his heart mostly belonged to astronomy.

From the astronomers mentioned the number of those who left no trace in the history of science or in any case not included in the DSB (Barry, Triesnecker and Wurm), is less than in the case of mathematicians. Since ca. 1790 Barry (only in Pogg, vol. 2), a Lazarist [a member of a brotherhood in Catholicism], was an astronomer in the Mannheim observatory. More influential, however, had been Triesnecker (ADB, vol. 38; Pogg, vol. 2), Professor of astronomy and director of an observatory in Vienna, and Wurm, an astronomer in Stuttgart although initially a theologian (ADB, vol. 34; Pogg, vol. 2). Wurm was a correspondent of Gauss who called on him in 1825 during his travel to south-western Germany to be personally acquainted with him. Both Triesnecker and Wurm had been known sufficiently for Humboldt to mention them in his *Kosmos* (1850, pp. 65 and 546; 243 and 443 – 444 and 483 – 484 respectively). Gauss had clearly indicated their merit, and no further description is necessary.

However, it seems necessary to say a few words about William Herschel whom Gauss called a German. Just the same, Florencourt could have certainly been called a Frenchman, but we ought to take into account the time when Gauss compiled his note, the time of an active national feeling generated by the previous occupation of the country by Napoleonic troops. Again, we should realize that in those times the German-speaking Austrians and Swiss had been called Germans[14].

When indicating the development of astronomy at the end of the 18th century, Gauss rightfully mentioned his own part in the rediscovery of Ceres after only 40 days of its observation [by Piazzi] and his new methods of calculating the parameters of planetary orbits. And, as noted above, he cited his epochal *Theory of Motion*.

Overall, § 103 leaves a still more unified and homogeneous impression than the mathematical section. Did it happen since here Gauss was able to extract more out of the general picture? In other words, was he able to be *entirely convinced in that* he really could *mention approvingly* fewer mathematicians? After the work of D'Alembert, Buffon, Lagrange, Laplace, Legendre, Clairaut, Monge, …, Newton, Bayes, Halley, Maclaurin, Stirling, Taylor, Waring[15], … here was no one else to mention; not many mathematicians of the first rank had been left if only he had no wish to praise himself[16].

It was different in astronomy. Gauss' own power here was in its theory, and he was able to acclaim wholeheartedly observers and discoverers. Again, in § 102 we find relatively many authors about whom hardly anyone speaks today which to some extent warrants Gauss' opinion about Euler (exactly at the bicentenary of his death[17]) and about the development of mathematics in Germany as well as it is justified to reprint this, in many respects meaningful and completely overlooked thoughts about the history of mathematics and astronomy [in Germany], from the standpoint of a Gauss in 1810.

The scientists mentioned by Gauss

Numbers 1 and 2 mean *cited in §§ 102 and 103 respectively*. The names of authors included in the DSB are italicized.

Barry, Roger, 1738 (?) − 1813, **2**
Bernoulli, Daniel, 1700 – 1782, **1**
Bernoulli, Jakob I, 1654 – 1705, **1**
Bernoulli, Johann I, 1667 – 1748, **1**
Bouguer, Pierre, 1698 – 1758, **1**
Bürg, Tobias, 1766 – 1834, **2**
Euler, *Leonhard*, 1707 – 1783, **1, 2**
Florencourt, Carl Chassot de, 1757 – 1790, **1**
Harding, Carl Ludwig, 1765 – 1834, **2**
Hausen Christian August, 1693 – 1743, **1**
Herschel, William (*Wilhelm*), 1738 – 1822, **2**
Hindenburg, Carl Friedrich, 1741 – 1808, **1**
Karsten, Wenceslaus Johann Gustav, 1732 – 1787, **1**
Kästner, Abraham Gotthelf, 1719 – 1800, **1**
Klügel, Georg Simon, 1739 – 1812, **1**
Kramp, Christian, 1760 – 1826, **2**
Lambert, Johann Heinrich (*Jean-Henri*), 1728 – 1777, **1**, **2**
Leibniz, Gottfried Wilhelm, 1646 – 1716, **1**
Mayer, Tobias, 1723 – 1762, **2**
Olbers, Wilhelm, 1758 – 1840, **2**
Pasquich, Johann, 1753 – 1829, **1**
Pfaff, Johann Friedrich, 1765 – 1825, **1**
Schröter, *Johann Hieronymus*, 1745 – 1816, **2**
Segner, Johann Andreas (*János-András*) von, 1704 – 1777, **1**
Süssmilch, Johann Peter, 1707 – 1767, **1**
Tetens, Johann Nikolaus, 1736 – 1807, **1**
Triesnecker, Franz von Paula, 1745 – 1817, **2**
Vega, Georg (Jurij), Freiherr von, 1754 (?) − 1802, **1**
Wolff, Christian, 1679 – 1754, **1**
Wurm, Johann Friedrich, 1760 – 1833, **2**
Zach, Franz Xaver von, 1754 – 1832, **2**

Notes

1. The most comprehensive bibliography of Gauss' works is still Poschek (1957), although its astronomical part should certainly be supplemented by Galle (1850). K.-R. B. See also Poggendorff, Bd. VIIa, Suppl., 1971, pp. 229 – 238. O. S.

2. I am thankful to my colleague, Doctor Horst Fiedler, for informing me about this work and the Gauss contribution. K.-R. B. The *other note* is not included in the translation. O. S.

3. It is quite wrong to consider the sensible and benevolent Jakob and his brother, the pathologically jealous Johann, on the same footing, see Wolf (1858). Savérien (1775), as Wolf (p. 144) noted, had stated:

Neither did the English, the German, the French, nor their authors understand at all the value of their discoveries. To Switzerland belongs the glory of producing two rare men, the brothers Bernoulli, who perceived the pertinent scope.

21 March 1694 Leibniz wrote Johann Bernoulli (Ibidem, p. 143): *Vestra enim non minus haec methodus, quam mea est* (It goes without saying that that method of yours is not worse than my own). O. S.

4. Gauss could have well mentioned Daniel Bernoulli's classical memoir of 1766 on the inoculation of smallpox. See also Mikhajlov (2005). O. S.

5. Lambert deserved much more attention. He published more than 70 contributions, and in 1965 – 2008 ten volumes of his *Philosophische Schriften* had been published in Hildesheim. His *Lettres cosmologiques* which appeared in 1761 had been praised even much later. Struve (1847), although criticizing Lambert on pp. 17 – 18, stated on p. 12 that his contribution was *remarkable for the clarity of exposition and penetrative views*. However, Daniel Bernoulli had stated, as one of his correspondents related in 1782, that Lambert's *Photometria was so obscure that he would have written it just as well as read it*, see Wolf (1860, p. 335). O. S.

6. Süssmilch was not a mathematician (and was not therefore included in the DSB, see below) and treated his statistical data clumsily, but Euler had successfully collaborated with him (Pfanzagl & Sheynin 1997). In the 18th century the calculus of probability began to be also applied to the treatment of observations (Simpson, Lambert, Daniel Bernoulli, Euler). O. S.

7. This statement should perhaps be qualified. When discussing the situation in the USA at the mid-19th century, Newcomb (1896, p. 108) stated that a *far greater* number of observatories had appeared *than, with the limited funds at disposal, can be kept in operation* and that it would have been much better *to concentrate on two or three large establishments*. O. S.

8. At that time Uranus and the minor planets had been treated alike, see Gauss' statement in the beginning of his § 103 about the *five main planets* and, also, Poisson (1837, § 110) who counted ten planets not including the Earth. Gauss called Herschel a German, see also below, but even so there were no *four* Germans. O. S.

9. Here, Gauss made a serious mistake. First, Herschel discovered two rather than six satellites of Uranus, and even now only five of them are known. Then, Herschel discovered Uranus itself (although thought that it was a comet). In addition, note that Herschel initiated stellar statistics.

10. It is certainly possible to take into account the circumstances of Leibniz' life and his mathematical estate, but only in a subjective sense. Objectively important is only what he had achieved. Indeed, Lambert would have accomplished much more had he not gradually poisoned himself by stupid and obstinate self-treatment, see Wolf (1860, pp. 352 – 353). O. S.

11. This statement is not documented and, anyway, it seems superfluous. O. S.

12. In the first place Gauss could have had in mind the following works: Wolff (1716; 1717), Segner (1758 – 1767 and 1761 – 1763), Kästner (1758; 1772 – 1801; 1796 – 1800), and Klügel (1770; 1778; 1803 – 1808). K.-R. B. Volumes 15/16, 17 and 18 of the *Dict. Scient. Biogr.* appeared after the publication of Biermann's paper. None of those scholars previously lacking in this source have been included. O. S.

13. See Gauss (1851). There also, on pp. 246 – 250 of his *Werke* is one of his reviews of Pasquich' tables of logarithms of trigonometric functions. K.-R. B. Biermann had not indicated this note in his main text and I myself placed it there. Many authors had exonerated Pasquich, see in addition H. C. Schumacher (*Astron. Nachr.*, Bd. 3, 1824) and Biermann himself (*Mitt. Gauss-Ges. Göttingen* No. 39, 1999). O. S.

14. Who exactly called then German? Lambert invariably considered himself a Swiss (Wolf 1860, p. 317). De Moivre, a Frenchman, and Herschel, a German (Jew), became Englishmen, and Biermann's criticism of Gauss is too mild. Then, all the three named representatives of the Bernoulli dynasty lived and worked outside Germany proper. And, if Herschel is a German, then why forget Goldbach, Jacob Hermann or Wolfgang Ludwig Krafft? O. S.

15. All scholars are listed in an alphabetical order but Buffon is the odd man out. De Moivre is left out, perhaps intentionally, cf. Note 12. O. S.

16. Yes, it was difficult for Gauss to praise someone, but this circumstance is only a partial justification. Kolmogorov (1935; 1938 and possibly later) had been able to write about himself and others.

Gauss' power was in theory (see below): this statement should be qualified. As far as persistent and prolonged observations are meant, Bessel left Gauss far behind. However (Subbotin 1956, p. 268)

Gauss and Bessel are the originators of a new trend in astrometry. After them, *everything is based on analysis of the instrument, on the fullest possible determination of its errors and on allowing for the influence these errors can have ...* And Krylov (1934/1951, p. 287) added: *Gauss introduced unprecedented precision into magnetic observations …*

Here are other testimonies. Schmeidler (1984, pp. 32 – 33) quoted Newcomb (1906, p. 343):

The fundamental idea of [the German school of practical astronomy] *was that the instrument is indicted* […] *for every possible fault and not exonerated till it has proved itself correct in every point. The methods of determining the possible errors of an instrument were developed by Bessel with ingenuity and precision of geometric method*.

Bessel, however, mentioned Gauss (B – G, 15 June 1818):

We are thankful to you for the most part of today's improvement of astronomy not only because of your least squares, but also because of wakening the feeling of fineness which seems to have disappeared since the time of Bradley and had only appeared 18 years ago. We only now came to the point of hunting down small errors or deviations lying beyond the boundaries of probability with the same attention as we previously did concerning the large ones.

At the very least Bessel recognized that it was Gauss who had initiated that German school of practical astronomy. A few remarks are in order. Did Bessel single out the year 1800 just for the sake of simplicity? Then, *boundaries of probability*: Encke (1834 – 1836/1888, pp. 43 – 47) used the short-lived term *boundaries of security* (Grenze der Sicherheit), i. e., *the probable bounds of the true value* (Gauss 1816, §§ 4 – 7). Again, Gerling, in a letter to Gauss of 19 Febr. 1838 and Bessel writing to Olbers on 28 June 1839 mentioned the same term without any comment. Concerning *true value* see Sheynin (2007).

The *improvement of astronomy* (and geodesy) also concerned the introduction of new methods of measurements in triangulation (Sheynin 1979, § 6). In those times the permanence of triangulation had not been ednsured, but at least Gauss included such structures as churches and bell towers in his measurements. However, no general reconnaissance was carried out in Hanover and the triangulation net in that kingdom became complicated and its precision did not reach the possible level, see for example Gaede (1885, p. 154). Gauss did not like this kind of geodetic work and it was difficult for him, but he was in charge of the whole enterprise, and he was to blame.

However, I am concluding on a pleasant note concerning *a trait peculiar to Gauss as an astronomer* (Subbotin 1956, p. 297):

The apparently striking underestimation and an almost complete oblivion [until the end of the 19th century] *of the works of Lagrange and Laplace* [on the determination of planetary and cometary orbits] *was caused by the fact that these authors restricted themselves by the purely mathematical aspect of the problem whereas Gauss thoroughly worked out his solution from the point of view of computations taking into account all the conditions of the work of astronomers and* [even] *their habits*. O. S.

17. Two hundred years after Euler´s death: the date of the publication of Biermann´s paper. Not exactly a proper way of expressing himself. O. S.

Bibliography

Allgemeine Deutsche Biographie, ADB (1882, 1887, 1898, 1894, 1894, 1895), Bde 15, 25, 34, 37, 38, 39 respectively. Leipzig

Allmer F. (1977), Von Göttingen nach Zagorica. *Mitt. Gauß-Ges. Göttingen*, No. 14, pp. 111 – 115.

Biermann K.-R. (1963), Aus der Vorgeschichte der Aufforderung A. von Humboldt von 1836 an den Präsidenten der Royal Society zur Errichtung geomagnetischer Stationen. *Wiss. Z. der Humboldt-Univ. Berlin*, math.-nat. Reihe

12, pp. 209 – 227.
---, Editor (1977), *Briefwechsel zwischen A. von Humboldt und C. F. Gauß.* Berlin.
Depman I. Ya. (1953, in Russian), Remarkable Slavic calculators, G. Vega and Ya. F. Kulik. *Istoriko-Matematicheskie Issledovania*, vol. 6, pp. 573 – 608.
Eichhorn J. C. (1805 – 1813), *Geschichte der Literatur von ihren Anfang bis auf die neuesten Zeiten*, Bde 1 – 6. Göttingen.
Encke J. F. (1834 – 1836), Über die Methode der kleinsten Quadrate. *Ges. math. und astron. Abh*., Bd. 2, pp. 1 – 200. Berlin, 1888.
Florencourt Carl Chassot de (1781), *Abhandlungen aus der juristischen und politischen Rechenkunst.* Altenburg.
Freudenthal H. (1966), Die eerste ontmoeting tussen de wiskunde en de wetenschappen. *Verh. Kgkl. Vlaamse Acad. Wettenschappen, Letteren en Schone Kunsten Belg.*, Kl. Wetenschappen, Jg. 28, No. 88.
Galle J. G. (1850), *Register zu von Zachs Monatl. Corr.* Gotha.
Gauß C. F. (1799), *Demonstratio nova theorematis omnem functionem algebraicam rationalem integram unius variabilis in factores reales primi vel secundi gradus resolvi posse*. *W*-3, c. 1 − 30.
--- (1801), *Disquisitiones arithmeticae.* Leipzig. *W*-1.
--- (1809, Latin), *Theory of the Motion* etc. Boston, 1857; New York, 1963.
--- (1813), Neue Aussicht zur Erweiterung des Gebiets der Himmelkunde. *Mitt. Gauß-Ges. Göttingen*, No. 14, 1977, pp. 26 – 29.
--- (1816), Bestimmung der Genauigkeit der Beobachtungen. In author’s *Abh. zur Methode der kleinsten Quadrate*. Hrsg, A. Börsch, P. Simon, 1887. Latest edition: Vaduz, 1998, pp. 129 – 138.
--- (1836), Erdmagnetismus und Magnetometer. *W*-5, pp. 315 – 344.
--- (1851), Einige Bemerkungen zu Vega’s Thesaurus Logarithmorum. *W*-3, pp. 257 – 264.
--- (Nachlass), [Plan und Anfang zum Werke über die trigonometrischen Messungen in Hannover]. *W*-9, pp. 401 – 405.
--- (1861, 1862), *Briefwechsel zwischen Gauß und Schumacher. W*/Erg-5, NNo. 3, 4. Hildesheim, 1975.
Gaede (1885), Beiträge zur Kenntnis von Gauss′ praktisch-geodätischen Arbeiten. *Z. f. Vermessungswesen*, Bd. 14, pp. 113 – 137, 145 – 157, 161 – 173, 177 – 207, 225 – 245.
Harnack A. (1900), *Geschichte der Kgl. Preußischen Akademie der Wissenschaften zu Berlin*, Bd. 1. Berlin.
Hausen Ch. A. (1734), *Elementa Matheseos.* Leipzig.
Humboldt A. von (1850), *Kosmos*, Bd. 3. Stuttgart.
Karsten W. J. G. (1767 – 1777), *Lehrbegriff der gesamten Mathematik*, Bde 1 – 8. Greifswald.
Kästner A. G. (1758), *Anfangsgründe der Arithmetik, Geometrie und sphärischen Trigonometrie und Perspektive*. Göttingen.
--- (1772 – 1801), *Mathematische Anfangsgründe*, Bde 1 – 5. Göttingen.
--- (1796 – 1800), *Geschichte der Mathematik seit der Wiederherstellung der Wissenschaften bis an das Ende des 18. Jahrhunderts*, Bde 1 – 4. Göttingen**.**
Klügel G. S. (1770), *Analytische Trigonometrie*. Braunschweig.
--- (1778), *Analytische Dioptrik*, Bde 1 – 2. Leipzig.
--- (1803 – 1808), *Mathematisches Wörterbuch*, Bde 1 – 3. Leipzig.
Kolmogorov A. N. (1935, in Russian), On some modern trends in the theory of probability. *Trudy Vtorogo Vsesoiuznogo Matematicheskogo S’ezda 1934* (Proc. Second All-Union Math. Conf. 1934), vol. 1. Leningrad – Moscow, pp. 349 – 358. **S, G,** 7.
--- (1938, in Russian), The theory of probability and ist applications. *Matematika i Estestvoznanie* v SSSR (Math. and Nat. Science in the Soviet Union). Moscow, pp. 51 – 61. **S, G,** 7.
Krylov A. N. (1934, in Russian), C. F. Gauss. *Sobranie Trudov* (Coll. Works), vol. 1/2. Moscow – Leningrad, 1951, pp. 279 – 297.
Küssner (1981), Der Gauß-Schüler Peter Paul Titel aus Ungarn (1784 – 1831), Direktor der Sternwarten Erlau und Ofen. *Mitt. Gauß-Ges. Göttingen*, No. 18, pp. 7 – 29.
Mikhajlov G. K. (2005), Daniel Bernoullis Leben und Werk. *Gelehrte aus Basel*

an der St. Petersburger Akad. der Wiss. des 18. Jahrhunderts. Vorträge des Symposiums … anläßlich der Feierlichkeiten 300 Jahre St. Petersburg. Aachen, pp. 77 – 87. **S, G,** 34.

Newcomb S. (1896), Abstract science in America, 1776 – 1876. *North Amer. Rev.*, vol. 122, pp. 88 – 123.

--- (1906), *Compendium of Spherical Astronomy*.

Pasquich J. (1790 – 1791), *Unterricht in der mathematischen Analysis und Maschinenlehre*, Bde 1 – 2. Leipzig.

--- (1799), *Elementa analyseos et geometriae sublimioris*, Bde 1 – 2. Leipzig.

Pfanzagl J., Sheynin O. (1997), Süssmilch. *Enc. of Stat. Sciences*, second edition. Editors S. Kotz & N. L. Johnson, vol. 13, pp. 8489 – 8491. Hobokan, NJ, 2006. The note somehow appeared anonymously.

Poggendorf J. C. *Biographisch-Literarisches Hdwb der exakten Wissenschaften*. A reference book in many volumes of biographies and bibliographies of scholars of various countries in mathematics and natural sciences . New volumes are still appearing.

Poisson S.-D. (1837), *Recherches sur la probabilité des jugements* etc. **S, G,** 53.

Poschek M. (1957), *C. F. Gauß. Eine Bio-Bibliographie*. Prüfungsarbeit der Bibliotheksschule Hamburg.

Salié H. (1957), Daten aus dem Leben und Wirken von C. F. Gauß. In *C. F. Gauß. Gedenkband*, pp. 15 – 36. Editor, H. Reichardt. Leipzig.

Sartorius von Waltershausen W. (1856), *Gauß zum Gedächtnis*. Wiesbaden, 1965. [Leipzig, 2012.]

Savérien A. (1775), *Histoire des progrès l'esprit humain dans les sciences naturelles et dans les arts qui en dépendent.* Paris.

Schmeidler F. (1984), *Leben und Werk der Königsberger Astronomen F. W. Bessel*. Kelkheim/T.

Segner J. A. (1758 – 1767), *Cursus mathematicus*, vols 1 – 5. Halle.

--- (1761 – 1763), *Elementa analyseos infinitorum*, vols 1 − 2. Halle.

Sheynin O. B. (1979), Gauss and the theory of errors. *Arch. Hist. Ex. Sci.*, vol. 20, pp. 21 – 72.

--- (2007), The true value of a measured constant and the theory of errors. *Hist. Scientiarum*, vol. 17, pp. 38 – 48.

--- (2013), *Teoria Veroiatnostei. Istoricheskii Ocherk* (Theory of Probability. A Hist. Essay). Berlin. **S, G,** 11.

Stargardt J. A. (1971), *Autographenkatalog*, No. 595. Marburg.

Struve F. G. W. (1847), *Etudes d'astronomie stellaire*. St.-Pétersbourg.

Subbotin M. F. (1956, in Russian), Gauss' astronomical and geodetic work. In memorial volume *C. F. Gauss*. Moscow, pp. 243 – 310.

Süssmilch J. P. (1741), *Die Göttliche Ordnung* etc. Berlin.

Tetens J. N. (1785 – 1786), *Einleitung zur Berechnung der Leibrenten und Anwartschaften*, Bde 1 – 2. Leipzig.

Vega G. (1783), *Logarithmischen, trigonometrischen und anderen zum Gebrauche der Mathematik eingerichteten Tafeln und Formeln*. Wien.

--- (1786 – 1802), *Vorlesungen über die Mathematik*, Bde 1 – 4. Wien.

--- (1793), *Logarithmisch-trigonometrisches Handbuch*. Leipzig.

--- (1794), *Thesaurus logarithmorum completus*. Leipzig.

Wolf R. (1858), Jacob Bernoulli von Basel. *Biographien zur Kulturgeschichte des Schweiz*, 1. Cyclus. Zürich, pp. 133 – 166. **S, G,** 43.

--- (1860), Joh. Heinrich Lambert von Mühlhausen. Ibidem, 3. Cyclus. Zürich, pp. 317 – 356. **S, G,** 34.

Wolff Chr. (1716), *Mathematisches Lexicon*. Leipzig.

--- (1717), *Auszug aus den Anfangsgründen aller mathematischen Wissenschaften*. Halle.

II

C. F. Gauß

Determination of the latitudinal difference between the observatories in Göttingen and Altona by observations with a Ramsden zenith sector

Bestimmung des Breitenunterschiedes zwischen den Sternwarten von Göttingen und Altona durch Beobachtungen am Ramsden'schen Zenithsector (1828).
Abh. zur Methode der kleinsten Quadrate.
Hrsg. A. Borsch, P. Simon (1887). Vaduz, 1998, pp. 152 – 189.
Also in Gauss, *Werke*, Bd. 9, pp. 5 – 64

0. Introduction

In 1821 – 1824 I had connected the observatories in Göttingen and Altona[1] by a chain of triangles most precisely measured in the Kingdom of Hannover along the meridian of Göttingen. These measurements will be made public in detail; here, I only remark that the sizes of the sides of the triangles rest on the base measured by Prof. Schumacher in Holstein with utmost precision and connected with the triangulation through its side Hamburg – Hohenhorn. The triangulation is oriented by the measurements with a passage instrument belonging to the Göttingen observatory since it and its northern meridian mire are triangulation stations.

Owing to a spectacular whim of fate the observatories in Göttingen and Altona are situated on the same meridian to less than the width of a house. Although the absolute polar altitudes are determined by fixed meridian instruments, it was still important to establish the latitudinal difference [between the observatories] by the same instruments but otherwise. I was indeed happy to be able to use the excellent Ramsden zenith sector which is known to have been applied in the English arc measurement for a similar purpose. The main aim of this paper is the [description of] my observations which I had carried out in the spring of 1827 and their results.

When observing by that instrument many stars in succession, it is not good to be without a proficient assistant, and Prof. Schumacher was kind enough to engage, by permission of His Majesty the King of Denmark, engineer-lieutenant von Nehus for observing in both places. This very skilful observer incessantly took care of reading the micrometer and of adjusting the Lotfaden whereas I observed the passage [of the stars] across the meridian hair and set the hair, perpendicular to the meridian, on the star. Only during the first two nights at Altona I had another assistant, but the pertinent observations were discarded since experience showed that different people differently estimate the pointing of the hair on an object.

The instrument is sufficiently well described by Mudge[2]. In Göttingen, but not in Altona, it was possible to install it in the observatory itself unter dem östlichen Meridianspalt (meridian mire?). In Altona it was installed in Prof. Schumacher's garden in which the local observatory is situated, under the same observational tent which Mudge had used for the same purpose. The firmness of the installation on framed posts left nothing better to desire. The levelling of the vertical and horizontal axes was checked daily and usually almost nothing had to be changed.

In Göttingen, the southern meridian mire was used to bring the plane of the [vertical] limb to the meridian. Although the mire was situated on the meridian of des westlichen Spalten, its azimuth at the place of the sector could have been calculated very precisely. In Altona, a similar method proved impossible. At first, knowing the absolute time, the limb was brought there very near to the meridian by means of a culminating star. Observation of many stars passing across the entire limb easily provided the yet necessary small correction, since, as noted above, each night the passing of the culminating stars across the meridian hairs of the sector was also observed and the right ascensions of those stars were known. Invariably, the correct position in the meridian was therefore securely checked. Only once an unimportant correction became needed. As a rule, from night to night the position of the limb, western or eastern, was changed. However, during a night and coming to the end of the observations, this rule was sometimes abandoned and the position of the limb changed once or more so that the numbers of those positions became about the same.

The barometer and the interior [in the tent] and exterior thermometers were read at least thrice during each night, at the beginning, in the middle, and at the end of the observations. Following the example of Mudge, the difference between the temperatures above and below the sector was also registered since the limb and the [metallic] radius change differently with the temperature. It is unnecessary to note especially that each stipulated caution concerning the instrument had been carefully complied with. For example the water vessel for the plumb line to hang in was properly kept full; whenever possible, the screw of the micrometer was read at its same thread[3] etc.

When observing each star the vertical hair was set at the upper graduation (at the centre of the graduated arc) independently from the previous observation, and, as a rule, the setting on the next graduation (or on both neighbouring graduations) was repeated many times over. The mean of the unequal readings of the micrometer screw, most of them coinciding to a few tenth of a second, was chosen.

1. The Observed Stars

At first, I had chosen for observation 38 properly situated stars. However, towards the end of my work at Göttingen I have added five more, since I took care that in case of unfavourable weather the observations at Altona can be dragged out since it will be impossible to observe often enough a considerable part of the 38 stars at culminations during daytime. This worry was only justified to a small extent: in Altona, only one single star was observed on one position of the limb.

I am providing here the mean places of these stars reduced to the beginning of 1827. Their declinations are the results obtained by observation with the zenith sector itself. For the present goal, a most precise determination of right ascensions was not important, and most of them were derived from a single observation with a meridian circle. Von Heiligenstein was kind enough to perform those reductions.

For the sake of convenience I designate the stars by consecutive numbers; numbers 8, 13, 15 and 31 are double stars. No. 8 is the second component of the star, NNo. 13 and 15 are the means of both components, and the second component of star No. 31 is so small as to remain invisible even with an experimentally darkened field of vision. However, Prof. Schumacher found that component at once when using a Reichenbach meridian circle with a telescope of a greater optical efficiency. At that time, we did not know that other astronomers had also recognized these double stars as such.

[The table mentioned above is not reproduced. It shows the declination and the right ascension of 43 stars for the epoch 1827 (for the beginning of that year, as stated in the main text). 16 stars out of those 43 are somehow left unnamed.]

2. The Observations

A full reprint of the journal of observations in its initial form would have more than twice lengthened this paper. I thought this superfluous and therefore provide the observations arranged according to the stars. The first column shows the observed zenith distances, i. e., just the reduced readings. Northern distances are considered positive, southern distances, negative.

The second column provides the refraction together with the action of the unequal expansion of the instrument caused by the inequalities of the temperatures above and below the instrument. The maximal differences were 1.°2 Réaum. (the temperature above was higher) and – 0.°6. For becoming capable of judging the agreement of the observations, I have spared no effort to calculate the correction of each separate one and I used some easily presenting themselves expedient calculating tricks.

The third column contains reductions to the mean places for the beginning of the year for aberration, nutation and precession, and, for some stars, for proper motion. The yearly motion in declinations was taken as −0.”42, 0.”33 and 0.”38 for stars 10, 25 and 37 respectively. The proper motion of the two first stars was established long ago, and for star 37 it is shown by comparing its place with the determination by Piazzi. If the latter is supposed to be correct, the value stated above cannot be questioned[4].

Calculation of the aberration, nutation and precession was based on Baily’s valuable tables. For each star an ephemeris with an interval of 10 days was calculated with the help of von Nehus and Petersen. Each of those was interpolated with an allowance of the second differences. Finally, the fourth column contains sums of the three first ones; that is, the true zenith distances of each observation for the mean place of the star at the beginning of 1827 only still corrupted by the collimation error.

3. Results

3.1. The simplest combination of the observations for obtaining the latitudinal difference between the places of observation consists in considering each star by itself[5]. Suppose, for example, that by the eastern (western) position of the limb[6] the observed zenith distances in Göttingen are a and a' and, in Altona, b and b'. Then the difference sought will be

$$\frac{1}{2}[(a+a')-(b+b')]. \qquad (1)$$

We will obtain as many results as there are fully observed stars. In our case, 42 results since in Altona star No. 5 was only observed in one position of the limb and therefore falls out.

For equally numerous observations which lead to the values a, a', b and b', the separate results ought to be supposed equally reliable so that the most probable latitudinal difference will be the simple arithmetic mean[7]. In our case this presumption does not hold, and unequal weights should be ascribed to the separate results.

If it is allowed to consider the errors of the separate observations independent from each other, then, assigning a unit weight to each of them, according to known reasoning, the weight of the result (1) will be

$$4 \div [1/\alpha + 1/\alpha' + 1/\beta + 1/\beta'] \qquad (2)$$

where α, α', β, β' are the numbers of observation which led to a, a', b and b'. Here are the 42 results with their weights. [The provided table is not reproduced.] The mean latitudinal difference calculated by taking into account the unequal weights is

2°0′56″.52, weight 213.41[8].

3.2. Denote n different determinations of a magnitude by A, A', A'', …, their weights by p, p', p'', … the mean calculated by taking into account these weights, by A^*, and by M the sum

$$p(A-A^*)^2 + p'(A'-A^*)^2 + p''(A''-A^*)^2 + \ldots$$

Then, according to the general theorem (Gauss 1823, § 38),

$$\sqrt{M/(n-1)}$$

will be the approximate value of the mean square [mittlere] error of an observation with unit weight. In our case, $M = 103.4126$ and the mean square error

$$\sqrt{103.41/41} = 1.''5882.$$

We will obtain the mean square error to be feared[9] by dividing this value by the square root of its weight which leads to the value 0.″1087 $[=1.5882/\sqrt{213.41}]$.

3.3. The collimation error of the instrument, as derived from observations of each star in Göttingen is 1/2($a' - a$) with weight $4\alpha\alpha'/(\alpha + \alpha')$, and in Altona, 1/2($b' - b$) with weight $4\beta\beta'/(\beta + \beta')$, see Table [not reproduced. It contains 43 values of the collimation error with their weights for Göttingen and 42 for Altona]. The mean values of that error are

in Göttingen 3″.75 with weight 455.17

in Altona 1″.40 with weight 432.18

The reality of the change of the collimation error is obvious and no doubt can be expressed in that, in spite of all possible precautions taken, it occurred during the transportation [of the instrument] from the first place to the second.

3.4. The results obtained for the latitudinal difference can completely reassure us but it is not useless to note, at least when keeping to theoretical considerations, that the combination of the observations in § 3.1 was not yet the best possible since not in each place each star was observed an equal number of times at each position of the sector. Actually, when determining the true zenith distance in Göttingen by the formula 1/2($a' + a$) it had weight $4\alpha\alpha'/(\alpha + \alpha')$. However, had the collimation error in Göttingen f be known, the true zenith distance and its weight would have been

$$\frac{\alpha(a+f)+\alpha'(a'-f)}{\alpha+\alpha'},\ \alpha+\alpha' = \frac{4\alpha\alpha'}{\alpha+\alpha'} + \frac{(\alpha-\alpha')^2}{\alpha+\alpha'}.$$

If $\alpha \neq \alpha'$, the weight will be greater than previously, and the same holds for Altona. Thus, even one-sided observations (like those of star No. 5) will contribute, although minutely, to the heightening of the precision.

The collimation error in either place is not known *absolutely* precisely, but it is easy to become convinced in that the [corrected] weight determined for the same mean result will be quite insignificantly lower.

3.5. If nevertheless it is desired to obtain a result completely satisfying the requirements of the rigorous theory, the determination of the latitudinal difference, the collimation error and the true zenith distance for each star in each place should be considered as a problem in which these unknown magnitudes (in our case, 46 of them) are determined by all the observed magnitudes (171), i. e., from the same number of equations, which should be combined according to the rules of the calculus of probability[10].

Denote the collimation error in Göttingen and Altona by f and g, the latitudinal difference by h, the true zenith distance of a star in Göttingen by k. The observations of this star provide four equations with weights α, α', β, β'. Then

$$a = k - f,\ a' = k + f,\ b = k - g - h,\ b' = k + g - h.$$

It is hardly needed to recall that it is more advantageous for calculation to replace these unknowns by the still required corrections to their values f^0, g^0, h^0 and k^0 determined with a very good approximation. We may then assume that

$$k^0 = \frac{\alpha(a + f^0) + \alpha'(a' - f^0) + \beta(b + g^0 + h^0) + \beta'(b' - g^0 - h^0)}{\alpha + \alpha' + \beta + \beta'}.$$

This rule, when applying the method of least squares to cases somehow taken together, should never remain out of sight. And the application of a proper *indirect* method of solution[11] is a very easy task whereas otherwise (ohne jede) a direct elimination [of the unknowns] will be unbearably protracted.

3.6. The success of this calculation whose detailed explanation is not necessary consists in that the previous determinations [see §§ 3.1 and 3.3] do not acquire considerable corrections. The improvement of the latitudinal difference is – 0".014, of the collimation error in Göttingen and Altona, 0".012 and – 0".014. So these are the new determinations:

Latitudinal difference 2°0′56".51, weight 217.67
Collimation error in Göttingen 3.76, weight 457.03
Collimation error in Altona 1.39, weight 437.64

According to the rule of § 3.5, almost all the changes of the true zenith distances of the stars in Göttingen are less than 0".01. It is not necessary to provide them here since they are the same from which the declinations of the stars given above have been derived when assuming that the polar altitudes at the place of observations was 51°31′47".92.

On the contrary, we indicate the differences [the residual free terms] remaining after the substitution of the derived values in the 171 equations. [A Table of those four differences for each of the 43 stars is provided. For star No. 1, for example, they were 0."07, 0."09, − 0."26 and 0."13. Above, only three magnitudes were mentioned, and four in § 3.5, the fourth being the true zenith distance.]

3.7. The sum of the squares of these 171 differences for the corresponding number of observations is 292.8249. According to the theorem mentioned above (Gauss 1823, § 38), the approximate value of the mean square error of a simple observation is the square root of the fraction whose numerator is that sum, and the denominator, the excess of the number of the compared observations over the number of the unknowns derived by the method of least squares[12]. (In our case, 171 – 46 = 125.) The mean square error will be 1."5308, only a little differing from the value derived in § 3.2. The mean square error to be feared of the end result for the latitudinal difference is therefore

$$1.''5308/\sqrt{217.67} = 0.''1038.$$

3.8. The calculations above presume that all the errors of the different observation can be considered completely independent from each other or purely random[13]. This premise is obviously not quite right; according to the nature of the instrument, all the α observations, which competed against one another for determining the value, *a* depend on the same graduation. Therefore, in addition to purely random errors of observation the error of that graduation is also involved. The same is true about a', b and b'. The errors of the graduations are unknown. In relation to the separate 171 results of observation they can also be considered purely random and independent from each other. Indeed, the cases in which different observations depend on the same graduation can be ignored owing to their small number. The consideration of this circumstance [of the existence of those errors] necessitates a modification of the calculations above, although practically speaking the result will not change at all.

Denote by m the proper mean square error of an observation appearing only from random causes and excluding the mean square error of the graduations, μ. The total mean square error of an observation is then $\sqrt{m^2+\mu^2}$. The same error of the mean of α observations, all of them depending on the same graduation, is $\sqrt{m^2/\alpha+\mu^2} = m\sqrt{1/\alpha+\theta}$ if $\mu^2 = m^2\theta$.

Let the weight of an observation *without allowing for the errors of graduations* be unity. Then the weights of a, a', b and b' will be

$$\frac{\alpha}{1+\alpha\theta},\ \frac{\alpha'}{1+\alpha'\theta},\ \frac{\beta}{1+\beta\theta},\ \frac{\beta'}{1+\beta'\theta}. \qquad (3)$$

The first method of combining observations provides the weight of the latitudinal difference derived from the observation of a star equal to

$$\frac{p}{1+p\theta}$$

where p denotes the expression (2) and the mean of the 42 determinations ought to be taken.

On the contrary, according to the second method of combining observations a weight determined by one of the fractions (3) should be assigned to each of the 171 equations. A change in the end result and its mean square error to be feared can obviously only occur if the new weights are not proportional to the previous weights. In the first method only the results of numerous observations were somewhat preferred, but after taking into account the error of the graduation the weights will become less discordant, and the less the larger is the assumed error of the graduations. When observing with an instrument in which this error considerably exceeds the error of the observation proper, we may only be satisfied by considering every determination equally reliable[14].

3.9. The indicated methods have no [inherent] difficulties if only the coefficient θ is known. Its approximate knowledge can be achieved by an indirect method about which we only provide a notion. First of all, we note that the observations themselves ensure a means for determining very reliably their proper mean square error m.

Actually, this m is independent from the error of the graduations in the differences of the *separate* values of which each a (or a', b or b') is the mean value von einander oder von diesem Mittel, bemerkbar and when α is very large, the sum of the squares of these differences of the separate values of a from the mean ought to be assumed as an approximate determination of $(\alpha - 1)m^2$.

In our case, if $\alpha \le 7$, one such determination can essentially differ from the proper value, but the sum of all the 171 partial sums (for all a, a', b, b' and for all the stars) ought to differ very little from

$$m^2[\Sigma(\alpha - 1) + \Sigma(\alpha' - 1) + \Sigma(\beta - 1) + \Sigma(\beta' - 1)] = 728\, m^2.$$

The sum of the 171 partial sums is 844.50 and a very reliable value of m is 1."0770 which is considerably less than in §§ 3.2 or 3.7[15]. It completely confirms the influence of the errors of the graduations so that the previously provided numbers cannot lead to any truthful results.

3.10. Without directly knowing the mean square error of the graduations the value of θ can be indirectly derived in such a way that by applying the first method of combining observations (§ 3.2) or the second method (§ 3.7), the mean square error of an observation whose weight is taken as unity, will again be equal to the derived value of m.

However, it does not really seem that such studies should be repeated until a complete coincidence is arrived at. On the contrary, it rather seems adequate, after other considerations, to show that the latter value of θ can only little diverges from 0.2[16] and to assign that very value when applying the first method of combining observations. Then we obtain

The latitudinal difference 2°0′56."50 with weight 104.29
The mean square error of an observation of unit weight 1."131

and therefore the mean square error to be feared in the latitudinal difference above, 0."1108 $[= 1''.131/\sqrt{104.29}]$.

The application of the second method of combining observations with the same value of θ will presumably provide a still better coincidence with the value of m above. The final latitudinal difference will perhaps decrease by 0."01 with its weight certainly increasing somewhat less (?). However, it is therefore senseless to repeat the calculations. We can assume the latitudinal difference as stated above with its errors being probably within ± 0."07[17].

3.11. When we keep to the value of θ given above, the mean square error of the graduation will be $m\sqrt{\theta}$ = [m = 1."077, see § 3.9] = 0."48 and the so-called probable error of a graduation can be stated as 0."32[18]. This value obviously only takes into account the irregular errors or the deviations of separate graduations from an imaginary and as precise as possible uniform distribution of the graduations whose *absolute* accuracy we are unable to question. Or, in other words, strictly speaking, the result obtained for the latitudinal difference and its precision only takes into account graduations of [an imagined] mean sector and remains dependent on their absolute accuracy.

The instrument does not offer the astronomer any independent means at all for checking this. If the astronomer believes (erwägt) that the manufacturer had set the end graduation with utmost care[19] and that only a small part of the whole arc is considered, then he ought to admit that the ensuing uncertainty of the derived latitudinal difference can only be very little increased.

The differences between the zenith distances of the 43 stars observed by me with a Reichenbach meridian circle and by the sector, and ordered according to their declinations show no trace of regularity. This, incidentally, is a check of the absolute accuracy of the graduation.

3.12. The centre of the sector at Göttingen was situated 1060 toises northward and 7595 toises eastward from the centre of the axis of the Reichenbach meridian circle. In Altona, on the contrary, those distances from the meridian circle there were 13,511 southward and 2578 westward. Reductions of the latitudinal difference from the sector to the meridian circle were 0.”07 and 0.”85 respectively. Therefore, the latitudinal difference between the Reichenbach meridian circles was

$$2°0′57.”42.$$

3.13. The derivation of the declinations of the stars by their observed zenith distances was based on the absolute polar altitude which was obtained by 89 observations in both culminations of northern stars with a Reichenbach meridian circle, both direct and reflected from a water surface. The observations of 1824 which make up the main part of them, are not yet known, and I am therefore putting together all the observations and only remark that mostly the stars had been observed when passing through the second, the fourth (the middlemost) and the sixth hair when observing directly, and the first, third, fifth and seventh hair when observing reflected images. Only the mean zenith distances reduced to the moments of culmination are here given. They were only corrected for refraction by means of the Bessel tables. The collimation error and the effects of the bending of the telescope are still included.

[There follows a Table of the zenith distances of northern stars observed on 13 May 1820 and April and May of 1824. Both the direct and reflected observations of both culminations (U. = unter, lower, O. = ober, upper), the location of the circle (western or eastern) and the number of observations are provided. The stars are not identified. Separately given are the changes in the declination of the northern stars as calculated by the Bessel tables.]

3.14. The bending of the telescope (or the change of the location of the optical axis projected on the plane of the circle with respect to its graduation) occurs owing to the effect of gravity on the interconnected components of the instrument[20]. Denote this bending when the optical axis is horizontal and vertical by f and g and assume beforehand that this bending is proportional to gravity. The entire effect is extremely small so that this premise seems quite safe.

Then, when the optical axis has inclination z, the expression of the bending will be *f*sinz + *g*cos*z*. Denote also the collimation error by *e* and the registered zenith distance [obviously] by *z*. The true zenith distance will then be

$$z - e + f\sin(z - e) + g\cos(z - e).$$

For an absolutely symmetric telescope $g = 0$. However, no human technical work is absolutely perfect; moreover, a perfect symmetry will to some extent be disturbed by the weight of the balancer. It is therefore quite possible to add one or a few tenths of a second to the value (betragenden Werten) of *g*. And when exactly calculating to tenths or even hundredths of a second, it will be inconsistent not to consider, if possible, the second part of the bending [depending on *g*].

3.15. The complement to 90° of half the difference between the zenith distance measured directly and in reflection provides this distance cleared from the collimation error and the first part of the bending. It only includes the second part of the bending, although with opposite signs depending on the eastern or western location of the circle.

This zenith distance is obviously relative to the vertical at the place in which the optical axis meets the water vessel and, imperceptibly differing for both culminations of northern stars, is 0.”05 northward from the axis of the circle. For our goal this combination is appropriate in so far as we do not keep to the premise of the collimation error remaining constant during the whole time of observation in 1824. Suppose that the number of observations, direct and reflected, are α and β. Then the weight of such a determination is [see formula (2)] $4\alpha\beta/(\alpha + \beta)$ insofar as the errors of observation are regarded as purely random and independent one from another.

3.16. Denote the polar altitude at the point of the water vessel by φ, the declination of a northern star at its lower culmination on 13 May 1820 by δ_1 and at its upper culmination on 20 April 1824 by δ_2. Then observations will provide [14 linear equations and their weights are written out. They connect δ_1 (or δ_2), φ and *g* with the observations. For the four unknowns δ_1, δ_2, φ and *g* six equations are derived and their weights provided.]

By the method of least squares[21] we arrive at the following values:

$$\delta_1 = 88°20'50.''33,\ \delta_2 = 88°22'18.''28,\ \varphi = 51°31'47.''90,\ g = 0.17$$

The weight of the determination of φ is 60.8. To obtain a criterion of sorts for the precision of the observations we substitute these values in the 14 equations from which the above six were derived. Then the following residual errors are[22] [14 *errors* are provided with the weights of the corresponding equations]. The sum of their squares with their weights taken into account is 9.6184 so that the approximate value of the mean square error of an observation is

$$\sqrt{9.6184/10} = 0.''981\ [10 = 14 - 4].$$

The mean square error to be feared of the final polar altitude, as far as it depends on irregularly acting causes, is therefore

$$0.981/\sqrt{60.8} = 0.''126.$$

However, the uncertainty of this result ought to be greater since the premise of the independence of all the observational errors taken without any order is not quite right. The observations of a certain culmination and such observations made during many days depend on almost the same reading. When using a vernier, almost always other graduations are appearing. When applying our method their irregular errors are included in the mean square error of an observation, in 0.”981, and it is natural that in different parts of the limb certain unequal average errors must predominate. Anyhow, these are very small.

In 1826 I had checked with extreme care 30 graduations 12° apart with four excellent Repsold microscopes. Each was included almost 200 times in variable combinations. The mean of the errors of two diametrically opposite graduations, A and A + 180°, so far as some regularity still ought to be determined, is expressed as nearly as possible by the formula

$$-1.''23\cos(2A - 28°28') - 0.''22\cos(4A - 47°56') \qquad (4)$$

and the residual error appears to be irregular. The square root of the mean of their squares is 0.”32. At first I thought of extending my study and additionally checking 30 other graduations, but the produced result was insignificant and it seemed that an extension will not deserve the time needed.

