

Bessel

It is hardly necessary to mention Bessel's great discoveries. Here, his biography is described, partly by himself, two of his popular writings are included as also a few pages of the translator which document Bessel's happy-go-lucky attitude. Two souls had been living in his breast!

F. W. Bessel

Biography; Two Papers; His Appraisal

Translated by Oscar Sheynin

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Introduction (O. S.)

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 downloadable Document i on my website www.sheynin.de or on its copy at Google, Oscar Sheynin, Home. Such documents are my translations 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 the contribution below.

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

I

F. W. Bessel

Brief Recollections of My Life

Kurze Erinnerungen an Momente meines Lebens.
Abh., Bd. 1. Editor, R. Engelmann. Leipzig, 1876, pp. XI – XXIV¹

[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 Rehme². 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. 1846³).

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 Thilo⁴.

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 δ 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 δ 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 secretly 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 incrustated 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 ivory⁶. 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 meridian⁷. 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 (beiwitem) lower than the actually obtained⁸. 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 directions⁹.

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 possess 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 your contribution should not remain unpublished, 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 work¹¹.

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 mathematics¹². 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 it¹³. 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 at 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 orbits¹⁴. 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 first 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 Lilienthal¹⁵.

[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 me¹⁶. 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 [...] 1806¹⁷ 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 ziehenden 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

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

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

The change in the hour angle due to the change in δ by $\Delta \delta$ and measured in units of time is

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

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 $\Delta \delta$ 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 $5^{\text{h}}5^{\text{m}}26^{\text{s}}.10$, $50^{\text{s}}.08$, $6^{\text{m}}13^{\text{s}}.48$, its yearly change due to $\Delta \delta = 0^{\text{s}}.921$, 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

$$\log_{10}[1/2(\operatorname{cosec} A/2)^2]$$

given the argument $90^\circ - A = 10''(10'')3600''$.

Then Bessel described the treatment of seven of his observations of the solar eclipse of 27 Aug. 1802. Each required about 1 1/2 hours. He concluded by providing the longitudes of some cities with respect to Paris (Mitau = Jelgava):

Berlin

$44^{\text{m}}16^{\text{s}}.5$

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

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II

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' satellites¹ [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 density² 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 stars³.

[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 stars⁴ 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 faultless⁵. 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 Hansen⁶, 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 observation⁷, 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 observations⁸ 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 distances⁹ were larger than those measured by Struve.

This noticeable difference prompted Bessel to a new long series of observations of the double star ρ 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 than $1''^{10}$. 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 ρ 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 Tralles¹⁴. 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 apparatus¹⁶) 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 units¹⁸.

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 kind²⁰.

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 problems²¹.

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 standing²² 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 time 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 affection²³.

[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 Gauss²⁴; 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 Gauss²⁵, 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 Hansen²⁶ certainly surpassed him in the depth and richness of mathematical speculations, perfection and elegance²⁷ 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 versatile²⁸.

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 become 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, 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 [vi, § 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 Bagratuni (1961, p. 14). Note that on p. 19 Bagratuni called the Gauss celebrated formula for the mean square error after Bessel.

Repsold [vi, § 23] mentioned an *Ausdehnungsmesser*, 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 [vii].

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. This seems to have been natural.

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 [v], 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

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III

F. W. Bessel

Letter to Professor Airy at Cambridge¹

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

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

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

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

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

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

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

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

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

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

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

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

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

is composed of two parts, the first of which is common to all the heavenly objects; the second depending upon the place of the star or planet vanishes in the Aequator (!), and is of small amount if the object is near the Aequator. The future correction of this part may be omitted for reasons similar to those alledged (!) for the omission of the correction of Aberration. The first part will be nearly without influence if the Longitude resulting from every group of Observations

is reckoned not from the apparent, but from the mean aequinoctial (!) point.