It is not necessary to recall that, when all the four verniers are read[23], as it always happened in my study, the first part of the regular error (4) is all by itself done away with. However, a real improvement occurs when the graduations are only read on diametrically opposite places, as I always do at present since considerably increasing the precision of reading the graduations by applying two Repsold microscopes instead of the verniers.

3.17. When preferring to suppose that $g = 0$, the polar altitude will decrease by 0.”07 and the weight of its determination will be 84.1. Observations made in another place seem however to confirm the sign and very nearly the magnitude of g provided above [in § 3.16]. However, this is not sufficient for deciding such a delicate matter.

The coefficients f cannot be determined by the existing observations without presuming that the collimation error did not change during the observations of 1824. If this premise is allowed, we will obtain 28 equations [§ 3.13] whose proper solution provides

$$\varphi = 51°31'47.''89 \text{ with weight } 60.9,\ f = 0.76,\ g = 0.23.$$

At present, the collimation error can be determined[24] each hour with a surprising precision and without turning the telescope by pointing it on the nadir, and therefore I reserve the right to further studies.

3.18. Future investigations can provide other corrections no doubt scarcely reaching half a second. Bearing this in mind, I set the polar altitude

In Göttingen

at the place of the water vessel when observing northern stars

51°31′47.”90

at the place of the Reichenbach meridian circle 51°31′47.”85

at the place of the zenith sector 51°31′47.”92

The last-mentioned value was applied for reducing the declinations of the stars situated near the zenith

In Altona

at the place of the zenith sector 53°32′44.”42

at the place of the meridian circle 53°32′45.”27

3.19. According to the trigonometrical connection of the observatories in Göttingen and Altona the latter is situated

115,163.725 toises northward and 7.211 toises westward from the former.

These are the distances between the places of the meridian circles, and they depend on the length of the Hamburg – Hohenhorn side of the triangulation, 13,841.815 toises which in turn depends on the base measured in 1820 by Prof. Schumacher in Holstein [this was already mentioned in the Introduction]. However, the comparison of his measuring bar with the Normal toise is not yet *definitively* completed and in the future the distances stated above should be changed in the same ratio as the base itself, but this change can only be very small.

The mean latitudinal degree between the two observatories is accordingly (?) 57,127.2 toises, noticeably larger than it should have been expected because of the mean values of the degree measured in France and England.

3.20. The arc measurement in Hannover has thus once more confirmed the now doubtless truth: the form of the surface of the Earth is not quite regular. The anomalies in parts of the English and the French arc measurements already proved that irregularity, and the anomalous polar altitudes in many places in Italy provided a still more powerful proof. In the Hannover arc measurement there are anomalies between Göttingen and Altona and considerably greater irregularities at Brocken, a triangulation station situated in between.

Suppose that my triangles are situated on the surface of an elliptical spheroid whose dimensions have been derived by Walbeck by considering all the previous arc measurements. According to our best present knowledge, it most perfectly describes the real form [of the Earth] (flattening 1/302.78, and a 360^{th} part of a meridian, 57,009.758 toises) so that the polar altitude at Göttingen is 51°31′47.”85, and the latitudes of Brocken and Altona are

51°48′1.”85 and 53°32′50.”79.

Astronomical observations in Altona provided a 5.”52 *smaller* polar altitude, whereas von Zach[25] obtained a 10 – 11” *larger* altitude at Brocken. In any case, only a small part of the latter’s deviation can be attributed to the instrument, or to the declination applied in the calculation. The comparison of the latitudinal difference between Altona and Brocken with the curvature[26], which corresponds to the spheroid best of all agreeing as a whole with the Earth, will result in a deviation of 16”.

Invariably only taking into account the local deflections of the vertical and as though believing them to be separate exceptions, means that the matter is, in our opinion, considered from a wrong standpoint. What we in the geometric sense call the surface of the Earth is none other than the surface which everywhere vertically intersects the direction of gravity and the surface of the oceans is its part. The direction of gravity in each point is, however, determined by the form of the solid part of the Earth and its unequal density and on the outer crust of the Earth being the only layer about which we know something. These form and density are extremely irregular; the latter's irregularity can easily extend considerably below the outer crust and completely invalidate our calculations for which we have hardly any data.

The geometric surface is the result of the combined action of these unequally distributed elements. Instead of being disturbed by the existing unambiguous proof of the irregularity, we should likely rather wonder that it is not still greater. Had the astronomical observations been capable of a ten or of a hundred times higher precision than they are nowadays, we would have undoubtedly established irregularities everywhere.

At present, nothing prevents us from considering the Earth as a whole as a spheroid of revolution from which the real (the geometric) surface everywhere deviates in heavy or light, in long or short undulations. Had it been possible so to say to wrap up the whole Earth in a single trigonometric net, and to calculate the mutual location of all its stations, the ideal ellipsoid of revolution will be that, for which the direction of the vertical as best as possible coincides with astronomical observations[27].

Nowadays we invariably remain far removed from this unattainable ideal, but future centuries will undoubtedly advance very considerably the mathematical knowledge of the figure of the Earth. The propagation of arc measurements has actually only begun so that only separate results are obtained from a small number of points situated along isolated lines. How much more fertile will become the yield when future trigonometric operations in various countries with choice aids [instruments] are interconnected and rounded up in a *single* large system.

It is perhaps not fantastic to prophesy that sometime all the observatories in Europe will become trigonometrically interconnected. Even now such connections exist from Scotland to the Adriatic Sea and from Formentera to Fühnen[28] although they are only partly made generally known. Let this circumstance be heeded more than it was until now, let not valuable materials which ought to belong to the scientific world escape its attention. They must not be exposed to the danger of destruction!

Addition to § 3.20[29]

Only two last sheets of Walbeck's determination of the dimensions of the terrestrial spheroid were published (1819). He treated the arc measurements in Peru, both measurements in East Indies, the French, English and the new Lapland arc measurements. As far as I know, his work is the only one which was carried out according to correct principles without any arbitrariness.

He had only considered each arc measurement in its entirety, or only the polar altitudes observed at its end points without taking into account many existing intermediate points, and in calculations he kept to the first power of the flattening. I have therefore recently persuaded Dr. E. Schmidt, already favourably known by his work, to calculate those arc measurements anew. He has indeed completed this calculation while the last sheet of this writing was printed. He considered higher powers of the flattening, took into account all the intermediate points [of the arcs] at which the polar altitude had been measured and included the Hannover arc measurement.

According to the principle stated above, he determined the ellipsoid for which the astronomically observed polar altitudes coincide with the geodetic measurements when inserting least possible required corrections, i. e., such an ellipsoid for which the sum of their squares is minimal. Here is his result:

flattening 1/298.39, a 360th part of the meridian 57,010.35 toises

The observed polar altitudes at 25 stations of the seven arc measurements and their minimal changes (see above) are [a Table follows. The sum of the changes over each arc measurement is zero.]

The numbers in the last column cannot at all be considered as the errors of the astronomical observations. They are the algebraic sums of these errors and of the irregularities in the directions of the vertical. When treating these overall deviations according to the same rules as those that exist for random errors, we find that the mean square deviation is 3."18 and the mean square error to be feared

in the denominator of the flattening 12.5 units

in the value of the 360th part of the meridian 5.0 toises

The so-called probable error can also be estimated as 8 units and 3 toises respectively for the mean latitudinal degree (bei dem mittleren Breitengrad)**[30]**.

The ascertainment of our knowledge about the precision which we are justified to ascribe to the dimensions of the terrestrial ellipsoid by drawing on all the previous arc measurements should be seen as an important result of Dr Schmidt, the author of most worthy contributions.

4. Determination of the Latitude of the Seeberg Observatory

Simultaneously with my observations in Göttingen and Altona, Hansen, the Director of the Seeberg observatory near Gotha, observed at my request the same stars with his 2 ft Ertel meridian circle. The resulting latitudinal difference between the observatories there and in Göttingen acquired additional importance since the former is connected with the Hannover system of triangles by a triangle measured under the guidance of Lieutenant-General von Müffling.

During the observations the circle had been few times [daily] turned but the collimation error had been independently determined daily, and mostly twice daily by setting the telescope on the nadir. In the autumn of 1826 Hansen had practically acquainted himself with that method.

Readings were made by microscopes rather than by verniers. The following table [not reproduced] shows the main results of those observations. The stars are numbered in the first column; the second shows the position of the circle [eastern or western], then follow the number of observations, the zenith distances which I reduced to the beginning of 1827 (for northern stars they are considered positive), and the latitude derived by the declinations indicated in § 1.

The reliability of these 60 values of the latitude certainly differs. For assigning them weights without any arbitrariness the ratio of the mean square error of the observations proper to that of graduations should be known. Suppose that this ratio is $1/\sqrt{\theta}$. Disregarding the small uncertainty still attached to the declinations, we have[31]

$$\frac{n}{1+n\theta} \qquad (5)$$

as the weight of one of the n observations depending on the same graduation.

Assuming that the weights are simply n, we get

50°56′5.”16

as the mean of the 206 observations.

Now, the observations allow us to recognize that the errors of graduation must be considerably larger than for the Ramsden zenith sector, whereas the errors of observation proper can rather be somewhat smaller. A determination based on a larger number of observations is given too much preference as compared with those only depending on one or two.

Once we want to take into account the influence of the errors of graduation, we must bear in mind that each determination of the collimation error involves a constant component which depends on the error of the appropriate graduation. It is clear, however, that when the circle is alternatively turned in both directions that component influences the polar altitude in opposite senses. We therefore ought to separate the observations made with differing locations of the circle, calculate the means for each of the two appearing series of observations with weights (5) and then take the simple mean of the two thus calculated means.

If certain knowledge of θ is lacking, that calculation is done under three hypotheses, θ = 0, 1 and ∞. The polar altitude will then be, respectively,

circle eastward	50°56′5.”75	5.”69	5.”71
circle westward	4.”62	4.”65	4.”65
mean	5.”18	5.”17	5.”18

When keeping to the rigorous principle the first result [50°56′5.”17] does not at all change noticeably so that we may hold on to it. The calculations above did not at all take into consideration the bending of the telescope. According to Hansen's testimony, it was equal to 1."00 when the telescope was horizontal and should be subtracted from the observed zenith distances; in our notation, $f = -1.''00$.

It is seen that, when allowing for this bending, the polar altitude becomes somewhat larger for the stars culminating northward from the zenith and smaller for those culminating southward. Northern stars were somewhat more numerous, so that the mean result will be increased by 0."02.

Since all observed zenith distances are small, the second part of the bending, or the bending when the telescope is vertical, can be regarded as a constant change of the collimation error. When applying our method, this part of the bending will therefore be eliminated all by itself just as it happens with the errors of graduation.

And so, these observations lead to the definitive value of the polar altitude

$$50°56'5.''19.$$

The mentioned trigonometric connection of the observatories, when applying the dimensions of the terrestrial spheroid as stated above, leads to the latitudinal difference

$$35'41.''86,$$

and, taking into account the determined polar altitude at Göttingen (see above), we arrive at the latitudinal difference

$$50°56'5.''99.$$

This value refers to the triangulation station or the centre of the axis of the passage instrument. The centre of the axis of the meridian circle is located 1.168 toises or 0."07 to the south. The polar altitude at the latter centre is, when derived from Göttingen through the trigonometric connection,

$$50°56'5.''92$$

or 0."73 larger than the value provided by astronomical observations.

For the longitudinal difference the trigonometric connection furnishes 47′9."20 or $3^m8^s.61$, which very well coincides with our knowledge derived from astronomical observations.

Finally, those measurements (?) provide the azimuth of the side of the triangulation Seeberg – southern meridian mire near Schwabhausen as 4."6 westward. This can be regarded as a good coincidence when taking into account the pretty large number of intermediate stations, the divergent information about some angles in the Prussian measurements, and the uncertainty about the location of the triangulation station exactly on the meridian.

Notes

1. Altona is now a district of Hamburg.
2. See the beginning of the book Mudge & Dalby (1799).
3. Corrections for the run of micrometers were not made then.
4. The correctness of that determination based on 8 observations is confirmed by its near coincidence with the information contained in the previous list of 1803 which was based on 6 observations. The exact value of the proper motion remains however somewhat uncertain since the year to which the mean of the observations corresponds is unknown. It is remarkable that a considerable proper motion was detected for a star of the seventh magnitude. In this respect, star No. 11 seems to be worthy of the astronomers' attention. C. F. G.
5. In § 3.8 this method of combining observations is called the first one. There also a second method is introduced.
6. In § 3.4 not the limb, but the sector is mentioned. A single term should have been chosen.
7. At first, Gauss (1809) justified the principle of least squares by deriving most probable estimators, but later he (1823) introduced instead most reliable estimators, and his mentioning the former approach is strange indeed.
8. Even in the beginning of the 20th century results had been stated with an excessive number of significant (and usually fictitious) digits, and Gauss was no exception. Karl Pearson was also in the habit of computing with an excessive number of digits. E. B. Roessler (*Nature*, vol. 84, 1930, pp. 289 – 290) pointed out that among statisticians *no uniformity of practice exists in the retention of significant figures* and a *very misleading impression of the accuracy of results* can occur. One of his examples concerned Fisher. A discussion followed in *Nature* (pp. 437, 483 – 484, 574 – 575).
9. Following Laplace, Gauss (1823, for example in §§ 7 and 39) already used this expression. Thus, in § 7: *Errorem medium mutuendum sive simpliciter errorem medium*. I have everywhere (also a few lines above) replaced *medium* by *mean square*, but am unable to say who had introduced that later term, *mean square error*. Gauss considered true errors just as he (1823, § 38) did previously. A few lines above Gauss stated that he had derived the approximate value of the error. Indeed, he (1823, § 39) noted that it was only possible to equate $M = \mathrm{E}M$.
10. It would have been better to mention the theory of errors, the term introduced by Lambert in 1760, but neither Laplace, nor Gauss ever used it (though Bessel did).
11. Gauss obviously bore in mind an iterative solution of the equations. In his letter to Gerling of 26 Dec. 1823 (*W*-9, p. 278) he described a version of such a solution, but here its description would have been appropriate.
12. According to Gauss (1823) himself, this error describes the precision of observations not necessarily treated by the method of least squares.
13. The expression *purely random* is not used anymore.
14. Or equally unreliable.
15. $1.0770 = \sqrt{844.5/788}$.
16. The derivation of this value is not explained and it also unclear how, a bit below, Gauss derived the latitudinal difference.
17. A somewhat vague statement. In the beginning of § 3.11 the definition of μ (§ 3.8) is used.
18. The probable error is calculated by tacitly assuming a normal distribution.
19. Belief in *utmost care* contradicts the very foundation of approaching the instrument, the new approach invariably attributed to Gauss himself and Bessel. Cf. Gauss's own statement in § 3.14: *No human technical work is absolutely perfect*.
20. That instrument was a Repsold meridian circle, see § 3.13. Bessel had also investigated the bending of the telescope (see for example [iii, Note 6]) and, moreover, of a measuring bar (1839), several feet in length, supported at two points. The weight of the bar bends it and changes its length, so where should the supporting points be for ensuring the minimal corruption of that length? Bessel solved this problem by means of appropriate differential equations.
21. Somewhat more convenient is here my method of 1828. C. F. G. This is the method of adjusting conditional observations.
22. Gauss followed Legendre and Laplace by loosely calling residual free terms *residual errors*.
23. From microscopes Gauss somehow passed on to verniers. Microscopes here

and above were apparently microscopes-micrometers. The mean of readings made in the opposite parts of the limb cancel its eccentricity.

24. Bohnenberger was the first to prove the practicality of the invaluable method applied here. He had invariably applied it for two years, C. F. G.

25. See *Monatl. Corr.*, Bd. 10, p. 203. In a point ca. 0.”5 southward from the trigonometric station this skilful observer derived the value 51°48′12.”12 by observing α Aquilae 188 times. He also observed the Sun obtaining the value 51°48′11.”17. C. F. G.

26. The radius of curvature can be used for calculating the latitudinal difference.

27. Subbotin (1956, pp. 272 – 273) quoted (in translation) the passage above and stated that Gauss had thus initiated physical geodesy (study of the physical properties of the gravity field of the Earth). I doubt however that due attention had been paid to that passage. On physical geodesy prior to the advent of satellite geodesy see Bomford (1952) and Pellinen (1979) who discussed the work of F. N. Krasovsky (best known for the Krasovsky ellipsoid computed in 1940 by his former brilliant student A. A. Izotov) and M. S. Molodensky, another and still more brilliant student of F. N. K.

28. Formentera is an island in the Mediterranean Sea and island Fühnen is a part of Denmark.

29. This *Addition* was placed at the end of the paper, but I have replaced it. Since Gauss had paid much attention to Schmidt, I note that Bessel (1837/1876, p. 41), who derived the dimensions of the terrestrial ellipsoid anew (and much more precisely), stated that that author had based his calculations on data whose correctness he, Bessel, partly questions. Schmidt had treated the Hannover arc measurement as well, so that Bessel possibly admonished Gauss, but, anyway, he did not prove anything. As it seems, the Bessel ellipsoid had been used the world over well into the 20th century (in the Soviet Union, until the 1940s).

30. Here again (see Note 18) the normal distribution is presumed, but 8.4 units and 3.4 toises should have been rather indicated. It is appropriate to mention here May’s (1972, p. 309) *conclusion: Gauss was satisfied to establish priority by private records, correspondence, and cryptic remarks in publications*.

The first derivation of a triaxial ellipsoid as the figure of the Earth was due to T. F. Schubert and published in 1859. He combined four arc measurements but treated his data arbitrarily (Strasser 1957, pp. 43 – 44). Danilov (1953, § 5) testified that the idea about *the triaxial ellipsoid runs through all of Krasovsky’s writings* for more than 40 years.

31. The ratio 1/√θ corresponds to notation of § 3.8 and formula (5) follows from formula (3) also given there.

Brief Information about Those Mentioned

Baily Francis, 1774 – 1844, astronomer

Bohnenberger Johann Gottlieb Friedrich von, 1765 – 1831, astronomer

Hansen Peter Andreas, 1795 – 1874, astronomer, geodesist

Mudge William, 1762 – 1820, geodesist

Müffling Philipp Friedrich Carl Ferdinand Freiherr von, 1775 – 1851, General-Fieldmarshall, geodesist

Petersen Adolph Cornelius, 1804 – 1854, astronomer

Walbeck Henrik Johan, 1794 – 1823, astronomer

Bibliography

Bessel F. W. (1837), Bestimmung der Axen des Rotationssphäroid etc. *Abhandlungen*, Bd. 3, pp. 41 – 47. Leipzig, 1876.

--- (1839), Einfluss der Schwere auf die Figur eines, auf zwei Punkten von gleicher Höhe auflegenden Stabes. *Abhandlungen*, Bd. 2, pp. 275 – 282.

--- (1844), Letter to John Herschel 22 June 1844. *London, Edinb. and Dublin Phil. Mag*., vol. 25, pp. 227 – 229.

--- (1846), Über die aus der Schwere hervorgehenden Veränderungen , die der Kreis … erfährt. *Abhandlungen*, Bd. 2. Leipzig, 1876, pp. 182 – 203.

Bomford G. (1952), *Geodesy*. Oxford. Fourth edition: Oxford, 1980. My Russian translation of the first edition: Moscow, 1958.
Danilov V. V. (1953, in Russian), F. N. Krasovsky. **S, G,** 50.
Gauss C. F. (1809, in Latin), *Theory of the Motion* etc. Boston, 1857; Dover Phoenix publ., 2004.
--- (1823, in Latin), Theory of the combination of observations etc. Transl. with Afterword by G. W. Stewart. Philadelphia, 1995.
May K. O. (1972), Gauss. *Dict. Scient. Biogr.*, vol. 5, pp. 298 – 315.
Mudge W., Dalby I. (1799), *An Account of the Operations Carried On for Accomplishing a Trigonometric Survey of England and Wales*. London.
Pellinen L. P. (1979, in Russian), F. N. Krasovsky and the development of astronomical gravimetric levelling in the USSR. **S, G,** 50.
Schmidt E. (1831), Von den Dimensionen der Erde. *Astron. Nachr.*, Bd. 9, pp. 315 – 316, 371 – 372.
Stewart G. W. (1995), Перевод статьи Гаусс (1823) with Afterword. Philadelphia.
Strasser G. (1957), *Ellipsoidische Parameter der Erdfigur*. München.
Subbotin M. F. (1956, in Russian), Gauss' astronomical and geodetic works. In memorial volume *C. F. Gauss*. Moscow, pp. 243 – 310.
Walbeck H. J. (1819), *De forma et magnitudine telluris* etc. Abo.

III

Kurt-R. Biermann

On the relations between C. F. Gauss and F. W. Bessel

Über die Beziehungen zwischen C. F. Gauss und F. W. Bessel.
Mitt. Gauss Ges. Göttingen No. 3, 1966, pp. 7 – 20

[1] The most important source needed for judging the relations between Carl Friedrich Gauss, the *princeps mathematicorum*, and Friedrich Wilhelm Bessel, the *great astronomer* of his time, as Alexander von Humboldt had called him, is the correspondence between these spiritual heroes (1880). Being extremely useful for the history of science, it can perhaps be without exaggeration considered the most important of all the published scientific correspondence[1]. Its letters contain a wealth of utterly interesting reasoning, results, observations, problems etc. pertaining to the history of science. Not surprisingly, it became sensational once the Berlin Academy of Sciences published it under the editorship of the astronomer Arthur Auwers.

A rarely seen but perceptible astonishment appeared by that time since Gauss had so often remarked about the reports on scientific discoveries, now and then somewhat laconically, that he found them long ago. Nowadays we understand that Gauss was not on any occasion guilty of scientific overstatements and that he had actually been far, far ahead of his time, but, when his correspondence with Bessel had appeared, a large part of the now published materials had still remained unknown.

The friends of Gauss never doubted that his announcements were absolutely truthful. However, already some of those contemporaries, who were more remote from him, sometimes expressed a slight suspicion that the great man from time to time exaggerated [his merits] and did not or could not have accepted the contributions of other scientists[2].

It is only natural that among the later generation, which had not anymore any personal relations with Gauss, such doubts had been strengthening. Indeed, until then, apart from the exchange of his letters with Sophie Germain published in 1879/1880, only the correspondence of Gauss with Schumacher had appeared, with a man who was hardly a scientific match for Gauss. For this reason no sufficient exoneration of Gauss had been offered, until, as stated above, the publication of the correspondence between Gauss and Bessel paved the way for a change.

Since then the superiority of Gauss over his scientific contemporaries has been demonstrated anew to his astonished readers with each new edition of his notes or letters. Take Weierstraß, a grandmaster of mathematics in his own right, so dissimilar from Gauss. He still had many common features with the latter; for example, he possessed many discoveries which only after some decades became generally known. Once, in a letter to H. A. Schwarz, he expressed his amazement at the fact *that Gauss*, *already in the* *beginning of this century, possessed the main ideas of our present analysis*. Weierstrass referred to a letter from Gauss to Bessel of 18 Dec. 1811 in which the sender clearly formulated the *Cauchy integral theorem* and described its importance[3].

However, the correspondence between Gauss and Bessel is so instructive not only in the scientific sense. It is also very important for judging both of them, their human peculiarities and unusual features as well as their interrelations. But exactly in this respect the letters leave many questions open, and, to answer them, we ought to fall back upon the letters exchanged between Gauss' friends. By using this possibility, I am trying here to contribute something to the interpretation of the relations between Gauss and Bessel. I especially draw on the letters between Bessel and Schumacher which until now only Johann Adolf Repsold had consulted when compiling their biographies [   ]. Regrettably, contemporary turmoil deprived these meritorious contributions of due attention.

Gauss and Bessel began to exchange letters in Dec. 1804 while the latter was still a commercial office worker in the Bremen firma Kulenkamp. The correspondence continued during Bessel's work as an assistant in Schröter's observatory in Lilienthal near Bremen (March 1806 – March 1810) and went on while Bessel had been Director of an observatory and ordinary Professor of astronomy in Königsberg (from May 1810). It ended one and a half years before he died on 17 March 1846. Gauss wrote Bessel 75 letters. Only one of them dated 28 Oct. 1843 was not included in their published correspondence (1880) since it was offered at an autograph market. That publication also contains 119 letters from Bessel to Gauss.

[2] On 28 June 1807 Gauss and Bessel became acquainted in Olbers' place in Bremen. Olbers was closely connected with both of them as most tightly with H. C. Schumacher. It was also he who, in 1804, *discovered* the twenty-year-old Bessel and, until his death in 1840, being 27 years older, fatherly took to heart his development and was deeply concerned about his life. And it was also Olbers whose mediation brought about the correspondence between Gauss and Bessel.

They only differed in age by seven years and were delighted by each other. This feeling strengthened even more during the next meeting of Bessel, Gauss, Schumacher and Olbers on 2 Sept. 1809 in Lilienthal. In 1808 Gauss and Olbers rescued Bessel from a threatened conscription. Then, being a professor in Königsberg without a doctorate, Bessel had initially experienced difficulties and Gauss ensured his receiving the doctor degree in Göttingen. Understandably, Bessel appreciated this friendship. *If it will only be possible, dearest Gauss, to prove to you once more how gladly I will be in use for you*, he wrote in his letter of 10 March 1811.

We should think that the foundation of a strong, lasting and unshakeable friendship had thus been laid the more so since the proof of Gauss' deep respect and affection for Bessel was not lacking (*semper totusque tuus* [invariably totally yours]). However, it occurred otherwise. In June and July 1819 Gauss and Bessel many times failed to meet either in Göttingen or Lauenburg. On the other hand, Bessel's new meeting with Schumacher in Lauenburg in August 1819 became the base for a close friendly connection as witnessed by their correspondence (535 letters from Bessel and 596 from Schumacher).

It became noticeable, however, that over the years the tone of the letters between Gauss and Bessel became less warm and indeed stiff and formal and less was written. About 2/3 of the letters was exchanged in the first half of the duration of their correspondence, and only 1/3, in the other half. On 31 Dec. 1831 Gauss made known the death of his second wife, and his letter still breathed the previous warmth:

For a very long time, my dear Bessel, I have not written you. You have favoured me with two of your priceless works, whereas I, as I believe, have not yet thanked you for the first one. I feel ashamed of my guilt although I am sure that you will forgive me and that even for a moment you could not have thought that I had forgotten to appreciate your scientific communications or the expression of your friendly cast of mind. You certainly know how high, how very high I set them both.

However, for a year and a half your poor friend has been a victim of heaviest domestic sorrows. The outcome of one of them you will easily guess by the colour of my applied signet ring[4] *applied now for four months. The other one, if at all possible, is still sharper and I hardly foresee any end of it apart of my own. But let me be silent about it. These circumstances agonizingly influenced all my scientific activities and brought almost to a complete standstill my correspondence*.

Bessel did not condole. That was one of his peculiar features: he was unable to write letters experiencing sympathy. He did not want to seem too soft-hearted and was rather not ashamed of looking cold, as Repsold had aptly remarked. However, in 1840, after the tragic death of Bessel's only 26-year-old son, Gauss did not find a word of compassion either. Between July 1833 and February 1839, for a full 51/2 years, Gauss had been completely silent. And from Nov. 1842 onward Bessel changed the previously friendly address to *Highly respected Sir* (Herr) *and friend*.

What was the decisive reason for such cooling which became apparent not only in the addressing and rarity of letters, but in the very form of communication?

[3] At first, Gauss. On 23 Dec. 1848 he declared that he would have wished to delete the address *Herr* (Sir) from the envisaged but not then brought about publication of Bessel's letters to him, and thus to give no occasion for any assumptions. He, Gauss, was sure that it was not he who initiated that formality. Gauss continued:

I reluctantly mention <u>*one*</u> *more letter which I would rather completely withdraw* [the letter of 28 May 1837, see below – K.-R. B.]. *At that time it seriously offended me and even still more by its improper tone rather than by the matter itself. Actually in its last lines the tone of Bessel's letter became unacceptable. I never allow myself such a tome with respect to a subordinate.* […] *As far as I remember, I did not answer that letter at all*.

Gauss went on to report that in a letter of 28 Febr. 1839 he expressed his *resentment* over Bessel's expressions after which Bessel, on 28 June, unsuccessfully, as Gauss understood it, attempted to justify himself. However, Gauss felt that it was a new groundless attack and that that second letter should also be withdrawn.

Here are the appropriate passages.

1. Bessel to Gauss. Königsberg, 28 May 1837

I have read with great interest your electromagnetic investigation published in the previous volume of Schumacher's [*Astron.*] *Jahrbucher* [Tübingen, 1836 – 1844]. *It was very important for me not only because of the certain and firm advance that will now follow but also since it corrected my false opinion.*

I have previously thought that you will turn your attention to the generality and simultaneity of the change in the terrestrial magnetism. Now I see that an exhaustive theory of the entire phenomenon of magnetism and its connection with electricity will be achieved. I can only wish that I will also see its significance set in bright light once you report to us your appropriate studies.

Although little am I justified to hope that my wish has some weight, I will not keep silent that it is only directed to my becoming acquainted with your present occupations as soon as possible. You have never recognized the obligation to promote the present knowledge of things by early reports about a certain part of your studies. You are living for the posterity which is completely contrary to my views. I believe that the more certain will become the fate of your still unpublished results the more completely the rights of the present time are recognized. When the new planets caused your Theoria motus, you had worked not only for the posterity to which your Disquisitiones [*arithmeticae* of 1801] *also testify.*

You would have surely never seen their success which you could have hoped for according to your own yardstick. However, you could not have remained ignorant how intense have the efforts been when trying to follow the path which you had outlined. Where would have the mathematical sciences find themselves not only in your place, but in Europe in general, had you expressed everything which you could have communicated! It is not necessary to continue the less so since I fear to repeat something which you were told a hundred times.

It was this last sentence that had especially hurt Gauss' feelings.

2. Gauss to Bessel. 28 Febr. 1839

I tend to fear that you […] *have been led to a wrong opinion about the aim which I wished to achieve by my work on the general theory of terrestrial magnetism. It is flattering that you appreciate that hasty publication*[5] *but I ought to complain about your sharp expression which you used. Only those may be reprimanded for delays who withhold something quite ready, i. e., ready for publication, if only publication is in their power. This is something which I never yet did in my life.*

It is a double entendre for a manuscript to be either ready in a fair copy or prepared for publication. For the latter I need time, much time, much more than you can probably imagine since I can only work slowly whereas my time is in many ways restricted, very restricted. And I also need to be in high spirits (need it much more for *preparation in this sense than for the first try) which is regrettably too seriously and in too many ways overshadowed.*

And I would therefore allow myself to ask you to judge me more justly.

Bessel had not directly used the word *delay*. In a letter to Schumacher of 23 Dec. 1848 mentioned above, Gauss surmised that there was one more letter from Bessel which he had not kept. However, it is conceivable that during the discussion Gauss only had in mind the *sense* of Bessel's reproaches.

3. Bessel to Gauss. 28 June 1839

At first, allow me to say that I did not mean delays in an unpleasant sense. I have really never even thought that you had wished to conceal the treasury of your ideas from the others. I myself have rather considered this matter exactly as you have explained it. And sufficiently often I had the occasion to be amazed by the maximal thoroughness with which you describe and form your contributions. But I have also understood that such maturity is not compatible with a quick succession of announcing [your results]. *Allow me therefore to say something in my defence.*

I do not dare to be insensitive either to the significance which a scientific investigation obtains by becoming fully ripe. However, when the time [of work] *increases by quantities of the first order, the* [studied] *magnitudes tend to their maximal* [ripeness] *by those of the second order. Will not the main idea itself appearing in a respectable, even if not in a maximally attainable description, more rapidly promote science than your postponement until the time favourable for the appearance of your highest degree of quality? Would have Euler achieved as much as he did had he published only a tenth of the great number of his ideas in an irreproachable form rather than the whole lot of them? Would have Lagrange acted better had he left his earliest writings (published in the Miscellanea Taurinensia) unknown for 20 or 30 years until they became completely ripe?*

I know that these questions belong to those which could be answered not unconditionally but only according to one or another point of view but I keep to the view from which follows your approval (?). *You look disinterested when something is taken away from your great treasure, and really have only to follow your own views, but you cannot be ignorant about the danger of complete loss to which you expose everything that will not be taken away from you.*

You can only await that in general all the friends of exact sciences balance their own benefit against your viewpoint. And in any case your contemporaries have even more grounds for wishing that you will be less rigorous. However, I came too far; I only thought of justifying myself.

We can certainly understand that a man as sure of his value as Gauss was, regarded such wishes as expressed by Bessel on 28 May 1837, and moreover formulated in a way smacking of a sermon, like an unbidden tutelage and a wrong assessment of the mode of his studies and publications. Among friends, however, such division of opinion should be allowed.

[4] Commentators have described Bessel as at times somewhat hottempered but, in contacts, a jovial, cheerful, brisk and sincere man. Often he went too far. His known argument with the Berlin astronomer J. F. Encke, a student of Gauss, who, due to Bessel, became Director of the Berlin observatory, shows that Bessel could have been implacable. Because of that quarrel even the friendship with his intimate friend, Schumacher, hung by a thread. However, as a rule he changed his tone and attempted to rectify *that which he* (?) *had spoiled while being in extremely ill humour* (his letter to Schumacher of 16 March 1838), and he did not shy away from expressing afterwards that the matter *really sorrows* him. And in his letter to Gauss of 28 June 1839 he had also changed his tone which certainly did not find in Gauss any requited love for his arguments.

We ought to mention as well that Bessel with his opinion was not at all alone. On 25 Jan. 1825 Olbers, who did not even slightly differ from Gauss, wrote to Bessel:

I am very curious about your discourse on the perturbations of the planets. It can easily happen that you will once more clash with Gauss as it really occurred about the determination of the bending of the instrument. About three weeks before Schumacher had received your letter, Gauss had orally explained it to me.

This is only a coincidence but otherwise our Gauss is often guilty himself when others overtake him with discoveries which he had also found. I am unable to praise you sufficiently, my dear friend, and many of my correspondents thankfully and admiringly recognize that out of sheer love of science you at once make known a new method, a new solution, a new and more correct viewpoint as soon as your genius and your studies discover it without taking into account that others will thus achieve things, solve problems, calculate that which had been too difficult or indeed impossible for them previous to your communications.

It seems however that Gauss invariably wishes to be the first to pick the best fruit to which his discovered and paved path led him before showing them to others. I think that this is a slight weakness of a man otherwise so great, the less to be explained since he had favoured us with so much from his inexhaustible riches[6].

Schumacher had also conveyed various similar thoughts to Gauss, certainly in his own tactful form, without offending him. Many years earlier Bessel had made known to Gauss his opinion shared by many others that a Gauss can spend his time more usefully than on geodetic work which can be fulfilled by less important people. Gauss answered him on 14 March 1824:

In many letters you have so strongly declared that the results of measurements are insignificant, and thus to a certain extent reproached me for wasting my time and wished me that its loss ends. God almighty, how wrongly are you judging me. However, it is much more important for me not to be falsely judged by <u>*you*</u> *than my reluctance to justify myself.*

To tell the truth, I agree with you about the matter itself. All the measurements taken worldwide do not offset a theorem which leads science really nearer to eternal truths. However, you ought to compare the relative rather than the absolute values. The *measurements undoubtedly possess the former. They should connect my system of triangles with that of Krayenhof and thus with the French and the English triangulations*[7]*. And, however unimportant is their value according to your estimation, to my eye it is higher than the value of the occupations which had been abandoned because of those measurements.*

Here (?)*, I am so far from being the boss of my time. I ought to share it between reading lectures* (*for which I have long since had an aversion, although not caused, but strengthened by the feeling which invariably accompanies me that I am thus losing my time*) *and work in practical astronomy. This latter always caused me so much pleasure, that you should admit that when any real help is lacking for the immeasurable number of small and smallest duties, the feeling of loss of time is removed if only you are certain of pursuing a considerable and important goal.*

However, for us, others, you made that difficult since you have overtaken us and done away with most of the desiderata in such a masterfully way that for us, others, only little is left aside by gleaning the remains.

So what is left me apart from fleeting hours for a work which I myself can highly appreciate? A person with another character, less susceptible to unpleasant impressions, or I myself if much will change, will perhaps secure more fleeting hours than I generally can. Under the circumstances I do not dare abandon an enterprise, although involving a thousand difficulties and perhaps exhausting my strength, since it is really useful. Someone else can certainly fulfil it whereas I will then under more favourable circumstances achieve something better. However, if I do not take that enterprise on myself, nothing at all will be done[8]*. And I dare not conceal from you a fact that to some extent equalizes the disparity between my earnings which remain the same in 1824 as those fixed for you in 1810 by Jérome*[9]*, and the necessities of a numerous family*.

This explanation did not bear much fruit. On 15 Jan. 1832 a surprised Bessel wrote Schumacher that

Gauss is so occupied with physics while having such a great mathematical treasure in stock. However, only in this respect do I find it so unusual.

Many contemporaries, for example von Humboldt, had also been surprised.

[5] In the abovementioned letter of 14 March 1824 written by Gauss we find no trace of an offence, so why fifteen years later had he reacted so sensitively? Is it sufficient to explain this change by a decrease in the readiness to conciliation with age? I do not think so. I rather perceive Gauss' later reaction in that he, since the dying away of his second wife and the emergence of the known difficulties with his sons by that second marriage (he alluded to this in his letter of 31 Dec. 1831 quoted above), he sometimes suffered from depression whose signs had been felt previously. He therefore became

A queer sort of a fellow [written by Schumacher in English – O. S.] *and somewhat more of an egoist than necessary for a pleasant contact, but at the same time he is exceptionally honest and incapable* *of any mean slyness or evasion*[10] (as Schumacher wrote to Bessel).

There are many instances of Gauss' changeable mood. For example, when he stated on occasion that he will not deal with a manuscript sent him since it was badly written. Actually, it was written clearly. This is a proof thereof just as the following description of Schumacher's visit to Gauss in May 1834, see his letter to Bessel of 30 May 1834; however, I am leaving out a drastic comparison[11]. Gauss, as it emerges, had time and time again expressed what he actually felt about Schumacher:

1. [I] *went at first not to him, but to Bessel.*

2. *With you* (with Bessel I) *lived fourteen, but with him only a few days*.

3. *Since you* [Bessel] *had still stayed* [in Berlin], *I have postponed my departure for a few days*. (*I cannot guess how he found it out if Encke had not written to him*).

4. *I left him* [Gauss] *during daytime* (*not at night*) *since I thought of living in a hotel rather than in his place*. *I had written to him from Berlin and asked whether he will allow me to live in a hotel since everyday life in his place was thrown into disarray by the death of his wife*. […]

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

In his answer of 4 July 1834 Bessel called the description above *curious* and continued:

But it follows that our friend is a crass egoist. How else can his foul mood occasioned by a random occurrence which he does not like show up in such a way that your statement [Bessel repeats Schumacher's allegory – K.-R. B.] *could have been to a certain extent confirmed*.

In 1842, when Bessel himself visited Gauss on his way to England, he was met not better [than Schumacher]. They did not see each other since 1825 when they had spent together only an hour in Rothenburg, on the post road to Bremen. It did not then come to the conversation desired by Gauss since many other astronomers were also present. It should be assumed that this time they will seize the opportunity to continue their previous talk and to rectify mutual offences. Nothing of the sort had however occurred. On 21 Nov. 1842 Bessel informed his friend Schumacher:

You know that I have spent a few days for making a detour and putting up at Göttingen. After having a meal and dressing myself up, I went to Gauss but found him <u>*caustic*</u>. *He spoke about* [my] *living a while in England and described the diet* [there] *as pernicious. I thought that I will have to adjust somehow* [my meals in England]. *I thought of having some soup and a beefsteak for breakfast and doing without regular dinners. When I mentioned the beefsteak Gauss began speaking about* <u>*teeth*</u> *exactly the same way as you have written me, so that I did not remain in doubt about the source of his remarks.*

It seemed very funny, but fine since otherwise I would have scarcely got the better of a temptation to remark soothingly about the defect of *his own teeth that* *biting* *is still enjoyable even if little is achieved*. […] *Next morning, however, he was quite amiable so that finally I thought it was nice to have come to Göttingen*.

Nevertheless, on 29 Nov. Schumacher took exception: he had corresponded with Gauss about a denture for approximately a year, but not anymore. And on 5 Dec. Bessel reassured him:

Leave both my and his teeth alone, they are not important. Gauss' foul mood must show somehow! I am very far from being upset by foul mood and I only related to you the curious way in which Gauss takes notice of the attention paid him because of that curiosity. He had previously responded exactly in the same manner to your similar attention. Incidentally, I myself am not invariably in high spirits.

Schumacher answered on 21 Dec.:

I had not intended to excuse Gauss' foul mood, I only wished to show that *I* *probably did not directly give him the arrow which he shot at you. He is the most unusual person in the world with whom, in spite of all his rough edges, you cannot really be angry. Attention, as you remark, and as I myself know by my own repeated experience, is usually met with an expression of foul mood. And I therefore find that it is much better just to remain exactly within the boundaries of usual politeness.*

You certainly did not know that your travelling through Göttingen was already an attention. And by your very presence you have transgressed those boundaries and had to take the consequences. Weber thinks that Gauss' foul mood sets in because of corns from which he suffers in an unusual measure and testifies that when they seriously bother him, he becomes as irritable and as angry as possible, but that in a few hours, when the pain disappears, he becomes amiability itself. I know by my own experience, that Gauss can indeed be amiable, although not often.

On 26 Dec. Bessel returned to this topic:

There is nothing to say about Gauss. A bit of foul mood is of no consequence. It can be completely forgotten even if it did not entirely disappear the following days. With a head so heavy and sickly legs, how can stable equilibrium always remain?

In spite of this amusing assurance ringing with truth, Bessel, as it seems, had not forgiven the initial unfriendly reception by Gauss. Indeed, his first letter to Gauss after that visit began with a dissociating form of address: *Highly respected Sir* (Herr) *and friend*. We are led to a suspicion that Bessel had harboured a grudge which was not rectified at their meeting. No wonder that we find in his letter to Schumacher of 30 April 1840 the following text:

I wish to confess gladly that at the time, when my astronomical troubled life had started at Königsberg, and I had resolutely thought of beginning something *important*, *one single approving word from Gauss would have greatly encouraged me. I regarded the abstention from sending me such a word as more than a chance inattention. Those, however, are* *bygone* *times and Gauss had won great claims on general respect. In comparison, my own did not last*[12].

Gauss acquired a great claim to general respect, and in comparison with him my own claim has disappeared, als das jene Zeit nicht längst spurlos untergegangen sein sollte.

It seems to me that this is a really remarkable statement. We recall the lack of a public recognition of the *absolute geometry* of Johann Bolyai so passionately wished for by his father, Wolfgang[13]. It is doubtful that no trace of Bessel's disappointment over his visit with Gauss had remained since his letters to Gauss became formal afterwards. And we may also suppose that Bessel thought that Gauss had not appropriately appreciated in writing the merits of his son in law, Adolph Erman.

[6] One more word ought to be added about Schumacher's quoted statement. He remarked that in his contacts with Gauss he remained *exactly within the boundaries of usual politeness.* This should be denied. In his letters to Gauss he always expressed himself with refined politeness, and, yes, it is often barely possible to gainsay there certain servility. This, however, is hardly a reproach. Apart from his extensive correspondence with the most important contemporary mathematicians, astronomers and natural scientists, Schumacher only left traces in the history of science as the founder and editor of the *Astronomische Nachrichten* (*A. N.*).

In astronomy his role was similar to that of A. L. Crelle in mathematics as the founder and editor of many years' standing of the *Journal für die reine und angew. Math*. Because of the deficiency in his knowledge and ability of judging, Schumacher was only able to bring the *A. N.* to a centre of scientific information by the support of his competent friends, of Gauss, Olbers and Bessel in the first place. He himself had wholly understood it. We are pleased to read for example in his letter to Bessel of 19 Aug. 1842 that if he *did something useful for science, it only is* [was] *as a middleman*.

And he always therefore endeavoured to act pacifically whereas his report about the ill-starred visit with Gauss in May 1835 was a rare event, and his spontaneous letter to Bessel shows how it worried himself sick. Otherwise the tenor of his letters was directed at preserving or establishing peace between those people on whom he depended. On 25 Apr. 1840 he wrote to Bessel quite characteristically:

[I gladly see], *so warm-heartedly see you and Gauss, the two outstanding people, being on intimate terms. Gauss invariably believes that you are underestimating him*[14]*, and I <u>know</u> how he values each favourable word from you.*

When in 1838 Bessel suspected Schumacher of being in cahoots with Encke to plot against him, Schumacher became horrified and turned, literally wringing his hands, to Olbers and Gauss requesting them to intervene. However, Bessel retracted his announced boycott against the *A. N*. and sent Schumacher a new manuscript. Schumacher's relief was indescribable.

This digression should only emphasize that Schumacher's advice (about contacts with Gauss) *just to remain exactly within the boundaries of usual politeness* and to do nothing else was only theoretical. He himself happily had not followed it. It is true however, that Gauss had not appreciated exaggerated eulogies. Nevertheless, I think that he had wrongly stated, in a letter to Schumacher of 23 Dec. 1848, that many compliments found in almost each letter from [the late] Bessel to him only reflected his beloved pursuit, *an attempt to say something pleasant to people or something that he believed they will be glad to hear*. And he knew quite well how much should be subtracted from his statement.

[7] We have no reason to suppose that Bessel's expressions of wonder and deep respect for Gauss' mathematical genius were exaggerated or even hypocritical. And in his letters to Schumacher in which, as we saw, he had not minced his words, we also find sufficient proof that he fully appreciated and respected Gauss' *mathematical power*. It could not have been otherwise. For example, when he heard from Poggendorff, to whom he was usually *well disposed*, a wrong appreciation of Gauss' paper on magnetic problems, he informed Schumacher about that on 31 Aug. 1839 and added that he was so angered that *most of all* wished to explain to everyone what had Gauss really meant although this is the business for Gauss himself if only he will consider it necessary.

On 9 Sept. Schumacher found such an intention very honourable:

But you also know what kind of a person he is. He will certainly do nothing. He is satisfied by putting down his works without troubling himself about them. If they are misunderstood, he will laugh inwardly and perhaps only become angry if his works are <u>deliberately</u> corrupted. In conversation he <u>never</u> argues when being in the right, but applies the entire art of dialectics to defend a wrong proposition which he had stated[15].

Other contemporaries sided with Bessel in criticizing Gauss for other matters. In a letter to Schumacher of 5 April 1835 Bessel found Gauss (1825) *excellent, worthy of admiration*, but he added that he did not understand why Gauss had not referred to Lagrange (1779).

Did not he know about the work of Lagrange? I believe in this possibility as little as I do in Gauss' denial of his knowledge had he been asked about it. This, however, is his habit not to be commended of naming no one else.

In a letter to Schumacher of 2 April 1836 Alexander von Humboldt reproached Gauss for the same reason: *Next to the map projections, as a threatening spectre, appears the ghost of Lagrange*. On 8 April 1835 C. G. J. Jacobi, the second after Gauss most eminent German mathematician of those times, wrote Bessel: For Gauss, <u>*de mortuis nil nisi bene*</u> [nothing but good should be spoken of the dead] *is replaced by* <u>*de mortuis et de vivis nil*</u> [nothing about either the dead or the living]. And on 21 Sept. 1849 he wrote to his brother Moritz about Gauss: *For over twenty years he had not ever quoted either me or D* [irichlet – K.-R. B.].

Gauss himself had named the reason for his behaviour. In a letter to Schumacher of 6 July 1840 he wrote:

I reluctantly express myself in detail about the achievements attained by others working in the same field as I did, if only not being entirely convinced in that I really may mention them approvingly.

And, again,

Nevertheless, I recognize […] *that I did not at all study critically* [the history of the theory of magnetism – K.-R. B.]. […] *As a rule, I am unable to decide just like that who should be favourably mentioned* *and thus to reinforce myself unconditionally. And, when desiring to provide authoritative connections, it would have been necessary to conduct* [prior] *literary studies for which I have neither time nor* (*I confess*) *inclination. Indeed, such investigations are not exactly to my taste*.

I can only say that the forbidden to a usual author should probably be allowed to a Gauss, and that at least we ought to respect his grounds.

[8] Finally, I would like to mention one more event, important for describing the relations between Gauss and Bessel and at the same time typical for both of them. Gauss had published a paper in Schumacher's *Astron. Jahrbücher* for 1836, Geomagnetism and the Magnetometer although he expressed his aversion to any popularization of scientific achievements. In this respect he was a *scientific aristocrat* through and through, but, to oblige Schumacher, Gauss had overcome his doubts. It occurred, however, that Humboldt had misunderstood various details, and in a letter of 15 April 1836 to Schumacher Gauss resigned himself to ascertaining that in spite of all his efforts, he apparently did not impart the necessary clarity to his paper.

Contrary to Gauss, Bessel gladly provided generally understandable essays on the state of knowledge[16]. In the same *Jahrbuch* for 1843 he published a paper (Bessel 1842) on geomagnetism. And then Gauss began to think that Bessel did not at all appreciate his contribution, as Schumacher informed Bessel on 26 Jan. 1843. These are his words:

I have informed him right away that I know the opposite from your previous statements, but I would like to ask you to send me a few words which I may communicate to our old friend. You yourself surely do not wish him to suffer from false ideas. He concludes them drawing on your paper in the Jahrbuch for 1843. He believes that it is as though a reproach to him, as though until now he did not inform the public about the new advances in the teaching of magnetism.

Nevertheless, you had mentioned his paper of 1836 (*which you therefore knew about*) *but merely since it discusses the connection between galvanism and magnetism. This discussion only occupies a quarter of the paper mentioned so that you ignore or negate the other three quarters. There, he at first based that doctrine, which in all known to him books was described confusedly, and then, which is the most important point, he managed to explain geomagnetism for laymen.*

What will you say about these conclusions? I have painfully seen how such a powerful mind can be deluded when it is buttoned-up and alone and only allows sullen ideas to take their course and does not suspect anywhere kindness or friendship. I am far from being able to laugh and will rather comfort and impart friendly views to him. His letter has no trace of anger or morbid vanity and is rather sorrowful since he sees himself misjudged. But it seems that there is nothing with which to oppose him except by showing him that he is not misjudged.

On 6 Febr. 1843 Bessel answered very reservedly since he intended to leave Schumacher the possibility to weaken Gauss' doubts by passing on his letter to others. However, in a supplement he expressed himself quite angrily:

I do not understand what G[auss] *really wishes to say. There is not a single word about the Theory of geomagnetism in his paper of 1836*[1]*. His intention, as it seems, was either for me to rewrite his paper or that I should have chosen to refer to his paper instead of adding a few words with which I explained the* [work with the] *magnetometer and the determination of the absolute intensity according to the essence of the matter*. […] *That I have otherwise deviated from his description – I cannot excuse it anymore.* […] *I knew long ago that even with the best will in the world it is possible to act otherwise and wrongly* towards others.

There are other proofs of the approach and strengthening over the years of the disagreement between Bessel and Gauss which however never diminished the mutual appreciation of their significance in the field of science. Nevertheless, the above is sufficient for answering the question which is felt between the lines of their letters as quoted above.

Two as distinctly complicated personalities as those of Gauss and Bessel spatially separated from each other, only meeting very seldom and under unfavourable circumstances and finding themselves on those occasions so changed into the bargain that they could have *not recognized one another anymore* (Gauss to Schumacher on 19 June 1842) perhaps inevitably had to become estranged from each other.

In old age Gauss, like Bessel, acquired an inclination which the latter must have thought of having even as a young man since he said: *I begin wishing to suck out poison out of roses*.

Had the intention to invite both Gauss and Bessel to Berlin been put into effect, their relation would have certainly turned out differently, but the finale became all the more peaceable. During Bessel's protracted and agonizing mortal cancerous disease Gauss kept silent, but, having learned about Bessel's death, he wrote to Schumacher on 25 March 1846 that he felt himself

Most painfully shaken, although we had to expect his death and to wish a speedy end of his suffering. Our contacts began in 1804 and now only a few old friends are left. So let us, dear Schumacher, all the more hold together.

Bessel's fundamental philosophy [fundamental appreciation of Gauss] was expressed in his letter to Schumacher: *Gauss is the open and clear truth itself.*

[9] One more remark is perhaps not useless. Here, in the extracts from the letters, there appeared much rarely spoken of but no belittling of either of the two heroes of the mind is perceived there.

Truth is indivisible. Even a Gauss and a Bessel are human which should not be either overstressed or put down. A biographer must also include that which does not coincide with the information transferred to him with an eye on the living and the picture he draws differs from the previous image, cf. the apt remark of Gerardy (1964, esp. p. 6).

We intend and may find out, we portray and judge the individuality and peculiar features, the lasting and the transitory, according to witnesses, be they contemporary or not. Such a portrayal brings Gauss and Bessel humanely nearer to us. Here, we may appeal to Gauss' own words from his letter to Schumacher of 30 May 1846:

Concerning the topic of your second letter [of 27 May – K.-R. B.] *it is not quite clear to me why are you so opposed to the publication of Bessel's correspondence. It certainly contains much information important for science, but posterity will also regard a correspondence that depicts not astronomers, but people as a very valuable legacy. The correspondences of Leibniz, Kepler, Euler, the* *<u>Commercium Epistolicum</u>*[17] *and so many other similar collections form a priceless treasury. Only suppress that, which can harm some living people, and it becomes possible to publish all the rest.*

Notes

1. Biermann himself (§ 9) quoted Gauss who had mentioned few extremely valuable collections of letters.

2. Cf. Jacobi's statement in § 7.

3. Korn & Korn (1961/1968, § 7.5.1) called that theorem after Cauchy and Goursat.

4. The colour of stamps and possibly signet rings, though perhaps not when used by individuals, had a heraldic meaning. Gauss apparently applied the black colour.

5. Gauss (§ 8) had nevertheless highly valued that *hasty publication*. There also Bessel declared that there (in that paper) was not a single word about the *theory of geomagnetism*. However, in his letter to Bessel of 28 Febr. 1839 (§ 3), Gauss mentioned his work *on the general theory of <u>terrestrial</u> magnetism*.

6. Biermann quoted the last paragraph in his later paper [iv, § 5]. Concerning the *clash with Gauss about the determination of the bending of the instrument* I can only add that Gauss determined it in 1828 [ii, § 3.14].

In a letter to Gauss of 28 June 1839 Bessel notes that much can be lost with Gauss's death, and it is opportune to add that Gauss himself stated the same in his letter to Bessel of 15 Nov. 1822.

7. Gauss (Nachlass; *W*-9, pp. 402 – 403) set high store on the unification of the separate triangulations which existed in various parts of Europe.

8. A few lines above Gauss stated the opposite. Another contradiction in the same letter concerns the significance of geodetic measurements: their value is higher than the value of the *occupation which had to be abandoned*, but, instead of these measurements, he could have *achieved something better*. Finally, someone else could have replaced Gauss, but, at the same time, nothing will be done without him.

9. Gauss should have mentioned Johann Schröter instead of the mysterious Jérome.

10. Biermann quoted the last paragraph in his later paper [iv, § 6].

11. Biermann's decision is unacceptable since the deleted passage was included in an unpublished source. And in § 9 he himself expressed an opposite opinion.

12. Bessel apparently forgot that Gauss had ensured his receiving the doctor degree and that Gauss and Olbers had rescued him from a threatened conscription, see § 2. Even in 1828 Bessel complained that Gauss had overshadowed him [viii, § 18]. Then, in 1843 Bessel, in correspondence with Gerling, attempted to establish his priority over Gauss in the adjustment of triangulation and accused Gerling of failing to mention his, Bessel's (non-existing) merits in the development of the theory of probability (Gerling 1861).

13. An explanation is needed. Gauss had not publicly stated his views about that *absolute geometry*. *Gauss wrote to Wolfgang Bolyai endorsing the discovery, but he also asserted his own priority, thereby causing the volatile Janos to suspect a conspiracy to steal his ideas* (May 1972, p. 302, right column).

14. At the same time (see below) Bessel had repeatedly complimented Gauss, a fact which the latter wrongly interpreted.

15. Biermann quoted the last paragraph in his later paper [iv, § 6].

16. I categorically disagree [x].

17. *Commercium Epistolicum* published by the Royal Society in 1712 was a collection of letters bearing on the priority strife between Newton and Leibniz. Biermann quoted the last sentence in his later paper [iv, § 3].

Brief Information about Those Mentioned

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

Bolyai Farkas (Wolfgang), 1775 – 1856, mathematician, a friend of Gauss

Bolyai Janos, 1802 – 1860, mathematician, one of the discoverer of the non-euclidean geometry, son of Farkas Bolyai

Erman Georg Adolph, 1806 – 1877, physicist, geophysicist, editor of the correspondence between Olbers and Bessel

Krayenhoff Cornelis Rudolphus Amandes, 1843 – 1921, general, physicist, engineer, geodesist

Poggendorff Johann Christian, 1796 – 1877, physicist, the author of the many-volume *Biographisch-Literarisches Handwörterbuch*,

Schröter Johann Heroymus, 1745 – 1816, astronomer

Schwarz Hermann Amandes, 1843 – 1921, mathematician

Bibliography

Biermann K.-R. (1963), Aus der Vorgeschichte der Aufforderung A. von Humboldt von 1836 an den Präsidenten der Royal Society zur Errichtung geomagnetischer Stationen. *Wiss. Z. Humboldt-Univ. zu Berlin*, Math.-Nat. Reihe 12, pp. 209 – 227.

Briefwechsel Bessel – Schumacher, Bessel – Jacobi. Nachlaß Bessel. Akad. Bibl., Berlin-Brandenburgische Akad. Wiss.

Briefwechsel Weierstraß – Schwarz. Nachlaß Schwarz. Ibidem.

Briefwechsel C. G. Jacobi – M. H. Jacobi (1907). Leipzig.

Bruhns C. (1869), *J. F. Encke*. Leipzig.

Gauss C. F. (1825), Allgemeine Auflösung der Aufgabe: Die Teile einer gegebenen Fläche auf einer andern gegebenen Fläche so abzubilden daß die Abbildung dem Abgebildeten in den kleinsten Teilen ähnlich wird. *W*-4, pp. 189 – 216.

--- (1836), Erdmagnetismus und Magnetometer. *W*-5, pp. 315 – 344.

--- (1880), *Briefwechsel mit Bessel. W/*Erg-1; K. E. Henrici. Berlin, Auktion-Kat. 120. 1927, No. 93; J. A. Stargardt. Marburg, Auktion-Kat. 558.1962, No. 579.

Gaede (1885), Beiträge zur Kenntnis von Gauss' praktisch-geodätischen Arbeiten. *Z. f. Vermessungswesen*, Bd. 14, pp. 113 – 137, 145 – 157, 161 – 173, 177 – 207, 225 – 245.

Gerardy Th. (1964), *Mitt. Gauß-Ges. Göttingen*, No. 1.

Gerling Ch. L. (1861), Notiz in Betreff der Prioritätsverhältnisse in Beziehung auf die Methode der kleinsten Quadrate. *Nachr. Georg-Augusts-Univ. und Kgl. Ges. Wiss. Göttingen*, pp. 273 – 275.

Korn G., Korn Theresa M. (1961), *Mathematical Handbook for Scientists and Engineers*. New York, 1968.

Lagrange J. L. (1779), Sur la construction des cartes géographiques. *Nouv. Mém. Acad. Roy. Sci. Berlin*, pp. 161 – 210.

May K. O. (1972), Gauss. *Dict. Scient. Biogr*., vol. 5, pp. 298 – 315.

Repsold Joh. Ad. (1918), H. C. Schumacher. *Astron. Nachr*., Bd. 208, 17 – 34.

Sheynin O. (2001), Gauß, Bessel and the adjustment of triangulation. *Hist. Scientiarum*, vol. 11, pp. 168 – 175.

IV

Kurt-R. Biermann

The change of our concept of Gauss

Wandlungen unseres Gaussbildes.
Mitt. Gauss-Ges. Göttingen, No. 28, 1991, pp. 3 – 13

[1] I will try to say something about how our picture of Gauss *as a human being* has changed during 3 1/2 decades since new sources have been discovered and the interpretation of the literature has deepened, and how does it change nowadays (Biermann 1978); [v]).

The high respect for the *scientific grandeur* of Gauss has not changed at all, and we may just as well say, as Richard Courant did here in Göttingen in 1955 on the occasion of the centenary of Gauss' death:

A hundred years have passed since Gauss' death, but his scientific grandeur remains as mysterious and incomprehensible as it should have been for his contemporaries. The prophetic intuitive originality, the depth and versatility of Gauss' achievements coupled with an incredible display of power and tenacity are unique both in his purely theoretic work and applied fields.

It seems to me that Gauss' mathematical work is somewhat more remote from today due to the rapid development of mathematics and its applications both in means and in form about which Gauss could not have had any notion. However, the wonder as expressed by Courant had not disappeared at all. But how do the matters stand with regard to Gauss *as a human being*, to the topic which interests us now?

In 1955, he had still been inconceivable, unyielding to measurement by human scales. For a century he had remained a marble statue of a hero and emanated a sense of calm and superior composure without any human warmth, required respect. The sculptors of this cold statue belonged to the inner circle of Gauss' surroundings during his last two decades: the physicists Wilhelm Weber and Johann Benedikt Listing, the biologist Rudolf Wagner, the physician Wilhelm Baum, the orientalist and Gauss' son in law Heinrich Ewald, Gauss' youngest daughter Therese who had been keeping his house, but, first and foremost, the mineralogist and geologist and Goethe's godson Wolfgang Sartorius Freiherr von Waltershausen.

The role of the last-mentioned which he played in Gauss' surroundings can be compared with that of Eckermann in the life of Goethe. He carefully recorded the important statements and removed or omitted everything that did not seem to fit the flawless marble statue. Thus he at once became the main source for feeding the biographies of Gauss and will certainly remain as such.

Had a member of the circle mentioned above out of some subjective interest really intended to reveal to the public a human side of that statue, an instantly arisen mighty storm of protest would have quenched any such project. We cannot imagine that Sartorius lied, but neither did he state the whole truth and his omission of everything contrary to the notion of reverence coloured the picture. A few examples will elucidate that conclusion.

Contemporaries certainly knew about the conflict between Gauss and his colleague and former friend Harding, the discoverer of the minor planet Juno, who was appointed extraordinary professor and inspector of the Göttingen observatory two years ahead of Gauss, together with whom Gauss had worked for 27 years. Sartorius did not say a single word about that conflict, whose deeper causes we still do not exactly know, although it strengthened with time and even led Gauss to think about leaving Göttingen.

Just the same, Sartorius kept silent about both the temporarily threatened break of Gauss' engagement to his future second wife and the estrangement between Gauss and Bessel. Then, he presented the establishment of the so-called Göttingen Magnetische Verein (Magnetic Union) as the result of an *encouragement* by both Gauss and Alexander von Humboldt but he passed over in silence an initial considerable ill feeling between them. Indeed, Humboldt realized that in a short time Gauss had mastered both theoretically and practically *that*, which he was accustomed to regard as his own field of knowledge. In addition, Gauss certainly noticed Humboldt's inadequate expertise in his initial opinion about the new Göttingen facilities for observing geomagnetic phenomena.

Then, Sartorius completely left out Gauss' strong clashes with his elder son by the second marriage and his serious worries about the future of his second son by the same marriage, his ensuing excitement and fears which for a long time poisoned his life. Sartorius affirmed that Gauss' faith was *unshakeable*, but his conversations with Wagner show that Gauss *envied* those who were able to believe *right off the heart* and asked: *Tell me, how to begin*.

Sartorius testified that Gauss *could have doubtlessly become an excellent finance minister*, but did not justify his statement. In any case, having been indigent and frugally paid (to compare: Humboldt earned six times more), he made about 500,000 marks by buying and selling securities. By our present yardstick, and taking into account the purchasing power at those times, at the moment of death he thus became a millionaire.

In Göttingen, his skill in increasing his fortune was almost proverbial. This is known from Moritz Cantor, a historian of mathematics, who attended a Gauss lecture together with Dedekind in the winter term of 1850/1851[1]. For Sartorius such communications were too banal. He concluded that Gauss was an enemy of any miserableness and naturally overlooked a fact discovered in 1977 by my late friend, Dr Theo Gerardy, an honorary member of the Gauss-Gesellschaft: until an objection was raised after a financial check, Gauss had regularly paid his dues in connection with arc measurements and geodetic work in usual low-value coins but received his payments in gold[2]. From 1825 to 1827 he thus gained 230 thalers.

[2] By leaving out everything which Gauss showed as a human being with his contradictions, doubts and attempts, not free from his moods, sufferings and struggles, Sartorius erected a monument to an *iron* Gauss which for a hundred years decisively determined the judgement of the posterity.

We also ought to consider that, during his later years in Göttingen, Gauss was high above his circle of friends. Witness Wagner:

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

For his part, Sartorius tells us:

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

These descriptions are surely somewhat prettified. However, in 1976 the surely highly impressive Gauss was turned into *an aging, shorty and somewhat stout man, a very German professor with many prejudices*. Then, in 1954 the conditions of his life were described as *meagre* with him only getting rid *of oppressive money troubles at a venerable age*. These descriptions are just delusions. I do not dwell on them since they had not practically speaking influenced our portrayal of Gauss.

But it should be remarked that it was not only his Göttingen circle of friends who recognized Gauss' absolutely special position. There rather was only quite a small number of eminent scientists in the history of science who, like Gauss, enjoyed recognition as such by their contemporaneous professional colleagues. Olbers affirmed that, had Gauss managed to come to Paris, he would be received better than *any scientist was until now*[3]. In Napoleon's outer office Lindenau heard that only Gauss would have been named a successor to Lagrange.

And Lagrange himself stated that the young Gauss had *by a single spurt raised himself to the rank of best mathematicians*. Laplace talked about *an unearthly spirit in a human body* and Bessel cried out: *What a day is coming, so Sie es wollen*!

In the letters of his learned friends we find a wealth of expressions of wonder at the *incomparable genius*, at his *not yet attained* [by others] *perfection,* at the *mathematical giant.* It was believed that he seemed to belong to *superior beings*, he was called *the master* of all professional colleagues. Already in 1804 Humboldt stated that only *one* person called Gauss can impart new lustre to the Berlin Academy[4].

The King of Hannover approved the legend on a medal commemorating Gauss that called him Princeps, Prince of mathematicians. These words were not recommended by a competent counsellor; quite simply, they were in keeping with the general conviction[1].

So how can we blame those who, due to the superiority of the revered man, are hindered from decreasing the distance to him, and wish to delete everything from the image of their hero that according to *their* yardstick can darken it? However, and we should not overlook this circumstance, the glasses through which we see Gauss have been manufactured not only by Sartorius and the circle of Gauss' friends, but also by Gauss himself. Conscientiously or otherwise, he powerfully assisted them, and I will prove it.

[3] In the second part of the biography of Gauss written by Sartorius, we find numerous verbal expressions exactly repeated in Gauss' letters, and here are a few examples.

As stated by Sartorius,

Although Gauss perhaps trusted analytical calculus more than any living person, he was considerably ill-disposed towards its mechanical applications of any kind. He attempted to restrict its use as much as it was possible under the circumstances. He often told us that he never takes up his pen for calculations until he completely solves the problem mentally. For him, analytical calculus only appeared as an aid which he uses when completing the task.

On 31 Dec. 1831 Gauss wrote Olbers about the same topic:

For geometers of the first rank calculus always is just the clothes in which they show what they had obtained not by its help but by meditation on the essence of the matter.

And in a letter to Schumacher:

I require that by each application of the calculus or notions one should always retain in mind the initial conditions and never consider the outcome [alle Produkte des Mechanismus] *exceeding his obvious right as his own property*.

Another example is provided by statements about the *antieuclidean geometry* as recalled by Sartorius on the one hand and by Gauss' letters to Gerling and Bessel on the other hand. Significant is also the coincidence of the report made by Sartorius about Gauss' interest in the mortality of babies and old men and his letter to Humboldt which discusses the same subject in about the same wording[5]. It is highly unlikely that Sartorius had seen that letter (only published in 1965) and neither did Gauss compile any summaries. This astonishing coincidence can only be explained when assuming that Gauss had insistently expressed the same thoughts in conversation and in the letter and that Sartorius made very conscientious notes.

Other cases of such facts can be provided, for example the coincidence of the motives for Gauss' study of the Russian language in his letter to Schumacher of 17 Aug. 1838 and in a communication by Sartorius. Or take the statement of Sartorius, *The thirst for truth coupled with a sacred drive for fairness mostly describe Gauss' noble nature*, and, on the other hand, the letter to Steinheil of 16 March 1836: *My theory is certainly dear to me, but infinitely dearer is the truth*.

[4] The often amazing statements more of which can be provided leave no other possible conclusion except believing that Gauss had verbally *and* in writing repeated definite maxims and reflections which especially captured his imagination to ensure their dissemination. I have long ago become convinced in that Gauss wrote and spoke for posterity just as, for example, Goethe and Wilhelm von Humboldt did. Understandably, he regarded these utterances as *publications*. With justified self-confidence he wrote to his publisher

Perthes[6] about his *Theoria motus*: *It will be studied in* [a few] *centuries as well*. And not without pride he remarked to Sartorius about his *Disquisitione arithmeticae* of 1801 that *it belongs to history*.

And when drafting his letters he thought not only about the recipients but about his future readers as well and almost always checked himself. We can infer this from his letter to his intimate friend Schumacher when this latter corresponded with him about the envisaged but only 35 years later brought about publication of his correspondence with Bessel. On 30 May 1846 Gauss explained:

Only suppress that which can harm some living people, and it becomes possible to publish all the rest so far as it is of some interest.

However, he wished to omit *one* letter from Bessel. It was exactly such that did not fit the picture of himself which Gauss intended to sketch and preserve. It concerned the question as to whether Gauss reasonably published only quite perfect materials or whether he could have greater contributed to the development of mathematics by lowering his requirements for the preparedness of manuscripts and publishing ideas even when they were not fully ripe and thus stimulating and supporting contemporaries.

Gauss became most highly annoyed and wished to omit the whole letter. Exactly this case can prove how Gauss himself took care of propagandizing his point of view. I am also thinking about letters which he wrote to Schumacher and his previous student, Encke. There he attempted to explain insistently not only them, but later readers as well, why does he hate overhasty publications and only intends to make known ripe materials, is not prepared to provide building blocks but prefers to erect finished structures. Such work requires *very* much unappreciated time, and he puts up with the ensuing delays and the danger that others will overtake him or that much will be lost after his death. His motto is: provide something perfect in essence by tying together the derived insights – or nothing at all[7].

Even much earlier Gauss had considered it very important to clarify his point of view. Indeed, in September 1814, during a trip to Seeberg near Gotha, he spoke to Encke who recalled that conversation more than 20 years later:

You had then explained your method of work and therefore did not approve of Euler's attitude. He published the results of his reflections perhaps just as they had first presented themselves and only remarked that he will repeatedly and often return to them. On the contrary, you always intend to attain perfection and intrinsic satisfaction both in essence and form.

It is therefore understandable how offensive was for him Bessel's rhetoric question of 28 June 1839:

Would have Euler achieved as much as he did had he published only a tenth of the great number of his ideas in an irreproachable form rather than the whole lot of them?[8]

It is not necessary to stress that Sartorius precisely described Gauss' efforts to attain perfection as he formulated it in his letters.

[5] I summarize: Gauss knew that Sartorius, not being a mathematician, will record his main statements and was convinced that his letters will sometime be published. He therefore took care that, by laying almost the usual stress in conversation and insistently emphasizing in his letters those principles will be preserved which he thought undoubtedly deserving to be saved. And he himself thus assisted in sculpting that statue which had been appearing to the amazed posterity for about a hundred years and presented an idealized hero, a *mysterious and incomprehensible* superman rather than a human being.

After those hundred years clear signs of a change of our perception of Gauss became visible. It occurred that an *iron* Gauss, a *bronze block* are out of the question. Gauss was rather extremely sensitive, influenced by his moods, doubting, seeking, not rarely suffering, sometimes however cheerful man (which Sartorius had not passed over in silence).

To be sure, some documents which had been known earlier did not quite well fit Sartorius' frames and, moreover, they were written by Gauss' most intimate friends. I recall for example a remark made by Olbers in his letter to Bessel of 25 Jan. 1825 about the abovementioned restrictions on the publication of letters. Gauss highly appreciated him both as an astronomer and a human being, and, for his part, Olbers had many times showed his high opinion of Gauss by many deeds rather than words. And this is what he wrote:

It seems that Gauss invariably wishes to be the first to pick the best fruit to which his discovered and paved path led him before showing them to others. I think that this is a slight weakness of a man otherwise so great, the less to be explained since he had favoured us with so much from his inexhaustible riches in ideas.

Such mostly affectionate criticisms which reappear on occasion or other testimonies, for example, Gauss' startling weep over his first wife, some letters to his sons by the second marriage which became known were simply disregarded or remained ineffective since they did not fit the Sartorius' picture. But in any case such facts multiplied until about 1955 a change had emerged. In such cases it is always difficult to set an exact date. I believe, however, that the beginnings of the great and not yet completed change can be fixed at about that mentioned year, 1955. It was then that two editions of the renowned biography of Gauss (Worbs 1955) had appeared. There, for the first time, his depression was discussed[9] and a note hidden in his mathematical records was published: *Death is preferably for me to such a life*. It was like a thunderbolt destroying an idyll, it was simply impossible to reconcile such a change with the then current picture of an unshakeable Gauss.

I am unable to present here in detail the new aspects discovered in the investigations concerning Gauss since 1955, and I restrict my description to some main points.

[6] The appearance of supplements to the previously published correspondence of Gauss with Gerling and Schumacher allowed the abovementioned Theo Gerardy to illuminate clearly a chapter in the life of Gauss which had previously remained largely in the dark. He described the disturbed relations of Gauss with both sons by the second marriage.

I have already said that even earlier some published documents made some conclusions possible but that these were not used. Gerardy showed how Gauss, helpless and confused, had to apply to his friends from other cities to settle problems which only properly concerned his family. How, first of all, his former student, the physicist Gerling from Marburg, weakened the panic by a sober and objective consolation, qualified the significance of the problem by life experience and showed the way to solve it by practical advice.

This way was indeed chosen and both sons acquired a possibility to prove their worth in the USA, to show that they are not at all *lost*, as Gauss initially thought at least about the elder son, Eugen. He was undoubtedly the most gifted of all Gauss' sons and the only one who inherited his father's visual perception of numbers.

Theo Gerardy quite justifiably summarized:

The relations between Gauss and his sons show a picture somewhat different from the heroic image which is described in his biographies. Only externally he is unshakeable and unapproachable; actually, however, easily hurt and then virtually helpless. Except [the possible case of] *rapid and clear decisions, in such situations which only properly concerned the parents, he has to ask advice from his friends. He cannot take advantage of his position for paving the way for his sons, he was loath to soliciting. He treated them justly and thoughtfully, but we can seriously doubt whether he loved them as much as his daughters*. *His thoughtfulness, fanatical striving for truth and* [possible] *opinions of the outsiders from his social surroundings could have robbed him of sympathy for the humanly forgivable weaknesses of his uncontrollable but in essence worthy and kindred son Eugen*[10].

Another essential cause for modifying our understanding of Gauss was the finding and use of the relevant correspondence of his friends about which I have reported [iii]. The letters gained the access to an almost unknown previously side of his nature: on the dependence on his moods. In his own letters, he often stressed his need for both cheerfulness and steadiness in his relations with others and judgements. He essentially depended on his mood which in turn was determined by external circumstances, but this was not taken into account in spite of his occasional statements that *We govern over our actions but not over the effects of life conditions on our soul*.

Family discord and illnesses, the need to decide his future, the appearance of sudden events, all kinds of deadline pressure, the duty to read lectures to ungifted students, hot or stuffy weather, − all this unfavourably acted on his mood. It cannot be doubted anymore that Gauss was a person influenced by circumstances rather than a hero, untouchable and existing above the everyday life as described by Sartorius.

Schumacher, probably his most trusted correspondent, knew well enough that association with Gauss without acquiring a foul mood was only possible for those who were able to remain exactly within the boundaries of usual politeness. And Schumacher came to understand that Gauss is *A queer sort of a fellow* [written by Schumacher in English – O. S.] *and somewhat more of an egoist than necessary for a pleasant contact, but at the same time he is exceptionally honest and* *incapable of any mean slyness or evasion*.

Gauss can be kindness itself*, although not often. In conversation he never argues when being in the right, but applies the entire art of dialectics to defend a wrong proposition which he had stated*.

I stress once more: all this comes not from someone of whom Gauss had disapproved, but from Schumacher, from a man whom he deeply respected and who remained as near to Gauss as hardly any other contemporary. Schumacher communicated his judgement to Bessel who confirmed it by his own experience. And the last conversations of Gauss with Rudolf Wagner published in 1975 was a step in the direction of a new portrayal of the former. They show Gauss as a weak man who had to attempt to keep cool under affected calmness shown to the outer world.

When the picture of Gauss is thus corrected, much of what seemed mysterious becomes clear. For example, *the contradiction* between the startling weep over the loss of his first wife and the new marriage contracted only ten months later. *The contradiction* between his melancholy mood after the death of his second wife and the disappointment over Eugen and the picture provided by the sister of Wilhelm Weber shortly afterwards. She described Gauss as a cheerful and almost lively person.

Justified become the words of Alexander von Humboldt, *For a free and agile nature like that* [of Eugen] *coexistence with Gauss was not as easy as desired. The contradiction* between Humboldt's judgement about Gauss as an *intolerantly sensitive,* and a *scientific despot* and, on the other hand, as *a fully warm-hearted softie*. Or *the contradiction* between the feeling for fairness and, as Jacobi once overstated, Gauss' habit of saying nothing about either the dead or the living[11].

[7] Allow me to mention two more inclinations of Gauss upon which new light has been thrown during recent decades. I bear in mind Gauss' tendency towards encoding both the achieved conclusions and minor matters, and on the other hand towards recording numerical results even of non-scientific origin. Both inclinations sometimes manifested themselves at the same time.

Thus, the probability that some outsider in Braunschweig was able to gain an insight into the number-theoretic findings of the young Gauss was practically zero and it was just as low concerning the significance of the count of steps from Braunschweig to Helmstedt (once Gauss counted 45,053 of them). The only reason for encoding both events was Gauss' pleasure in his game [with numbers]. He was a *homo ludens*, a playing man delighted by even useless games with numbers, delighted *to act as though* someone was hunting for his newest discoveries and as though he ought to prevent their efforts by encoding.

Being 25 years old, Gauss himself, in a letter to Franz von Zach, admitted that he was a *lusus ingenii* (an inborn player) and 45 years later he wrote to his intimate friend Schumacher:

In general, I am lenient with imagined games. […] *No, I do not deny that I sometimes amuse myself in a similar way but I will never publish anything of that kind*.

When Gauss recorded in how many thousands of numbers he had counted the number of primes during a day, he encoded not only the result, but also the relevant dates so that it certainly was an amusing game. When he also noted the number of weekdays on which he counted those primes and encoded those days by numbers 0, 1, …, 5, 6, it was a game just as well. In addition, he assigned number 1 to Wednesday possibly because he first saw the light of day on a Wednesday.

Allow me to insert a word about how I became able to decipher the encoded dates. It was known that Gauss congratulated Humboldt on his 30,766 day of life, or at the age at which Newton had ended his *terrestrial career*. I knew that Gauss could have represented dates by numbers, so I began to check whether they can conceal a date. Soon I struck gold. I came across the number 7219 and established that the date on which Gauss defended his dissertation, 16 July 1799, was exactly 7291 days after his birth. Any residual doubt has therefore disappeared: Gauss had indicated number 7291 or 99-VII-16 and added the letter D (doctor).

Soon afterwards Dr. Gerardy sent me a reproduction of a handwritten table which Gauss had inserted in a table of logarithms and called it *Count of days*. For non-encoded numbers, such as 1777 April 30 (Gauss' birthday) this means that, for example, 64768 = 4 Wednesday (here, Wednesday was not denoted by 1 so that Gauss was not consistent in such things[12].

So here was a Rosette stone of sorts, a bilingual concordance. The table provided the number of days which passed from Newton's birth to given dates. I am sure that many numbers in Gauss' posthumous manuscripts which have nothing in common with the mathematical contexts are actually dates. Thrifty Gauss put them into printed texts and thus economized on the relatively expensive writing paper. Why did he indicate this or that date is certainly not easy to determine, and furthermore it will be necessary to establish on which weekday their count began with a zero.

I cannot here describe the decoding of *combinations of letters* and restrict my account by referring to my relevant contributions[13] in which I had indicated in detail Gauss' pleasure of encoding. However, I would like to present a typical example of his table of numerical results, a reproduction of a page from Gauss' *Mathematical Diary* [omitted in translation], Nieders. Staats- und Univ.-Bibliothek Göttingen. Code Ms. Gauß Math. 48Cim.

I believe that this page is suitable for clearly showing us Gauss' pleasure of playing [with numbers]. There are grounds for stating that it was mainly written before the autumn of 1799, but it also includes insertions dating back to 1784 as well as later additions up to 1808. At the top of the page we find information about a walk from the gate […] to gate […], a table showing the times of day and therefore the time required for that walk. Under that table are some numbers, the letter B and fragments of two words. Then follow the words *Newton's Epitaph* and two lines by Pope:

Nature & nature's laws lay hidden in night. God said, Let Newton be & all was light.

So early had Gauss' admiration for Newton been manifested. […]

At the left margin there is a table compiled on 6 April 1801 which indicated the time required for a walk from […] in Braunschweig to […]. We see that Gauss was a fast pedestrian who did not shuffle his feet, his marching speed was about 5.6 *km/h*.

In the middle of the page there is a table showing the *rounded off distance* counted in steps (1 step ≈ 0.75 *m*) from Braunschweig to Helmstedt separated in eight intervals, probably estimated or reckoned by a map. (His exact *count* of those distances in steps is on another page.) […]

Also in the middle of the page there is a table providing the mileage of the trips until the autumn of 1799 separated into walks, trips in waggons and on horseback […], 239 miles in all. […]

[8] So what is new, where can we see the elements of the changed understanding of Gauss? We see now a human being experiencing pleasure in playing [with numbers] but not as a superman. Only now his motives and actions became clear, but had it not diminished his greatness, or the fascination he holds for us? On the contrary. The admiration for his achievements which to a large extent depended on the atmosphere surrounding him, only strengthened since now we know and understand that he compiled his immortal contributions under circumstances which, according to his feelings, did not at all foster mental efforts. Hard work under hindering circumstances constituted a considerably greater part [in achieving success] as compared with brilliant intuition than it was admitted previously.

Indeed, the vulnerable, receptive, sensitive man had to wrestle not only with those unfavourable conditions but with himself just as well. The Gauss biography compiled by Sartorius will always remain a valuable primary source but it ought to critically used and supplemented by other sources.

This is especially true regarding the interpretation of Gauss' political views. Only weak initial signs of a new understanding have emerged. Until this day there dominates a conviction based on the report of Sartorius that Gauss, having been inspired by the demand of the Duke of Braunschweig, was (became?) a conservative and disliked any changes.

[9] A minor sensation occurred when some years ago it was discovered that two men from Gauss' immediate surroundings, whom he wholly trusted, namely his mechanic [specialist in astronomical and geodetic instruments] Moritz Meyerstein and his colleague and former student Moritz Abraham Stern, belonged to the circle of friends of Paris left radicals.

And now Gauss' statement of 20 April of the revolutionary year 1848 in a letter to Bolyai, a friend of his youth, can be seen in a new light:

The powerful political and social earthquake which extends ever wider and overturns every European custom (until now your fatherland understood in a strict sense, I mean Transilvania, is not yet affected). *Nevertheless, I confidently feel that after all pleasant fruit will appear, but the transitional period will at first cause much distress and* (*quod tamen deus avortat* [God forbid]) *can last a long time. At our age it is always very doubtful whether we will live to see* *the Golden Age*.

How to explain this statement so strikingly contrary to his other stock remark handed down to us about revolutionary upheavals? Should not some traces of Gauss' discussions with Stern and Meyerstein be seen here? At present, we can only raise this question without answering it.

A detailed study of the life conditions of Gauss requires considerably more *knowledge about his companions*. In this connection I would especially like to recall the contribution of the members of the Gauss Gesellschaft and first of all I name the regretfully already late Martha Küssner, Horst Mischling and Dr Gresky.

I allow myself to adduce *one* example[14]. We know from Sartorius that Gauss had reproached Goethe for want of principles and ideals and did not appreciate too highly his *lyrical poetry*. I can show that that low appreciation had to do with a similar opinion of Goethe about Gauss. Indeed, when in 1817 Goethe had revised the comedy *Die Bestohlenen* (The Robbed) by August von Kotzebue for the stage he changed a place in the text in which Leibniz and Gauss were mentioned on a par. Kotzebue wrote: *Had you ben as learned as a Leibniz or a Gauss*, but Goethe's ill humour about Gauss' silence over his theory of colours prompted him to replace Gauss by Kant: *Had you been as perfect as Leibniz and as great as Kant* (*Goethe's Jb.*, Bd. 92, 1975, pp. 195 – 219, see p. 204)[15].

Future investigations of the great mass [of unpublished statements and letters] kept here in Göttingen will certainly further change his portrait. I am convinced that a new approaching understanding of Gauss will be deeper, more objective and more appropriate that the conventional hackneyed respect due to a hero. Theodor Fontane in vail warned contemporary biographers against *beautifying forever*. Nevertheless, much was irrevocably lost with Gauss' death as he himself prophesied in 1832. But even now we can safely say that our admiration for that outstanding genius, analytical power, for his purposeful persistence, his use of mathematical experiments[16], his intuitive discovery of hidden connections and applications, as well as for his ensuing deepness and versatility, − that everything mentioned will remain eternally.

Notes

1. See Dedekind (1901/1933, p. 305) who described Gauss' lectures: *Especially clear description of the development of the main notions and main propositions of the calculus of probability*. And here is May (1972, p. 307, left column): *Teaching became less distasteful* [for Gauss], *perhaps because his students were better prepared and included some, such as Dedekind and Riemann, who were worthy of his efforts*.

2. I do not know anything about those dues.

3. In a letter to Gauss Legendre called himself the inventor of the method [of the principle] of least squares since he was the first to publish it. Gauss did not reply and the much older Legendre became indignant, mostly because of that silence. After that, French mathematicians dealing interested in the treatment of observations including Poisson (but not Laplace), to their own detriment, started to ignore the relevant work of Gauss. Reich (1996) stated however that at least from 1836 this attitude had changed. Legendre died in 1833. On 17 Oct. 1824, in a letter to Schumacher Gauss wrote: *With irritation and distress I* […] *read that the old*

Legendre, an ornament to his nation and his time, was deprived of his pension.

4. Earlier noticed by Dunnington (1955, p. 348). The commemorative medal was issued just after Gauss' death. Its inscription read (in translation): *George V, King of Hanover, to the Prince of mathematicians*. During 2005, a century and a half after Gauss' death, the newspaper *Göttinger Tagesblatt* published 49 popular articles about Gauss and his works, then issued all those articles as a booklet called *Mein Gauss* (published by Gauss-Gesellschaft E. V., the place and year of publication apparently Göttingen, 2005 or 2006). I can only say that this booklet, if only obtainable, deserves to be scanned through. I am grateful to Professor Ulrich Krengel for sending me a copy. Stamps commemorating Gauss were issued at least in Eastern Germany and in the united Germany. There also appeared a commemorative five-mark coin and a 10-mark banknote.

5. See [v].

6. Usually only Perthes is called the publisher of the *Theoria motus*, but Biermann [v, § 1] correctly named both of them: Perthes and Besser.

7. In a letter to Olbers of 30 July 1806 Gauss stated that his motto was *aut Caesar, aut nihil*.

8. Recall Gauss' earlier enthusiastic appraisal of Euler's achievements [i, The Gauss text].

I am not satisfied with Biermann's conclusion about the attitudes of Gauss. *First*, Gauss had published two classical contributions, the *Disquisitiones arithmeticae* of 1801, and *Theoria motus* … of 1809, both of them perfect or almost so, in form and essence, so he was probably quite unwilling to lessen his standard. Indicative is his explanation (1807, p. 161) of delaying the latter:

Many esteemed astronomers insistently asked me to publish the method that I had applied [for rediscovering, in 1801, the minor planet Ceres], *but* [various circumstances] *as well as my intention to treat this matter in detail and my hope that further studies* […] *will offer an opportunity to bring various parts of the method to a higher degree of perfection, generality and ease, are the causes why I am only now satisfying those friends.*

It is worth noting that Gauss had certainly encountered difficulties in translating his text from German into Latin. Indeed, much later, when preparing his Latin memoir of 1823 for publication (and calling it by its finally abandoned title *New justification of the method of least squares*), Gauss (G – O, 14 Apr. 1819) remarked that *The brittle Latin language often resists natural effortless expression of thoughts.*

May (1972, p. 309, right column) inconclusively stated that Gauss *did have high standards* but published *all that was ready for publication by normal standards.* Anyway, discussing Gauss' memoir (1823), Stewart (1995, p. 222) reasonably decided: *It requires great generosity on the part of the reader to conclude that he actually proves anything* [in his §§ 12 and 13].

In § 7 and in the beginning of § 8 Biermann concludes that Gauss experienced pleasure in playing [with numbers]; elsewhere [v] he added that playing soothed him. I venture to suggest that by introducing numbers he transferred irregularity into order (his counts of primes or of people struck down by lightning [v, § 2]), and order, perfection was what he wished to see in his manuscripts. Finally, he valued harmony in the results of geodetic measurements (Gaede 1885, p. 180).

These considerations stress the *otherwise* in Biermann's statement that Gauss had *conscientiously or otherwise powerfully assisted* in portraying himself as a marble statue. Then, Biermann [v] reasonably remarked that, when collecting scientific or even useless data, Gauss attempted to order apparently random occurrences. This circumstance could have strengthened his desire for perfection.

Certainly, however, that Gauss was a *scientific despot* (Humboldt, end of § 6), a *scientific aristocrat* (Biermann [iii, § 8]), a *crass egoist* (Bessel [iii, § 5]). Indeed, how else can we explain his inhuman demand imposed on his sons (Note 10)? Recall also Note 7.

Second, below, in § 9, Biermann notes that Gauss eagerly wished that the revolution of 1848 will eventually bring about the Golden Age, that he was not a conservative at all (as stated by Sartorius). Here is an unjustified contradiction (perhaps issuing from Sartorius as well): *During the revolution of 1848 Gauss stood guard with the royalists* (May 1972, p. 307 left column). And (May, p. 309 left column) Gauss was *hostile or indifferent to radical ideas in mathematics as in politics.*

Concerning mathematics, I adduce a sudden comparison of Gauss with Chebyshev (Novikov 2002, p. 330):

Endowed with a brilliant analytical talent, he was a pathological conservative. He *scornfully spoke about the newfangled disciplines like the Riemannian geometry and complex analysis*.

9. Klein (1926, pp. 11 – 12) noted that Gauss had sometimes suffered from morbid depression. Bashmakova et al (1978/2001, 51) quoted a comment on Gauss' *Mathematical Diary* from Klein (p. 33):

Here we see not the inaccessible, closed, cautious Gauss as he appears in his published papers. Here we see what Gauss was like when he experienced and conceived his great discoveries. He expresses his joy and pleasure in the liveliest manner, bestows laudatory epithets upon himself, and shows his mood in enthusiastic exclamations.

10. Gauss's sons *reported that he discouraged them from going into science on the ground that he did not want any second-rate work associated with his name* (May 1972, p. 308 right column). An inhuman demand!

11. However, Gauss highly appreciated Jacobi (and Dirichlet, to whom he had not referred either). He attributed to the former *sagacity, penetration and elegance*, see his letter to Crelle of 1828 as reported by May (1972, p. 304 right column). He was also much impressed by Dirichlet's *eminent talent*, see his letter to Encke of 8 July 1826 as reported there also. Now, Gauss *wrote and spoke for posterity* and regarded these utterances as publications (Biermann, the very beginning of § 4).