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

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

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

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

You will perceive, dear Sir, by what I have said, that, were I to superintend the business, I would prefer to exhibit the Results without complicating them by the introduction of a single undetermined quantity. But permit me to add a few words about the method by which a continually increasing approximation to the true values of the Elements of Reduction will be obtained. Some of these Elements have been the subject of repeated inquiries, every-one of which has afforded a new determination somewhat different from others extant.

Proceeding in this manner, and *rightly* combining the results of earlier inquiries with those of a later date, we shall undoubtedly arrive, in some future time, at every desirable degree of approximation.

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

– In the present state of our knowledge of the Elements of Reduction their yet admissible errors are so narrowly limited that further corrections can only be expected from long continued observations made expressly for the purpose. The nearer the approximation is, the more difficult will be a further correction, and the less probable will be the supposition that the Result of every new inquiry will approximate yet more to the truth; – continual oscillations within the limits of unavoidable imperfections are, on the contrary, agreeing with the very nature of Results derived from observations³.

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

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

– The observed mean place of the Planet may be reduced to geocentric Longitude and Latitude and the former related to the mean aequinoctial-point by the subtraction of the Nutation taken always from the same Table which constantly has been employed in the whole computation.

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

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

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

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

Notes

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

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

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

Brief Information about Those Mentioned

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

Olufsen Christian Friedrich Rottboll, 1802 – 1855, astronomer

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IV

F. W. Bessel

On the Calculus of Probability

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

[1] Since I intend to talk to the respected Physical Society¹ about the calculus of probability, I ought to presume such an interest in this subject which will be characteristic of an exception to a rule easily derived by experience: neither a calculus, nor even its result is suitable for an oral presentation. And this is what I really believe in.

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

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

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

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

But still our information about Caesar and those kings are *of the same kind*. Our knowledge only differs in the measure of its strength². It is so precarious concerning the seven kings that we do not dare believe in their existence whereas the information about Caesar is so robust that any doubts seem unreasonable. Strictly speaking, however, his former existence is only *much more* likely than that of the kings. The doubt about him is not more unreasonable than the hope to extract at random one single white ball from an urn also containing many millions black balls. That doubt is therefore not really unreasonable

but only very weak. In ordinary life such doubts are completely ignored, but stronger doubts occur oftener.

So where is the boundary, the level of probability for two events both called certain³? Be it possible to determine this level we will be able to assign correct places to each event and numerically establish which event is *more probable*. In history, however, and in all matters which cannot be reduced to numerical relations, it is difficult to establish the amounts of probability. Historical events can be dated, but no other number denoting their probability can be assigned them.

On the contrary, there exist very many things whose probability can be measured, and I will say something about the means which may be applied for this. The entire theory of probability⁴ rests on what is usually called chance. Will a tossed coin fall on one side or on the other? The outcome, as we say, is the effect of chance.

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

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

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

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

conjectandi, in 1713, paved the way and prompted various later mathematical investigations. The great work of Laplace which appeared some years ago (vor einigen Jahren) combined all of them⁵.

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

On the contrary, a die with three or more faces oftener rests on white than on black and the appearance of white is certainly more probable. For two faces the probability of each outcome is $1/2$; for three faces, the probabilities are $2/3$ for white and $1/3$ for black etc. For a die with 12 faces, 7 of them white and 5, black, those probabilities are $7/12$ and $5/12$. Playing with such a die and gaining 5 talers in case of white, I ought to lose 7 talers in the opposite case. If I pay less, I will probably win; if more, I will probably lose since there is no reason (or at least it is thus assumed in the calculus of probability) for one face to appear rather than the other⁶.

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

So here we have the means to judge precisely the probability or improbability of an event. However, its application usually leads to serious, and often unsurmountable difficulties since we often do not have the data on which our judgement is dependent. As stated above, the probabilities of white and black for a die with 7 white faces and 5 black ones are $7/12$ and $5/12$. Roll such a die many thousand times, and then the ratio of the occurrences of these faces will be 7:5, and the nearer to it the larger is the number of the rolls. When we do not know how many white and black faces the die has [how large is their ratio] it can be derived [from the experiment]. The result will be the more reliable, the more rolls are made. And so, there are two means for discovering the number of faces of a die⁷: either count them, or observe the result of the rolling.