Nevertheless, *Gauss typically acknowledged the help of Weber* [in compiling an important contribution on terrestrial magnetism] *but did not include him as joint author* (May 1972, p. 305 right column).

12. Biermann had decoded some notes written by Gauss, and now, in turn, readers should decode his description. The number 64,768 is the number of days from the introduction of the Gregorian calendar to the birth of Gauss [v, § 5].
The Rosetta Stone enabled to decode ancient Egyptian hieroglyphs since its inscription also contained the text in ancient Greek (and a Demotic script as well).

13. See the very beginning of § 1.

14. Goethe was not a companion of Gauss (cf. above).

15. *Leibniz and Kant* seems more proper than *Leibniz and Gauss*. Then, Biermann did not prove that Gauss knew about the described episode. Finally, May (1972, p. 307, right column) stated that Gauss had a *rather narrow cultural outlook* and that (p. 309, left column) *did not care for Byron or Shakespeare* […], *disliked Goethe and disapproved of Schuller*.

16. Biermann mentioned mathematical experiments in § 2 as well, but he probably meant empirical calculations.

Brief Information about Those Mentioned

Eckermann Johann Peter, 1792 – 1854, Secretary of Goethe and his friend

Encke Johann Franz, 1791 – 1865, astronomer

Ewald Heinrich, 1803 − 1875), orientalist and theologian, Gauss′ son in law

Fontane Theodor, 1819 − 1898, writer

Harding Karl Ludwig, 1765 − 1834, astronomer

Kotzebu August von, 1761 – 1819, playwright, writer

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

Listing Johann Benedikt, 1808 − 1882, physicist

Meyerstein Moritz, constructor of optical instruments

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

Steinheil Carl August von, 1801 − 1870, physicist, inventor, astronomer

Stern Moritz Abraham, 1807 − 1894, mathematician

Wagner Rudolf, 1805 – 1864, physiologist, anthropologist
Weber Wilhelm Eduard, 1804 – 1891, physicist

Bibliography

Bashmakova I. G., Rudakov A. N., assisted by A. N. Parshin, E. I. Slavutin (1978, in Russian), Algebra and algebraic number theory. A chapter (translated by A. Shenitzer & H. Grant or either of them) from *Mathematics of the 19th Century*, vol. 1. Editors, A. N. Kolmogorov, A. P. Youshkevich. Basel, 2001, pp. 35 – 86.

Biermann K.-R. (1978), Gauß als Personlichkeit. *Abh. Akad. Wiss. DDR,* No. 3, pp. 39 – 49.

Dedekind R. (1901), Gauss in seiner Vorlesung über die Methode der kleinsten Quadrate. *Ges. math. Werke*, Bd. 2. Braunschweig, 1931, pp. 293 – 306.

Dunnington G. W. (1955), *C. F. Gauss, Titan of Science.* New York. [Math. Assoc. America, 2004.]

Gauß C. F. (1807), Deutscher Entwurf der Einleitung zur *Theoria motus* … *W*-12, pp. 156 – 162.

--- (1823, in Latin), Theory of combination of observations least subject to error. Latin and English. Translated with Afterword by G. W. Stewart. Philadelphia.

--- (1863 − 1930), *Werke,* Bde 1 − 12. Hildesheim, 1973 – 1981.

--- (1975 – 1987), *Werke*, Ergänzungsreihe, Bde 1 − 5. Hildesheim.

--- (1976), *Gauss' mathematisches Tagebuch*. Leipzig. Ostwald Klassiker, No. 256. Leipzig, 1985.

Gaede (1885), Beiträge zur Kenntnis von Gauss′ praktisch- geodätischen Arbeiten. *Z. f. Vermessungswesen*, Bd. 14, pp. 113 – 137, 145 – 157, 161 – 173, 177 – 207, 225 – 245.

Gerardi T., Editor(1964), *Ch. L. Gerling an C. F. Gauss*. Göttingen.

--- (1969), *Nachträge zum Briefwechsel zwischen C. F. Gauß und H. Ch. Schumacher.* Göttingen.

Klein F. (1926), *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert*, Tl. 1. Berlin.

May K. O. (1972), Gauss. *Dict. Scient. Biogr.*, vol. 5, pp. 298 – 315.

Novikov S. P. (2002, in Russian), The second half of the 20th century and its result: the crisis of the physical and mathematical community. *Istoriko-Matematicheskie Issledovania*, vol 7 (42), pp. 326 – 356.

Reich K. (1996), Frankreich und Gauß, Gauß und Frankreich. *Berichte zur Wissenschaftsgeschichte*, Bd. 19, pp. 19 − 34.

Stewart G. W. (1995), See Gauss (1823).

Worbs E. (1955), *C. F. Gauß. Ein Lebensbild*. Leipzig.

V

Kurt-R. Biermann

An inborn player in the scientific work of C. F. Gauss

Lusus ingenii im Schaffen von C. F. Gauss.
Mitt. math. Ges. Hamburg, Bd. 12, No. 2, 1991, pp. 329 – 346

[1] *A spectacular whim of fate*, as Gauss had remarked on occasion, is that the observatories in Göttingen and Altona are situated *on the same meridian to less than the width of a house* [ii, Intro.]. And a similar whim, as I may say, is that I found out by chance that Gauss was a member of the Mathematical Society of Hamburg [1, p. 8] at the same hour as I received the honourable invitation to report on a subject of my research at the tercentenary of this Society rich in tradition.

For more than 30 years I have been studying the work of Gauss so that that chance coincidence led me to write about the Prince of mathematicians. Quite apart from the fact that the subject *Gauss* can always count on attracting the attention of mathematicians, be they more or less historically minded, the Prince had maintained many-sided relations with Hamburg and Altona.

First of all I should perhaps mention that Gauss' main astronomical contribution, the *Theoria motus*, was published by Perthes & Besser in Hamburg. It was in Hamburg that Gauss had come to his first thoughts about inventing the heliotrope for reflecting sunlight and thus for serving as a sighting target for geodetic measurements. In October 1818 , in Lüneberg, Gauss noticed that *the* [western – K.-R. B.] *window of the uppermost gallery of the Michaelis tower illuminated at that moment by the Sun was seen as a shining ray of light* [2/4.2, p. 47].

In 1821 Gauss had seriously thought about becoming director of the new observatory in Hamburg [2/4.2, p. 81]. Nothing came out of it actually for the same reason that his repeated intention to be invited to Berlin did not realize: those responsible hesitated and economized. Edmund Landau stated on occasion that the mathematical centre of the German language area was situated *in the triangle Göttingen – Berlin – Hamburg* [3, p. 202]. Allowing a free rein of imagination, we may speculate how would that centre shifted had Gauss really moved to Hamburg.

We should also mention Gauss' visits to Hamburg and Altona, and first of all his correspondents there certainly beginning with his closest friend, the Altona astronomer Heinrich Christian Schumacher, the founder and first editor of the still existing *Astronomische Nachrichten*. He had also played a certain role in the history of mathematics as well. We may recall that the dramatic contest between Abel and Jacobi in the construction of the theory of elliptic functions was partly held on the pages of that periodical.

It is also possible to mention Repsold, the highly esteemed by Gauss manufacturer of astronomical instruments. He was also the head of the city fire brigade and lost his life in the great fire of 1830. I can, but will not also name half a dozen other correspondents from Hamburg and Altona, but the mentioned above is sufficient proof for having good grounds to report about Gauss here and today. From the almost inexhaustible for a historian range of topics relating to Gauss I have chosen a problem area which can most of all excite listeners. Namely, I will deal with two Gauss' inclinations both of which seem playful: a predilection for recording numerical outcomes and, until age 40 or thereabouts, a tendency to encode his results.

[2] Gauss – a playing man? This will astonish those whose image of Gauss was formed from the picture systematically drawn by his trusted friends and companions in Göttingen and consciously, as I have shown [4, p. 44], assisted by himself. Such inclinations do not at all fit that picture of a *bronze block* [5, p. 45][1].

Incidentally, at age 25 Gauss himself admitted playing with fiction when in 1802 in a letter to Franz von Zach he stated about the so-called Titius – Bode law on the mean distances of the planets from the Sun [6, p. 504 (Gauss); p. 444 (Humboldt (1850)]: *It should not be disapproved at all when such approximate coincidences are searched for in nature. Greatest men of each time have indulged in such approximate coincidences*.

And even 45 years later he [2/5.5, p. 394] wrote to his intimate friend Schumacher:

In general, I am lenient with imagined games. […] *No, I do not deny that I sometimes amuse myself in a similar way but I will never publish anything of that kind*. *To such amusements belong for example my thoughts about the inhabitants of celestial bodies*.

However, I will dwell not on such imagined games concerning astronomical matters, but on his records and encoding as a means of unwinding and relaxation. Four categories ought to be discerned in his numerical tables and records. At first I should mention the results of observation which Gauss applied *for inductively discovering arithmetical relations* [7, p. 5), for example the table of the frequencies of primes, of *cyclotechnie*[2] and for decimalizing fractions [Gauss, *W*-2, pp. 435 – 443, 477 – 496 and 411 – 434].

A riddle is contained here: how could have the young Gauss revealed *concealed connections without some theoretical viewpoints* [7, p. 66], or [8, p. 37] *fish out from time to time number-theoretic theorems from the great pond of his tables*?

For Gauss, compilation and effective arrangement of his auxiliary tables for rapidly checking calculations [8, p. 44] was a point of taste and aesthetic pleasure. In a lecture on the method of least squares he stated half in jest that there is *certain poetry* in compiling tables of logarithms [9, p. 444], and even prolonged adjustments of geodetic measurements provided him satisfaction [2/1, p. 412].

After tables of wide number-theoretic interest we should mention Gauss' records of numbers intended to open up new applications for mathematics[3] [10, p. 89]. Even in 1802, being 25 years old, Gauss expressed his hope for editing censuses, of data on births and deaths in Braunschweig both for his own *pleasure* and for becoming *useful* [2/4.1, p. 106]. Later he compiled tables which should have served for discovering regularities in the mortality of infants and people of extreme old age [10, p. 89]. In a letter to his friend Humboldt of 15 April 1846 [11, p. 95] he wrote:

Had I been a Rotschild, I would have donated a million with the interest being yearly distributed among 400 oldest inhabitants of a large country under the condition that their age and life be most perfectly studied.

In the same letter Gauss also stated that for him exact and detailed statistical data on the mortality of babies would have been *something just as* (*or much more*) *interesting as the determination of a new planetary orbit.*

And when for many years Gauss had recorded the number of aces dealt out to gamblers in each set of whist in which he himself had participated [9, p. 444], it was his intention of checking the coincidence of frequency and probability.

His record of storms [10, p. 89] was compiled owing to his intention of discovering regularities in seeming disorder. A record of the monthly receipts of the Hanover railroads (Ibidem) and the daily reading of home and foreign newspapers for registering the fluctuations of the prices of securities [9, p. 444] was founded on his aspiration to subject conjunctures and crises to calculus. We know how successful Gauss was in his studies of booms and slumps from the fact that, initially indigent and always frugally paid (to compare: Humboldt earned about six times more), he made something like 500,000 marks [12, p. 237]. By our present yardstick, and taking into account the purchasing power at those times, at the moment of death he thus became a millionaire many times over. In Göttingen, his skill in increasing his fortune was *almost proverbial* [9, p. 444].

However, Gauss was interested not only in recording suchlike data; over and above that he attempted to gain other exact figures [2/5.5, p. 325], for example about the number of people struck down by lightning and the frequency of the lightning bolts per area unit [11, p. 96].

Tables of the third category show his efforts *to base everything on numbers* [10, p. 89]. These are lists borrowed from the literature, and I mention as a typical example a list of 78 peaks and places or regions [13, p. 73] from Chimborazo [in Ecuador] to Montblanc, from Brocken to Oderbruch in Harz.

Finally, the fourth category is comprised of such tables which were compiled as a jocular amusement, and here is an example, typical in my opinion [14, sheet 8^v]. After the last page of his famous *Mathematical Diary* (Gauss 1985) in which he had recorded his findings during 1796 – 1814 *there are some sheets with both mathematical and non-mathematical statements* (*W*-10/1, p. 572; [15, p. 25]). […][4].

[3] Only a few words about the distances measured in steps and found on a page full of jocular elements. It is reported that Gauss, in later years as well, recorded *the distances in steps from the observatory* [in Göttingen] *to those places which he had visited more often* [10, p. 89]. On 31 Dec. 1837 he wrote to Schumacher [2/5.3, p. 190] that during those counts he was able to occupy himself otherwise. He read the indication of the French astronomer Lalande that an astronomer engaged in practical astronomy ought to be certain of his counts of seconds to the extent of being able to walk [a few steps], write something down and even speak without interrupting his count or being mistaken[5]. This statement prompted Gauss to remark in the same letter:

I can do much more, I can think coherently about quite other matters, or count something quite independent from the first count or read a book or a letter. […] *However, I do not dare talking, or talking more than a few words without getting out of the count*.

Table 2 also taken from an appendix [14, sheet 14^r] to the *Mathematical Diary*[6] lists the stages [of a walk] with the relevant minutes and numbers of steps. Thus, we can imagine how Gauss, apparently in October 1798, walked from Braunschweig to Helmstedt. He came to Bornum in 180 minutes, to […] and to Helmstedt in 370 minutes having counted 45,053 steps and at the same time thinking about, for example, his proof of the theorem that each algebraic rational whole function (?) of one variable can be expanded into real factors of the first or the second degree, − the proof [of the main theorem of algebra] that he offered a bit later in the dissertation defended in Helmstedt [16]. His speed amounted to ca. 5.7 *km/h*; recalling that he covered 35 *km*, we conclude that this should be called a sporting achievement of a 21-year-old man.

The page with the mentioned numerical results also contains various tests of the pen and a copy of a French love poem (probably written by Jean-Baptiste Rousseau) and of its German translation by Friedrich Wilhelm Gotter. The poem ends thus: *When you open your lips beats my entire heart, touching your hand jerks me to the sky*.

So Gauss had not lived only in the world of numbers at all. Further proofs of this statement are found not only in his letters [2/2, pp. 16, 61 – 62], but also in his notes. For example, in a field record book of his Braunschweig triangulation of 1803 he had repeatedly written down the name of his future first wife, Johanna Osthoff [17, pp. 15, 17], with whom he had just been acquainted, with whom he fell in love a year later, who died only five years after that, deeply mourned and never forgotten by him.

[4] Here is another appearance of an inborn player. Among the supplements to the *Math. Diary* there is a table [14, sheet 29^r] which I [18, pp. 8 – 14) have interpreted as an indication of the number of thousands in which Gauss had counted the number of primes on certain days. The compilation of this table (see Fig. 3) began on 15 Dec. 1791, when he was even younger than fifteen, earlier than Gauss (*W*-2.2, p. 444) had recalled almost 60 years later and it ended on 28 Nov. 1797 after Gauss had studied 56 thousands.

There are no uncoded indications and the dates in the first column are provided not in the usual way but as four-digit numbers denoting the number of days beginning with Gauss' birth. Hints are actually offered by coincidences on the second line from above and on the sixth line from below: 97.4.15 C (chiliaden [thousands – O. S.]) A (Abzahlungen, counts) 7291, and 8113; 99 VII 16 D (Doctor).

Calculation shows that on 15 April 1797, the day up to which Gauss had studied 20 thousands, 7291 days, and on 16.7.1799, the day on which he had defended his dissertation, 8113 days have passed since his birth. The second column shows the increase in the number of the studied thousands and the corresponding day, in the third column are those numbers since the beginning of the counts.

The fourth column contains much mysterious, for example crosses of various kinds, other signs and words. The fifth column consists of weekdays denoted by numbers, the two next columns show the day of the month and of the days of the count and therefore offer an additional means of checking. This column provides a special amusing play insofar as the weekdays are shown not in the then usual form, that is, not denoting Sunday by 1, Monday by 2 etc, but otherwise: Tuesday was 0, Wednesday was 1 etc. […] We can only speculate why had Gauss denoted Tuesday by a zero, although possibly because he was born on a Wednesday.

[As mentioned above], Gauss had concluded his table on 28 Nov. 1797, but later he *very often spent a free quarter of an hour for studying a thousand here or there* (*W*-2.2, p. 445). Or, more properly: studied them in an unintended way. At first, as I said, he inserted the day of the defence of his dissertation, then another date (3 April 1801) which, according to some information, could have referred to his paper [19, pp. 136 – 140] in which he (p. 140) had derived the

condition for the existence of a limit of a countable set as a quite special case by issuing from its invariably existing upper and lower boundaries.

Four more dates are given and an additional calculation is provided. It mainly corresponded to his geodetic measurements of 1824. We do not know why Gauss had considered his calculation important. Among other abbreviations there is the letter Z which possibly denoted Zeven, Gauss' temporary accommodation, highly valued by him in contrast to other quarters in which he lived during his triangulation measurements since 27 June 1824.

[5] Until old age Gauss had kept to his usual peculiar notation of dates by the number of days since his birth. For example, he had thus calculated Eisenstein's age at death [20, p. 7]. From Sartorius […] we know that Gauss had *compiled a list of the duration of lives measured in days mostly of eminent people, namely, of his friends* [10, p. 89]. As far as I know, that list is not yet published, but I [21] published the *count of days* written on a blank page of his own copy of a logarithmic table of 1811 (Fig. 4). He calculated the number of days of Newton's life to find out on which date Alexander von Humboldt will arrive at Newton's age at death, − on 9 Dec. 1853. Next to his own day of birth (30 April 1777), on which 64,768 days have passed since the introduction of the Gregorian calendar [in 1582 – O. S.][7] Gauss also found place in his table for indicating the day of his dissertation's golden jubilee.

Basing himself on this method of dating, Gauss [11, pp. 113 – 114] had stated in his letter of congratulation:

We, Germans, celebrate with pleasure, perhaps more than any other nation, certain days which have some temporal connection with our dear people or events such as birthdays, jubilees, a. o.

Even now this rings very topical. Gauss continued:

Representatives of the quantitative science, in whose eyes indefiniteness and arbitrariness are always considered repulsive as opposed to clarity and stability, find a small deficiency in that the ground for establishing for celebration exactly this day rather than another one more or less depends on arbitrariness […] *and, in the final analysis, on the circumstance that we have exactly five fingers to each hand*.

Humboldt's joy over the *astonishing* congratulation on the occasion of his *dreadful 30,766 days of life* was restricted (who would be pleased to be reminded of his old age?) but he discerned *something peculiar to the great man* [21, p. 165].

[6] I doubt that avoidance of arbitrariness was the only decisive argument for the fifteen-year-old Gauss when he began to provide dates of his life measured in days from his birth. I rather believe that it already was his inclination to encode, his tendency to erase each trace (to follow Kronecker [22, p. 42]) and, as Philipp Maennchen [23, p. 105] had put it, *to insert complications even in jokes*.

Along with the search for the primes' law of distribution the young Gauss had been mostly fascinated by *playing with the arithmetic-geometrical mean* [24, p. 45]. I will only briefly dwell on his relevant notes as far as they were *intentionally* compiled *in a puzzling form* [19, p. 12].

From 1796 until 1816 Gauss had been without explanation using artificial words such as GEGAN, WAEGEGAN, GALEN and groups of letters, for example WAE AZ ACLN L in his *Math. Diary* and notebooks. I [25; 26; 27] have attempted to find out plausibly that all those letters relate in various ways to his great discovery of the connection between the lemniscate, arithmetic-geometrical mean and power series as well as to the resulting elements of the general theory of elliptical and modular functions. I will not go into details since they are documented in my publications (Ibidem).

Being based on circumstantial attempts at interpretation, they are inevitably hypothetical and therefore questionable. It is thus understandable that other explanations are offered [28]. They coincide with my interpretation insofar as they also issue from a connection between the artificial words and the arithmetic-geometrical mean, but the essence of their statements is very general and, most important, the freely existing inner interrelation of all the decoded words is lost.

My assumption that by his encoding Gauss had attempted to prevent outsiders from gaining an insight into his mental workshop has also been criticized. It was accepted that,

when working under great stress, or being enormously joyful over discoveries, Gauss had no time or inclination for formulations in detail and in such cases he often used abbreviations [28, p. 18].

However, exactly the attention paid by Gauss to write down his keywords or encoded combinations of letters in *adorned capital letters* [29, p. 24] to a certain extent indicated the possibility of leisure. The danger that some outsider in Braunschweig was able to gain an insight into his notes certainly did not exist and in Göttingen that danger hardly existed. But it was exactly *the acting as though* that appealed to him. I therefore consider Gauss' inclination to encode not as a corollary of his attempt to economize time, but as the act of an *inborn player*. For someone as extremely skilful at, and experienced in calculations as Gauss was, this attitude led to an increase of the required time; instead of the usual dating he had to find out the number of days passed from his or someone else's day of birth[8].

In this connection it should be mentioned that in 1812 Gauss had deviated from his principle of publishing only quite ripe materials [2/5.2, p. 94; 30, p. 40]: he made known an encoded [31] conclusion in which he was not quite sure. It was the only occasion on which he had revealed *coram publico* [to the public – O. S.] his inclination to encode. His cryptogram should have meant that *the main motions of Jupiter and Pallas are in a rational ratio of 7:18* [2/1, p. 170]. My published modest attempt at decoding [32] differs from other endeavours [32, p. 156] in that I understand the encoded message not only as stated above, but as also including the date of the discovery, 3 April 1812.

I hope that my explanation has thrown light on two points: Gauss liked to deal with numbers even without setting objectives since it entertained and soothed him; and, until reaching maturity he had a weakness for encoding. Both inclinations expressed his strive for playing.

I also wish to indicate that even today it makes sense to study the unpublished notes of that probably unique genius. Such work can be essentially eased by the publication of the catalogue of his manuscripts kept at the Staats- und Unibibliothek Göttingen. Its author, my friend Theo Gerardy (1908 – 1986) had not completed it. I would like to drop a hint for simplifying this work. We may assume that, while attempting to economize on expensive writing paper, on blank spaces [for example, in published tables] Gauss had written the results of his collateral calculations, as they are thought to be, and then inserted four- or five-digit numbers with abbreviations which had no connection with the initial aim of the record. Actually, they denote dates of his life and contributions or of the lives of others. Quite generally and unchangeably valid is still the statement [33, p. 73]

It is really probable that the scope of important ideas is not yet understood and will only become fruitful in the future[9].

Nevertheless, as Gauss prophetically foresaw in 1832, much had been irrevocably lost with his death [30, p. 41][10].

Notes

1. See [iv].

2. Cyclotechnie is connected with the expansion of numbers into products of primes, see explanation in the source mentioned.

3. Application of mathematics (more precisely, of the theory of probability) to demography, see below, was not new at all. I (Sheynin 1979, pp. 81 – 63) have described Gauss' study of the laws of mortality.

4. I have omitted more than a page of the author's text also contained elsewhere [iv, § 7].

5. Chronographs were still unknown and observers had to use the method called *eye – ear*; I myself used it while being a student of the Moscow geodetic Institute. The observer memorizes the indication of his chronometer and simultaneously counts the seconds according to its ticking, then observes and registers the passage of a star across the crosshairs of the ocular of his instrument.

6. I have omitted both the tables and the reproduction of pages from Gauss'

Mathematical Diary.

7. Catholic Europe officially passed on to the Gregorian calendar in 1582; actually, however, European countries introduced it later (and not at all simultaneously).

8. Note however that the lost time was more important for an able calculator. Maennchen (1918b) stated that Gauss had often made mistakes in his calculations since he did not check himself (apparently in less important cases).

9. Possibly Yang Qing Zhi et al (1997) is here useful. Now, however, this source is not easy to get hold of.

10. Gauss foresaw it in 1822 [iii, Note 6].

Brief Information about Those Mentioned

Eckermann Johann Peter, 1792 – 1854, Goethe's secretary and friend

Eisenstein Ferdinand Gotthold Max, 1823 − 1852, mathematician

Gotter Friedrich Wilhelm, 1746 − 1797, poet

Landau Edmund Georg Hermann, 1877 − 1938, mathematician

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

Zach Franz Xaver von, 1754 − 1832), astronomer

Bibliography

Note: *Materialen* (1911 – 1920) is not definite enough. Their issues have been published either as books or as papers in periodicals. And I have found *Materialen* which appeared after 1920.

[1] Michling H. (1979), Aus der Bücherei des Gymnasiasten J. F. C. Gauß. *Mitt. Gauß-Ges*., No. 16, pp. 5 – 16. Johann was a name which Gauss never used.

[2] Gauß C. F. (1975 – 1987), *W/*Erg-1 – 5.

[2/1] *Briefwechsel mit Bessel*, Bd. 1,

[2/2] *Briefwechsel mit Bolyai*, Bd. 2,

[2/3] *Briefwechsel mit Gerling*, Bd. 3,

[2/4] *Briefwechsel mit Olbers*, Bd. 4/1, 4/2,

[2/5] *Briefwechsel mit Schumacher*, Bd. 5/1, 5/2, 5/3.

[3] Biermann K. R. (1988), *Die Mathematik und ihre Dozenten an der Berliner Universität 1810 – 1933*. Berlin.

[4] Biermann K. R. (1978), Gauß als Persönlichkeit etc. *Abh. Akad. Wiss. DDR*, No. 3N, pp. 39 – 49.

[5] Küssner Martha (1977), Gauß' Umzug von Braunschweig nach Göttingen etc. *Mitt. Gauß-Ges*., No. 14, pp. 30 – 47.

[6] Gauß C. F. (1802), Brief an Fr. von Zach 16 Okt. 1802. *Monatl. Corr*., Bd. 6, p. 504; Humboldt A. (1850, p. 444).

[7] Bachmann P. (1922), Über Gauß' zahlentheoretische Arbeiten. *W*-10/2, No. 1.

[8] Maennchen Ph. (1918a), Die Wechselwirkung zwischen Zahlenrechnen und Zahlentheorie bei C. F. Gauss. In *Materialien* (1911 – 1920), see issue of 1918, No. 6).

[9] Cantor M. (1878), C. F. Gauß. *Allg. Deutsche Biogr*., Bd. 8, pp. 430 – 445.

[10] Sartorius von Waltershausen W. (1856), *Gauß zum Gedächtnis*. Wiesbaden, 1965.

[11] Biermann K. R., Editor (1977a), *Briefwechsel zwischen Humboldt und Gauß*. Berlin.

[12] Dunnington G. W. (1955), *C. F. Gauss, Titan of Science*. New York. [Math. Assoc. America, 2004.]

[13] Reich Karin (1977), *C. F. Gauß 1777/1977*. München.

[14] Staats- u. Uni. Bibl. Göttingen, Gauß Math. 48 Cim.

[15] Biermann K.-R. (1986), Wissenschaftliche Beziehungen von C. F. Gauss. *Sitz. Ber. Öster. Akad. Wiss*., Abt. II, 195, No. 1 – 3, pp. 25 – 40.

[16] Gauß C. F. (1799), *Demonstratio nova theorematis omnem functionem* *algebraicam rationalem integram unius variabilis in factores reales primi vel secundi gradus resolvi posse*. *W*-3, pp. 1 − 30.

[17] Gerardy T. (1977), Die Anfänge von Gauss' geodätischer Tätigkeit. *Z. Vermessungswesen*, Bd. 102, No. 1, pp. 1 – 20.

[18] Biermann K. R. (1977b), Aus unveröffentlichten Aufzeichnungen des jungen Gauß. *Wiss. Z. Techn. Hochschule Ilmenau*, Bd. 23, No. 4, pp. 7 – 24.

[19] Schlesinger L. (1912b), Über Gauß' Arbeiten zur Funktionentheorie. In *Materialen* (1911 – 120), issue of 1912, No. 3.

[20] Galle A. (1917), Gauss als Zahlenrechner. In *Materialen* (1911 – 1920), issue of 1919, No. 4.

[21] Biermann K. R. (1974), Humboldt zu Newton in Beziehung gesetzt durch Gauß. *Mitt. math. Ges. DDR*, No. 1 – 2, pp. 162 – 167. Staats- u. Uni. Bibl. Göttingen 8° Cod. Ms. philos. 34, p. 4.

[22] Kronecker L. (1901), *Vorlesungen über Mathematik*, Tl. 2/1. Leipzig.

[23] Maennchen Ph. (1934), Zur Lösung eines rätselhaften Gaußschen Annagramms. *Unterrichtsbl. Math. u. Naturwiss.*, Bd. 40, pp. 104 – 106.

[24] = [8]

[25] Biermann K. R. (1963), Zwei ungeklärte Schlüsselwörte von C. F. Gauß. *Monatsber. Deutsch. Akad. Wiss.*, Bd. 5, pp. 241 – 244.

[26] Biermann K. R. (1969), Versuch der Deutung einer Gaußschen Chiffre. Ibidem, Bd. 11, pp. 526 – 530.

[27] Biermann K. R. (1976), Schlüsselworte bei Gauß. *Arch. Intern. Hist. Sci.*, t. 26, No. 99, pp. 264 – 267.

[28] Schumann (1976), Vicimus GEGAN, Interpretationsvarianten zu einer Tagebuchnotiz von C. F. Gauß. In *Intern. Z. Geschichte Ethik, Naturwiss., Techn. Med.* (NTM), Bd. 13, p. 17 – 20.

[29] Schlesinger L. (1912a), C. F. Gauß. Fragmente zur Theorie des arithmetisch-geometrischen Mittels etc. In *Materialen* (1911 – 1920), issue of 1912, No. 2.

[30] Schering E. (1887), *Gauß und die Erforschung des Erdmagnetismus*. Göttingen.

[31] Gauß C. F. (1812), Über Störungen der Pallas. *Gött. gel. Anz.*, pp. 657 – 660.

[32] Biermann K. R. (1971), Zum Gaußschen Kryptogramm von 1812. *Monatsber. Deutsch. Akad. Wiss*., Bd. 13, pp. 152 – 157.

[33] Rieger G. J. (1957), Die Zahlentheorie bei C. F. Gauss. In *C. F. Gauß. Gedenkband zum 100. Todestag*. Leipzig, pp. 37 – 77.

[34] Biermann K. R. (1986), Wissenschaftliche Beziehungen von Gauß. *Sitzungsber. Österr. Akad. Wiss*., Abt. II, Bd. 195, No. 1 – 3, pp. 25 – 40.

Gauß C. F. (1985), *Gauß' mathematisches Tagebuch*. Leipzig. *Ostwald Klassiker*, No. 256. Leipzig.

Humboldt A. von (1850), *Kosmos*, Bd. 3. Stuttgart.

Maennchen Ph. (1918b), Gauß als Zahlenrechner. *W*-10/2, No. 6.

Materialen (1911 – 1920), *Materialien für eine wiss. Biographie von Gauß*. Eight issues prepared by F. Klein, M. Brendel, and L. Schlesinger.

Sheynin O. (1972), C. F. Gauss and the theory of errors. *Arch. Hist. Ex. Sci.*, vol. 20, pp. 21 – 72.

Yang Qing Zhi et al (1997, in Chinese, Engl. summary), Analysis of the reasons why many of Gauß' mathematical ideas were never published. *Math. Rev.* 1998j.

VI

F. W. Bessel

Brief recollections of my life

Kurze Erinnerungen an Momente meines Lebens.
Abh., Bd. 1. Editor, R. Engelmann. Leipzig, 1876, pp. XI – XXIV[1]

[1] I was born in Minden on 22 July 1784. My father was a [local] government secretary (Regierungssekretär), manager of various funds (Kassen), legal advisor of the then existing ecclesiastical parish of the Johanniter-Malteser order from which he received the title of counsellor-at-law. In Westphalia he became the first registrar of the tribunal. After happier times had returned, he moved to Paderborn where I saw him for the last time in 1819. My mother was the daughter of a pastor Schrader in Rehme[2]. For me, both parents had always remained models of honesty and had been generally recognized as such.

My father was not only honest, he was also clever. My mother presented the most perfect picture I have ever seen of self-sacrificing love for others. In my ripe years I have often recalled her attitudes and was unable to remember even a single wish of hers which was not shelved off to the last.

Only by extreme thrift was the income sufficient. Surmounting essential hindrances my parents had brought up three sons and six daughters. My father's serious thought and extremely intensive activity had often been necessary for earning a living and for teaching of so many children. Had he lived longer, he, a subordinate clerk of a judicial board, would have been happy to see his eldest and youngest boys achieving worthy positions as presidents of district courts in Kleve and Saarbrücken respectively whereas his middle son, although not versed in law, was honoured by many distinctions. Three daughters out of the six have married and two of the other three have died so that seven members of the family are still living (on 12 Febr. 1846[3]).

Our family is noble. One of my ancestors, my great-grandfather as I think, had not used the customary distinctive sign of such families [apparently, the *von* – O. S.] (I do not remember having heard the reason why), and both my grandfather and father followed suit. The latter and my brothers told me that, nevertheless, we may still claim that sign but none of us seemed to desire it although our cousins have successfully done it.

Our family also owns considerable territories held in fee (Lehngüter). Parts of them are near Petershagen, near Minden and, as I think, in Pomerania. Our Lehnsvetter [Vetter – cousin – O. S.] claimed them back even in our century. Later, however, I do not know why, they lost their claim. Presumably these territories had not been recognized as feuds anymore or the cousins had agreed to be recompensed.

I do not anymore remember anything remarkable about my youth although I recall being somehow distinguished from other school students of the same age. Many times in the lower classes up to the *unter Tertia* [to the fourth class], after which I left school, I was kept in after school which was quite proper since I had always detested the rudiments of Latin.

To avoid them, I explained to my father that I was strongly disposed towards calculation and therefore wished to become a merchant. In spite of his sense of justness my father would have hardly satisfied the wish of a lazy schoolboy, although having this inclination coupled with a special calculating skill, had I not been supported by one of our teachers, the assistant principal Thilo[4].

He was an enthusiast of mathematics and natural science, but, as I had later easily understood, highly ignorant although being endowed with an active speculative mind. Once I had for so long rubbed with sand a round piece of window glass that to a certain extent it began to concentrate sun rays. I showed it to Thilo and asked him how to turn it into a real lens. The glass, although barely effective, inflamed the enthusiast, and I am thankful to him for his support which proved decisive for my later life. Father agreed, took me out of the school and allowed me to study further writing and arithmetic as well as the French language and geography,

[2] From that period of my life (13 − 14 years old) I recall an episode which I wish to put on record since it shows the sharpness of my eyesight from which I had been able to expect much without exhausting its power. For making out the constellations I compared the sky with its picture on a plane in an old geographical atlas. After coming to the Lyra I suddenly noticed that one of the two stars that together with Wega form an almost equilateral triangle consisted of two stars. I called my elder brother so that he would also be delighted by this astronomical discovery. However, he did not see those *two* stars but rather, with effort, *one elongated* star.

His eyesight was apparently already weakened by diligently doing his homework. These stars were ε and 5 Lyra known to be only 1/8 (viertelhalb) of a minute apart. I have often glanced at them to notice how the weakening of my eyesight was going on. Already in Lilienthal I was hardly able to see those stars separately, and later I saw only one elongated star whereas now I see it only with effort.

Recently Argelander has zealously and minutely studied the picture of the sky as seen by the naked eye and mapped it in his book (1843) along with a list of stars with their most thoroughly determined magnitudes. His maps show only one star instead of the two mentioned, ε and 5 Lyra, and, accordingly, they are listed as a single star of the fourth magnitude. It is thus stated that the usual eyesight can only see both stars as a single object, and I have good cause to believe that eyes which are able by attentively looking to see these stars separately, are unusually sharp. If, however, this is possible at once, the eyes are sharp to a rare extent.

[3] In 1798 a friend of my father obtained a promise from a respected commercial firm Andreas Gottlieb Kulenkamp & Sons in Bremen to take me on as a trainee in exchange for *seven years* of working for free. My father himself brought me to Bremen. We arrived there on 1 Jan. 1799 and next day I was shown my place in the office (in einem Comptoirpulte). I found myself in a new world which intensely seized me. What I learned in the parental home was extremely restricted, it only concerned the well-being, or rather the difficult preservation of the thought-over meagre things. On the contrary, in Bremen considerable commercial deals, about which I had gradually learned from copies of business letters, went by before my eyes.

The grandeur of these deals interested me so strongly that I, even after working hours, remained in the office and studied all the business accounts to achieve an overview of the entire process of trade. Soon I succeeded and on many occasions, when some detail escaped the memory of other employees of the office, had put to good use my procured knowledge.

And so I earned some standing and by the end of the year received a bonus of 5 Friedrichsd'or. Later that bonus invariably increased and in 1805 reached 30 Frd'or. My father and both my brothers approved of, and deeply respected me which flattered my modest pride so that I had rather been prepared to sacrifice anything than imperfectly fulfil some duty.

In 1799 the British and the Russians had invaded North Holland and Kulenkamp received an order to provide the necessary grain for man and horse. The extent of the business had essentially widened and accordingly my workload increased. I still gladly remember that my powers had strengthened due to tension and allowed me to fulfil both the usual and the new work easier than to carry out previously only the former.

I had now only lived for the deals but then during the same year serious difficulties occurred because of the crisis. Numerous large firms in Hamburg and Amsterdam went bust which impeded and often slowed down the turnover of bills of exchange. The Kulenkamp firm found itself in an awkward situation: it was feared that its acceptances for large deliveries of grain will not be sufficient for the duration (which was prevented by a delivery of silver from England).

My attention to these deals had intensified and I found out the possible measures to aid firms [on such occasions].

[4] After the capitulation and embarkation of the landed army business had returned to its previous state, and it soon turned out that I was not sufficiently occupied so I began to think about my future. Being without any means, I saw only one good prospect, of becoming a competent *cargadeur* (hirer of ships in expeditions). In those times Hanseatic cities had been fitting out expeditions to French and Spanish colonies and China, and I began to study writings providing instruction on commodity research and natural history, or the general history of the emergence of usual commodities. From these writings I went over to descriptions of countries outside Europe and of the essence of the trade with them. I studied the reports of travellers, Raynal (1770) and suchlike works, acquired a good knowledge of geography and recorded appropriate notes.

At the same time I had learned English in two or three months of intense oral instruction; I was compelled to save on the cost of a longer study. I also attempted to learn Spanish by studying its grammar and reading a Spanish book. I also came across a man who had previously lived in Spain but who at that time worked as an apprentice in a gun smithy in Bremen. He was patient enough with my questions about pronunciation.

Along with these efforts I had thought that, although navigation was not the business of a cargadeur, some knowledge of it can prove useful for him. I decided that at least it will not harm me to be able to determine with a sextant and a timepiece with a second hand, independently from dead reckoning, the place of the ship as often as the location of the Sun and the Moon enabled it.

In those days Hanseatic sea captains had been ignorant of this *modern art*. I spoke with many of those whom I met in connection with [our] trade, but invariably heard that that art was absolutely unnecessary, that dead reckoning coupled with observation of the latitude at midday was sufficient and that the main point was to *pay attention* when approaching coast. Their opinion was sober for short voyages across familiar European seas, but it was not difficult to realize that longer voyages required other navigational means as well. The diligence with which the English trained their seamen in some astronomy additionally proved to me that that *modern art* could not have been as unnecessary as our ignorant captains thought and the acquisition of that art seemed still more important to me. I thought that, if the usual practice did not ensure sufficient certainty, I will be able to inspire the captain to trust the new art by daily showing the place of his ship on a sea map and inducing him to resort to my map and thus to enjoy the ensuing advantages.

[5] I had therefore decided to learn the astronomical part of navigation and turned to the then available book Moore (1807). It only contained *instructions*, and, if the reader additionally got hold of practical directions, he would have been really able to determine the place of the ship by observing celestial bodies. Without such aid the book would have remained in most cases fruitless, and, furthermore, it did not provide an *insight* at all into the matter the less so since it did not dwell on the principle of spherical trigonometry.

And so, I had learned much from my copy of Moore, but not enough at all for being satisfied. I mostly attributed this insufficient success to my ignorance in the main *astronomical* notions and attempted to help myself by a *popular* astronomical book written, if I am not mistaken, by Voigt. Again I learned much even if only reading it secretively since I feared to be mocked by other employees of the office for venturing in *astronomy*[5].

But the best of what I had thus learned was the title of Bohnenberger (1795) and that it mostly dealt with the application of the mirror sextant. This was exactly what I wished to learn from Moore. I procured that new book which brought me to new light. I distinctly saw that it provided *mathematics* and that that science was useful for solving navigational problems.

I found a book on the beginnings of mathematics written perhaps by Münnich and devoured it in a few days. At the end of the book I also read very attentively historical information placed there with numerous hints far exceeding the boundaries of that textbook [translation of the not really understandable context].

And now the study of Bohnenberger became quite easy. Its next fruit was my attempt to construct an instrument for measuring the altitudes of stars and a rather bad pendulum clock with a second hand. Both were made with the assistance of a carpenter and a watchmaker. The latter was so unskilled that he became almost unemployed, but, exactly for this reason he treated me in the best possible way, that is, most submissively fulfilling even poorly paid work.

[6] With their help I made a mahogany sextant incrusted with ivory with a fixed telescope and fastened it to a rod by the window. A vertical thread showed the graduation which I marked with sufficient diligence on the ivory[6]. The clock was separated from its striking mechanism. I found the place for this instrument in the house of my friend, Helle, who attended all the classes of the Bremen grammar school but had to abstain from entering the (?) university owing to the death of his father. For the time being he was compelled to carry on father's craft workshop. More precisely, since he was scarcely versed in its business, to supervise the workers there. Later he adapted himself to metalwork of the workshop and definitively gave up further studies to devote himself to the workshop.

For me, friendship with this educated young man had been highly desirable, and he, in turn, welcomed my zeal for astronomy. We mounted the instrument in the state of best possible repair and had been rewarded by enjoying the achieved determination of time. The method which I applied was the only one suitable for my instrument. I observed two stars with the *same* altitude and almost the same declinations but situated on the opposite sides of the meridian[7]. Many pairs rapidly following each other ensured a check of the precision of the result by comparing the obtained corrections of the clock derived from each pair. The outcome astonished me since I expected that my instrument will provide a precision much (beiweitem) lower than the actually obtained[8]. More important, however, was the thus achieved skill of trigonometric calculations.

Once I managed to see through my weak telescope the ingress of a bright star into a dark edge of the Moon and impatiently awaited the results of other observations of the same event. They were finally published by von Zach's *Monatliche Correspondenz* and Bode's *Astronomische Jahrbuch*. Now I had to determine the longitudinal difference [between the places of those observations] and Bremen for which Bohnenberger provided sufficient and clear directions[9].

Happily my determination of that difference coincided with the known value to one or two seconds [of time] and I triumphed over the success of my first attempt at solving a problem of practical astronomy. You should posses the flame of youth to grasp how this success gladdened me! I am certainly not mistaken when presuming that the die that determined the rest of my life was thus cast.

I have mentioned the *Mon. Corr*. and the *Astron. Jahrb*. and am reporting that both periodicals had clearly noted (fesselten) my attentiveness. I discovered there so many novelties inaccessible to my knowledge. This should have prompted me to study further. For the time being I had not allowed this circumstance to disturb me, but

[now] it inclined me, by means of the mentioned popular book (Bohnenberger 1795), to attain a *better overview* of astronomy. Ensuring this aim by connecting the [available] hints presented no difficulties since in those times my memory was excellent and after reading it a printed word did not easily escape me.

As happy chance would have it, I came across and bought a copy of Lalande (1775)**[10]**. And when reaching one of the innumerable flaws in my knowledge, of something unclearly represented in my *overview*, I opened the appropriate chapter of that book and invariably satisfied myself.

[7] And I have thus compiled a reasonably complete knowledge of astronomy from separate fragments which I transferred to their proper places in the overview. This, however, was the only suitable for me method. I learned only that which I either meant to apply or thought to be needed for understanding my sources. I never learned astronomy as a science so that my present astronomical knowledge would have had many more flaws than it actually has, had not all parts of that science been so closely interconnected that my long-time work necessarily involved all of them.

In a supplement volume of the *Astron. Jahrb.* I found the observations of Harriot concerning the comet of 1607 (the Halley comet) and discovered by von Zach in the archive of an English family. A wish to treat them up to the calculation of the orbit of that comet arose in my mind. Instructions provided in Lalande's book together with Olbers' famous contribution on the easiest method of determining cometary orbits became my guide. After reducing those observations I had experienced no serious difficulties on the way to my goal.

On this occasion I ought to admit that I have complied with many instructions without bothering to justify them by Lalande. This, however, was a consequence of my entire general viewpoint on science: I wished to perceive its results rather than to learn it. I studied earnestly but not for being examined but for the fruit which irresistibly attracted me. I did not even dream that astronomy will someday become my profession, I only searched for pleasure which consisted in gathering the fruit.

[8] Bremen was distinguished by its scientific orientation which (at least in those times) it would have been futile to look for in other German commercial cities. It first manifested itself, as I think, in the *museum* established by two or three patriotically inclined citizens who were able to discern the worth of that direction. Artefacts pertaining to natural history and books had been collected there, evening conferences were held and talks took place from time to time. Olbers was one of the first who started fostering the aims of the museum. The zeal became widespread and the membership had to be restricted to 200 with many more invariably wishing to enter.

The overseas connections of an important commercial city ensured a rapid expansion of the collections. Gifted books and money from the 200 members rapidly filled [/helped to fill] the bookshelves. The townspeople had been proud of the museum and it soon became the nicest ornament to the city. At the beginning of this [the 19th] century it built for itself a grand and imposing house, transferred there its grown-rich collections and was able to increase its membership accordingly.

More newspapers and scientific journals had appeared and were displayed in reading rooms for the members. *Weekly* scientific talks (from which religion and politics were excluded) were held and attracted a large number of listeners from all sections of the population. Among the lecturers shined such figures as Olbers, Albers, both brothers Treviranus and Mertens. No wonder that after the scientific orientation had consolidated, and the only still living man of those scientists left the city to adorn our universities at Breslau and Bonn, a younger generation filled up the gaps left after the death of those who had previously reared the scientific spirit of Bremen.

I see the scientific orientation of Bremen, of that invariably dear to me city, as its only lustre which at least in those times distinguished it from the larger and in many respects more important Hamburg. And this circumstance assisted in making more natural my turn from the office to scientific work.

[9] For me, Olbers had been a bright star and I had burned with desire to become personally acquainted with him. After concluding my study of the comet of 1607 and cleanly rewriting it, I plucked up my courage and crossed his path. He walked slowly along a street whereas I met him after more quickly passing to a next one [and returning back] and asked his permission to bring him a brief astronomical essay. He agreed and an hour later, on a Saturday, 28 July 1804, he received my manuscript. Next day afternoon, being free from the office, anxiety about the possible effect of my essay on Olbers prompted me to a long walk. Towards evening I returned home and found a letter from Olbers and many books which he had sent me since they contained unknown to me information about comets. I am now copying his letter.

Bremen, 29 July 1804

With great pleasure I have read your excellent work on the comet of 1607 [No. 1/1]. *I have acquired not only an idea about your exceptional mathematical and astronomical knowledge and excellent skill in the most difficult parts of the calculus. You yourself also exceptionally interest me. If I ought to reproach you, it is only that you had spent much more time, effort and thought on treating the observations of Harriot and Torporley than they deserved. You took into account tenths of a second whereas their precision hardly came to half a minute.*

However, your work, since it is done, is all the more valuable and we therefore exactly know what can be gleaned from the observations of Harriot. This is just the reason why <u>your contribution should not remain unpublished</u>, and I am asking your permission to send it to von Zach or Bode.

The observations of Kepler and Longomontanus of that comet are much less perfect than those of Harriot. In his book (*Halley 1749*), *which you possibly did not previously have, you will find how that man of genius applied those observations. It will please you to note* *how nearly do the elliptical elements calculated by him coincide with those obtained by you, − nearer than should have been expected from such rough observations if only the rapid apparent motion of the comet had not lessened the influence of the errors.*

I am also sending you the book of Longomontanus (*1622*) *since it is perhaps worthwhile to compare your* [calculated] *elements with his observations made on September 18 and 21. If you wish to study Kepler's original observations, see his book* (*1619*).

With greatest thanks I accept your kind-hearted offer to help me from time to time with astronomical calculations, and will avail myself of it on the very first occasion. Concerning the requested permission, I would like to receive a positive answer and with deepest respect I am offering my good offices.

No need to say that this letter gladdened me not less than previously the result of my determination of time, the observation of the occultation of a star or the calculation of the longitudinal difference relative to Bremen did. I hurried to Olbers, thanked him wholeheartedly for his leniency and went back not before acquiring an impression of the courtesy of his character and behaviour, an impression as strong as made much earlier on me by his astronomical weight.

From then onward Olbers became the object of my sincerest respect. I considered him as my second father and this is how I respected him until his death. Often had this respect prompted me to travel a long way from Königsberg to Bremen, the last time in August 1839, seven months before he died.

[10] Had I not been tired from writing down this report about my life or hampered by the advance of my illness, you would have heard much more about the relations between Olbers and me. However, I adduce my short note about Olbers as published by my friend Schumacher in [his] *Astronomische Nachrichten* [   ]. I had read it out at the conference of German natural scientists and physicians in Bremen in 1844 in accordance with the desire of Senator Olbers, the worthy son of my immortal friend and second father.

As mentioned in his letter included in § 9, Olbers had sent me the book of Longomontanus (1622). There, the author published his observations, imperfect but made three days before those of Harriot which thus essentially increased the scope of the observed geocentric motion of that comet of 1607. I began to determine its orbit anew and took into account both those and later observations of that astronomer. Olbers sent to von Zach the thus improved contribution about the comet and it was published in 1804 [No. 1/1]. In an adduced note [von Zach] friendly introduced the young amateur astronomer to the professionals and they concurred with Olbers' lenient opinion about my work[11].

Just after concluding this investigation I turned to the comet of 1618. Harriot had essentially studied its motion as well and von Zach discovered his observations in the abovementioned archive of [see § 7] English family. My new work was much more extensive since much more observations had to be treated. However, my skill in calculations of all kind had increased, and happily led me to the conclusion of my work which was published by Bode [No. 2].

[11] I had also plunged into astronomy when attempting to familiarize myself with *navigation*. I had not found it in a book devoted to the latter and took up a better source, the book of Bohnenberger, although it did not especially treat navigation. The book did not fail to turn to the hardly previously felt mathematics[12]. It thus opened up new possibilities for those parts of astronomy which I had previously no intention of more closely going into. And now I did not anymore really think about any *restrictions*.

I was satisfied by my acquired knowledge and convinced that as a cargadeur I will be able to determine the place of the ship each time that the celestial bodies allow it[13]. I could have left both navigation and astronomy, but the new knowledge induced me to try to delve deeper into its (ihr) field.

And now I ought to add something about how did I learn astronomy. It is very difficult to explain convincingly the real *initial* motive of an action, but in this case navigation had undoubtedly led me to astronomy. Nevertheless, I cannot answer just as persuasively whether navigation was the *only* incentive. Even in my early youth I had an idea about the motion of the Earth and of the planets [in general], and I knew that they moved not in an unknown manner but rather that astronomers had the means for *calculating* their motion. Then I acquired some skill in calculations, but was unable to find any connection between them and calculations in astronomy. The discovery of such connection seemed to me most highly desirable, but my pertinent childish thoughts had necessarily been fruitless until I began to sense [the need to apply] mathematical means.

The drive to lead myself essentially to astronomy undoubtedly prompted me to understand something about the essence of mathematics. This aim was ensured by navigation and consequently brought me to the book of Bohnenberger, but I cannot say whether something else would have not later done the same. I would not have adduced these thoughts had not the idea that an obliging chance became an essential condition so often crossed my mind. Without wishing to understand astronomical calculations I would have undoubtedly remained in the field of navigation. So I did not abandon *astronomy*.

Prompted by the *cometary astronomer* Olbers and following his wish, I investigated anew the orbits of some older comets which he thought were not satisfactorily determined since the possibilities of the existing and mostly very imperfect observations were not exhausted. In most cases I was only able to convince myself that those observations were insufficient. I achieved a somewhat better success when studying the second comet of 1748, and my brief investigation was published in the *Astron. Jahrb*. for 1809.

[12] I do not al all believe that my communications should only consist of various brief notes about new discoveries, observations or other events interesting for astronomy, but I ought to make an exception in the case of both comets discovered in the last quarter of 1805. Both, the so-called Encke and Biela comets, later became extremely remarkable.

During the night after 1 Nov. 1805, having received the three necessary observations from Olbers in the evening of that day, I calculated the preliminary elements of their orbits[14]. Later more observations became known which led to difficulties and barely successive work. My investigation of the *first* comet appeared in the July issue of 1806 in *Monatl. Corr.* It was quite impossible to describe its observations by a parabolic motion; the deviations from that notion were so irregular that the imperfection of those observations was doubtless. Most of all I became interested in two observations made by Olbers on 12 and 13 November since the difference between the [calculated] right ascensions almost amounted to 3 minutes. In spite of the expressed doubts, the approximate correctness of those observations compelled him to believe that there was some singularity in the appearance of the comet which impeded the precision of observations.

Furthermore, apart from those observations, irregularities were shown by the observations of Thulis in Marseille which could not have been explained by any kind of regular motion. This circumstance scared me away from abandoning the parabolic hypothesis and only deducing elliptical elements. I also wish to remark that in those times the idea of a comet completing its motion around the Sun in not a great number of years was still quite strange. The period of return of the Halley comet, 3/4 of a century, was thought to be the only exception from the rule that assigned to the comets a much longer if not an unbounded period of return.

Later Encke discovered that the comet named after him was seen in 1819 with its period of return being 1207 days and that its observations of 1805 can be made to coincide sufficiently well with such an orbit after improving the observation of 12 November made by Olbers by 10 time seconds and in addition if 7 out of the 18 observations made by Thulis were for an unknown reason considered worthless. A misprint of 10 seconds in that observation of 12 November was indeed discovered in Lalande's catalogue (1801/1847).

And thus the observations of 1805 of the first comet of that year from which it was impossible to derive any reliable result later proved weighty for determining its motion. On the contrary, it was possible to bring into concord the observations of the second comet of 1805 with the presumption of its parabolic motion so that there was no decisive doubt about it although Gauss had found out that there appears another coincidence of calculation and observation if the parabolic hypothesis is abandoned and an elliptical motion is looked for instead.

Such an investigation led to an ellipse with a period of return of 1732 days. In 1772 there appeared a small comet for which we only have a small number of barely satisfactory observations. Still, they were sufficient for a more precise determination of the elements of its motion first accomplished by Lalande. Later, after my new reduction of those observations, the elements of that comet became so similar to those of the *second* comet of 1805 that the identity of both comets was suspected.

I had therefore been prompted to study anew the observations of both comets under the supposition that the second comet was a repeated return of the fist one moving along an ellipse with a period of 33 years. However, the success of my investigation showed that the difference between the elements of the comets of the years 1772 and 1805 cannot be made as small as was needed for explaining it by the action of the planets during the period between those years. I have therefore thought not about the identity of both comets against which Gauss had justly reasoned that in the interim the comet could have many times returned without being detected and come near to some planet whose action could have explained the difference mentioned.

Later when Biela rediscovered that comet and found out that its period of return was indeed short (2465 days) and established that in both cases, in 1772 and 1805, it was the same comet.

I think that, had I been more cautious and less prone to the then prevailing premise that the period of return of comets amounted to hundreds or thousands of years, I could have arrived at the correct hint concerning its motion and therefore studied why such hints were not seen. I detect similar blunders of a greater or lesser extent when recalling my early youthful attempts. Such mistakes were so numerous that I have long ago been sick and tired of sharply criticizing them by issuing from my invariable drive to a single aim and from the stored experience.

My cometary studies invariably turned me to *solar tables*. Their *application* was not really difficult for those who quite understood their underlying theory; for those who partly understood it; and even for those for whom it was completely strange. I belonged to the second category. I knew both the essence of elliptical motion, the analytical expansion of the canonical equation of the ellipse and the expression for the radius vector. Concerning the perturbations occasioned by the action of the planets and the Moon on the elliptical elements of both (?), I had not only a general notion but *to a certain extent* understood what Lalande (1764/1792) had to say about them.

[13] However, neither my insufficient knowledge of celestial mechanics collected from hints in various sources, and from an incomplete understanding of the twenty second book of Lalande (1764) could have satisfied me. I decided to gain a better understanding of that discipline by venturing to study Laplace's *Mécanique Céleste*. My slight knowledge of mathematical analysis would have probably scared me away from this brave attempt even if I had studied it properly.

But surmounting a contribution which envisages some mathematical knowledge had misled me. My calculation of the appearance of the Halley comet in 1607 showed me that the true anomaly of the orbit's deviation from a parabola cannot anymore be determined with sufficient precision by the Simpson table of corrections. I found reprints of this table in later works devoted to the study of comets.

I was therefore compelled to turn to the burdensome indirect solution of the problem: *To calculate the true anomaly of the deviations of a comet from a parabola for a given time*. This did not, however, suppress my intention to study the easier Simpson method as thoroughly as was necessary for applying it if possible. And after completing the calculations concerning the comet of 1618 (which happened at the end of 1804) my wish prompted me to study that method. My mathematical knowledge proved sufficient as seen by my publication of 1805 [No. 3].

This success encouraged me to study the immortal *Méc. Cél*. But I soon understood my mistake. It may be excused since I had no possibility to imagine the difficult expansions applied by mathematical analysis and situated beyond the region which became accessible to me. So I attempted to broaden my mathematical knowledge hoping to attain that goal by means of the textbooks written by Kästner (1772 – 1801). Only much later I found out that those by Lacroix (1797 – 1800) would have been much more useful.

The manner of applying those textbooks was the same as I had used for achieving an aim (§ 7). I only invariably intended to reach my goal and the necessary means seemed worthy to me *only* as far as they enabled that. So I *devoured* Kästner's elements of the analysis of finite magnitudes, differential and integral calculi and of higher mechanics not for thoroughly learning the subject but to orient myself in it and be able to *find* later the necessary material. In this case my method of studying was not as totally blameworthy as on other occasions since I have already mastered the notions which the various parts of those books had interconnected.

However, the switch-over from Kästner's textbooks compiled as a series of lectures to the comprehensive analysis of the *Méc. Cél*. could only be arduous. At first I only encountered difficulties and, if my efforts were unable to perceive there Laplace's idea, I often had to skip for some time the hard places and to understand them by the following exposition. The advance was extremely slow, but my courage was sustained by noting that to my inexpressible joy the understanding of the following chapters became ever easier. And thus I have worked through the first two volumes of the *Méc. Cél*. although leaving for the future the details of the theory of tides. I have devoted to this study the most part of 1805 and the beginning of 1806 and I think that never I will be able to spend so much time so usefully and successfully.

I have now concluded what I had to say about my scientific pursuits in Bremen. Soon afterwards I left that city, my second home town, to spend a few years with Schröter in Lilienthal[15].

[14] However, I should not end the story about my life in Bremen. Everyone interested will like to understand some not yet mentioned circumstances. One of them is the compatibility of my astronomical pursuits with those required by my duties, inclinations and by the idea of their need for my future life which only lately left me[16]. As a rule, these duties occupied the time from 8 in the morning until 8 in the evening although usually two or three hours out of the twelve remained free. Sunday afternoons, when all the work in the office and warehouses stopped, had been devoted to walks or meeting friends and therefore remained useless for astronomical studies with seldom exceptions when those studies became especially urgent.

Nights had thus been necessary for helping me and little did I object to this practice since night was the usual proper working time for astronomers. As a rule, I returned to my room after supper (at half past eight or at nine) to devote six hours, until three or half past two in the morning, to calculation and books. I invariably followed this rule from the beginning of 1804 until […] 1806[17] when I left Bremen. This allowed me to combine both of my so differing occupations not only completely, but without any unease. The undisturbed night calm was favourable for attention whereas my body required no more than five sleeping hours as witnessed by my uninterrupted health.

I apparently ought to mention how I managed to pay for clothes and scientific books; housing and food were provided for free by the firm. I longed for relieving my father of paying for those needs as early as possible, and when, after three years of employment, my yearly bonus had risen to 12 Frd'or, I thought that my wish had come true.

Notes

1. See the correspondence of Olbers and Bessel (Erman 1852, p. IXff). R. Engelmann (R. E.)

2. Rehme: it is now included in Bad Oeynhausen, in North Rhein – Westphalia.

3. So this is when Bessel started to write down his recollections. He died only a month later (on 17 March) which certainly excuses some if not all of the hardly significant shortcomings below.

4. See Bessel's letters to Thilo in further Notes.

5. The same remark could have apparently been made about the book of Moore (above) and other books (below).

6. On **6 May 1803** Bessel described his first instrument in a letter to Thilo (Wichmann 1860, p. 168ff):

If this happens, which I do not doubt, I will open my own shop and manufacture quadrants [sextants – R. E.]. *Already a few years ago they gave me pleasure. Little had I understood about them, but happily a mahogany frame with an ivory limb was made for me for 3 thalers. I was unable to make a sextant all by myself and my rashness frustrated me. A sextant should be made* (zurichten) *according to Müller* als ich mich eine bessern besann. *And I decided to use somehow my sextant. I lowered a brass cone in its centre and became able to determine much more precisely the midpoint of the* der zu ziehended Kreise *and began to mark the graduations devoting to it almost all early mornings and being often thus occupied for four weeks.*

I have now concluded it; there are 96 graduations 15' apart. I reliably fulfilled that job with an Uhrmacher- or Federcompass which is much preferable to the very imperfect Haarcompass. A good graduation certainly cannot be achieved with the last-mentioned instrument.

My sextant will be without an alidade with only a lead plumb line so that I will have to measure the smaller parts (?) *by means of the telescope micrometer. It's a pity that such an idea did not cross my mind from the beginning since then it would have been so easy to order a sextant. And now I had to give up altitudes either lower than 30° or higher than 60°. I have quite naturally given up the former to be able to regulate easier the instrument and to determine more precisely the time during nights. Then, a crosshair with a single thread is the simplest possible, and, as I believe, just as reliable as any other since an instrument with a plumb line should be vertically set and the instrument can be as precisely as in other cases directed on the sighting target.*

I do not yet have the telescope lenses, a 13 lines (1 line = 1/10 or 1/12 of an inch) objective lens of 17 lines *or an ocular with focal length of 10 or 11 lines*. *Bremen mastery is insufficient for manufacturing them which is one more reason for availing myself of your kindness. Perhaps you will be able to tell me where is it possible to manufacture such lenses.*

28 July 1803

My sextant is now completely ready for service, only small changes are needed. I *hope to begin on 9 August by observing ε Arietis and it will then be obvious whether the instrument is useable or not. If my expectations come true, I will be much pleased to be able to help you with the determination of the longitude of Minden.*

Measurement of the altitudes of many stars when having a pocket timepiece with a second hand will hopefully determine the time well enough, and I will then certainly carry out the somewhat tiresome calculations. I have recently come across an idea which will not be completely unworkable: to measure longitude during storms. This problem was accomplished with the help of flashes of fire, so why not use lightning bolts? They appear for free, without any efforts and allow repetition of the measurement many times over. Places not farther apart than 6 or 8 or possibly 10 miles can certainly be thus determined.

26 August 1803

You asked me to describe my instrument. As you know, it is a sextant in 18 Paris inches radius with no alidade but with a fixed telescope and a screw micrometer in its focus. A silver thread is stretched from the centre to the graduations and can be set precisely on a graduation by the screw of the micrometer. Then this movable thread is set on the observed star by the screw of the micrometer.

My telescope consists of two lenses of which the ocular is bad in the first place (*I got it from a passing by glass grinder*) *but it still works much better than I suspected with a magnification of 15 times and ensures a bright picture. With a powerful illumination of the threads I quite clearly see even weak stars, for example fairly well the double star Alcor in Lyra.*

It is possible satisfactorily to determine the time by my instrument and it deserves to be recommended owing to its low cost. The mahogany frame with an ivory limb costs 3 thaler. Micrometer, Vorrichtung am Mittelpunkt. The axis around which the sextant is rotated, 2 thaler 36 groten; the telescope lenses, 1 thaler; and the frame, 5 thaler. In all, 11 thaler 36 groten.

Its manufacture is not difficult and the graduation is not as tedious as usually thought when having a good Federcompass. An observer having a window looking south does not need the frame. I encountered the main difficulty in that our house had no such windows; my own goes on exactly north. The window also ought to be high with a sufficiently wide window sill. When I first came here, I became acquainted with a young man called Helle whose father was a gun smith.

[…] *His house has a perfectly located room with large and high windows looking east, south and west. My sextant is now there and also from there I had observed the solar eclipse* [of 17 August – R. E.]. *Having no good pendulum clock, I borrowed a pocket timepiece with a second hand and measured 18 altitudes of the Sun. I did not yet have the screws and therefore had to set the thread on the graduations of the limb by the tripod screw. The necessary rigid position was therefore lost which certainly helped to make four observations completely useless.*

In addition, this was my first work on practical astronomy so that a better result could probably not been expected than that indicated below. I also remark that the obtained corrections of the clock are probably not quite exact since the collimation error of the instrument is not yet precisely determined. After rejecting the bad observations I got […] [Results of 14 observations are provided. They lasted about 70 minutes and the correction of the clock changed from $22^m25.^s7$ to $23^m12.^s3$.] *The clock was very slow and the change of its correction was so regular that it can mostly only be attributed to the functioning of the clock rather than to the observations.* [In one case the correction decreased by $3.^s7$ in 4 minutes.] *The clock had indeed functioned very badly since apparently it went slower in the first series than in the second.* [Where are these series? – O. S.] R. E.

7. Bessel described this method in a letter to Thilo on 10 Febr. 1804 (Wichmann 1860, p. 177):

I am most eagerly awaiting the next day [the solar eclipse – R. E. A second eclipse, see Note 6? O. S.] *and still hope for better weather. Since the day before yesterday it rains almost all the time which did not earlier prevent me from determining the time very well and quite reliably by observing the passage of many stars. Since the Sun is too low for making corresponding observations of altitude, I have applied another method which seems quite reliable. I observe equal altitudes of stars on both sides of the meridian so that the instrumental errors compensate each other just like when observing corresponding altitudes. During half an hour I can thus determine the time as reliably as by observing corresponding altitudes during* *no less than 4 or 5 hours. Calculation is naturally more difficult but if everything is prepared they can be completed in an hour. After observing for example 10 altitudes of a star only three of them need to be treated since the rest can be easily joined* [by interpolation] *with the second differences being considered.*

The same method can be applied for treating the altitudes on the other side of the meridian. The differences between the corrections of the clock are then attributed to the instrumental errors. This indirect method of calculation is much easier than the direct solution of the problem of determining time given unknown but equal altitudes of two stars. R. E.

It is opportune to mention that N. Ya. Tsinger (1884) suggested to determine time by observing stars having corresponding (equal) altitudes and situated to the east and west of the meridian with the sum of their azimuths being near 180 or 540°. For the sake of comprehensiveness I also note that in 1887 M. V. Pevtsov (Tsvetkov 1951) introduced a method for determining latitude by observing stars on equal altitudes situated to the north and south of the zenith with the sum of their azimuths near 360°. Subbotin (1956, p. 266) stated that Tsinger issued from the ideas of Gauss (1808a; 1808b). O. S.

8. Bessel checked the functioning of his clock by the Olbers method of observing a disappearance of a star. In a letter to Thilo of 29 Febr. 1804 (Wichmann 1860, p. 180) he wrote:

I am now determining the moment when a star disappears behind a tower and applying this method. After observing two stars having an equal altitude I determine the time and then observe the disappearance of a star, record it in sidereal time and calculate its hour angle. This angle changes with time since the deviation [declination] *of the star changes.*

Denote the polar altitude by φ; *the deviation of the star*, δ; *hour angle*, *t*; *and the parallactic angle, p. Then*

$$\frac{\cos t}{\text{tg}\varphi} = \text{tg}\psi,\ \text{tg}p = \frac{\cos\psi\sin t}{\text{tg}\varphi\cos\ (\psi+\delta)},$$

$$\sin\ p = \frac{\cos\varphi\sin t}{\cos h},\ \ \cos\ p = \frac{\sin\varphi\cos\ (\psi+\delta)}{\cos h\cos\psi}.$$

The change in the hour angle due to the change in δ *by* Δδ *and measured in units of time is*

$$\Delta t = -\frac{\Delta\delta\text{tg}(p+i)}{15\cos\delta}, \qquad (1)$$

where i is the inclination of the tower from the vertical circle. If the place in which the star had disappeared was vertical, then i = 0.

After observing the disappearance of a star, I reduce [the observation] *according to formula* (1) *to 1 Jan. 1800, write down the positive or negative changes in the hour angle caused by the yearly change of* δ, *see formula* (1). *I multiply the coefficient of* Δδ *by the aberration and nutation as provided by the Metzger table and determine, for example, for Regulus* [α Leo] *on 7 February the hour angle on 1 Jan.* 1800 was $5^h5^m26^s.10$, $50^s.08$, $6^m13^s.48$, *its yearly change due to* $\Delta\delta = 0^s.921$, *equal to – 0.05341*. R. E.

9. On 31 Dec. 1803 Bessel wrote Thilo (Wichmann 1860, p. 162):

The Bohnenberger formulas for calculating longitude certainly cannot be shortened, but I came across a method for making them more convenient. [Bessel wrote out these trigonometric formulas and informed Thilo about some of his auxiliary formulas. One of them indicated the value of

$$\log_{10}[1/2(\text{cosec}A/2)^2]$$

given the argument 90° – *A* = 10″(10″)3600″.

Then Bessel described the treatment of seven of his observations of the solar eclipse of 27 Aug. 1802. Each required about 11/2 hours. He concluded by providing the longitudes of some cities with respect to Paris (Mitau = Jelgava):

Berlin	$44^m16^s.5$

Vienna	$56^m 1^s.5$
Mitau	$1^h\ 25^m 29^s.8$
Kremsmünster	$47^m 19^s.7$
Prague	$48^m 29^s.5$
Lorenzberg near Prague	$16^s.6$.] R. E.

10. *I cannot desist from inserting some remarks about the book of Lalande. It is now dated, but its properties lack in later published general astronomical treatises. Lalande was an astronomer who worked in all branches of astronomy and he invariably cited the contributions of others in each of those branches. He had thus acquainted his readers with the knowledge of his time as well as with its historical development and made possible further studies by means of diligently and reliably chosen sources.*

These excellent qualities seem to be ever more lost with time. I cannot excuse it by the widening of the scope of science since it should only lead to the enlargement of treatises. However, I ought to acknowledge that authors will find it ever more difficult to treat science from the same viewpoint as the worthy Lalande did. At the same time I do not at all keep to the viewpoint of the authors of later main books of the same title [Astronomy]*, viz., that they could be written by someone not versed in every branch of that science.*

For such an author it will not be so difficult to follow historically the advance of astronomical knowledge and do the deserved justice to each who had indeed contributed to its development by fully indicating the title and the essential contents of his work. The later so-called guidebooks to astronomy mostly testify to the one-track minds of their authors. Some tempt their readers into looking for science in a pile of expansions of trigonometric formulas; others, in the knowledge of pictures of astronomical instruments; or in some applications of celestial mechanics. Finally, another one, free from one-sidedness, offers a lifeless compilation so remote from showing the needful historical development that he is able to explain in his Preface that he provided no names since otherwise each page would have been overburdened with them.

The scope of science uninterruptedly widens, and I often thought that a contribution that thoughtfully and completely separates astronomy into its branches, mentions the literature essential for each and describes each work briefly but correctly, will be extremely useful for students and scientists alike. Such a contribution, in spite of its comparative brevity, will foster serious work and knowledge and lead the reader to the destination ensured by his background rather than scare him away from it. F. W. B.

11. See Note to contribution [No. 1] on its p. 1. In his biography of Bessel, Wichmann (1860) in detail describes this work. Calculations occupied there not less than 330 pages. R. E.

12. *The curriculum of the Untertertia* [of the fourth class] *of the Minden grammar school included elements of geometry, but I think that without their prolongation they were unsuitable for generating an idea about the true essence of mathematics. The beginnings of the general art of calculation and algebra would have been better adapted.* F. W. B.

13. *Since I became acquainted with Olbers, I had sufficient possibilities to exercise the use of a navigational instrument, of a mirror sextant. His occupation as a practitioner of medicine prevented him from directly determining the time which was sometimes needed, and I had therefore attempted to be of use to him whenever my duties in the office and the warehouses allowed it.* F. W. B.

14. *Monatl. Corr., Jan. 1806. In this paper, Olbers mentioned those calculations completed in a few hours as proof of my skill in such work. This ease of arriving at a result still better proves the adaptability of the Olbers method.* F. W. B. The author of that paper was Bessel, but apparently Olbers added his comment. O. S.

15. In this connection the youngish Bessel wrote to Thilo on 12 Oct. 1805 (Wichmann 1860, p. 149):

Today I am writing […] *you to let you know about something important for me and interesting for you. I am moving to Lilienthal to fill Harding's post. In February or March, after completing our books* [our ledgers?], *I will be able to devote all my time to divine astronomy, to undertake works whose immensity I have until now only considered with a sacred shame*. R. E.

16. This sentence is somewhat awkward.

17. The date in the manuscript is lacking and can only approximately be fixed as being between January and 15 April, see letters No. 27 – 30 of the correspondence between Olbers and Bessel. Erman [Editor of that correspondence].

Brief Information about Those Mentioned

Bode Johann Elert, 1747 – 1826, astronomer
Encke Johann Franz, 1791 – 1865, astronomer
Harriot Thomas, 1560 – 1621, astronomer
Mertens Franz Karl, 1764 – 1831, botanist
Schröter Johann Heronymus, 1745 – 1816, astronomer
Torporley Nathaniel, 1564 – 1632, priest, mathematician, astrologer
Treviranus Gottfried Reinhold, 1776 – 1837, naturalist, botanist
Treviranus Ludolf Christien, 1779 – 1864, botanist, brother of the former
Zach Franz Xaver von, 1754 – 1832, astronomer

Bibliography

Argelander F. W. A. (1843), *Neue Uranometrie*.

Bohnenberger J. G. F. (1795), *Anleitung zur geographischen Ortsbestimmung*. Göttingen.

Erman A., Editor (1852), *Briefwechsel zwischen W. Olbers und F. W. Bessel*, Bde 1 – 2. Leipzig.

Gauss C. F. (1808a), Methodus pecularis elevationem poli determinandi. *W*-6, pp. 37 – 49.

--- (1808b), Über eine Aufgabe der sphärischen Astronomie. *W*-6, pp. 129 – 140.

Halley E. (1749), *Tabulae astronomicae.* London.

Kästner A. G. (1758 – 1769), *Anfangsgründe der Mathematik*, Bde 1 – 4. Göttingen. Sixth edition, 1800.

--- (1772 – 1801), *Mathematisches Anfangsgründe*, Bde 1 – 5. Göttingen.

Kepler J. (1619), *De cometis libelli tres*.

Lacroix S. F. (1797 – 1800), *Traité du calcul différentiel et du calcul integral*, tt. 1 – 3. Later editions: 1810, 1814, 1819.

Lalande J. J. (1764), *Astronomie*, tt. 1 – 2; 1771 – 1781, tt. 1 – 4; 1792, tt. 1 – 3. New York – London, 1966.

--- (1775), *Astronomische Handbuch oder die Sternkunst in einer kurzen Lehrbegriff verfasset.* Leipzig. Translated from the second French edition (1771?).

--- (1801), *Histoire céleste française*. Second edition, 1847.

Longomontanus C. S. (1622), *Astronomia Danica*.

Moore J. H. (1807), *New Practical Navigator Being a Complete Epitome of Navigation*. Reink Books, 2015.

Raynal G.-T. F. (1770, 1774, 1780, 1820, 2006, 2010), *Histoire du commerce européen dans les deux Indes*.

Subbotin M. F. (1956, in Russian), Gauss' astronomical and geodetic work. In memorial volume *C. F. Gauss*. Moscow, pp. 243 – 310.

Tsinger N. Ya. (1884), *Ob Opredelenii Vremeni po Sootvetstvuyushchim Vysotam Raslichnykh Zvesd* (On Determining Time by Corresponding [Equal] Altitudes of Different Stars). Petersburg.

Tsvetkov K. A. (1951), *Prakticheskaia Astronomia* (Practical Astronomy). Moscow. Second edition.

Wichmann (1860), Beiträge zur Biographie von Bessel. *Astron. Z. f. populäre Mitt.*, Bd. 1.

VII

R. Engelmann

[Supplement to Bessel's *Recollections*]

F. W. Bessel, *Abhandlungen*, Bd. 1. Leipzig, 1876, pp. XXIV – XXXI

[1] In the beginning of 1806, following Olbers′ wish and suggestion, Bessel filled the post of inspector at the private observatory of Schröter in Lilienthal and thus became, forever and completely, a professional astronomer. For the first time and at the proper moment, being thoroughly prepared, he got [was able to use] larger astronomical instruments. Naturally, they were only reflecting telescopes barely suitable for micrometric measurements. Nevertheless, they were the best of their type and exactly the simplicity of the measuring device induced Bessel and aroused such an exceptional degree of his insight into observations.

Apart from observing comets and planets by means of a registering micrometer Bessel turned special attention to Saturn. He thoroughly investigated and applied a Schröter micrometer (artificial images at variable distances from the eye, a crude device according to modern notions) which provided measurements of the distances of the Huygens' satellites[1] [from that planet], and, as a corollary, an essentially reliable value of the mass of Saturn. He also diligently investigated the previous observations and took into account the perturbations which only heightened that reliability.

After he got hold of a large heliometer as a measuring instrument of the first rank, other contributions followed that first one published in 1812 [No. 82/17]. He determined [other] parameters of Saturn and studied the motion of its sixth (the fourth of the previously known) satellite and investigated the Saturn system in general, see also [No. 386/22]. Already then his work on the figure of Saturn, which allowed for the attraction of its ring [No. 14/154], convincingly testified to the depth of his penetration into mathematical analysis.

For a few decades Bessel thoroughly and without question decisively liking this subject, continued his first studies in Bremen by calculation and determination of the orbits of comets and of Saturn. He contributed to the theory of motion [of heavenly bodies] by his very first work [No. 3] on the true anomaly for near-parabolic orbits and especially by his most important studies of perturbations in a paper of 1807 devoted to comets and only published three decades later in the *Astronomische Nachrichten*.

[2] At the grievous and troubled time, robbed of power and of the best part of his territory, the courageous Friedrich Wilhelm III of Prussia in close cooperation with the fund of the Berlin University established and richly installed an institute in Königsberg. It should have remained remote from any earthly hubbub as though invariably showing the way to eternity, caring for, and attempting to maintain the ideal goodness of mankind.

No one worthier than Bessel was found for directing the not yet existing observatory in the spirit of its noble founder. In spite of many other tempting possibilities and requests, the young astronomer from Lilienthal gladly responded to the offer and in May 1810 moved to Königsberg as astronomy professor and director of the observatory. Unavoidable delay caused by the erection of the observatory and the scant possibility of proper observations was perhaps less regrettable since it allowed to continue investigations. Brought to perfection, they yielded superb fruit which Bessel had picked from the tree of astronomical knowledge.

Even in May 1807 Olbers had suggested to Bessel to compile a list of stars for 1750 by issuing from the Greenwich observations of the great Bradley. Bessel gladly caught at this idea and assiduously began the work which unexpectedly grew at his hands, led to investigations in more than one direction and to results which even today essentially constitute the basis of our astronomical knowledge.

After compiling the necessary auxiliary tables, reducing the observations for determining the location of the Bradley's transit instrument of 1750 – 1765 and establishing the temporary Greenwich polar altitude by the new and the old quadrants, Bessel already in June 1807 turned to the derivation of the absolute right ascensions of the 14 Bradley's main stars. A certain difference detected by Bürg during his investigation of the right ascension of α Aquilae between the two equinoxes prompted Bessel to go more precisely into refraction.

At first he determined the constant of refraction for [altitude] 45°, the horizontal refraction and the thermal coefficients by issuing from Bradley's observations and the Laplacean theory. A comparison of these coefficients provided by Kramp under either of the two main hypotheses about the change in the air density showed however that they cannot be quite brought to correspondence. Bessel therefore developed a theory based on a presumption about the decrease of the air density[2] and compiled a table of refraction which almost completely corresponded to the Bradley's observations up to altitude 85°. This table was the first and one of the most important results of the discussion of the Bradley's observations. It became the foundation of more extensive Bessel's tables in 1818 [No. 130] and 1830 [No. 248]. Most astronomers are known to apply the latter even today almost without any changes.

With the same thoroughness and precision Bessel determined the right ascension and declination of the 14 Bradley's main stars, the polar altitude in Greenwich [No. 85/111] and the obliquity of the ecliptic. Various checks of these results confirmed the excellent quality of both the Bradley's observations and the calculations made by Bessel. Bessel concluded these investigations in 1808 and published them in 1812 [No. 84/28]. They only constitute a relatively small part of work still needed for completing the catalogue of the Bradley stars[3].

[3] At various times the determination of the spherical constants necessary for reducing the observed places of stars to their mean places, the precession, nutation and aberration, had been investigated previously by astronomers and geometers, but Bessel could not and dared not be satisfied by the known and possibly less reliable data. He felt obliged to derive them from the Bradley observations but drawing in addition on the most reliable new determinations. During 1811 – 1813 he was mainly occupied by investigating precession. Their results are published in a classical work on the magnitude and influence of the precession of the equinoxes which was crowned by the Berlin Academy [No. 104/37]. There, as also in the later studies of nutation and aberration, apart from numerical justification, he developed the theory in many ways by applying new original expansions, for example, by considering previously neglected terms of a higher order.

Recalling also the considerations about proper motion, as for example the derivation of the place of the Polar star, we would at least agree that he thus in a most general way described the foundation of the large catalogue of 3222 stars published in 1818 [No. 130]. This study will remain forever as a brilliant proof of what can be achieved by zeal, industry, patience, acumen, methodical and helpful spiritual power, or when the benevolent fate furnishes, in short, the sum of those qualities to one single man.

Works of such kind are monuments in the kingdom of sciences all the more valuable the rarer they are. To some extent the *Tabulae Regiomontanae* of 1830 [No. 248] ought to be considered as its continuation. There, Bessel intended to offer both the observers and calculators reliable determinations of various spherical constants achieved by himself and others such as the places of the fundamental stars and in a more convenient tabular arrangement all the elements necessary for the transition from observed to mean places. Since then at least in Germany the forms for calculations, suggested partly by Bessel and partly by Gauss, are in general usage. Bessel's contribution [No. 248], and the tables compiled by Wolfers (1858) are irreplaceable auxiliary sources for each astronomer.

At the end of 1813 Bessel began his observations in Königsberg. For a few decades the determination of the place of the Sun, of the 36 Maskelyne main stars and of both polar stars[4] by all instruments at his disposal had become the main aim of himself and his observatory. It is tempting and instructive to see how the obtained results were invariably improved with the increasing quality of the instruments and the further development of their theory. For a practical astronomer, exactly this side of Bessel's achievements, this direction of his natural abilities are the most exciting and most amazing since here Bessel appears as a creator.

[4] In essence, there was previously no theory of instruments, no art or criticisms of observations. Previously, even the most thorough and shrewd astronomers had then been thinking that their instruments, when coming from their manufacturers, were faultless[5]. For them, instruments had only been the means for achieving their aims and investigation of such means seemed unnecessary. Only Bessel maintained and practically proved that an astronomical observation was only worthy when the astronomer observed thoughtfully; when he knew what should be observed and which means could be applied in his work; when he considered his instrument, so to say, spiritually compatible with the observed object; when he regarded his instrument as an entity whose peculiarities, merits and defects were investigated, understood and checked, − only when all this was accomplished observations really became reliable and usable. The greatest astronomers, Gauss and Hansen[6], less exceeded him in purely mathematical matters than he exceeded them in everything concerning observations.

When, in November 1813, the Königsberg observatory opened, the pool of its instruments was meagre. According to modern notions, the quality of both the main instruments, of the Cary complete circle and the Dollond 4 ft transit instrument, was barely satisfactory. Until then, Bessel knew almost only the simplest measuring device, the position micrometer, although he certainly studied and applied it most carefully, and he thought that those instruments, and especially the Cary circle, were very good. However, their investigation, which he began at once, revealed his mistake.

Errors of the graduations, eccentricity, ellipticity of the pivots variations of the collimation error lead to essential errors in the [thus] imperfect or unreduced observations. New and meaningful methods allowed Bessel especially to determine or exclude the errors of the graduations so substantially that the residual errors became ten times less and could have been almost completely explained by the unavoidable random errors of observation[7], and Bessel had similarly investigated the Dollond transit instrument.

As a first result he determined the polar altitude by observing the Polar star (16 Nov. 1813 – 22 June 1814) [No. 95?] and the obliquity of the ecliptic (summer solstice of 1814) [No. 95/158]. He soon ensured a reliable check of the former by observing a long series of circumpolar stars.

[5] Not a few astronomers and especially Piazzi detected a difference reaching 8” between the obliquity of the ecliptic at the winter and summer solstices. This prompted the appearance of many daring conjectures and explanations, but already the first value of that obliquity at the winter solstice of 1814/1815 measured by Bessel was the same as at the previous summer solstice (only 0.”67 less). This was a new proof of how apparently real events or differences often disappear after careful and critical observations by investigated instruments.

Bessel’s year-long occupation mostly consisted in thorough and continuous studies of the solar motion by meridian instruments. The Carlini tables (1810) then in general use proved so erroneous that Bessel decided to correct them by long and possibly continuous solar observations. The reliability of the required constants essentially depended on a most precise knowledge of the instrument and especially on the polar altitude and the obliquity of the ecliptic. During five first years they had been established and checked by the Cary circle whereas the right ascensions of the 36 fundamental stars and both polar stars had also been observed by the Dollon instrument.

The results of these 5-year observations[8] were summarized in a catalogue of the right ascensions of the fundamental stars for the epoch of 1815 [No. 134, 136/86]. In 1820 [correction by hand: 1819; below, 1819 is mentioned once more] the observatory received a meridian circle ordered by Bessel and manufactured by Reichenbach

& Ertel. For the next eight years it was used for observing the Sun and those fundamental stars.

Bessel once more applied new methods for determining the [instrumental] errors and especially the bending of the telescope which possibly was not taken into account when investigating the Cary circle. Refraction was determined anew by observations carried out with that same instrument and two catalogues of the fundamental stars were the result of continuous and sophisticated observations with the new instrument. One of them listed the declinations for 1820 [No. 159], the other one, right ascensions for 1825 [No. 202, 203].

These observations did not wholly convince Bessel in that the obtained places of the fundamental stars were important for the knowledge of motions in the Solar system. Those determinations were twice repeated, the first time, in 1836 – 1840 by Busch, again by the same Reichenbach meridian circle, and the second time, by the new Repsold meridian circle received in 1841. It was Bessel's preferred instrument which he investigated most thoroughly and precisely.

Fundamental determinations made by Bessel who applied that instrument during his last years belong to the most reliable and in general they are the best known in astronomy. In many respects they are not surpassed even today.

[6] When, in 1819, Bessel received the Reichenbach meridian circle he set himself as one of the main tasks the observation of all stars up to the ninth magnitude with declinations between – 15 and 45°. After compiling a precise plan of observations and reductions as well as mounting supplementary devices on the telescope and limb, he began observations on 19 Aug. 1821. They lasted uninterruptedly for more than a decade and ended on 21 Jan. 1833 after observing 536 zones 2° wide, just like those of Lalande.

They were mostly observed by Bessel who was only assisted at first by Argelander, then by Busch. They read the limb and calculated. This great work which embraced 75,011 separate observations proved most convincingly that Bessel possessed endurance, vitality and even physical strength. Argelander later extended these zones north and south just as carefully and tirelessly.

Apart from the direct benefit provided by these observations to the knowledge of the bodies in the Solar system and their motions, for a long time they had been founding the studies of the variable state of the stellar world. Directly connected with those observations were star charts which, following Bessel's suggestion [No. 207/96], had been drafted by many astronomers. In 1828 – 1859 the Berlin Academy published such charts although they only included zones with declinations between – 15 and 15°.

A new epoch in the art of observation began with the large Fraunhofer heliometer which the Königsberg observatory received in 1829. Bessel had strongly felt the lack of devices for very precise micrometer measurements. Now, the heliometer was undeniably preferable to other measuring instruments because of its wider applicability and, in addition, probably a special liking for complicated instruments particularly appealed to the acumen of the observers and prompted Bessel to test it. Unlike others, for example, Struve, Bessel opted for a telescope of a mean optical power, but the heliometer measured large [angular] distances as precisely as small ones, which was only possible to achieve by a crosswire micrometer.

Known and partly mentioned above is the investigation of the heliometer both in general, as an equatorial telescope, and in particular, in all of its details, as well as the results achieved by Bessel's observations of the Sun, Saturn and its sixth (the fourth of the previously known) satellite, the Halley comet and other bodies. Bessel especially valued the comparison of the heliometer with instruments based on other principles. Simultaneously with Struve in Dorpat [Tartu] who had a new Fraunhofer refractor with a crosswire micrometer he observed with exceptional precision many double stars. Position angles almost coincided; on the contrary, with a single exception all the distances[9] were larger than those measured by Struve.

This noticeable difference prompted Bessel to a new long series of observations of the double star p Ophiuchi, and he became satisfied in that his measurements were free from a constant error.

[7] However, the most important and at the same time most arduous and difficult investigation with the heliometer was the measurement of the parallax of 61 Cygni. Even in 1806 and 1808 in Lilienthal, Bessel from time to time unsuccessfully, as should be expected, investigated the parallax of brighter stars. Later, in 1814 and 1815, he measured the right ascensions of 61 Cygni and other bright stars, again naturally without success. He only established that the parallax was smaller then 1"[10]. Now, having a measuring instrument of the first rank, he had to solve this problem. Bessel began to measure the parallaxes of α Bootis and 61 Cygni, then, since August 1837, he concentrated on the latter. Already in the spring of 1838 he convinced himself in the reality of its parallax of about 0."5.

A rigorous calculation of all the most precise observations (i. e., of the comparison with two neighbouring stars), which continued until 1840, finally provided parallax 0."348 with mean (mittlern) error 0."014[11]. This number, owing to the method of its derivation, for the first time deserved and earned full trust.

One of the last investigations, most extensive and penetrating in itself, and followed up by most important work in stellar astronomy, was devoted to the change of the proper motion of Sirius and Procyon [No. 372]. Best observations, and especially those newest made with a Repsold meridian circle, and the following most precise reductions convinced Bessel, especially with regard to Sirius, that there ought to be some objective physical cause for the curious irregularities of their proper motions. His theoretical investigation proved that that irregularity was explained by the existence of considerable (dark) masses in the near neighbourhood of these bright bodies, that, in other words, both Sirius and Procyon were real double stars. Later calculations (Peters, Auwers) as well as direct observations are known to have confirmed Bessel's prediction.

[8] Much more work on spherical and stellar astronomy such as the theory of instruments can only be sketchily discussed here. In 1841 – 1842 Bessel had published a series of most important and most extensive works [No. 350]. Apart from the abovementioned investigations of the Königsberg heliometer, of the double star p Ophiuchi and of measurements of the 37 double stars (Vergleich-Doppelsterne), this contribution includes articles about the influence of refraction as well as of precession, nutation and aberration on the results of micrometric measurements; on the apparent figure of a partly illuminated planetary disk [No. 282]; on the places of the 53 stars of the Pleiades [No. 347]; the determination of the mass of Jupiter [No. 348]; an analysis of eclipses a. o.

Most of them, including masterpieces of thorough analytical treatment of astronomical problems, had been called forth by the need to provide sufficient precision for all the elements of reductions of the most precise (heliometer) measurements. Perhaps exactly for this reason Bessel had simultaneously refined and developed practice and theory. His study of the Repsold meridian circle [No. 369] and his last investigation of the distortion of the vertical circle due to the influence of gravity [No. 370/76, 191/63][12] proves how pleasant it was for him, in his last years, to see the perfection of pure observations and the theory. An earlier contribution of 1824 [180/48?] that should not be underestimated once more stressed the applicability of transit instruments in the prime vertical for determining the polar altitude or declination.

By nature, Bessel remained more distant from pure mathematics and most mathematical problems which he handled had been derived from astronomical observations. Nevertheless, when striving for comprehensiveness, he went over to mathematical considerations, left the special astronomical background and for a while wholly devoted himself to the general and mostly analytical treatment of the problem.

His investigations of factorials [No. 83/109], attraction (Anziehung), expansions into series were prompted by purely astronomical problems and the study of logarithm integrals (Bessel functions)[13] was possibly the only one which had not been thus provoked.

[9] More extensive and more significant and fruitful owing to their influence were certainly Bessel's investigations and results in geodesy and physics of the Earth [in triangulation and the figure of the Earth], in particular, his studies of arc measurements, of the length of the seconds pendulum and on the Prussian unit of length. In many respects the applied methods and their execution belong to the best of his works and of the scientific arsenal in general.

Already in 1817 Bessel had determined the coordinates of some geodetic stations around Königsberg and checked the values of the angles measured by Textor (1810). In 1824 he measured a baseline adjoining the older triangulation, detected enormous errors in that geodetic work founded by the baseline and unquestionably proved that new and more precise measurements were needed.

Almost at the same time he continued the determination of the seconds pendulum which had been begun by Tralles[14]. For this goal Repsold had manufactured an excellent pendulum apparatus. Coupled with new original methods of observation and their treatment and allowing for the previously wholly neglected air resistance he attained, for the first time ever, a precision necessary for further reliable conclusions, especially those concerning the flattening of the Earth [of the earth ellipsoid].

In 1825 and 1826 Bessel determined the length of the seconds pendulum in Königsberg, and in 1835, in Berlin [No. 290]. Both results belong even today to the most precise and delicate measurements, but he had to surpass most serious difficulties connected with passing over to a new field of research. The Berlin Academy published these investigations (on the length of the seconds pendulum, in 1826; the investigation of the force with which the Earth attracts substances differing in constitution, in 1830 [No. 250; 264/139][15]; on the length of the seconds pendulum in Berlin [see above]).

From 1832 during many summers Bessel had been engaged together with Baeyer in geodetic operations and measurements which from time to time had to be abandoned due to the abovementioned investigations or pure astronomical work. Here also the most superb instruments (especially the Repsold baseline apparatus[16]) whose most precise and critical investigation as well as the applied methods of observation and the mathematical treatment of the results obtained allowed Bessel and Baeyer to attain previously unknown precision.

The relatively small arc thus measured in Eastern Prussia became one of the most important among the wide set of such measurements for the derivation of the parameters of the size and the figure of the Earth. These results which cannot be here considered in detail were published in 1838 in the joint work of Bessel and Baeyer [No. 322/135][17]. The numerical values of the parameters of the earth ellipsoid which Bessel deduced from his own [and Baeyer's] and the other most trustworthy arc measurements are still considered as the most reliable. Only now, mostly due to work initiated by Bessel, as it ought to be recognized, they underwent inevitable but in general slight changes.

Finally in a closest connection with the above investigations is the study of the Prussian unit of length and its relation to the toise of Peru which Bessel had described in his book of 1839 [No. 334]. The most essential practical result was here the determination of the original normal standard (3 Prussian feet) installed in the building of the Ministry of Commerce. Until now, it has been the foundation of the Prussian system of units[18].

Bessel often encountered purely physical problems during astronomical investigations (especially concerning refraction). Here also he introduced his own new aspects and methods, for example when studying the calibration of thermometers [No. 217/41[19]] generally accepted even today.

[10] Above, although superficially and insufficiently, we described the work of Bessel the observer and investigator, but we ought to add a few words about his biography and nature.

Bessel always consciously and gladly carried out the duties entrusted to him as to a professor of the Königsberg University. Together with his celebrated university colleagues M. H. Jacobi and Neumann he raised the mathematical and physical disciplines to quite a high level. In Germany, since his days the Königsberg mathematical school is considered as a leading institution of its kind[20].

Bessel very highly estimated the significance of the noble popularization of science which he himself experienced during his life in Bremen. A series of popular reports which he read especially later (and which Schumacher published in 1848, after his death [No. 385]), informed a wide circle of listeners about astronomical phenomena and processes and explained them. In essence, his style cannot be called easy or fluid, but it was clear and sound and each word testified to the perfect command of, and penetration into the subject. His reports were always specimens of generally comprehensible presentations of rigorous and sometimes complicated scientific problems[21].

Fortune had been richly granting him pure joy, noble enthusiasm for science, a pleasant family life and warm friendship. At the same time it did not spare him from blows or pain that afflict each human being. The last years of his life had been agonizing owing to deep incurable sorrow and even physical pain.

Soon after his departure from Lilienthal, in 1812 he discovered a faithful partner for life in Johanna Hagen from a respected Königsberg family who until now mourns his death. During their happiest marriage she presented him two sons and three daughters although not all of them outlived him. In 1840, the death of his only adult son Wilhelm who gave hope was a heaviest blow for him.

For a long time, his own health in spite of his sensitive constitution had remained sound but then began to suffer under excessive strain and exhausting activity of his tireless spirit. Gradually and noticeably since 1844 it led to the formation of a tumour in the peritoneal area, and on 17 March 1846 it snatched the great man from us.

Bessel's nature and personality which the later-born can only incompletely discern or assess was described by his long-standing family doctor, Dr. Kosch (1846), under the freshest impression of Bessel's death:

Bessel, the great astronomer of our century, had only arrived at the 62nd year of life. He was a man of short stature, weakly and skinny, with a noticeably pale and deeply furrowed face. His head was covered by silvery-grey hair hanging down in a rich body and reaching his bushy eyebrows. The upper part of his body was slightly stooped to the front and for many years superficial viewers would have seen him as an old man. However, as soon as spoken to, his calm and somewhat rigid features brightened up radiating kindness and mildness.

The clear typical look of his gleaming eyes, agility of movement and the rapid flow of his melodious voice sufficiently testified that a powerful spirit with a still youthful force dominated its frail shell and prematurely wore it out. The spiritual elasticity covered the defect of physical strength and provided toughness and endurance to the weak body which enabled it to cope with quite unusual strains.

Bessel worked for the most part of the day with short interruptions and thus founded his immortal glory in science, and, until his last years, he observed the sky for a large part of night time. Even in the beginning of his last illness, he had not given up the pleasure of *hunting and often, gun in hand, rambled for many hours. Almost daily he went for long and rapid walks without feeling especially tired. Sleeping for many refreshing hours restored his expended strength and the early morning found him fresh and cheerful, puffing away at a pipe, mostly standing*[22] *once more at his working place.*

His usual way of life was plain and moderate. Being however quite remote from anxious pedantry he did not scorn the pleasure of social intercourse at a well laid table. Invariably the soul of the company in which he found himself, he brightened it up by intellectual talk. Then, unburdened, the same evening he took up his interrupted work and observed until late into the night. The liveliness of his spirit allowed him to feel almost no tiredness or not to heed it.

In inviolable order and regularity he always eagerly devoted himself to the solution of the most difficult problems which science continually poses to its selected servants. Great and perhaps for a long time unattainable in his scientific field, he was at the same tine most charming in social life. He conveyed the proper feeling of his worth not by proudly isolating himself or by striving to favour others by posh condescension.

Who came near to Bessel was delighted by his good will, friendly nature and the most direct contradiction between the heat of an argument and his fascinating mildness and fineness although sometimes not without stubbornness with which he attempted to convince his opponents. With these qualities he combined vigour and firmness of character and a rare strength of his soul. He therefore aspired for high and noble aims and for keeping to a gained conviction. Here were the roots of deep respect and trust to which he steadfastly kept with regard to those to whom he once felt an affection[23].

[11] This account squares with the image which appears from his long-standing and instructive correspondence with his fatherly friend Olbers. Eagerness and passion, vitality and willpower allowed him to study exhaustively the undertaken scientific matters and achieve his aims in, and stand the tests of life. Warm feelings permitted him to remain faithful to his friends Olbers, Schumacher and Gauss[24]; love of truth and sense of justice allowed him to recognize willingly the special aptitudes or merits of others.

In wonder, he gracefully saw the greatness of Gauss[25], with respect he adhered to Olbers, and closest affinity tied him with the kind and wise Schumacher. And with real and effusive love and patriotic enthusiasm Bessel the Prussian looked up to the King. The exalted of the earth gladly show deep respect for geniuses and the King repeatedly, and also when the great astronomer was lying on his deathbed, expressed it to him in the most reasonable and personal manner.

Bessel was great not only because of the quality of his spirit, an aspect of his natural talent; his greatness should be found in the harmonious connection and fusion of his most various aptitudes and skills of spirit, character and body. Laplace, Gauss and Hansen[26] certainly surpassed him in the depth and richness of mathematical speculations, perfection and elegance[27] of the display of analysis;

William and John Herschel and Struve had been near him in the talent and keen perception of observation(s); Encke, in the skill of calculation; Argelander perhaps reached Bessel in diligence, endurance and disposition natural for an observer. However, in anyone mentioned those separate abilities had not been joined together to form a single one as they did in Bessel who therefore was exhaustively versatile[28].

It is questionable whether Bessel consciously and unshakeably thought of a definite aim, as for example of a most general proof of the Newton law of universal gravitation as many others did. His choice of various studies in the field of attraction as special problems is also arguable. In general, when judging his goals and their underlying ideas it is best to recall the own words of the Master [No. 350, Intro.] and thus to end my account:

When astronomy began to attract me, it fascinated me not by some particular kind of work which its admirers have carried out, but by the possible results. Even later no predilection for any special astronomical occupation had occurred and when occasionally I had been prepared to devote more time to calculations or to increase my fund of astronomical observations, it was always caused by a strive for becoming better acquainted with a certain topic or for removing a clearly appeared obstacle which hindered the increase of knowledge of many themes.

Apart from the lacking inclination to collect data without any idea about its usage, I had early and forever became convinced in that obtaining astronomical results was not a necessary condition of success, but at least the most possible reliable guarantee that my defects thus revealed can be made up by inducing me to eliminate them.

Notes

1. Huygens had discovered only one satellite; now, not less than ten of them are known.

2. A table of refraction can only correspond to the Bradley observations if compiled for the area of Greenwich, and only for the same time of day during which he carried out his observations and for the same air temperature. So how did Bessel manage? Olbers, in a letter to Bessel of 2 Nov. 1817, remarked that anomalies in refraction more or less depended on the location of the observatory.

3. I do not know whether that catalogue is now completed.

4. In § 5, the author once more mentioned two polar stars. Without explanation Fricke (1970) named them: α and δ Ursae Minoris.

5. This is wrong, see Sheynin (2009, § 1.1.4) for an incomplete discussion. And Tycho and Bradley, see [i, Note 16], if not Hipparchus should be mentioned as well.

6. Hansen, a great theorist, offered a theory of the Moon, measured the solar parallax and studied perturbations in the Solar system.

7. Nothing in essence is said about the elimination of the errors of the graduation and the description of the obtained results is unconvincing. A few lines above and a few times afterwards the author introduces the term *theory of instruments*. Actually, he meant the theory of investigating instruments, but even so does such a theory exist?

8. These observations (see above) did not concern the fundamental stars.

9. *Distance* here means the angular distance between the components of a double star.

10. Already Bradley knew that the parallax of stars was less than 0.″5 (Blazko 1947, p. 203).

11. Comparison with other stars means that Bessel had determined the relative parallax (Blazko 1947, p. 204). On Bessel's measurement of parallaxes see [No. 318/120, 319, 321/83, 337/84, 338, 338*].

12. In 1844, Thomas Galloway informed Bessel that at a meeting of the Royal Astronomical Society the translation of his letter to Sir John Herschel [No. 370] *on the effect of gravity in obtaining the shape of a meridian circle was read and* [one word is undecipherable] *with great interest* … See Sheynin (2001, pp. 170 – 171).

13. Concerning logarithm integrals see [No. 58/106, 81/108]. Bessel functions constitute a particular case of cylinder functions, but after cursorily reading Korn & Korn (1961/1968) I did not find any connection of those with the logarithm integrals. Then, what exactly is the *mean* (mittlern) error? And the number of significant digits in the value of the parallax is certainly excessive.

14. Bessel continued the work of the deceased Tralles [viii, § 8]. During ca. 1900 – 1925 pendulums had been protected against the motion of the air (cf. below), and later they were observed while oscillating in vacuum (Bomford 1952, § 6.01). And the method of registering time had changed (Ibidem).

15. See the very end of this contribution.

16. This apparatus was later called after Bessel, see [ix, Note 20] and Bagratuni (1961, p. 14). Note that on p. 19 Bagratuni called the Gauss celebrated formula for the mean square error after Bessel.

Repsold [viii, § 23] mentioned an *Ausdehnungmesser,* a device for measuring deformation in the elements of constructions used when measuring baselines. I can only mention a mechanical device [No. 322/135, p. 69] whose purpose is not known to me.

17. It was Baeyer and Bessel who jointly carried out the arc measurement, but only Bessel was the author of the book [No. 322/135].

18. A unit of length cannot by itself be the foundation of a *system* of units.

19. I was unable to understand the calculations in this contribution.

20. The history of that school is certainly little known.

21. I resolutely disagree, see [x].

22. In those times, as I have seen in some film, clerks (and possibly scientists) had been working in a standing posture.

23. Bessel completely trusted Kosch, see his letter to Humboldt of 19 Apr. 1844 (Feiber 1994).

24. See [iii].

25. On 26 Oct. 1818 he wrote to Olbers (Erman 1852, vol. 2):

Gauss was able once more to form a marvellous opinion about secular changes. At his hands everything takes a new look. When reading his works it often seems incomprehensible why others had not hit upon the same idea. This, however, should indicate a true genius who does not miss a most natural idea. I am sufficiently convinced in that Gauss is at least a divine genius. R. E.

26. The author had already mentioned Hansen at the end of § 4.

27. Laplace and elegance? First, his contributions are known to make extremely difficult reading. Second, here is an appropriate judgement (Gnedenko & Sheynin 1978/2001, p. 224):

Laplace's exceptional intuition […] *enabled him to arrive at correct conclusions using non-rigorous and, now and then, simply confused reasoning*.

28. The author could have well mentioned Mudge [ii], Bouvard and Airy.

Brief Information about Those Mentioned

Jacobi Moritz Hermann, 1801 – 1874, physicist, inventor

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

Baeyer Johann Jacob, 1794 – 1885, geodesist

Bürg Johann Tobias, 1766 – 1834, astronomer

Busch August Ludwig, 1804 – 1855, astronomer

Dollond John, 1706 – 1761, optician

Fraunhofer Joseph von, 1787 – 1826, physicist

Hansen Peter Andreas, 1795 – 1874, astronomer, mathematician

Neumann Franz Ernst, 1798 – 1895, physicist
Peters Christian August Friedrich, 1806 – 1880, astronomer
Reichenbach Georg Friedrich, 1771 – 1826, manufacturer of optical instruments
Schröter Johann Heronymus, 1745 – 1816, astronomer

Bibliography

Bagratuni G. V. (1961), In Bessel (1961, pp. 5 – 21).

Bessel F. W. (1838), *Gradmessung in Ostpreussen* etc. [No. 322/135].

--- (1961), *Izrbannye Geodesicheskie Sochinenia* (Sel. Geod. Works). Moscow. Editor, G. V. Bagratuni.

Blazko S. N. (1947), *Kurs Obshchei Astronomii* (A Course in General Astronomy). Moscow.

Bomford G. (1952), *Geodesy*. Oxford.

Carlini Fr. (1810), *Nuove tavole de moti apparenti del Sole*. Second edition, 1832.

Engelmann R. (1876), Allgemeines Verzeichniss der Schriften Bessel's. In Bessel F. W., *Abhandlungen*, Bd. 3. Leipzig, 1876, pp. 490 – 504.

Erman A., Editor (1852), *Briefwechsel zwischen W. Olbers und F. W. Bessel*, Bde 1 – 2. Leipzig.

Felber H.-J., Editor (1994), *Briefwechsel zwischen A. von Humboldt und F. W. Bessel.* Berlin.

Fricke W. (1970), Bessel. *Dict. Scient. Biogr.*, vol. 2, pp. 97 – 102.

Gnedenko B. V., Sheynin O. B. (2001), Theory of probability. A chapter in *Mathematics of the 19th Century*, vol. 1. Basel, 2001, pp. 211 – 288. Editors, A. N. Kolmogorov, A. P. Yushkevich. First published in Russian in 1978. First English edition, 1992.

Korn G. A., Korn Theresa M. (1961), *Mathematical Handbook*. New York, 1968.

Kosch (1846), *Bessel's letzte Krankheit*. Königsberg.

Sheynin O. (2001), Gauss, Bessel and the adjustment of triangulation. *Hist. Scientiarum*, vol. 11, pp. 168 – 175.

….--- (2009), *Theory of Probability. Historical Essay.* Berlin. **S, G,** 10.

Textor J. C. von (1810), *Beschreibung des Verfahrens bei der trigonometrisch-topographischen Vermessung* …

Wolfers J. P. (1858), *Tabulae reductionum*.

VIII

Joh. A. Repsold

Friedrich Wilhelm Bessel

Astron. Nachr., Bd. 210, No. 5027 – 5028, 1920, columns 160 – 214

[1] The eldest representative of the Bessel family was colonel Jobst von Bessel born in Livland [now, parts of Latvia and Estonia] who lived at the end of the 15th century. The line of those living in Minden began with Johann von Bessel at the beginning of the 17th century (Schumacher 1889, p. 152). Bessel himself provided further information about his family and his youth until age 25 in an essay [vi] shortly before his death which was first published in his correspondence with Olbers (Erman 1852, pp. IX – XXX). We reprint this essay without its Note 10 since Schumacher found out that its author was not Bessel[1].

[2] Bessel's autobiography certainly cannot be altered, but, for describing his honourable place among astronomers which he already occupied when moving to Lilienthal, we note that, through Olbers' mediation, Since Dec. 1804 he began corresponding with Gauss after volunteering to help him calculate the places of the Sun for studying the motion of the three new [minor] planets, Ceres, Pallas and Juno. In a short time the correspondence of the 20-year-old Bessel with the seven years older and praiseworthily known Gauss[2] and with his fatherly friend Olbers, 27 years older, became relaxed.

Again through Olbers Bessel became acquainted with von Zach and visited him while on a commercial journey. However, von Zach was absent at the observatory in Seeberg near Gotha and Bessel met his assistant, von Lindenau who later became the editor of the *Monatliche Corrrespondenz* (Schumacher 1889, p. 99) whereas Bessel was its author [No. 1/1].

And so, in astronomy Bessel was not anymore unknown. He gradually had been reaching the decision to abandon his commercial activities for totally devoting himself to science. On 28 Jan. 1805 he wrote his former teacher and friend Thilo in Münster who had then been building a small observatory for himself (Schumacher 1889, p. 99):

Who will have to observe the sky there? […] *Had I devoted myself to astronomy a few years ago, there would have possibly been some hope for me, but now I ought to give up this pleasant idea. I would be very glad to be able to change now my occupation*.

It should be assumed that Olbers had guessed this secret wish and that he himself attempted to encourage Bessel. Indeed, on occasion he had recommended Schröter[3] in Lilienthal near Bremen to invite Bessel and acquaint him with the observatory there, be with him for a night and show him the instruments at work. A few days later, 18 July 1805, Bessel wrote Olbers:

And so, the time for deciding where will I live, here, there, or elsewhere, comes nearer. On this decision depends my future.

He (Schumacher 1889, p. 100) wavered between fear and hope. Such was the situation when Harding, Schröter's assistant, left Lilienthal. And now, Olbers understood that a decision ought to be made. On 10 Oct. 1805 he wrote Bessel:

Can you tell me something else about the possibility of work with Schröter from whom I have received a detailed letter? I really wish to know your answer before 8 in the morning since I will then write to Lilienthal.

Bessel answered at once. He did not hesitate to offer himself whereas Schröter does not want him[4] (Schumacher 1889, p. 101):

Tomorrow you intend to complete the business with Lilienthal which is a clear proof of your magnanimity and my gratitude is as boundless as my respect which I feel for you. I have nothing more to say.

[3] And thus was the problem solved, but Bessel was only able to move to Lilienthal on 19 March 1806 since there remained much to do at the firm. He only still worried about being able to support himself rather than having to burden his father. Only a half of Harding's previous salary could have been given him, but, with occasional earnings for reviewing, which he had already begun, he hoped to manage.

Schröter's essential practical activities were split up between his duties which concerned agriculture and his observations, whereas Bessel wholly applied himself to observation and treatment of phenomena in the heaven as well as to theoretical studies which should have allowed him to use his observations in the best possible way. He had many instruments at his disposal; however, except the quadrants they were only fit for observation but hardly suitable for measurement. A micrometer described as a star-gauge seemed inappropriate, presumably being too sensitive and having a too narrow range of measurements.

In any case, Bessel preferred to work with a 15 ft reflector manufactured by Gefken[5] with Schröter's additional measuring device. It made possible the comparison of the observed object seen with one eye with a grid seen with the other eye. The grid could have been shifted within the range of the accommodation of the eye on a graduated rod parallel to the optical axis of the ocular [No. 82/17]. Bessel called this method of measurement the only possible one in Lilienthal.

Bessel's work mostly concerned comets and the newly discovered minor planets, but included everything that excited his interest, and Olbers' advice was decisive for him. Bessel only kept in touch with Schröter and his elderly sister, and Schröter, although not unfriendly, was barely communicative. Still, life in the old monastery yard with a church in its middle was not bad, especially in summertime. And when Bessel needed diversion and movement, he was always able to go hunting. He was somewhat longsighted, but his eyesight was excellent, which was useful [also] for hunting and remained good until his death[6]. All the other time Bessel devoted to eager work. Visits were seldom and he all the more valued his gradually extending correspondence.

In the spring of 1807 Olbers advised him to take a rest by going to Minden to the wedding of his eldest sister. While there, he found out that Gauss was expected at Lilienthal. Full of joy to become personally acquainted with his highly respected friend, he hurried home. Gauss, however, had delayed his journey and they only met on 28 June 1807 at Olbers' house where both of them lived for two days. At that time Gauss wrote to his wife (Schumacher 1889, p. 110): *This Bessel is a most delightful fellow*. Then Schröter himself came in his own coach to invite Olbers and his guests to Lilienthal, and Gauss remained there for three days more. After parting, Bessel complained: *Today I have followed* [accompanied] *you from one place to another for the whole day*.

One day Olbers (O – B, 10 May 1807) suggested Bessel a theme: a compilation of a star catalogue for 1750 from Bradley's observations. Bessel eagerly took in this idea and immediately began preparations to this lengthy and laborious undertaking and went on with it whenever possible along with his current work.

[4] With time, in spite of all his work, Bessel became depressed by his loneliness. In 1808 Schröter had been extremely busy with establishing a new fen settlement with a mill and with other economical work. Except for observations, he was therefore unable to look thoroughly after Bessel's work and often they had no topic for a lively conversation. In addition, in the spring Bessel lost his friend, Johann Heinrich Helle in Bremen, who had been very helpful with the making of his first measuring device, the sextant. A letter from Bessel's elder sister Amalie[7], who was near to him, informed Bessel that Helle died on 1 July 1808 of an ill-fated heart trouble. Amalie complained that Bessel wrote her ever shorter and rarer. He answered however (Schumacher 1889, p. 115):

In Bremen, I had always been happy. Whatever happened which could have hurt me, no one tried harder than I myself to excuse it. Here, in Lilienthal, everything is in a different way. No one harms me, but I am still seized by an inclination to suck poison out of roses.

The political circumstances had also been highly dismal and dispiriting and for a long time Bessel had even been in danger of being conscripted. Finally, in the middle of 1808, Gauss and Olbers relieved Bessel of that danger after pleading for him with the Westphalian state councillor von Müller.

Bessel became so praiseworthy known that many institutions had desired to win him over. At first he was asked to [head?] a college in Düsseldorf opening under new authorities. This offer did not take to him and he finally declined. Then came offers of extraordinary professorship from Leipzig and from Greifswald, but especially attractive was von Lindenau's somewhat indefinitely formulated wish to invite him to Gotha. All this was discussed on 2 Nov. 1809 in Lilienthal among friends when Olbers with Gauss and Schumacher on their way to Hamburg were present there.

However, almost immediately after that a new invitation had arrived. Wilhelm von Humboldt, the director of the department of creeds and education[8] of the Prussian Ministry of public education asked Tralles, an academician of the Berlin Academy, *with the aim* (B

– G, 9 Nov. 1809) *of inviting me to Königsberg for erecting an observatory. The business was soon concluded since all my requirements were met. My salary is 800 thalers with free housing and heating*.

The exceptional trust which underlay the responsible invitation should have extremely satisfied Bessel, but all the excitement of that year had got on his nerves and after deciding to move and calming down he fell ill for a week (O – G, 27 Febr. 1810).

[5] On 27 March he became able to take leave of Schröter and journeyed first to Minden for picking up his sister Amalie to accompany him[9], then through Göttingen where Gauss once more [cf. § 3] found him *quite a good man* (G – O, 15 Apr. 1810) and Gotha to Berlin. There he was met very nicely.

Here in Berlin it is good for an astronomer. It is a pity however, that Bode is so feeble. […] *Tralles is a splendid person, highly talented and practically skilful* (B – O).

Bessel discovered that the plans for his observatory had been prepared, although (B – O, 26 Apr. 1810)

I have asked to have a voice which was gladly given to me. Now I will make no mistakes at all since I was promised that in this business only my opinion will be heard.

To fulfil the wish of his parents, Bessel ordered a small gypsum relief of himself which was made by Leonhart Posch (Schumacher 1889, p. 153). […] On 11 May 1810 Bessel with his sister came to Königsberg where they felt themselves well *among many friendly disposed people* (B – G, 24 May 1810).

Soon Bessel chose two places, both suitable for his observatory, and began waiting for the final decision [from Berlin]. He had already revised the plan of the observatory (B – G, 26 Aug. 1810) but the bought instruments, the pool of [the late] Count von Hahn from Remplin (*Mon. Corr.*, Bd. 14, p. 285), had not yet arrived.

A difficulty appeared in that Bessel had no doctorate whereas the elder professors considered it absolutely necessary for carrying out the duties of professor. After all, there existed institutions which were able to confer him a doctorate at once, but he feared of having to pay dearly and this he did not wish to do. So he asked Gauss to arrange his doctorate at Göttingen which the latter achieved with some bother but without any further steps from Bessel.

Soon Bessel became accustomed to Königsberg and in August 1810 he wrote to Gauss:

I like it very much here. I feared lecturing, but it lost its unpleasant aspect. I read rather gladly and always to a full audience.

Until that time Bessel’s contributions had mostly been published in the *Mon. Corr*. or in Bode’s *Astron. Jahrbuch*, but in 1810 he published his first separate work [No. 60] about the comet of 1807 which he had much studied in those years. And now the postponed Bradley’s observations came into their own as far as the observations in the [not yet existing] observatory allowed it.

At the end of 1810, in spite of the hard times, the more suitable but more expensive of the two building sites suggested by Bessel was prepared for work by the purchase of the hindering mill, and the work had indeed begun. The instruments had also arrived. They were (B – G, 27 Dec. 1810)

Beyond expectation excellent. The (Dollond – J. A. R.) *transit instrument is better than the one which I saw previously. It is furnished by devices which make me think that it can be better than that in Seeberg. The* (Cary – J. A. R.) *circle is excellent by construction and graduation. It is similar to that possessed by Piazzi, has a level and an excellently fastened Bleifaden. No verniers, but an external micrometer with which angles can be measured unbelievingly precisely*. […]

Pleasant indeed is the (Dollond – J. A. R.) *equatorial telescope with its heliometer having a 27 ft lens* (B – O, 12 Jan. 1811).

The other instruments are less important, but all of them taken together win respect for von Hahn's pool. Only the Klindworth clock seems to be much worse than the Repsold clock (which Bessel bought in 1810 – J. A. R.)

Bessel found a dusty Dollond 7 ft achromat in the [city?] library and, after polishing up the glasses, he brought it to a proper working condition (B – O, 12 Jan. 1811).

However, in the middle of 1811 the erection of the observatory ground to a halt owing to money shortage and, in spite of the hard times[10], Bessel felt himself obliged in its interest to accept the offer of a job arranged by Olbers (O – B, 31 Oct. 1811) from the observatory in Mannheim [of heading it?] and thus to ensure an immediate completion of the observatory or to demand his parting (B – O, 7 March 1812; suppl. to Bd. 183 of *Astron. Nachr*.). The problem was attended to, money was found and besides that Bessel received 300 thalers of additional payment (B – O, 26 March 1812; same suppl.).

So he decided to stay put. Naturally, something else could have determined that decision: not long ago Bessel had fell in love with Johanna Hagen, a daughter of a medical officer of health in Königsberg. On 10 Nov. 1813 Bessel became able to open the observatory, and a year earlier he had celebrated his wedding.

[6] Already in the end of 1811 the Paris *Institut* [*de France*] awarded Bessel the Lalande prize for his table of refraction compiled from the Bradley observations (see [vii, Note 2]). Certainly still more welcome was his appointment to one of the eight full mathematical members of the Berlin Academy coupled with the prerogative of living in Berlin just as local members did (B – O, 8 July 1812).

The winter of 1813 was *dreadful*.

Everyone is restless, everyone is ill and many dear to us people died but we have been spared and I escaped with a very slight nervous fever. However, coupled with a cold which gripped me in the beginning of winter, it violently attacked my breast, and more than once frightened me of consumption. But the returned spring very much improved my condition.

Do not criticize me, dear darling Olbers, for having much worked during winter in spite of my illness. It was not too much, and I had to conclude finally my Bradley Ana which I did six weeks ago (B – O, 26 Apr. 1813).

Olbers had in a most friendly way urged Bessel to spare his strength and health, but Bessel (B – O, 2 Febr. 1814) answered:

But what should I do? *Should I, having plenty of work, indulge myself by refusing to do anything? May I, even if that was my wish? Will I thus fulfil the expectation of the King and his councillors who in this* [hard] *time built me the observatory*?

Yes, he would have gladly had an assistant but did not know how to find one. Finally, he asked Olbers to advise him as a physician.

In November 1814 Bessel had almost edited the Bradley catalogue but encountered difficulties with finding a publisher (B – O, 7 Nov. 1814). That work [No. 130] appeared two years later as a subscription edition (B – O, 23 Apr. 1818). In 1815, after issuing from this contribution, Bessel published a study of the precession of the equinoxes [No. 104/37] rewarded by the Berlin Academy.

Observations were very pleasant for Bessel but he soon founded out that his English circle was a *changeable instrument*. Already in March 1814 he asked Reichenbach about a four feet circle; he did not want a repeating instrument but a meridian circle resting on two supports (B – O, 7 March and 2 June 1814). However, the series of observations of the solar altitude already begun by the Cary circle were most carefully continued, and, according to Bessel's principle not to amass observations, already after a year he *eagerly published* them (B – G, 18 Febr. 1815). Struve, who visited Bessel in November 1814, counted for his own pleasure these observations and *had already found 8 thousand of them* (B – G, 7 Nov. 1814).

An annoying inflammation of the eyes, a consequence of overzealous work, was soon successively dealt with after taking Olbers' expert advice (O – B, 9 June 1815). Already in September Bessel published the first section of his observations [No. 106] for 1813 and 1814. During the next fifteen years 20 similar volumes had been published, at first a year apart, then more rarely.

For his numerous small communications Bessel greatly missed the von Zach's *Mon. Corr*., which ceased publication at the beginning of 1813. Happily, from the beginning of 1816 von Lindenau and Bohnenberger began publishing the *Zeitschrift für Astronomie*, but it only lasted until the end of 1818. Bessel apparently did not willingly turn to Zach's *Correspondance astronomique*.

[7] Bessel's correspondence with Gauss had incessantly been very friendly and concerned most differing theoretical and practical themes. It would appear that Bessel was prompted to examine in more detail his English equatorial telescope with the superior twofold lens by Gauss' first trials with his Fraunhofer heliometer and thus had acquired a strong interest in such measuring devices.

In Munich two meridian circles of the same construction were manufactured [by Reichenbach] one after the other for Königsberg and Göttingen. This ensured the possibility of a comparative examination of these instruments of the new type, but it never was an examination in itself since observations were also needed. Before Bessel began using his meridian circle he had completed his five-year long measurements with the Cary circle. He wrote about this (B – G, 18 July 1816):

I have barely thought that such useful observations can be made *with my instrument. It is unbelievable what even secondary instruments can accomplish when they are meticulously known. This, however, will never happen when having instruments in abundance*.

[Gauss (G – O, beginning of April 1819) noted that the quality of Bessel's instruments was not high.]

Typical for the frank correspondence between Gauss and Bessel was the latter's remark which he made when they, without knowing it, studied the same mathematical topic, the Kramp factorials (B – G, 12 Jan. 1812):

Concerning this topic, it is pleasant for me that you are interested in a subject which for some time has been delighting me. It would be still more delightful had I not been tempted to publish my ideas. Indeed, what sense can it have since you wish to occupy yourself with the same or a related matter? It goes without saying that I cannot imagine directing my ambition to deal with something as nice and exhaustive as that which we accustomed to wonder in your contributions.

On occasion, the considerate Olbers did not quit admonishing his favourite to take care of his much claimed strength (O – B, 26 Apr. 1816):

My dear friend! Moderata durant [only the moderate survives]. *Indeed, such stress as you have until now been experiencing cannot be endured for a long time. Occupy yourself with science, with your family and friends! Try to find a skilful assistant as soon as possible: he will make your work somewhat easier*.

Olbers had reason for that admonishment since Bessel (B – G, 5 Oct. 1818) wrote: *I note that my body is not anymore as durable and untiring as previously*, although Argelander, *a very good disciple*, had been helping him.

Meanwhile Bessel had luckily escaped a great danger. In January 1818 his own dog had bitten him in the thumb of his right hand. The dog was ill, and some symptoms indicated the beginning of hydrophobia. An autopsy showed that it was not rabies, but the physicians did not dare waste time and right after the bite administered the strongest antidote[11] which badly injured the thumb and provoked strong suffering of the entire body. During the healing period observations became impossible and Bessel calculated new tables of the Polar star (B – O, 25 Jan. and 9 March 1818).

[8] Since 1816 Olbers and Bessel had been discussing Bessel's journey to his old home town [Bremen] but something always prevented it, and especially Bessel's unwillingness to leave the observatory standing idle and unguarded. Now (?) observations were interrupted since the observatory had to be rebuilt for the expected meridian circle and the supervision of that work could be left to Bessel's disciple Gotthilf Hagen (a cousin of his wife).

Bessel decided to go in the spring of 1819, and not alone, but with his wife, sister and eldest son Wilhelm, Olbers' godchild. They should see their (?) parents, Olbers, Gauss and Lindenau, and, on the way back, Schumacher and Repsold.

He announced his intentions to his friends. In the beginning or mid-July, he informed Gauss, who, however, depended on Schumacher's preparations to their common geodetic measurements in Lauenburg and was unable to say definitely when they will begin although did not doubt that this will happen *about the end of this month* (June), *perhaps earlier* (G – B, 10 June 1819).

From Berlin Bessel went to Gotha and remained there somewhat longer than planned since Lindenau was certain to receive the news about Gauss' departure. Then on 28 June he came to Götttingen whereas Gauss, in the morning of that same day went to Launenburg. 30 June Bessel expressed him his regret and his hope to see him either at Olbers' place or in Lauenburg. *If this happy event will take place owing to a slight change of your plans of about a few days, I will regard this as a pleasure.*

He journeyed at first through Westphalia to visit his family [his parents] then, on 21 July, to Olbers who was very glad to have him for a long stay. Then, accompanied by Olbers, Bessel travelled to Lauenburg and came there on 1 August. Gauss, however, arrived there on 1 July. On 18 July he returned to Göttingen and wrote to Olbers that same day:

It is endlessly regrettable not to see Bessel. […] *But I still hope that our Bessel will decide to travel back through Göttingen*.

Bessel is greatly distressed by not attaining one of his main goals of his journey, wrote Olbers to Gauss on 18 July 1819. And so they expected to see each other, but none made the decisive step to bring about this desirable meeting.

Schumacher amiably and obligingly greeted Bessel in Lauenburg and on 3 August or a day later accompanied him to Altona. Repsold, whom Bessel wished to see, was absent; he presumably went to Cuxhaven to have a look at the lighthouses on the Elbe.

On 21 August Bessel returned to Königsberg and in a few days wrote to Schumacher: *Among my recollections* […] *you, and that which I saw and enjoyed with your help occupy one of the first places*.

From that time their correspondence became livelier and very friendly.

About Bessel's external appearance at that time we know something from Encke who first met him in Seeberg. He wrote Gerling:

Bessel's visit had extremely gladdened us. […] *He is a bit taller than I am*[12] *and dark-haired. He is quite the opposite of the impression that I formed from his letters. He is highly jovial and merry, full of enthusiasm for his science which he never forgets. In his letters he appeared so restrained and formal but in conversation he comes out fresh and it is really pleasant that he dares to express his contrary opinion just as free and open*. […] *Bessel is very glad* [that he will] *speak to Gauss once more* (Bruhns 1869, p. 92).

[9] Upon returning home, Bessel saw that the [rebuilding of] the observatory was not yet accomplished. The meridian circle had arrived later than stipulated (and only installed in November). Its bearings were erected most carefully, their common foundation overlaid with a wooden floor which only rested on that foundation but not on the supporting wall (B – G, 12 Sept. 1819).

Reichenbach later arranged a release of the limb (des Teilkreises) from its clamp (Deklinationsklammer), but Bessel discovered that other changes were also necessary, especially the elimination of the unequal loading on the bearings by a counterbalance. In addition, he greatly missed the possibility of reading the limb by a microscope which he learned to value highly on his Cary circle. For zonal observations for which he thought to begin using the new instrument, in spite of Reichenbach's special liking for verniers, he ordered two micrometric microscopes from Fraunhofer. For solar observations, just like he did when working with the English circle, Bessel used a sunshade which only left the objective lens free. Along with these preparations for the meridian observations, Bessel began negotiating with Fraunhofer about a large heliometer which could have been used as an altazimuth.

In January 1820, he informed Olbers about all that. And in a few weeks, on 14 February, he sent him another letter which began thus:

For some days now, I am feeling the need to write to you, but I was unable to find the proper tone. Seized by the news from there (?), I am unable to say something consolatory, and will therefore try to divert you for a minute. […]

Then followed a long discussion about the theory of conic sections. The *news* concerned the death of Olbers' wife and only at the end of the letter Bessel added:

Allow me, most respected Olbers, to write something comforting! My wife, who feels herself fine, my sister and our dear Wilhelm deeply feel the loss which you have just now experienced and they most sincerely sympathize with you. I am convincingly asking you to trust firmly the strength of your soul and not to disregard its uncommon aid.

In a similar way Bessel had expressed himself a year ago when Olbers' daughter had died (B – O, 3 Apr. 1819). And when he himself lost his father, and shortly afterwards his father-in-law, both of whom he highly respected, he thus informed Schumacher about it (B – S, 8 Apr. 1819):

However, resorting to my way of thinking which is known to you, I try to forget the inevitable.

In other instances we find the same failure to express himself and evasion of painful impressions which is amazing given Bessel's strong and resolute character. And the *forgetting* should certainly be understood as a means for getting the better of pain by reliably keeping it in memory.

Similar behaviour occurred on occasions of serious illnesses. When Schumacher informed him that Olbers was very frail, Bessel did not write to him for a long time and on 16 May 1832 explained to Schumacher:

To understand this, you ought to learn about my special peculiarity. I cannot at all write to someone whom I love and respect as soon as I find out that he is in mortal danger. For this reason I did not write to my father during the last months of his life, and when I intended to write to Olbers, I was unable to bring myself to do it. This can only be a ridiculous weakness, but here I am not my own master.

On the other hand, on 14 Oct. 1840 he was able to write [to Schumacher?]:

I am telling you that the thought about old age and the ensuing death does not frighten me although I do not at all belong to such pious people who will grin and bear the inevitable. I am prepared to endure it as such.

Bessel was fairly remote from church life[13]; indeed, he jokingly called himself a *half pagan*, whereas his wife was *as pious as is allowed to a good wife*. However, since Bessel felt his *special peculiarity* as a weakness, it might be explained by an anxiety not to dominate sufficiently his own excitement and thus not to disturb an ill man (or someone affected by a heavy loss). In that letter to Olbers he was only able to speak about the loss of Olbers' wife after many pages of mathematical content and then to ask him to calm down rather than to help him to achieve calmness.

[10] He did not want to appear too weak and was not afraid to seem rather cold. Indeed, he was not only frank and truthful, highly appreciating, as he himself stated, the honesty which he inherited from his parents [cf. vi, § 1] and [cf. vii, end of § 10] as long as possible believed that a [certain] man was only capable of goodness. Once he said (Bruhns 1869, p. 272): *Those whom I trust, can say or do very much before I quit trusting them*.

Sometimes sharply, but cordially and sympathetically he stated that he cannot doubt either that which exists according to oral tradition, or to the words of his friends, especially Olbers, Schumacher and Gauss as well as of his student Steinheil (in his correspondence with Bessel) who respected him, and Anger (1846). On p. 15 the last-mentioned stated:

His attractive nature won him respect and favour even in wider circles of the society. He never had enemies. He readily acknowledged worthy efforts and achievements even of those who belonged to alien fields of knowledge and willingly argued about subjects beyond his speciality, earnestly and ingeniously defending his views when his opponent did not agree with him. […]

He often diligently worked in his garden and (wobei) *took pleasure in discussing astronomical themes with his students, answering their questions, hearing out reports on their results. He was prepared to fulfil any wish of such kind, but was loath to interrupt his astronomical work or at least his observations*.

Steinheil worked in the Königsberg observatory in 1824 – 1825, and Anger, in 1827 – 1831.

In 1820, Bessel lively supported the proposal about collective observations of the Moon to which Gauss had already attracted Nicolai, Soldner and Encke. Bessel (B – G, 10 Jan. 1820) wished to participate and hoped that these observations

Will connect astronomers and observatories. Much of what is unfit and what just disgusts me, since I willingly, actively and intensively wish to work in a collective, can be eliminated. Give us more of the same, and a tight connection will soon emerge instead of the present stupid egoism. The time will return when a man was delighted by the work of another one.

By March 1820, Bessel became ready to observe with his own meridian circle after inserting there new threads by his own method.

First of all, he intended to check the invariability of the collimation error by very carefully changing the position of the telescope, then to begin the prepared *observations of the polar altitude, refraction etc, then scan all the sky zone after zone.* […] *Help* [participation in that work] *would have been pleasant*, […] *but only by a quite similar instrument. On this point I would willingly hear your opinion* (B – G, 5 March 1820).

Gauss, however, neglected the obvious hint although already on 24 June 1818 he wrote to Olbers about the *revision* of the *Hist. Cél* [*française* by Lalande (1801)]:

It seems that it will be best of all if many astronomers will participate. Then I will willingly offer to revise one or two thousand stars.

Bessel and Gauss continued to share their experience in the work with the new instruments. Thus, for eliminating [the influence of] the bending of the telescope they both observed with mirror telescopes. At first, Bessel applied a [mineral] oil horizon, then, more successfully, a bowl of water, 3 ft in diameter, and, finally, following Pond, a flat bowl with mercury (G – B, 20 March and 30 Apr. 1820) [No. 150/62].

Bessel (B – O, 11 May 1820) wished to find an assistant for the zonal observations and Gauss likewise was in the same need which can explain his restraint [see above]. He (G – B, 28 June 1820) experienced

The burden weighing on the life of a practical astronomer, who works without an assistant and often too intensively. Most annoying, however, is that I am hardly able to be engaged in a coherent and serious theoretical work.

Bessel (B – G, 10 July 1820) argued however:

You are certainly right when you say that the life of a practical astronomer is burdensome. I had felt it long ago but disregard it since I think that observations are extremely important and that our practical astronomy is still lagging behind theoretical astronomy. As soon as the art of observation replaces the skill of counting the seconds (see [v, § 5]) *theoretical studies will in many aspects become less important than they are now*. […] *Meanwhile, I hope and believe that you will never prefer practice to theory*[14].

The Hanover measurements began in the spring of 1821 and Gauss certainly had to devote much time to them. He had to abandon the investigation of the meridian circle which greatly disappointed Bessel.

[11] In the winter of 1821 Walbeck came to Königsberg, and Bessel arranged comparative observations of the stars' movement across the field of view of the telescope. The result was astonishing [No. 176/61]: Walbeck registered all those movements a second later than Bessel. Bessel had begun a similar study even in 1819 in Seeberg together with Lindenau and Encke, but it had to be abandoned owing to unfavourable weather.

Bessel (B – O, 8 Febr. 1821) asked Olbers' opinion about this mysterious phenomenon and later, 11 Dec. 1823, he wrote to Gauss asking him as well to explain the results obtained, but got no answer. And so was the personal equation discovered. Maskelyne had noticed it earlier, but explained it by inattention of his assistant and sacked him.

It is now appropriate to quote Bessel (B – S, 30 Apr. 1823):

Time is as much distressingly absolute as it is comparatively easy to determine it. I have been convinced in this long ago and, besides, I think that a solution is difficult to come by and moreover it only remains valid in a particular case [?]. *If the time in two different places ought to be compared with absolute certainty, nothing can be done except interchanging the instruments and the observers*.

Or another of his statements (B – S, 9 May 1832):

Drawing on my experience of many years I believe that it is better to eliminate an error from observations at once rather than to get rid of it during calculations. And I do not doubt that it is better to determine time by a transit instrument of 12 or 18 inches with the position of its telescope changeable at any moment than by a meridian circle.

Nowadays all this seems evident, but in those days not at all so.

In May 1821 Bessel experienced a cruel suffering owing to the death of his sister who, after moving to Königsberg, had been living in his house. *In many ways she was a devoted companion*. […] *She would have hardly imagined how much* […] *we have lost* (B – G, 18 June 1821). For a week he was seized by sorrow as also testified by his words (B – O, 7 June 1821):

Because of my children and my work I would have willingly remained here [among the living?] *several years more and I am therefore attempting to reinforce my strength and rest as much as possible*[15].

In June 1821 the instrument for the zonal observations had finally arrived. Apart from two screw micrometers, each mounted on an arm connected with the axis of the telescope (auf der Fernrohr-Achse zu klammender Arm). It restricted the movement [of the telescope] between two stops by the width of the zone. A patter prevented to reach the allowed boundaries with a jolt.

For checking the precision of graduation Pistor had sent four other microscopes which could be mounted on the alidade, and, for determining refraction Bessel applied a thermometer, but its readings occurred erroneous since the inner diameters of its tube were unequal. Bessel investigated the thermometers by his improved method applied for the first time [No. 217/41] (see [vii, Note 18]) using a truncated mercury column according to Gay – Lussac.

On 19 Aug. 1821 Bessel began observing as far as possible all the stars down to the ninth magnitude. Working with an assistant he was able to observe hourly about 120 stars. Using a meridian mire, he had also begun a thorough study which established that *the Earth's axis of rotation deviated from the main axis*[16] *probably not more than by* 0."5 *if at all* (B – G, 18 Oct. 1821).

Concerning Bessel's observations, Gauss (G – B, 26 Dec. 1821) noted:

Your zonal observations of the starry heaven is a serious undertaking whose significance I recognise, but I still wish and insistently ask you not to work too zealously. Dear Bessel, you are certainly working much too much. Take care of yourself for the benefit *of your family, your friends and science*.

And Bessel really worked very intensively although in the very beginning of the zonal observations he felt himself sickly and had to miss a few nights. For leaving the nights free for observations, he investigated the meridian circle as far as possible during daytime although sufficient work was then always needed.

[12] In the winter of 1822 Bessel's health was not good either but he recovered by keeping to an expedient way of life (much movement, hunting) and autumnal sea bathing will additionally help (B – G, 14 Apr. 1822). He greatly missed the previous much appreciated investigation of the meridian circles in parallel with Gauss and regretted that Gauss spent much time on geodetic work (B – G, No. 137 between 16 Dec. 1822 and 14 March 1823):

Such loss of time is not for you to experience, you only ought to take on what is necessary for completing the appropriate theory. […] *The rest should be the business of NN rather than Gauss.*

Later Gauss stated that one important theorem is of more significance than all the measurements made worldwide, but he still considered his geodetic work relatively valuable, more valuable than the studies which he had to interrupt.

The realization of practical astronomical work for an essential aim

Is now complicated for us since you have overtook us and so masterfully carried out most of the desiderata that for us, for the rest of us, almost only gleanings are left (G – B, 14 March 1824).

A slight smell of envy is felt here although Bessel had only wished to work together with Gauss and even stated (B – S, 11 March 1824) that he *had never seen anything written by Gauss which I* [which he] *would have not willingly signed*.

Meanwhile, in September 1821, Schumacher published the first issue of the *Astronomische Nachrichten* whereas Bessel (B – O, 9 Apr. 1821) had stated at first that *He regarded his observatory in a way that prevented him*[17] from working especially for that newspaper (Zeitung). Indeed, he was still unable to put in order all the delayed.

Nevertheless, Schumacher's journal soon became a very valuable outlet for publishing his numerous smaller current contributions. Moreover, it further strengthened his friendship with Schumacher. Their correspondence became more lively, and Bessel, who lived after all in a somewhat isolated way, greatly valued his correspondence, especially with Gauss and, certainly, Olbers whom all of them respected as an old friend.

[13] In 1822, the death of Tralles interrupted some of his preparations for determining, on the instruction of the Berlin Academy, the length of a seconds pendulum[18] by the Kater method. The Academy was willing to charge Bessel with the continuation of this project, but *he did not trust Kater's experiments* (B – G, No. 137 between 16 Dec. 1822 and 14 May 1823) and would have only agreed to take the work over if granted complete freedom of action. The Academy did not concur and Bessel withdrew.

However, during the discussions Bessel became so interested in pendulum observations that he decided to carry them out independently. Already in 1823 he asked Repsold to manufacture an appropriate pendulum apparatus and thoroughly, although not going into details, formulated his wishes (B – S, 3 March 1823).

Meanwhile Bessel mainly busied himself with the meridian circle and zonal observations. He did not seek *happy discoveries* which Gauss had wished him since he had no desire to repeat his observations[19], but noted (B – G, 17 Apr. 1823) that

When it concerns the widening of knowledge, you and I are accustomed to leaving behind our own precious ego. Who had even begun to fear self-sacrifice is half-lost for science.

There were no offers to participate in that long work which Olbers saw as near in spirit to [observatories in] Dorpat [Tartu], Mannheim, Bogenhausen [near Munich] and which Struve and Walbeck thought about [No. 155/94]. Goodwill only occurred in England where really suitable instruments were lacking so that Bessel (B – O, 9 Oct. 1823) apparently did not want any participants from there.

In the mid-year of 1823 Argelander, who had previously assisted Bessel with the zonal observations, was invited to Abo, and Rosenberger replaced him. By the autumn of 1824 the observation of the zones between declinations 15° and – 15° was completed with only some gaps being left. Bessel entrusted Steinheil, who at that time worked in the observatory [as well], with a preparation of a star chart extending over one hour, sent it as a specimen to the [Berlin] Academy and asked them to take over the publication of such charts for all the prepared zones. The Academy agreed and selected a commission which in November 1825 compiled a sketch of necessary steps and then urged to compile such charts for the rest 23 hours (*A. N.*, Bd. 4, p. 297). However, publication had been slow and irregular; the last sheets only appeared in 1852 and two of them were left unpublished.

Bessel thought of continuing the zonal observations to the north until 45° but remarked (B – G, 23 Oct. 1824) that

The three years of severe and unstable weather had regrettably influenced my health.

This continuation was completed by the end of 1835. All the work taken together included 75 thousand stars and now Bessel began preparations for continuing observations up to the pole and employed Busch as a *permanent observer* (B – G, 15 Jan. 1833; 24 Sept. 1835).

With all of his numerous tasks on his hands, Bessel found time for directing a small military geodetic measurement […] having as its aim the verification of the previous survey made by von Textor [vii, § 9]. With an apparatus similar to the Munich base-measuring equipment, officers measured a baseline three thousand feet long and discovered a very large error. Bessel (B – G, 14 June 1824) remarked that he took on this work mainly to have *sometimes a daylong breath of fresh air*.

[14] The invitation of Gauss to Berlin had been discussed in 1810 and resumed in 1823. In 1824, it greatly affected Bessel and he asked [the Berlin Academy] why this problem was dragging out. The Academy still hoped to get Gauss, but (B – G, 14 June 1824)

An obstacle had occurred, which I, in my latest letter, called absurd (the candidature of General Müffling). Bessel was privately asked about all this[20]. (See [ii, § 4 and vii, § 9].)

I have naturally answered in the negative, mostly because the question was based on a misunderstanding, on the presumption that an astronomer can occupy the place of a mathematician. And I could have indicated that there was no less proper management than beginning something and later abandoning it for an excessively long time.

However, the invitation had protracted so long that in the beginning of 1825, since *the situation has really liberally improved it became possible to stay firmly put in Göttingen* (G – B, 15 Jan. 1825).

And on 26 January Bessel wrote Schumacher that it was *unboundedly regrettable that Gauss had shattered our hopes and remains in Göttingen*. Soon afterwards, in February, Bessel was invited to fill the post in the Berlin observatory that became vacant after [the resignation of] Bode.

However, here everything is going on exactly according to my wishes and it would be unreasonable to accept the invitation (B – G, 12 February 1825).

He declined and proposed Encke instead. And Gauss and Bessel thus remained apart from each other. In the long run, personal contacts would have possibly affected their relations more favourably than correspondence. They could have happily supplemented one another. The somewhat inaccessible Gauss had long since detested lecturing and, when being occupied with practical astronomy and *lacking any real help,* almost always felt himself losing time and only wished to pass the rest of his life *working in my* [in his] *study without distractions by petty everyday problems* (G – B, 14 March 1824; 15 Jan. 1825).

And the other man, a lively, open-hearted Bessel, a subtle and tireless observer, devoting all his strength to the enrichment of astronomy, willingly meeting the scientific efforts of his students (Anger 1846, p. 15) and highly respecting Gauss. However, in his impulsive manner he was sometimes unable to choose his words carefully enough so that from time to time the sensitive Gauss jarred on them, and later their relations often became shackled. During personal contacts Bessel's amiable nature would have easily overcome [such] small [?] hindrances.

Gauss' letters (G – B, 21 March 1825) testify that he himself was not quite satisfied by his decision to remain in Göttingen, and for Bessel it would have in many respects been better to live in Berlin than far from the capital. Furthermore, he was unable to avoid [completely] Berlin. It seems that both he and Gauss had feared the worries of a large city but later discovered that their isolation was a self-inflicted obstacle.

[15] Bessel had at last received word from Repsold that the pendulum apparatus expected long ago was ready. In April 1825 he thought of going to Hamburg, taking it personally and discussing with Repsold the method of working with it. He was impatient and understood that his hope of taking everything easier at the age of 40 had been unjustified (B – S, 10 Febr. 1825). Still, he remained inspired by work and was able to say (B – S, 1 March 1824) that *Astronomy is indeed beautiful, it always offers so much of essential* *and interesting*. And when Schumacher once unfavourably mentioned the behaviour of a certain astronomer, Bessel (B – S, 10 October 1830) excused him: *For me, whoever found something essential in the sky, is worthy of respect*.

Bessel and Repsold met for the first time and sincerely took to each other. Bessel was once more staying with Schumacher[21] who made every effort to please his guest although Bessel asked not to pay too much attention to him (B – S 10 Febr. 1825):

This is not necessary at all if only you are prepared to stand patiently my dietetic oddities. I am living quite modestly, unwillingly go to crowded gatherings, drink [daily] *two glasses of light wine and many glasses of water, take only one meal and fear your rich* [wine] *cellar*.

A room on the ground floor of Schumacher's house served for Bessel's preparatory work with the pendulum. It went on successively but took up almost all his time. He was only able to visit Olbers in Bremen for a short time.

Exactly then Gauss had been occupied with geodetic work in Hanover and asked Bessel to visit him for a day in his place of stay, Zeven. Bessel, however, had no time anymore and Schumacher arranged their meeting for a few hours in Rothenburg, on the post road to Bremen, where Gauss could have easily arrived. However, by some unlucky chance other astronomers, Encke, Hansen, Thime, gathered there, so that Bessel's calm talk with Gauss became barely possible (G – O, 26 Apr. 1825).

They both were very disappointed by this second failure. Bruhns (1869, p. 108 note) reported that when they had finally been able to separate themselves their conversation had been interrupted for an hour owing to the difference of their opinions about mathematical problems. I think however that his statement is very unlikely, and nothing of the kind was reflected in their correspondence[22]. On the contrary, Gauss (G – B, 25 Apr. 1825) stressed that he would have willingly discussed the Berlin matters with Bessel in more detail.

On his way back, while staying in Berlin, Bessel became able to assist Encke in filling the post at the observatory there instead of himself. Somewhat later Encke became an academician and, besides, the permanent secretary of the Academy's physical and mathematical class. He filled these posts rather timidly and asked Bessel, who regarded him very friendly, not to deny him, as an academician, advice and support in case of need (Bruhns 1869, p. 271). Somewhat later Encke wrote Bessel:

Let heaven make me happy by living a long time under your eyes whereas Bessel invited Encke to Königsberg to acquaint him with his instrument for zonal observations (Ibidem, p. 272).

[16] During their meeting in Altona Bessel and Schumacher became even friendlier and Bessel (B – S, 16 May 1825) stated:

I would prefer to be always together with you! As soon as the calm times begin[23]*, we will possibly work together more than now. We can well deal with each other and I have now become even more convinced that we are so much alike and can even do without trouble*.

Bessel therefore valued their correspondence and on occasion said

(B – S, 20 Apr. 1840)

I would like very much if you will not so often leave my questions etc without any notice. My only aim is to find out your opinion and I try to satisfy your similar wishes as well[24].

In August 1825 Bessel got the long since expected pendulum apparatus [cf. § 15] and began at once working with it. For a long time this activity had been discussed in his correspondence with Repsold and on 21 August 1825 Bessel wrote Schumacher:

You and Repsold and the pendulum apparatus belong to me all together, and I cannot think about one without recalling the other ones.

Schumacher even previously wished to incline Bessel to common pendulum observations which would provide him with a foundation (with a natural measure) for the assigned transformation of the Danish system of weights and measures. So now he followed Bessel's work with special interest and hoped to make use of [the new methods] for his own goal. Bessel (B – S, 25 March 1828) however sincerely stated:

The idea of a natural measure cannot be realized at all. For carrying into effect the King's will, a roundabout way ought to be chosen[25]. *As soon as something measured becomes a measure the business is completed if only we can prove that the measurement was done mathematically precisely* (B – G, 30 Nov. 1827).

Gauss (G – B, 1 Apr. 1827) also thought that *the introduction of a natural measure into* [everyday] *life is extremely unsuitable*[26].

[17] In 1826 Bessel (B – O, 20 Jan. 1826) reluctantly parted with his assistants, Rosenberger and Scherk, who were invited to Halle and in June of the same year Fraunhofer's death disturbed him in connection with the large heliometer ordered way back in 1820 about which he had for a long time no news. He asked Steinheil who had moved to Munich to find out about the instrument and got a rather satisfactory information, but in 1827 he decided to go himself to Munich for achieving a clear agreement with Utzschneider.

Steinheil promised to look from time to time after the work [after the manufacturing of the instrument] and check it whereas Bessel resolved that in the interests of the orphaned optical Institute Steinheil ought to participate permanently in its leadership which conformed to his inclinations. His negotiations with Utzschneider annoyed Georg Merz much respected by the former. Bessel's proposal was finally declined.

In the autumn of 1826 Schumacher visited Munich[27] and in a similar way successfully proposed Th. Clausen as the leader of the Institution's optical calculations. Bessel, however, did not know that. On the advice of his physician Bessel went from Munich to Marienbad [Marianske-Lasne, the Czech Republic] to drink the mineral water. The advice proved successful, but Bessel felt himself miserable over work and domesticity. He had a small transit instrument but barely used it.

In the autumn of 1827, after at last overcoming the main difficulty (of accounting for the air resistance[28]), Bessel managed to obtain a satisfactory result (B – G, 30 Nov. 1827) [No. 219/237]. Earlier, in

January, he wrote Schumacher:

God knows that because of this damned pendulum I became a quite another person and am unable at all to write to you diligently.

Already in Dec. 1826 he complained to Gauss: because of these pendulum observations *almost everything else had to be put aside*. He all but regretted having taken on himself the determination of the length of the [seconds] pendulum, but at the same time he (B – S, 13 Oct. 1927) recognized that he *cannot postpone the unfinished. For me* [for him] *it is impossible*. Bessel thankfully recognized the tireless help rendered him by Repsold (B – S, 11 Nov. 1827): *In any case, without his apparatus I would have been unable to discover the truth*.

And he made another series of observations, especially (?) concerning the action of gravity on various substances [No. 350; 264/139], see [vii, § 11]. In July 1828, just as he completed this work, Schumacher came to him to acquaint himself with the application of the pendulum apparatus and to take it to Copenhagen for the intended observations there.

[18] After concluding his pendulum observations Bessel mostly devoted himself to a tiresome collection of all the tables needed for reducing astronomical observations and his work [No. 248] is still widely used whereas Gauss (G – B, 9 Apr. 1830) stated:

Your auxiliary aid suitable for a hundred years (hundertjährigen) *for reducing astronomical observations*[29], *is a sacrifice on your part, but a highly deserving work for the science. The views and principles reported in your letter are written all over my soul*.

Exactly then, because of Aleksander von Humboldt's efforts, it became once more possible to invite Gauss to Berlin. Neither Bessel, nor Schumacher had been satisfied by the previous repeated attempts and Bessel (B – S, 16 Nov. 1828) noted [another aspect of the problem]:

Something improper for me had probably occurred: much was allowed to Gauss, whereas I invariably got a bad mark.

This is hardly understandable, but it ought to mean that Bessel justifiably felt (Grund hatte) that his work had been [relatively] neglected.

In Copenhagen Schumacher was unable to find a suitable room for pendulum observations and finally decided in favour of Güldenstein, a castle in Holstein. In 1829 he turned to Bessel for help since he felt himself diffidently. Bessel however waited for the ordered heliometer and had to oversee the final stage of the erection of a building for it, so he could not come. He himself directed the installation of a sliding cupola which at last was done according to his wishes.

Bessel and a previous assistant of Repsold who moved to Königsberg, Steinfurth, took on themselves the installation of the instrument. On 21 Oct. 1829 Bessel became able to report to Schumacher: *Victory! The heliometer is installed*. The time for its investigation and slight correction had come. It was very pleasant to lead the instrument through these trials […] *although it would have been better if it had not deserved it*.

In the beginning of 1830 Schumacher repeated his request for Bessel's participation in his pendulum observations which still remained unpleasant for him. Observations made during previous years which he had to carry out alone ought to be repeated and completed. However, before Bessel had time to answer, the deeply touched Schumacher informed him about the sudden death of his old friend, Repsold. A fire had burst out and a stone wall crumbled down and hit him only a few minutes after he had cordially spoken with Schumacher.

Bessel answered on 7 March, still under the impression of this sudden death:

I will not forget our old friend either. […] *A hundred times everything that I heard from him and saw while being with him, had passed before my eyes*.

Schumacher repeated his request once more and Bessel (B – S, 15 Apr. 1830) promised to come in August. Now, however, he was tied up for fourteen days by Encke's visit who managed to come only then. Bessel invited him perhaps hoping to improve and strengthen their relations which began to form since Encke had moved to Berlin [see § 19].

[19] Because of his remoteness from the capital, Bessel had to restrict essentially his participation in the publication of the star charts as stipulated by the commission of the Berlin Academy and especially by one of its members, Encke. This circumstance led to annoyance lasting for years. Even on 9 June 1828 Bessel reported to Schumacher:

It occurred that since his move to Berlin all my contacts with Encke have a bitter taste the real cause of which I do not know.

Actually, after Encke had asked Bessel to help him by advice, he became displeased by the latter's statement formulated in his typical free and easy manner: Encke ought to go his own way; Bessel is always lively and prompt. Encke did not hurry to process Bessel's business letters (Drucksache) which passed in Berlin through his hands and considered them when it suited him. Bessel wished to direct his junior friend but Encke did not desire it[30], although he recognized Bessel's superiority. His forthcoming visit to Bessel could not have seriously improved the situation: characters do not change and they had not suited each other but on the contrary spurned one another[31].

After protracted negotiations for and against Bessel's visit to Güldenstein it was fixed for the end of July. He went with wife and daughter and, after the work was completed, they travelled to Altona and managed to visit Olbers. They stayed there until returning home on 21 August 1830.

Many times the delivery of the *Astronomische Nachrichten* by the Prussian postal service prompted Bessel to complain, and he asked Schumacher to support one of his complaints by a *presentable* letter (of 31 Jan. 1831 (?)). We see therefore how highly he valued this periodical. Thus, B – S, 30 Jan. 1831,

Astron. Nachr. is […] *a necessary condition for a happy blossoming of our astronomy. Previously the von Zach's journal and then the periodical of Lindenau had played a similar role. Our astronomy therefore came to the fore and our neighbours can now learn much from us. Astron. Nachr. is a step higher than its predecessors since we ourselves have risen a step. In addition, the*

Astron. Nachr. is advantageous in that it is being sent by separate sheets [1 sheet = 16 pp.?] *and it can replace correspondence for those who do not practise it*.

Earlier, on 29 Jan. 1826, feeling self-respect, he wrote Schumacher: *Astronomers ought to learn German, and you can compel them to do it*[32].

For simultaneous zonal observations and observations with the heliometer Bessel needed one more assistant, but was unable to select anyone. He was prepared to give up on his desire rather than take on someone *not passionately carried away by astronomy* (B – O, 13 Apr. 1831).

[20] The summer of 1831 was alarming. Cholera swept over Germany and broke out in Königsberg. It mightily scared the inhabitants of that city and led to ill-considered instructions. Thus, a cholera cemetery was established near the observatory, only 27 toises from the building for the meridian circle. Bessel strongly protested but was unable to convince the city council since its chief-president resisted.

And Bessel with family went to the countryside and only managed to return in October, when the epidemic had petered out. In spite of the alarm and troubles his health improved apparently since he had been unable to work as much as usual.

Complying with the intensions of the Russian government, it was decided in 1830 to carry out geodetic measurements in East Prussia under Bessel for connecting its network with the existing Struve triangulation built up in [the territory of the present Baltic states]. Nevertheless, until the cholera epidemic of 1831 only initial preparations had been done. Bessel together with Baeyer only became acquainted with the region and ordered the necessary instrument indicating the desired details.

Observations began in the spring of 1832, and from 21 May to 11 September with short interruptions Bessel had to devote to them very much time and the comparison of the Prussian and Danish measures [of length] proposed by Schumacher had to be postponed. On the contrary, zonal observations had been continued as promptly as was possible and should have been completed by the winter of 1833 up to declination 45°.

During these months Bessel found time to sit for his portrait. Professor Joh. Wolf skilfully painted it and E. Mandel prepared an excellent copperplate later appended to the *Königsberger Beobachtungen* for 1856. A portrait of Bessel's wife was also made.

In January 1833 Peters came to Königsberg on Schumacher's recommendation for concluding his studies under Bessel and acquire the degree of Doctor of Philosophy. Some difficulties were encountered since Peters was actually a self-educated person but Bessel and M. H. Jacobi overcame them. At the end of the year Peters returned to Hamburg and had been working at the observatory there until 1839 after which he was invited to Pulkovo. Later he often spoke of Bessel with great respect.

The arc measurement [in Eastern Prussia] had resumed in the spring of 1833 but demanded much time because of the inclement weather.

The angle measurements continued all summer and the beginning of the next summer and in the autumn of 1834 the polar altitude and azimuths were measured in Memel [Klaipeda] and a base measured for the second time nearby [No. 322/135][33].

[21] In April 1834 Bessel met Schumacher in Berlin according to his wish. They lived there for fourteen days and, taking into consideration the Prussian system of weights and measures, preliminarily discussed the transformation of the Danish system. For Schumacher, it was impossible to postpone still more the command from Copenhagen. […]

In the autumn, after the very hot weeks in Memel, Bessel (B – S, 15 Oct. 1834) took pleasure in his

Idea of definitively concluding the geodetic work in the near future. No, I cannot [he cannot] *say that they are unpleasant, but I feel the need to return completely to astronomy*[34].

In November 1834 the Bessel family happily celebrated the wedding of his eldest daughter Marie who married the Berlin professor Adolf Erman[35].

The Administration for Commerce, Industry and Construction in Berlin asked Bessel to establish a new Prussian system of weights and measures for which pendulum observations in that same city were needed. They should have been carried out in 1835 with his pendulum. He came to Berlin on 17 May, stayed there and prepared for observation a small house in the garden of the local observatory.

Encke had already got the pendulum (?) and helped Bessel with the necessary preparations and took upon himself the determination of the time by means of a small Repsold transit instrument brought by Bessel since the Pistor meridian circle was not yet ready[36].

Schumacher came later [see above] for 14 days for consulting with Bessel about how best to compare the two systems whereas Bessel preliminarily convinced himself in that both linear measures were almost identical and in the prescribed ratio to the Paris measure which Bessel had obtained for his observations in 1824 from Fortin and was thoroughly compared in Paris with the toise. That Paris measure should have also been applied during the new pendulum observations.

For the most precise observations Bessel ordered a comparator from Baumann in Berlin although they could have only been carried out later, along with pendulum observations[37]. Schumacher went home believing that after concluding his work Bessel will come to Altona and repeat the observations together with himself (with Schumacher) and Oerstedt. Together with Schumacher the last-mentioned had been given the task of transforming the Danish system of weights and measures.

Meanwhile, persistently, as was his wont, Bessel continued the observations in Berlin and regularly complained to Schumacher about their tiresomeness. He definitively established the coincidence of the previous measures of length sufficient for the desired aim and patiently with much effort introduced a new endpoint measure, a steel rod with sapphire tips which were screwed into nuts. Their gradual tightening led to the necessary distance [between the tips] which was in the most possible precise ratio with their toise. The length of the new three-foot endpoint measure was 3·139.13035 lines [1 line = 1/10 or 1/12 of an inch] as compared with the demanded length of 3·139.13 lines[38].

Bessel was happy and completely satisfied in that he had finally achieved his aim, but Schumacher felt himself bound by the royal command and Oerstedt's doubts. He had not calmed down and wished to compare in Altona the initial measures once more. Angered letters were exchanged. Bessel complained about tough and unfair reproaches and declared unceremoniously that he will not busy himself with new corrections just for dispersing Oerstedt's doubts. On 20 August he went to Königsberg, and Schumacher had to rest content with the accomplished.

[22] During his three-month stay in Berlin, Bessel naturally met Encke many times. However (Bruhns 1869, pp. 281 – 282),

Very soon a difference of opinion had manifested itself in their conversations. Bessel, so outspoken and lively, fully made known his ideas and views even in the presence of others, and Encke many times felt himself insulted. Nevertheless, they had friendly associated with each other and when Bessel went home Encke saw him off. […]

Bessel thanked him in a letter of 15 Oct. [1835 – J. A. R.] *for his essential help in everything, and especially for determining time. Encke, however, decided that Bessel had allowed himself much too much and complained to a friend that even in the presence of others he had to restrain himself to avoid quarrels. When remaining alone, Encke thought about all the spoken and became seized with a serious and chronic low spirits*.

Nevertheless, Bessel's student Anger (1846, p. 16) appraised the same situation contrarily:

Bessel's dialectic had not depended on personalities and could have never offended anyone […] *since it became at once evident that he did not reason in the spirit of contradiction but only expressed his true inner conviction*[39].

In discussions, Encke apparently felt himself restrained by Bessel's superiority and became annoyed.

On 23 August 1835 Bessel returned to Königsberg and at once began to calculate definitively the length of his normal rod[40], attained a somewhat better approximation and wrote Schumacher about it. After fourteen days, having received no answer, he wrote to him once more and impatiently (B – S, 11 Sept. 1835): *I am awaiting letters from you, my dear old friend*. An answer [to his previous letter] soon arrived and he (B – S, 13 Sept. 1835) cried out:

Victory! You are satisfied once more. The foot should now be a third part of the new standard (*without any new explanations.* Is this Repsold's rtemark?). *The result of the comparison: 1 foot = 139.13 lines of the pendulum toise. With this we agree and will come to agreement about the rest. If something else is taken instead of the toise, that number, 139.13, will naturally change as well*.

In the winter of 1836 Bessel made preparations for the zonal observations and was especially occupied with the Halley comet (*Astron. Nachr.*, Bd. 13) [No. 293/13; 294].

Meanwhile Schumacher began doubting anew the measure of length which Bessel had alone carefully compared a year ago. On 22 Jan. 1836 he expressed his wish to come to Berlin with Oerstedt when Bessel will be there and assist him in definitively completing the comparison of the Danish standards. Bessel did not agree; on the contrary, in his answer, he indicated that, as Schumacher ought to know, the necessary comparison had already been made.

After this concluded part of the work, the second part should follow whose only aim will be to ensure an easy and reliable reproduction of the standards. For completely attaining this goal two additional devices are needed. They were ordered long ago but not yet manufactured: an Ausdehnungsmesser [see vii, Note 15] *and a comparator*. […] *And when I will get the new original of the Prussian standard it will be very desirable to find you in Berlin to amend and finally establish your original*.

In a supplementary letter of 14 Febr. 1836 Bessel all but regretted that he had informed Schumacher about this matter. […]

Schumacher had to be contented. However, Oerstedt, also responsible for the task, was not mentioned, which Schumacher did not approve and allowed himself to *interpolate* [insert] him in his report to Copenhagen.

[23] The business had nevertheless not ended at all. On 15 April 1836 Bessel wrote that the Ausdehnungsmesser was still lacking. In the same letter he thanked Schumacher for his attempts to *prevent* an unpleasant quarrel *between me and Encke*. Basing his considerations on the motion of the Pons comet[41], Encke (*Astron. Nachr*., Bd. 13, p. 265) suggested that the space medium is resistant whereas Bessel (p. 6) thought that its existence is doubtful. To end this dispute, Encke (p. 274) [noted that]

Simply mentioning a hundred other possible causes including those, provided by Bessel (A. N., Bd. 13, p. 274)*, will not explain that cometary motion*.

Bessel (p. 350) argued however that any further discussion of that subject was *fruitless*. At the same time he (Bruhns 1869, p. 283) wrote Encke that he hoped that his answer *will at least satisfy* others. Encke (Ibidem) only answered in a few months[42]:

Already a few years ago it became clear to me that our views are regrettably contrary in many aspects. I cannot at all understand how is it possible that the way along which you endlessly worked is wrong [?]. *However, there are many ways and I feel the need to follow that which alone suits my character*.

On 20 Nov. 1836 Bessel complained to Schumacher:

There is not a single letter in which Encke forgets to say that he resents me. I do not understand this business at all. It began exactly when, in 1835, I left Berlin, but it continued, so I wrote him that there is no call for being displeased and that he is greatly mistaken. He could have at least shown respect and trusted me. Indeed, I never lie intentionally. […] *Until my departure he was quite candid* (*as far as his nature allowed it*) *and saw me off until Vogelsdorf. However, just after that he began behaving as though bitten by a tarantula.* […] *I regard all his arguments quite simply. Nowadays he believes himself so highly placed that does not need my considerations anymore. He* *puts on airs, imagines that he is independent, and thinks that he thus increases his weight*. […] *I suspect that he thus attempts to slander me*.

On the other hand, we can notice that Bessel had sometimes expressed himself too freely and somewhat rashly and that under some circumstances he could have been wrongly understood. For example (B – S, 26 Dec. 1831),

Nothing disgusts me more than acting intentionally (nach Vorsatz), *according to duties or a system. Everyone acts as he wishes.* […] *I would have lied had I stated that I was not annoyed afterwards by being stupid enough to act out of duty*.

I only adduced these lines to show that Bessel, after carving his way by himself, preferred to go ahead freely, confidently feeling that he will certainly find for himself a sure and suitable path without bothering about any statutes. We may say that he aspired to *moral beauty* which Schiller (letter to Körner of 19 Febr. 1793) called the *maximal perfection of character* which is *only attainable when duty becomes its nature*. Now, rather than in 1831, when Schiller's views were chronologically nearer, this [Bessel's] statement could have been easily understood inconsiderate and self-willed[43].

[24] In 1836 Bessel (B – S, 14 Dec.) had devoted much time to a new theory of comets. Once he made known his attempt to illuminate the nadir by Steinheil's method, i. e., by a flat glass placed at an angle of 45° to the ocular and thus obtaining not much but sufficient light. For investigating the terrestrial refraction he (B – S, 15 Apr. 1836) fastened a thermometer to a mast of variable height and thus measured the air temperature at different heights above the earth. He read the thermometer from a distance of 100 ft through a telescope.

Bessel worked much but felt himself well enough (B – S, 25 Sept. 1836):

I am once more occupied by something new which is just excellent. I am fresh and healthy and capable of attaining something.

The Baumann measuring device was only manufactured in 1837 and Bessel asked him to come to Königsberg to arrange everything easier. He invited Schumacher as well to participate in the still forthcoming correction of the Danish measure [of length], but finally began working only with the arrived Baumann (B – S, 3 Sept. 1837).

In his yearbook [*Astron. Jahrbuch*] for 1839 published in 1837 Encke published unpleasant recollections about Bessel in connection with his, Encke's, determinations of time during pendulum observations of 1835. When giving Encke his transit instrument, Bessel pointed out that, when the position of the telescope was changed, the instrument slightly changed its position and recommended to apply a meridian mark, as he himself did. The cause of this change, as Bessel later thought he had established, was that, owing to temperature changes or some other random effects, the instrument's screws did not exactly fit their cavities although this uncertainty disappeared if the screws were placed freely (*A. N.*, Bd. 15, p. 124). Encke (*Jahrbuch*, p. 269) wrote:

Later, when the instrument was taken back to Königsberg, the same uncertainty persisted and Bessel decided that he had discovered its *real cause. However, this variability seemed to me not quite probable and perhaps somehow self-contradictory and in addition it did not at all influence the observations here*.

Bessel (B – S, 15 Apr. 1836) remarked that

In itself, his article does not appreciably concern me and I could have paid no attention to it, but it was prompted by my statement which therefore I ought to strengthen. Since Encke has done it, I ought to block his statement. And I will do it, naturally without feigning insult. Encke's character essentially differs from mine. He can be very good but we badly suit each other. […] *There was a period when I had regarded Encke very well but later he showed himself not as I would have done*.

Only after his Königsberg friends and Schumacher advised him, Bessel decided to comment on Encke in the *Astron. Nachr* (Bd.15, p. 121). He explained the variability just as stated above and especially objected to the *self-contradiction* which Encke unjustifiably wished to find.

Encke had sent objections to Schumacher who decided that he certainly ought to publish them as a continuation of his previous note. Bessel became outraged by Encke's self-confident tone very different from the tone of his previous letters, but almost even more by Schumacher's agreement to publish those objections (*Astron. Nachr*., Bd. 15, 1838, p. 173 [No. 174]). Indeed, Schumacher only considered Encke's previous remark (Ibidem, p. 121) as a defence against an attack which was impossible to repulse in the same source (in the yearbook).

The influence of Bessel's Königsberg friends (especially Neumann, the brother of his wife, and C. G. J. Jacobi) strengthened his outrage. They, just as he himself, reproached Schumacher (*Astron. Nachr.*, No. 4970, p. 28). Bessel decided that it stood to reason that he did not dare send his new current works to the *Astron. Nachr*. since it will injure his dignity.

He even blamed his friend, although hoping that they will not personally move away from each other, for becoming influenced by Encke and his followers. He sent a brief objection to Schumacher, who did not refuse to publish it (*Astron. Nachr*., Bd. 15, p. 231), see B – S, 3 March 1838. For Schumacher that letter became *a bolt from the blue*. He was unable to consider himself guilty. *Let your letter soon return me my old friend* (S – B, 5 March 1838). And in this manner they tormented each other for a fortnight, wrote letters one after another, did not sleep at night, remained miserable. Through Humboldt Schumacher (S – B, 9 March 1838) vainly attempted to persuade Encke into making a reconcilable explanation.

Finally, on 16 March Bessel wrote: *I ought to try once more to mend everything which I made rashly*. He regretted that he had too hastily written Schumacher for the second time without awaiting further explanations.

This is an indication of my still remaining hot blood. Owing to its consequences I sincerely feel sorry that it manifested itself.

However, he cannot imagine that Schumacher, even if without realizing it, did not fall under Encke's influence. Bessel returned to this episode on 23 March: Encke had drawn him, Schumacher, into his *plot*. And they were unable to agree about Bessel's further collaboration with the *Astron. Nachr*. Schumacher suggested to Bessel to ask the opinion of Olbers and Gauss, but the result was inconclusive: they both answered vaguely. However, Schumacher reasonably did not hurry into making a decision and was happy when Bessel, after receiving an inducible opinion from Olbers (!), sent a new manuscript to that journal.

[25] The unanimity among Bessel and Schumacher was at least achieved once more and a few months had not passed before they agreed to meet in Berlin in the spring of 1838. But still, Bessel never wished even to hear about Encke. He had intended and tried to remain friendly to Encke but satisfied himself in that Encke acted towards him neither cordially nor respectfully. He was unable to forgive Encke, but did not disclose their quarrel since it only occurred in essence because Encke did not reach his, Bessel's, level of mastering the art of observation.

Bessel prepared his instrument for Encke and warned him about its delicateness so that when the latter did not cope with it he could have had it out trustingly with Bessel before compiling his report for the yearbook. *Much ado about nothing*, as Gauss wrote to Olbers on 5 Apr. 1838. Bessel did not mention the quarrel either to Gauss or Olbers, but, on the contrary, in strongest expressions and quite openly informed his close friend Schumacher about it. Occasionally some words excusing Encke had also occurred in his letters to Schumacher but they did not change anything since his correspondent had to be very careful. Indeed, for a long time Bessel could have still harboured his suspicion of him, Schumacher, having for some time been under the influence of Encke and his friends.

Bessel paid no attention to Encke's repeated attempts at rapprochement and only formally thanked him for his letter of 10 Sept. 1845 with its friendly compassion for Bessel's illness (Bruhns 1869, p. 285).

The arrival of Bessel and Schumacher in Berlin in the spring of 1838 allowed them to conclude definitively the problem of the measure of length. They lived in the same house. For a long time after that journey Bessel felt himself sickly and complained about *incessant tiredness* (B – S, 29 May 1838). However, the Marienbad mineral water which he regularly drank at home improved his health and in August he once more began to work diligently and became occupied with the theory of probability of observational errors [with the theory of errors] [No. 317/119].

In October another *great event* had occurred: the heliometer, after being nine years much in operation, was with Steinfurth's assistance completely taken apart, cleaned out and somewhat improved and the cupola of its building was reconstructed and made more expedient. [A detailed description of this second work follows.]

Already on 4 November observations became possible once more and during the night of 20 November Bessel worked with the heliometer for seven hours (B – S, 21 Nov. 1838). During that year, after seven years of serious work, he also became able to complete his book on the arc measurement [No. 322/135] and send it to the publisher.

[26] In spite of their breakdown occasioned by Encke, the trusting relations between Bessel and Schumacher remained as they were previously. This statement is proved by Bessel's decision which he reached in February of 1839 to come in summertime to Altona with his son. He feels himself well everywhere if only allowed to smoke his pipe (B – S, 16 Febr. 1839).

And on 11 July he began his four-week journey. In Altona, after all the happily endured troubles, they were naturally met with joy. However, soon discussions about a meridian circle similar to the one recently manufactured for Pulkovo had to begin in Hamburg. Bessel wished to acquire in addition an eyepiece micrometer and devices for observing in the nadir and for changing the position of the telescope without touching either it or the circle.

Following Hansen's advice, Bessel thought of having auxiliary graduations or the usual ones 5′ apart to simplify their investigation but he finally decided to have them 2′ apart since er nicht auf jedesmalige Einstellung zweier Teilstriche verzichten wollte.

It was certainly necessary to visit Bremen and Bessel felt special pleasure in taking his son once more to the son's godfather, Olbers. A detour to Göttingen became then impossible and Bessel had apologized to Gauss beforehand. Their correspondence had gradually become much less lively and more formal. Thus, Gauss only informed Bessel about the death of his second wife four months later, on 31 December 1831, and only in a roundabout way. Bessel (B – S, 15 Jan. 1832) had to ask Schumacher about it. In accordance with his *special trait* he had not found words for expressing sympathy.

Bessel did not understand Gauss and reproached him for turning away from mathematical astronomy to [geo]magnetism and reproached him in the same letter:

It is indeed unusual that, having such great mathematical riches, he rather devoted himself to physics. True, I only consider it relatively unusual.

In July 1834 he wrote to Gauss:

I have heard from Schumacher that you are long since happy for being in good health but are moving ever further from astronomy.

[27] On 28 May 1837, in a letter to Gauss, he discussed with interest Gauss' electromagnetic experiments but added: [see iii, § 3].

And on 4 Jan. 1839 Bessel wrote Gauss:

Von Boguslavski told me that in your investigations of the magnetism of the Earth you have approached a point which pleases you. I understand the meaning of that word, and I wish you the happiness of a most complete success and cherish the hope that you will not keep it to yourself for a long time.

Gauss had not written to Bessel for 5 1/2 years whereas Bessel sometimes added a letter when sending him printed matter. Bessel's repeated frank statements, although based on their long-standing friendship and high respect, obviously touched Gauss unpleasantly, and only on 28 Febr. 1839 he decided to write to Bessel once more. His letter was also apparently meant as an answer to Bessel's request

to be acquainted with his, Gauss', work: [iii, § 3].
This letter, sent after a very long interruption, see above, can hardly be considered as a friendly continuation of their correspondence. Bessel (B – G, 28 June 1839) attempted to exonerate himself: [iii, § 3]. Their correspondence resumed. Letters were exchanged regularly but not often, and the previous warmth had disappeared. And Bessel (B – S, 30 Apr. 1840) once wrote to Schumacher: [iii, § 5 and Note 11].

[28] In August 1839 Bessel returned home from a journey affected by a severe chill and was unable to work for a few weeks (B – S, 28 Sept. 1839).

A pity that I have lost so much time. Heaven favoured me with a good and robust health to save once more some of the great loss [?].

Finally, by October Bessel felt himself well, better than before his journey during which he was

Nervously enfeebled. Nothing is better understandable than my way of life and my temperament. I am never at rest. My occupations accompany me when I go to bed and when sleep deserts me they meet me at once.

In the evening he should refrain from work, as his doctor told him. Either rest more, or make no claims to health (B – S, 26 Nov. 1839). Olbers' death (on 2 March 1840) profoundly shocked him. He had thankfully respected Olbers as a father. *I knew no weaknesses in him. I see him before my eyes, majestic and marvellous* (B – S, 9 March 1840).

A spa treatment in the spring of 1840 was successful and during summer Bessel diligently occupied himself with the necessary reconstruction of the building for the Repsold meridian circle. He was happy to be professionally assisted by his son who came for a short visit after splendidly passing an examination for a constructional conductor. Bricks were laid for a pillar on which the circle will rest, and Bessel's son prepared sketches for the building, all that according to Bessel's indications (Busch, *Königsb. astron. Beob*. 27, Tl. 1, VI).

Troubled days occurred in September 1840. The King Friedrich Wilhelm IV came to Königsberg to take the oath (Huldigung) and Bessel was unable to avoid completely the festivities. [At that time] Humboldt often visited the observatory, and on a clear but noisy evening came the King. Bessel was especially honoured and his salary was raised by 500 thalers[44]. However, the very hot weather and the ensuing commotion which burst into his house worsened anew the state of his health and led to severe spasms in his breast and essential weakness.

In the beginning of October, just as he began to feel himself better, Bessel received news from Berlin about a severe illness of his promising son. After an apparent improvement he died on 26 October. Bessel staunchly endured the heavy shock and his health did not directly suffer. In December he even became able to resume the work with the heliometer.

A letter concerning that instrument from Johnson, the director of the Radcliffe observatory in Oxford, which he had recently visited [?], especially excited Bessel. Johnson inquired about the possibilities of ordering a similar instrument and Bessel advised him to order the lens in Munich and all the rest from Repsold. He also listed the desirable innovations: the halves of the lens to move over a cylindrical surface concentrically to the focus (?) and the [possibility of the] Positionsdrehung of the entire telescope.

[29] Bessel began to work with the Fraunhofer heliometer for which he prepared a dioptric paper on the determination of the focal length based on [his?] previous theoretical investigations and sent it to Schumacher for urgent publication. However, the latter knew that Gauss had just sent *dioptric investigations* on the same subject but arrived at differing conclusions. To avoid disorder, Schumacher asked Gauss' permission to show Bessel his manuscript. Gauss decided that it was not necessary whereas Bessel had no wish to postpone the publication of his paper and it had indeed appeared [No. 340/169] before he became acquainted with the work of Gauss.

Bessel's letter (B – G, 20 Jan. 1841) proved that Schumacher had no call for worrying: Bessel calmly and candidly acknowledged the superiority of the Gauss' paper and only complained [noted] that *it is not easy to clash with you*. He also asked Schumacher to publish an additional remark stating that he had sent his paper on 30 Dec. 1840 but that its appearance was delayed. He thus defended himself against [possible] accusations of plagiarism (B – G, 28 Jan. 1841). And besides he (20 Jan. 1841) informed Schumacher that

My [his] *health is rather good but my courage is broken. I feel that I am not young anymore, that only strive for work has remained*.

In March 1841 Bessel worked very studiously but complained about the *increased immovability* and recalled the time when he was able *to stir a hundred joints at once* (B – S, 4 March 1841). Schumacher mentioned journeys but Bessel did not even want to hear about them and at best thought of coming with his family to visit for a few weeks his youngest brother in Saarbrücken.

He remained at home, drank mineral water, sometimes went hunting or to the seashore. By autumn he was quite prepared to install the impatiently awaited new meridian circle. Adolf Repsold came himself and Steinfurth was to help. They started work in the beginning of November and concluded it in a fortnight. Bessel most approvingly mentioned the new instrument but then suddenly exclaimed: *Give me an axis and a cartwheel, and I will be able to observe just as well*! That mischievous joke meant that he was in high spirits. He talked much about the heliometer needed in Oxford [see § 28], but the preparation of the drafts was not yet possible. Still, Bessel's experience and advice were thus taken into account.

Bessel was a most amiable host and the weeks in Königsberg had remained forever in Repsold's recollections. While there, he wrote his wife: *It is* [will be] *difficult to find elsewhere such a trusting and cosy life that is prevailing here*.

At the end of 1841 eight of Bessel's papers had appeared under a common title [No. 350]. They were partly written previously, and partly unsuitable for the *Astron. Nachr*. because of their extent. Six papers were published next year [No. 356].

After the meridian hall was prepared and the investigation of the instrument completed in winter, Bessel avidly began observations in the spring, thus found a desired diversion and sometimes became as cheerful and brisk as previously.

However, Bessel at his instrument was not Bessel in the peacefulness of his study. […] *Pain had gnawed there at his wounded soul* (Busch, *Königsb. astron. Beob.*, 27, Tl. 1, VII).

[30] And so, Bessel (B – S, 20 Apr. 1842) did not even think about journeying although Schumacher would have willingly gone with him to Vienna to observe a solar eclipse:

I have an irresistible aversion for any travelling. […] *I would have never left Königsberg anymore*.

However, in four weeks he (B – S, 22 May 1842) added:

Man proposes, God disposes. I am indeed going, and even to England and France.

Minister von Schoen suggested to the King to send Bessel and [M. H.] Jacobi to Manchester, to a conference of the British Association for the Advancement of Science, and did not wish to listen to any refusals. Bessel found himself in a predicament which however became ever more endurable. He wrote to England and, owing to his feeble health, asked to allow him to remain somewhat apart.

When Schumacher found about these plans, he became frightened and insistently begged Bessel not to subject himself to the tensions of the journey and the English festivities[45]. Bessel however had already decided to go; even previously he expressed his desire to visit England. And on 6 June 1842 he, together with his second daughter Elise and son-in-law Erman, went through Göttingen to see Gauss (who had been in low spirits), Ostende and London to Manchester.

At the conference, he was met with great honour and little was left of his good intention to remain apart. During the eleven days all kinds of visits in England and Scotland had taken place and Bessel received tokens of attention. On the way back Bessel spent two days in Hawkhurst with John Herschel to whom he took a great fancy.

In London he spent *many hours with Dente* and visited Greenwich for half an hour. In Paris, Bessel did not find Arago but was able to see him during his last day there. *A competent chap completely at the mercy of his humane heart. I intended to understand something scientific from him*. He also came to like the old Bouvard. Mathieu *became a good deputy but did not even hint at astronomy*. Gambey deserved the grade *good, able*, and Winnerl, *excellent* (B – S, 9 Aug. 1842). Bessel also mentioned his missed opportunities: he had not met either Simms the sign of whose firm he saw daily or Breguet.

[31] In spite of the tension, the journey, as Bessel thought, positively influenced him. For a long time his letters did not mention health and soon he got accustomed to work once more. Perhaps he overdid it since by the beginning of 1843 he once more started feeling spasms in his breast. In March he complained about tormenting rheumatic headaches, complete (gänzlich) absent-mindedness and irresistible listlessness.

However, in May Bessel again remained at the meridian circle *day and night* and on 14 September 1843 thus ended his letter [to whom?]: *Now, once more to the observatory! The weather has cleared up* *wonderfully*. From the end of May he (B – G, 17 Oct. 1843) had

40 times observed most of my [36 – J. A. R.] fundamental stars, 10 times directly and 10 times in reflection at each position of the axis. […] *During cloudy days I investigated the errors of the graduations*. The result of all that work was a list of declinations [of those stars], much more reliable than the previous ones.

Then followed considerations about the bending of the limb [of the sector? (Teilkreis)[46]].

In February 1844 Bessel found time for a popular talk, the last of a series of 15 talks for a wide circle of acquaintances which began in 1832 and which Bessel regarded as fragments of popular astronomy (see [x]). After his death Schumacher collected and published them [No. 385]. In his Introduction he indicated that on 28 February 1840 Bessel had reported about the planet only discovered in September 1846 and called Neptune: the anomalies in the motion of Uranus which he revealed by calculations were occasioned by that planet. However, neither health nor time had allowed Bessel to continue his work.

The state of his health fluctuated ever stronger but he steadfastly resisted. For him, hunting remained a desirable and often refreshing remedy. With difficulty he resumed observations in April 1844, but, complying with the request of his physician, abandoned them until he began drinking mineral water (Busch, *Königsb. astron. Beob.,* 27, Tl. 1, VIII).

In March 1844 the Bessel family joyfully celebrated the engagement of his second daughter Elise to Lorenz Lorck, a son of a family friendly with them for many years and respected in Königsberg. Much later one of Elise's sons presented Bessel's letters to the Berlin Academy.

[32] Unwillingly Bessel carried out Schumacher's request for compiling a sketch of Olbers' biography for the *Astron. Nachr*[47]. Olbers was so close to Bessel and so highly respected by him but he still resisted a detailed and frank description of that, which he considered self-evident with respect to his fatherly friend. And eventually he became dissatisfied by his text [ ]. *I do not like beating about the bush and prefer to reach the essence by faltering steps*. Most of all he would have simply repeated his own words written just after Olbers' death [see § 28]: *I knew no weakness in him. I see him before my eyes, majestic and marvellous*[48]. Foreseeing his future he (B – S, 30 June 1844) wrote:

I see so much which I would not like to lose, and I will not therefore regret having a few more years to live.

Heinrich Schlüter, Bessel's assistant in working with the meridian circle, regrettably died in the spring [of that year]. In September 1844, a jubilee of the Königsberg University was celebrated and Bessel was awarded the star to the order of the Red Eagle (Stern zum Roten Adler-Orden). At that time he was unwell but hunting refreshed him. Then he became *very busy,* but in October had to give over the almost concluded observations with the meridian circle to Busch. However, he soon consoled himself by obtaining them after their completion.

[33] In the beginning of December Bessel *fell ill and is ill now also*

(B – S, 2 Dec. 1844)[49]. *Some parts of the body are now stronger and there seems to be no general dropsy. Sleep and appetite are good*. […] *I do not know what's the matter*. Achieved little, managed to read more (B – S, 15 May 1845). He did not say anything about observation.

The disease crept up on him. In May 1845 suffering is *unbearable*. In June the King sent him his personal physician, the celebrated Schoenlein, but Bessel's state is *as bad as previously* and in August it remained *without any essential improvement* (B – S, 24 Aug. 1845).

After a few difficult weeks, on 6 November, he became able to write without any help and remarked that a year had passed since the time when he had decided that his disease has *wholly manifested itself*. He was again anxious mostly because the year had passed almost completely for nothing.

The little that I was able to attain is a part of a new article on the theory of the system of Saturn [No. 386/22]. *And I had to endure excessive suffering and pain. A thousand times I have asked heaven to weaken my suffering which became so severe that from one week to another I had hoped to die. But I must patiently bear my heavy burden* (B – S, 8 Nov. 1845).

In December 1845 Bessel's state fluctuated but improved so much that he sent Schumacher a long article concerning pendulum clocks[50] and a detailed report about that improvement.

I am still living with a good hope. My supposed delusion is so serious that in the reassuring case I am considering most various measures (B – S, 21 Dec. 1845).

He wished to furnish his room anew and asked Schumacher about mahogany furniture and the upholstery and thought of ordering all that in Hamburg and besides, as many times previously, about buying wine. The disease went on with improvements and worsening. Negotiations about the furniture were cancelled and resumed anew and scientific remarks had occurred [in correspondence].

The King presented Bessel his portrait (painted by Franz Krüger) with a *lovely* holograph letter, as Bessel reported to Schumacher in his last letter of 22 Febr. 1846. He ended it with the words

I am gravely ill and a mosquito can irritate me. Do not take me either badly or even unjustly since the mosquito will disappear at once. For more than two years now I see you as my sheet anchor which must hold even in quicksand.

Severe suffering went on for many weeks (cancer of abdomen) until on 17 March 1846, at half past six in the evening the expected end had occurred. Bessel (Anger 1846, p. 29)

Was fully conscious until the end and expressed his pleasure about this to his wife and (the youngest) daughter Johanna who remained with him. Already three days before he died he changed very much. His pulse was barely perceptible and he was almost all the time slumbering. And his death took place in a manner in which he always wished it to occur.

Bessel was buried near the observatory, about a hundred meters to the north-west from the meridian hall. In 1885 his wife was buried nearby. She lived to be 91 years old.

Hamburg, August 1919

Notes

1. These recollections are reprinted here in accordance with the author's wish and at his expense so that everything concerning Bessel's portrait occurs in a single source. Schumacher [Editor of the *Astron. Nachr.*].

These recollections were here abridged; in particular, all the accompanying notes were omitted. O. S.

2. After 1801 Gauss became one of the first if not the very first mathematician in the whole world.

3. Johann Hieronymus Schröter was born in 1745 in Erfurt and was not an astronomer by profession. In 1764 he entered the Göttingen university to study the law [did he graduate? – O. S.], but with a special liking he took to hearing Kästner's astronomical lectures. In 1770, after filling various minor posts he was sent to Herzberg as an assistant of an official. The possibility of studying agriculture occurred there.

In 1777 Schröter was appointed secretary of the Royal chamber in Hanover. Being a music-lover, he became acquainted there with the family of the oboist Isaac Herschel, the father of Wilhelm [William] Herschel, about whose great success in England achieved with home-made astronomical instruments he passionately recounted.

Schröter began to read Kästner's books once more and then, being helped by Dietrich Herschel, the younger brother of Wilhelm, managed to acquire a 3 ft Dollond telescope and installed it himself with a *lunar and solar micrometer*. His enthusiasm for astronomy strengthened, and when in 1781 it became possible to become a senior official in a fen village Lilienthal, about a mile from Bremen, he agreed at once. Indeed, his decision corresponded to his inclination to occupy himself with farming as practised by a previous monastery and in addition provided him the possibility of unhinderedly following Herschel's example.

He moved to Lilienthal and soon arranged a small house for observations with his Dollond 3 ft quadrant which he (Schumacher 1889, p. 53) *applied most successfully instead of a mural quadrant and a transit instrument.* In 1784 through Dietrich Herschel he received a 4 ft Newtonian reflecting telescope from Dietrich's brother Wilhelm (Ibidem, p. 51) and, in 1786, a mirror with aperture of 6 inches and focal length of 7 ft (and installed it himself) with 10 eyepieces, and *an excellent Sternausmesser with a best screw micrometer* and a similar [mirror] manufactured by Joh. Christ. Drechsler in Hanover (Ibidem, p. 55). Schröter himself made a *Scheiben-Lampe micrometer*.

He published his observations made from 1785 onward in the *Berliner astron. Jahrbuch*, but they did not always correspond to those carried out by Herschel. This prompted Schröder to obtain a larger reflector similar to Herschel's. Happily, he met Professor J. G. F. Schrader from Kiel who repeatedly ground mirrors. He visited Schröder and recommended to install larger mirrors. Four 7 ft, a 12 ft and a 13 ft mirrors were manufactured and, shortly before Schrader's departure (in January 1793), a 19 ft mirror was cast. It was possible to charge the gardener, Harm Gefken, with its grinding and polishing since he assisted Schrader in the treatment of the other mirrors and learned that art. Later, in 1806, Gefken very successfully coped all by himself with a 15 ft reflector (Schumacher 1889, p. 104).

Until 1796 Schröter almost always had been working alone but after the number and the sizes of his instruments essentially increased an assistant became desirable. When looking for a tutor for his ten-years-old son he found a suitable man for both occupations, Carl Ludwig Harding, a candidate of theology, who had also attended Kästner's lectures and since then readily occupied himself with astronomy. Schröter thus found himself a willing assistant who remained in Lilienthal for nine years.

Being busy with astronomical investigations, Schröter did not at all lose his practical grasp. He had gradually spent so much means on his observatory, that no more was left. However, in 1799 he decided to take over the establishment of a large fen colony and attempted to sell his instruments to the Hanoverian – English government on the condition that they will remain in his use. Government circles were well informed about his laudable activities and not only did he succeed, he also arranged the admittance of Harding to civil service as inspector of the observatory with a salary of 200 thalers. He thus freed himself of that burden.

In 1805 Harding gained a professorship at Göttingen and his work in Lilienthal came to an end. However, his connection with the observatory was not completely broken off and he continued to draw a half of his salary as an inspector. Schröter needed another assistant so that Olbers, as mentioned above, helped Bessel to fill that post. J. A. R.

4. No explanation provided.

5. Gefken was mentioned in Note 3.

6. Bessel distinctly saw everything situated at distances of 10 inches and farther [No. 82/17]. J. A. R.

7. Amalie's letters show that in common parlance Bessel was called *Fritz*. J. A. R.

8. I named Wilhelm von Humboldt's post according to the third edition of the *Great Sov. Enc.* (vol. 7, 1972). This edition is available in an English translation.

9. Bessel apparently visited his parents as well.

10. It was rumoured at that time that Napoleon had ridden through the town and was very much surprised that an observatory rather than, say, a blockhouse was being built, and remarked: *So the King of Prussia still has time to think about such objects* (Anger 1846, p. 16). J. A. R.

11. So rabies had been somehow prevented even before Pasteur.

12. According to Bessel's still preserved passport dated 10 April 1810, his *height was 1.68 m.* J. A. R.

13. Bessel [No. 378/184] hoped that the Jews will be soon granted full civil rights. Did his hope correspond to the views of the Catholic or protestant Church?

14. Without providing an exact reference, Galle (1924, Epigraph) quoted Gauss:

Science should be the friend of practice but not its slave, should give presents to practice rather than serve it.

15. At that time Bessel was 37 years old.

16. The Earth's axis of rotation is inclined by 65 1/2 degrees to the plane of the ecliptic and describes a cone whose axis is perpendicular to that plane (Blazko 1947, p. 118).

17. Here and many times below the author grammatically changed the quoted passages. Bessel certainly did not use the third person when writing about himself.

18. The author several times mentions the seconds pendulum without specifying the appropriate latitude.

19. I can only refer to Bessel's contribution [No. 344].

20. Since Gauss refused to lecture, the university was unable to pay Gauss a part of his salary, which, when complemented by the means provided by the Academy, would have reached the required level whereas the King did not approve a grant of special means (O – G, 22 Sept. 1824). This information came from Prof. Dirksen (Berlin) who visited Olbers (O – G, 12 Oct. 1824). Dirksen later told Olbers that that difficulty was overcome since *a fund for advisable expenses* was discovered (O – G, 18 Oct. 1824), but apparently too late. J. A. R.

21. Schumacher lived in Altona (now, a district of Hamburg).

22. Even on 2 Nov. 1817 Olbers expressed his regret to Bessel that his relations with Gauss were hardly satisfactory:

I will be very sorry if some prolonged coolness between the two […] *greatest German astronomers and mathematicians will occur*.

Bruhns mentioned a witness (Ohrenzeuge) who heard that *Gauss had harshly fell on Bessel*. Bruhns himself noted that their correspondence had not included anything of the kind.

23. A similar statement was contained in a letter B − S of 12 May 1825.

24. Elsewhere Repsold (1918, pp. 24 – 25) quoted a similar letter B – S of 1828.

25. The *roundabout way* is not explained. Gauss expressed his opinion (see a bit below) in a letter to Olbers of 8 Dec. 1817:

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

26. This contradicts the previous Note.

27. During this journey Schumacher met Bohnenberger in Tübingen.

He is a pleasant man and, if only I were not writing this letter to you, I would have said, a second Bessel (S – B, 12 Dec. 1826). J. A. R.

28. See [vii, Note 14] and [No. 254/138].

29. Later Bessel (B – G, 1 Nov. 1845) stated that his catalogue will be useful up to 1850.

30. See Encke's opposite wish in § 15.

31. However, in a letter to Humboldt of 2 June 1830 Bessel (Felber 1994) wrote that Encke's and Struve's visits had greatly pleased him.

32. Since Daniel Bernoulli and Lambert had published in 1776 their astronomical works in German, Lalande (1802 – 1803/1985, p. 539) stated that astronomers ought to study German.

33. Why was it necessary to repeat the measurement of the base?

34. Gauss certainly realized that geodetic measurements were important, which was one of the reasons why he engaged in that work for a few years. But he also held that *all the measurements taken worldwide do not offset a theorem which leads science nearer to eternal truth* (G – B, 14 March 1824), see [iii, § 4]).

35. In 1832 Bessel published a paper [No. 261/83] describing Erman's scientific journey to Siberia and Kamchatka.

36. This is not altogether correct, see end of § 11.

37. On the use of the comparator see end of § 22.

38. The number of the significant digits is doubtful.

39. This description of Bessel's personality does not essentially differ from Encke's impression of 1819 (end of § 8) and it also corresponds with the opinion of Kosch [vii, § 10], the last family doctor of Bessel (*Abh*., Bd. 1, p. XXX). In 1834 his doctor was still Motherby (O – B, 2 Apr. 1834). Kosch wrote: *Who came near to Bessel was delighted* … But then, Kosch stated that Bessel was *of short stature, weakly and skinny* …. It seems however that Bessel's *noticeably pale* face was a special trait in his family which manifested itself in one of his daughters and one of his great grandsons (Hagen). In general we ought to recognize that Kosch judged Bessel in his last and difficult years. In his young years he can be imagined as a diligent hunter, gardener in his own garden and in general a fresh and lively man. But he had unprecedentedly exerted himself and Kosch felt that *his still youthful force dominated its frail shell*. J. A. R.

40. The author did not use this term previously.

41. Three comets rather than one were named after Pons.

42. Where are these letters?

43. This explanation seems hollow.

44. Bessel (B – O, 3 March 1811) stated that his salary was 1200 thalers, but in §§ 4 and 5 he named 800 and then 1100 thalers.

45. This Association was established in 1831, which means that the ten years of its existence were celebrated. Elsewhere Repsold (1918, p. 30) named Glasgow rather than Manchester and here the same author added that Bessel had visited Scotland as well. Finally, Bessel himself [No. 354] later mentioned Manchester. The conference was possibly held in both cities in turn.

46. Cf. [vii, Note 12].

In addition to these pleasant results achieved during the last years, we happily possess his successful portrait. Its original and its relief casting are in possession of Dr. E. Hagen, a son of Bessel's youngest daughter. He deserves sincere thanks for allowing its reproduction and not less for many very desirable small communications and hints about his grandfather. Concerning the portrait of 1843 he noted:

The original, a daguerrotype with the size of the head being 20x22 mm, was taken by Ludw. Ferd. Maser, physics Professor at Königsberg University. In April 1880, his nephew presented the original from his uncle's archive to my father. J. A. R.

47. Cf. however [ix].

48. This is repeated from § 28.

49. Bessel fell ill in December, and on 2 December he was still ill!

50. I was unable to ascertain this information.

Brief Information about Those Mentioned

Anger Carl Theodor, 1803 – 1858, mathematician, astronomer

Bode Johann Elert, 1747 – 1826, astronomer

Bohnenberger Johann Gottlieb Friedrich von, 1765 – 1831, astronomer

Bouvard Alexis, 1767 – 1843, astronomer

Breguet Louis Clément François, 1804 – 1883, physicist, watchmaker

Busch August Ludwig, 1804 – 1855, astronomer

Clausen Thomas, 1801 – 1885, astronomer, mathematician

Dent Edward John, 1790 – 1853, watchmaker

Dirksen Enne Heeren (?), 1788 – 1850, mathematician

Dirksen Heinrich Eduard (?), 1790 – 1868, jurist

Fortin Jean Nicolas, 1750 – 1831, manufacturer of scientific instruments

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

Gerling Christian Ludwig, 1788 – 1864, astronomer, geodesist, physicist

Hahn Friedrich von, 1742 – 1805, landowner, philosopher, astronomer

Hansen Peter Andreas, 1795 – 1874, astronomer, mathematician

Harding Karl Ludwig, 1765 – 1834, astronomer

Jacobi Carl Gustav Jacob, 1804 – 1851, mathematician

Jacobi Moritz Heinrich, 1801 – 1874, physicist, inventor

Johnson Manuel John, 1805 – 1859, astronomer

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

Kästner Abraham Gotthelf, 1719 – 1800, mathematician

Klindworth Johann Andreas, 1742 – 1813, mechanician, watchmaker

Kramp Christian, 1760 – 1826, mathematician

Lalande Joseph Jerome François, 1732 – 1807, astronomer

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

Mathieu Claude Louis, 1783 – 1875, mathematician, astronomer

Merz Georg, 1793 – 1867, optician

Müffling Friedrich Karl Ferdinand Freiherr von, 1775 – 1851, diplomat, geodesist

Nicolai Friedrich Bernhard Gottfried, 1793 – 1846, astronomer

Oerstedt Hans Christian, 1777 – 1851, physicist

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

Pond John, 1767 – 1836, astronomer

Pons Jean-Louis, 1761 – 1831, astronomer

Rosenberger Otto August, 1800 – 1890, astronomer, geodesist

Scherk Heinrich Ferdinand, 1796 – 1885, astronomer

Schlüter Heinrich, 1815 – 1844, astronomer

Simms William, 1793 – 1860, manufacturer of scientific instruments

Soldner Johann Georg von, 1776 – 1833, physicist, mathematician, astronomer

Schrader Johann Gottlieb Friedrich, 1763 – 1821, physicist
Steinheil Carl August von, 1801 – 1870, physicist, inventor, engineer, astronomer
Tralles Johann Georg, 1763 – 1822, mathematician, physicist
Utschneider Joseph, 1763 – 1840, engineer, businessman
Walbeck Henrik Johan, 1793 – 1822, astronomer, geophysicist
Winnerl Joseph Thaddäus, 1799 – 1886, watchmaker

Bibliography

Anger C. T. (1846), *Erinnerungen an Bessel's Leben und Wirken*. Danzig.

Blazko S. N. (1947), *Kurs Obshchei Asstronomii* (A Course in General Astronomy). Moscow.

Bomford G. (1952), *Geodesy*. Oxford.

Bruhns C. (1869), *Joh. Franz Encke*. Leipzig.

Felber H.-J., Editor (1994), *Briefwechsel zwischen A. von Humboldt und F. W. Bessel*. Berlin.

Galle E. (1924), Über die geodätische Arbeiten von Gauss. *W*-11/2, Abt. 1. Separate paging.

Lalande J. J. (1801), *Histoire céleste française*. 1847.

--- (1802/1803), *Bibliographie astronomique*. Osnabrück, 1985.

Repsold J. A. (1918), Heinrich Christian Schumacher. *Astron. Nachr.*, Bd. 208, No. 4970 – 4971, pp. 17 – 34.

Schumacher H. (1889), Die Lilienthaler Sternwarte. *Abh. Naturwiss. Vereins Bremen*, Bd. 9, No. 1.

Sheynin O. (2001), Gauss, Bessel and the adjustment of triangulation. *Hist. Scientiarum*, vol. 11, pp. 168 – 175.

Supplement No. 1. Bessel's Honorary Medal from the London Astronomical Society

Memoirs Astronomical Society of London, vol. 4, 1829, pp. 217 – 221.
Medal presented by the President, John Herschel

Gentlemen, The next Medal which has been awarded by your Council is a Gold Medal to Professor Bessel, for his observations of the stars in *zones*, made by him at the Royal Observatory of Königsberg; − a vast undertaking, and one which would alone suffice to confer immortal honour on a name, which has already so many other independent claims to astronomical distinction. The attention of astronomers, in fixed national observatories, up to a late period, was almost exclusively confined to observations of the sun, moon, and planets, and a moderate number of the principal fixed stars. The smaller stars, the minor host of heaven, were systematically neglected, and the conspicuous ones only deemed worthy of being observed in any other than a desultory way. Their utility for the purposes of nautical astronomy might of course be expected to draw upon the most remarkable ones a proportionate attention; but astronomers, like the vulgar, had been too much influenced by appearances and by glitter, and had fallen into habitual neglect of the rest, or contented themselves with rough approximations to their places, sufficient to mark them down in maps, or include them in lists and approximate catalogues; but inadequate to the determination of any delicate question as to their proper motions, parallax, &c. To this, however, one splendid exception must be made in the Catalogue of Piazzi. This record of the places of more than 7000 stars of all magnitudes, determined with an excellent instrument, with all the care of a diligent and cautious observer, and from several observations of each, is one of the finest monuments of astronomical research. Nor ought the labours of Lalande to be forgotten. His examination, indeed, was extended to an enormous list, to no fewer than 50,000, and was conducted, like Professor Bessel's, in zones. It has been rendered available, also, to astronomers, by tables of reduction, of the simplest possible kind, published by Professor Schumacher, and is indeed a most useful and valuable collection. It labours, however, under the disadvantage of a great inferiority in an instrumental point of view, and therefore can be nowise regarded as superseding or anticipating the more refined inquiries of Professor Bessel.

It would be quite superfluous to speak here of the general merits of Professor Bessel as an astronomer, or of the excellence of the observations regularly made in the observatory under his direction. We know and appreciate them; but they are not to be made the subject of our remarks or our praise on this occasion. The observations for which your Medal is awarded to him were commenced in 1821, and have been continued with little intermission ever since, at the Royal Observatory at Königsberg, with the meridian circle of Reichenbach, having a magnifying power of 106 applied to a most excellent telescope. This instrument being confined to a zone of about two degrees in breadth, is made to oscillate or sweep up and down continually, while the heavens pass in review before the observer by their diurnal motion, and all stars, down to the ninth magnitude, which pass the field, are taken at once in right ascension and declination, and read off by the clock and limb of the circle. This mode of observing presents two capital advantages, − viz. multitude of objects, and facility of reduction. Of the former we may judge by the fact, that in some of the zones we find between three and four hundred objects observed at a sitting: with respect to the latter, a little table, of the simplest use and most compendious form, is attached to each zone, and by its aid the readings of the clock and limb are at once reduced (by a calculation comprised in three lines) to the mean right ascensions and declinations of the objects at a fixed epoch, freed from instrumental error, and ready for the catalogue. Those only, who know by experience the labour of reducing observations not made on this system, can imagine the saving of toil and drudgery thus arising. Nay, more – it renders the observation-book itself available as a catalogue; for, by the system of indexing the zones, any point in the heavens may at once be referred to, and every object there at once reduced, without need to turn over the book, to enter into any inquiry, or in any way refer beyond the page before us and the table of reductions in the beginning of each volume. This is the perfection of astronomical book-keeping.

This course of observations was commenced, as I have already said, in 1821; and you may judge of the industry and perseverance with which it has been prosecuted, by the fact, that, by the end of 1824, the whole equatorial belt, of 30° in breadth, of the heavens, had been swept, and between 30 and 40,000 stars observed. But this did not satisfy the zeal, or exhaust the patience, of M. Bessel. He has since continued the work northward with unabated ardour, and is extending his zones to the 45th degree of northern declination: thus embracing, in the whole, sixty degrees of the finest part of the heavens.

A great many double stars, some of them very delicate ones, have been detected in these sweeps; they are included in M. Struve's splendid catalogue of these objects. Nor is it at all improbable, that many new planets may have been seen, and, on a repetition of the observations, will be found missing. In a word, we have in this collection, one of those great masses of scientific capital laid up as a permanent and accumulating fund, the interest of which will go on increasing with the progress of years. It is a harvest sown, and already springing, but of which the ripened produce is destined for after generations. Yet the crop, if a remote, is a sure one. It will neither be uprooted by political convulsions, nor stinted by neglect, nor spoiled by premature gathering in. The language of such a record is like that of a prophecy. It is written, but we cannot yet read it. It is full of truth, but not for us. It contains the statement of a vast system, but future generations must develop it. Could it be permitted us to look forward and draw aside the veil which a few centuries interpose between us and its interpretation, we might expect to see all the great questions which agitate astronomers set at rest, and new ones, more refined, and grounded on their solution, arising. Some minute and telescopic atom will perhaps have become the stepping-stone between our system and the starry firmament – its parallax will mark it for our neighbour – and either its fixity will demonstrate the equilibrium of our immediate sidereal system, or its proper motion reveal to us the nature and extent of the forces which pervade it. The orbits of those remarkable stars which are ascertained to be really *erratic*, or which have a proper motion too large to be overlooked, such as 61 *Cygni* and μ *Cassiopeae*, will become known. They will be seen to deviate in their paths from great circles of the heavens – their convexity or concavity will mark the directions, and their changes of velocity the intensities, of the forces which urge them. Already, since the date of the first catalogue of fixed stars, the former of these wonderful objects has moved over no less than four degrees of the heavens. Had it been accurately observed but once in a century, what might we not have known! Let this consideration stimulate astronomers to follow up the splendid example Professor Bessel is setting, and complete and pursue the gigantic task he has carried on so far, but which is beyond the power of any one man to go through, much less to repeat. How much is escaping us? How unworthy is it of those who call themselves philosophers to let these great phenomena of nature – these slow, but majestic manifestations of the power and glory of God – glide by unnoticed, and drop out of history, beyond the power of recovery, because we will not take the pains to note them in their unobtruding [unobtrusive] and furtive passage; because we see them in their everyday dress and mark no sudden change; and conclude that all is dead, because we will not look for the signs of life; and that all is uninteresting, because we are not impressed and dazzled.

We must not, however, be hasty in our reproaches. There is a general sense afloat among the continental astronomers, of the necessity of laying a foundation for the future sidereal astronomy, as deep and as wide as the visible constituents of the universe itself. Nothing less than ALL will be enough – *quicquid nitet notandum*. To say, indeed, that every individual star in the milky [Milky] way, to the amount of eight or ten million, is to have its place determined and its motion watched, would be extravagant; but at least let samples be taken – at least let monographs of parts be made, with powerful telescopes and refined instruments, that we may know what is going on in that abyss of stars, where, at present, imagination wanders without a guide. Let us at least scrutinize the interior of sidereal clusters. Who knows what motions may subsist, what activity may be found to prevail, in those mysterious swarms? Or if we find them to be composed of individuals at rest among themselves – if we are to regard them as quiescent societies of separate and independent suns, bound by no forcible tie like that of gravity, but linked by some more delicate and yet more incomprehensible [less comprehensible] cause of union and common interest – the wonder is all the greater. We walk among miracles, and the soul yearns with an intense desire to penetrate some portion of these secrets, whose full knowledge, after all, we must refer to a higher state of existence, and an eternity of sublime contemplation.

Supplement No. 2. Schumacher's Honorary Medal from the London Astronomical Society

Ibidem, pp. 221 – 224.
Medal presented by the President, William Herschel

Astronomy is a science peculiarly in unison with the German national character. The persevering industry which forms so striking a feature in it, is the quality, of all others, requisite for an astronomer – that diligence which never wearies, and which, working slowly, and destroying nothing that is done [Beschäftigung die nie ermattet, die langsam wirkt doch nie zerstört, &c – Schiller] goes on adding grain by grain to the mass of results, and accumulating them with a kind of avarice to swell the heap; − that painstaking scrutiny which penetrates through all details, and will not be satisfied till perfection is attained. And, on the other hand, an enthusiasm seemingly incompatible with this plodding turn, yet often coexisting with it in the same mind; a love of systems for their own sake; a spirit of speculation, sometimes bordering on wilderness; and an ardent inherent love of the vast and wonderful. Among minds of this turn it is no wonder that astronomy should flourish – with enough of sublimity and mystery to exhaust the wildest imagination, and enough of laborious detail to keep in employment the most patient industry. Accordingly, Germany has always been fruitful in astronomers, and (regarding as Germans all who are bound in the common family tie of language and manners) German astronomy has at present reached a pitch of eminence, which only national pride prevents our acknowledging to be unexampled in the history of the science – whether we consider the researches of their theorists, the activity of their computers, or the number and importance of their national observatories; or those of Russia, several of which are manned (so to speak) with directors and assistants who have been educated in the German school, and transplanted from German observatories, and from the personal tuition of their most illustrious men, who have worked with them as their friends and pupils, rather than as mere assistants, and who look up to them with the veneration of the scholar to his master.

Among all these, and among those numerous and talented individuals throughout the continent, and in England, who are attracted to astronomy professionally, or from love of the science, the *Astronomische Nachrichten* of Professor Schumacher establishes a point of concourse – a complete bond of union: we have there a theatre of discussion of whatever is most new and refined in the theory and practice of astronomy – the utmost delicacies of computation and scrupulous investigation of instrumental errors are given by those most competent to supply and to judge of them. To its pages observations of every kind find their way, especially those which depend for their utility on corresponding observations, or which lose their interest and importance by long suppression. Not a comet appears but *there* we find its elements handed in from all quarters with emulous rapidity – occultations – moon-culminating observations – computations of longitudes and latitudes – disquisitions on practical points – descriptions, advertisements, and prices of instruments – in a word, everything which can awaken and keep alive attention to the science – everything that can facilitate the contact of mind with mind. Everyone who has attended to the progress of knowledge in recent times must feel all the importance of such an engine. But it cannot be kept in action without a strong presiding power. In any inferior hand it would languish, and soon fall into disrepute and inaction. Professor Schumacher is, of all men, that one whom the voice of Europe would have fixed on for the conduct of such a work: an excellent astronomer himself, and presiding over an observatory in which everything is delicate and exquisite, he possesses that practical and theoretical knowledge which commands respect, and gives his acceptance or rejection of contributions a weight from which there is no appeal. He has, moreover, the eminent but merited good fortune to possess the full and effective support of a Government deeply impressed with the importance of astronomical science. With this powerful aid, which would have been accorded to no other, he has been enabled to establish sure and regular communications with every part of the civilized world – and to face an expenditure which, under similar circumstances, no private individual would have ventured to undertake. He has thrown his whole weight into the scale of advancing science; and the effect has been, the establishment of a great European republic, with a common feeling, and a sense of common interests.

But the services rendered by M. Schumacher to astronomy are not limited to this publication. A numerous and useful collection of tables has been edited by him, under the title of *Hilfstafeln*, or assistant tables, and others. One of these volumes is devoted to facilitate the reduction of the observations of Lalande in the *Histoire céleste* [*française*], on the same plan with those used for the reduction of Bessel's zones. This truly useful work rescues from oblivion the labours of Lalande, and renders his observations available to science. M. Schumacher, liberally assisted in a pecuniary point of view, by the Royal Danish Hydrographic Office, has also followed up the example set by the Coimbra Ephemeris, of the publication of lunar distances from the planets, − thus rendering available a new branch of nautical astronomy, and hastening the period when observations of the planets at sea would have naturally been called for.

In the computation of the assistant tables, M. Schumacher has had most active assistance from several accomplished Danes; of whom I may mention Hansen, Clausen, Ursin, Nissen, Nehus Zahrtmann, and Petersen. In honouring the principal, we honour the accessories; and we trust that the tribute of this passing notice will not be displeasing to them and their coadjutors.

Captain Smyth, − As you are kind enough to act as proxy for Professors Bessel and Schumacher, receive for them these their respective medals; and, in transmitting them, take care to convey to them the expression of our gratitude and admiration for the services they have rendered to our science, and our wishes that their brilliant and useful career may be prolonged yet many years, with increase of glory, and with health and prosperity to enjoy it.

IX

F. W. Bessel

About Olbers

Über Olbers. *Abhandlungen*, Bd 3. Leipzig, 1876, pp. 479 – 481.
First published 1844

In these recollections about an eminent astronomer, who remains the pride and joy of Bremen, I do not dare to describe convincingly the high position which he occupies in science. He was generally respected partly for his special successes but still more for his peculiar ability never to leave the path leading to them. For him, the danger of losing his way had not existed since he did not take a single step without knowing that he proceeds in the outlined direction.

Olbers was never tempted by the lustre of sudden ideas, he did not delve into prolonged investigations. Neither the hope of a chance achievement nor the picking of numerous fruit seemed to him as valuable as a planned cultivation of the desired. In my opinion, the study of the logic of his separate steps which led Olbers to his results ought to be the aim of those who recalls him. However, without a thorough investigation of the new riches for which astronomy should thank him, such studies will remain unconvincing.

Instead of such investigations I will make known his statement which he repeatedly expressed to me: he was delighted by the attempts of Lambert, Bradley and Tobias Mayer[1] and considered *their achievements as pure gold*. They attracted him in the first place, and who heard his stories about them did not fail to feel his similarity with those scientists.

Even Olbers' first step into the realm of astronomy was remarkable. Newton had made the theory of cometary motion completely clear. He discovered the laws of their rotation around the Sun and showed that the motion of each comet was defined by six elements whose knowledge was necessary and sufficient for completely describing their appearance. However, the transition from observing such an event to these elements remained a most complicated mathematical problem. Newton himself indicated one of its solutions provided that the mean time of three full observations of the places of a comet precisely coincides with the mean time of its extreme observations.

Later geometers of the highest calibre had dealt with this problem in most various ways whereas Olbers, even when being a student, revealed a property of the apparent motion of a comet whose introduction essentially simplified the difficulty of that problem. It became possible to solve it without introducing the Newton restriction and much easier as well.

Olbers had first applied his method while looking after his ill university friend. Later, in 1797, he published his method and it became generally applicable. It was impossible to add to it anything substantial. True, the solution could have been partly simplified by introducing another form of calculation which still did not change either the essence of the method or its result.

His paper was remarkable for a complete achievement of its aim but not less for a thorough treatment of similar previous attempts. Such successes fostered the preference of the investigated goal and all his life Olbers had indeed mostly studied comets. He unearthed simple means applicable without prolonged preliminary efforts for determining the places of comets on the celestial sphere by issuing from observations and showed that they deserved respect. Apart from his own observations of comets he enriched astronomy by the results of others. In rare completeness his library included works about comets which often allowed him to save from oblivion contributions essential for our knowledge of those celestial bodies. Without his care they would have possibly never be known.

On cloudless nights Olbers mostly hunted comets. He is known to have been often rewarded by happy results. The most important of these is the discovery of the comet of 1815 whose observations established that it rotates around the Sun in about 74 years and that consequently it will be first seen in 1887. It is deservedly called by the name of its discoverer and from time to time it will remind our descendants about him, and they will mention it as respectfully as we are now discussing the Halley comet.

Finally, Olbers' predilection for cometary astronomy is seen in his numerous excellent memoirs in which he explained various topics more or less related to comets. A future biographer of Olbers will find many possibilities to honour deservedly his natural viewpoint, not prettified by any prejudices, which disclosed the problems presented to him in his incessant occupations with comets.

Olbers mostly devoted his works to comets, but he thought not less thoroughly about all the other branches of astronomy. He scrutinized each essential contemporary achievement so deeply that he became possible to formulate his own opinion about its separate parts even remote from the area of his occupation. During an hour he quite instructively recounted to me the subject of a new volume of Laplace's *Mécanique céleste* and of the Greenwich observations.

Unanswered problems of natural phenomena lively stimulated his curiosity. He was the first to study mathematically the possibility and probability of the lunar origin of meteorites[1]; to develop the method of the treating the observations of meteors carried out by Benzenberg and Brandes etc. The few leisure hours, which he had been able to devote to astronomy[1], left traces of his own work in each of its part. He justifiably restricted the field thus enriched by him and never came out when unable to add something essential to the known.

Without the testimony of those who enjoyed a personal acquaintance with him we would possibly doubt that Olbers had felt himself on the peak high above all the region of astronomy. On 1 January 1801 Piazzi discovered Ceres which however soon became invisible in the solar rays. Olbers avidly and successfully participated in its further search and [then] incessantly followed its motion. Acquainting himself with the faint stars in the vicinity of the celestial sphere through which Ceres should have moved, he discovered a star previously unnoticed by him. And thus, soon after the rediscovery of

Ceres, on 28 March 1802, he discovered a second new planet, the Pallas.

That was an excellent time for astronomy! Delightful activity rapidly accumulated remarkable results. Gauss did not rest content with his rare combination of unsurpassed mathematical power and a perfect knowledge of the field where he should apply his methods of determining the paths of the new planets. Incessantly employing them he followed the everyday successes of the astronomers. His insight compelled them to achieve the highest possible precision and convinced them in that their attempts were useful. The knowledge of the paths of Ceres and Pallas soon became perfect[1].

Special star charts had hardly been considered necessary for simplifying future observations of the new planets, but Harding had already occupied himself with designing them and on 1 September 1804 he thus discovered the third new planet, Juno. Olbers found himself in the midst of this challenging animation. The most fervent astronomer, he was able to direct it lively and properly and possessed personal traits which deserved him unquestionable trust.

Even if the discovery of the new planets is assigned to happy chance, which however could have only favoured diligent investigators of the heaven, the discovery of the fourth one, Vesta, on 29 March 1807 crowned the long and systematic efforts by deserved success. The paths of the new planets approached one another which prompted Olbers to surmise that sometime they could have possessed a common point whose trace was still seen in this approach in spite of the perturbations caused by the larger planets. And he noted that, if the fragments of a [not anymore existing] large planet have scattered under the influence of some external or internal cause, there should be a common point of the paths of all the three [known new] planets.

Olbers reasonably assumed that it was improbable for such a happy chance, which allowed the discovery of three similar planets during a short period, to exhaust their number. He therefore decided to discover many others, and turned his attention to the region of the sky in which the paths of Ceres, Pallas and Juno approached one another. For many years, month after month, he surveyed the faint stars in that region and he thus *should have* discovered all the bodies moving through it. And he had indeed discovered Vesta.

Olbers enjoyed respect without sharing it with non-existing rivals. He enriched our knowledge of the Solar system by discovering two planets and he should have been thankful to his active spirit and insistent efforts rather than a happy chance.

I had to restrict my attention to indications for those who will create him a monument more durable than made of bronze. Everyone knows more [about Olbers] than I was able to say here, but, anyway, I had not suppressed that which I offered so that astronomers will not forget the circle in whose centre Olbers had shone during his life. Those, who were included in that circle, will find rich material in the description of his medical and humane significance for expressing their esteem.

I have esteemed him as well. He was my noblest friend. By reasonable fatherly advised he guided my youth. I am keeping his letters, 171 in all, which justify my right to extend my respect beyond science. Hundreds of hours passed in his presence became unforgettable and with each of them I connect a noble expression, a vivid opinion and a lenient attitude towards others. I see him majestic and invariable, in his prime in 1804 and as an old man in 1837. Let someone more skilled preserve for posterity the picture which he created and left in my heart.

Editor's note. *The Biographishen Skizzen verstorbener Bremischer Ärzte und Naturforscher* tells us that this year (1844) the society of Bremen physicians published a memorial collection of papers for the 22nd conference of German naturalists and physicians. Schumacher [Editor of the *Astron. Nachr*.].

Olbers was born in 1747 and died in 1840. O. S.

Notes

1. Only a small part of the meteorites are *lunar*; however, in those times all of them were considered lunar.

2. Bessel did not say that it was Gauss who had ensured the rediscovery of Ceres.

3. Olbers was a practising physician.

4. Blazko (1947, p. 345) mentioned Olbers in this connection.

Brief Information about Those Mentioned

Benzenberg Johann Friedrich, 1777 – 1846, theologian, astronomer

Brandes Heinrich Wilhelm, 1777 – 1834, meteorologist, astronomer

Piazzi Giuseppe, 1746 – 1826, astronomer

Blazko S. N. (1947), *Kurs Obshchei Astronomii* (Course in General Astronomy). Moscow.

X

Oscar Sheynin

The other Bessel

Friedrich Wilhelm Bessel (1784 – 1846) was an outstanding astronomer and an eminent mathematician. I (2000, p. 77) have briefly listed his achievements in astronomy, but focused on his unforgivable mistakes. Thus, I have discovered 33 mistakes in arithmetic and elementary algebra (except those noticed by the Editor) in his *Abhandlungen* (1876). They did not influence his conclusions but they throw doubt on his more serious calculations. Here is just one of them (1876, Bd. 2, p. 376): $\sqrt{4}: \sqrt{5} = 1/1.409$; actually, however, 1/1.118.

One more example, this time concerning Bessel's reasoning (1818; 1838). He presented three series of Bradley's observations, 300, 300 and 470 in number, and stated that their errors almost precisely obeyed normal distributions. Actually, he was wrong and it is difficult to believe that he was mistaken. Moreover, he thus missed the opportunity to discover an example of long series not quite normally distributed errors of precise observations. Later, scientists gradually discovered such series, especially see Newcomb (1886).

Bessel's contribution included a proof of a version of the central limit theorem (rigorously proved only by Liapunov and Markov). Bessel stated that, given more observations, the deviation from normality will disappear. Did not he notice that he thus undermined the essence of that theorem?

I have since discovered other examples of Bessel's misleading statements in his popular writings. True, at least one of them pertains to the time of his fatal illness, but I venture to suppose that a very ill person should all the more try to avoid mistakes.

1. Bessel (1843). This is his report of the same year read out to the physical section of the Königsberg physical-economic society in which he had been very active. Schumacher published the texts of these reports (1848b), and Bessel (1848a), about which I say a few words below, is included in that collection.

And so, Bessel (1843) described the life and work of William Herschel. Among other things, he properly discussed Herschel's hunt for double stars and his attempt at counting the stars in the Milky Way, but he did not explain that there are two types of double stars nor did he say that the Milky Way is only one of the countless galaxies.

Herschel came to understand that his telescope did not penetrate to the boundaries of the sidereal system whereas Bessel (p. 474, left column) stated quite the opposite. Another mistake concerned the discovery of the planet Uranus. Contrary to Bessel's statement (p. 469, left column), Herschel discovered a moving body and decided that it was a comet. In 1810, Gauss made the same mistake [i]. Finally, Bessel (p. 470, right column) mentioned Caroline, the sister of William, and remarked that she was still alive and assisted her brother. Actually, Caroline died several decades later than he.

2. Bessel (1845). This is a newspaper article which had nothing to do with astronomy. Bessel stated that under such parameters as territory, climate etc (political system not mentioned) only mental development of the population determined its acceptable maximal number. However, a territory becomes more or less populated when people turn from hunting to farming (Bessel's own example), but are farmers more mentally developed than hunters?

Then Bessel turned his attention to the United States and provided his own data taken out of thin air and damnably wrong about the population of Native Americans.

3. Bessel (1848a). The date of the report is unknown. Bessel mentioned Delambre's *Astronomy* which was not quite definite, but sufficient for stating that the report was read in 1821 or later.

The significance of Jakob Bernoulli's law of large numbers was not discussed, Lambert's preference of maximum-likelihood estimators over the arithmetic mean (p. 401) was mostly imagined and Laplace's *Essai philosophique* of 1816 was not even mentioned. Population statistics studied, for example, by De Moivre, Nicolaus and Daniel Bernoulli, was completely left out. It is difficult to conclude that Bessel's quite elementary exposition had satisfied his listeners.

In his correspondence, Gauss several times indicated Bessel's shortcomings.

1. G – O, 2 Aug. 1817. Bessel had overestimated the precision of some of his measurements. On 2 Nov. 1817 Olbers *confidentially* informed Bessel about Gauss' opinion.

2. Gauss (G – S, between 14 July and 8 Sept. 1826) stated the same about Bessel's investigation of the precision of the graduation of a limb.

3. Gauss (G – S, 27 Dec. 1846) negatively described some of Bessel's posthumous manuscripts. In one case he was *shocked* by Bessel's *carelessness*.

Recall ([viii, § 15 and Note 22]) that in 1825 Gauss *had harshly fell* on Bessel.

I am at a loss: how was it possible to pass these statements over? And, again, how was it possible for Bessel to be at once a great scholar and a happy-go-lucky scribbler? Cf. Goethe (*Faust*, pt. 1, Sc. 2): *Two souls are living in his breast*.

Bibliography

Bessel F. W. (1818), *Fundamenta astronomiae*. Königsberg. See a fragment in a German translation (Schneider 1988, pp. 277 – 279).

--- (1838), Untersuchung über die Wahrscheinlichkeit der Beobachtungsfehler. In Bessel (1876, Bd. 2, pp. 372 – 391).

--- (1843), Sir William Herschel. Ibidem, Bd. 3, pp. 468 – 478.

--- (1845), Übervölkerung. Ibidem, pp. 387 – 407.

--- (1848a), Über Wahrscheinlichkeitsrechnung. In Bessel (1848b, pp. 387 – 407).

--- (1848b), *Populäre Vorlesungen über wissenschaftliche Gegenstände*. Hamburg. Editor, H. C. Schumacher.

--- (1876), *Abhandlungen*, Bde 1 – 3. Leipzig. Editor, R. Engelmann.

Newcomb S. (1876), A generalized theory of the combination of observations. *Amer. J. Math.*, vol. 8, pp. 343 – 366.

Schneider I., Hrsg (1988), *Die Entwicklung der Wahrscheinlichkeitstheorie von den Anfang bis 1933*. Darmstadt.

Sheynin O. (2000), Bessel: some remarks on his work. *Hist. Scientiarum*, vol. 10, pp. 77 – 83.