[5] I hope that the respected Physical Society will excuse me for discussing all this somewhat extensively, since it was indeed necessary for stressing the true point of contact of the calculus of probability with occurring events. When considering this in a more general setting, the unknown numbers of white and black faces represent the favourable and unfavourable causes of some event. When counting the occurrences of that event we obtain the number of the cases in which it happened and did not happen. The derived ratio

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

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

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

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

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

[6] Mathematicians⁸ have made a very significant step forward by discovering a means for determining by calculation the reliability with which we may reckon on an event found probable by observation. This reliability obviously heightens with the number of the observed cases. When we roll only a hundred times the abovementioned die with 7 white and 5 black faces we will be much less certain about the possibility that the ratio of white and black outcomes is very near to $7:5$ than after 1000, 10,000 or 100,000 rolls. However, it is possible to calculate how *reliable* is that ratio as derived from a 100, a 1000, ... rolls. This reliability heightens so much that the boundaries of the probable error⁹ will soon become so near to each other that the derived ratio will not noticeably differ from the probability anymore.

Observations, whose reliability is clearly determined, can only be obtained by applying this theory. And only during the last years we have learned how to derive much use from it, and I will hardly be mistaken when supposing that after a sequence of years the first chapter of each textbook on science based upon experience will be devoted to the application of the calculus of probability to the art of observation¹⁰. The data for such applications will surely not be available at once since it is easy to show that much which is today called observation hardly deserves this name. However, new observations require time, often very much time. In medicine, national economy and in similar matters in which the general rule is essentially corrupted by numerous chance events, it will only later be possible to understand reliably what exactly should be obtained for trusting the observed result. Unreliable is much of what in usual life is thought to follow from experience and judged and justified by everyday occurrences but what still is completely wrong. Thus, everyone says that a full moon changes the weather and believes that he personally had made many suitable observations, but there does not exist anything so unjustified than that statement as proved by actual counts covering 50 years¹¹.

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

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

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

All this is revealed by the observations: repeat today's work

tomorrow, and its result will be a bit different, and the day after tomorrow, different once more. At the time of the forefathers of astronomy such differences amounted to half a degree, at Tycho's time, a few minutes. Now, having such aids¹² as those in my observatory, it is possible to reckon with considerable reliability that observations made today and tomorrow will not differ more than by a second.

In spite of such precision, I, just as Tycho, cannot maintain that my observation provides the truth. I am nevertheless looking for the truth, so which of the two observations should I prefer? Obviously, both are equally wrong since there is no reason why one of them should be chosen. We therefore take the mean of all those made, and this rule can be rigorously justified, although the great Lambert had objected to it¹³.

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

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

[8] The answer to this question is offered by the calculus of probability. It teaches us that among the possible uncountable (?) paths we ought to discover the one which has the highest probability. That calculus leaves no room for arbitrariness. Previously, before that theory [that calculus] was developed, the computer had to be satisfied to choose, in accord with his prudence and ability, a path more or less conforming to the observations. Nowadays, he has complete power over choosing quite methodically the *best* path derivable from the observations. Moreover, he will be criticized if not arriving at the very

best to which he could have freely approached.

In the first case, he thus certainly strengthens his reputation for ability, but not when he acts otherwise even if he manages to keep very near to observations. The *astronomer* will thus lose as much as *astronomy* wins, and we should not doubt that, owing to this invention (?), observations acquire quite different weights¹⁴ and astronomy can advance more in *one* year than formerly in a decade.

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

Suppose that someone determined that the orbital period of a comet is 100 years with a probable uncertainty of 1/4 of a year, and that someone else determined that that period was 102 years with a probable error of 1 year. The choice between these determinations is not arbitrary anymore: the first one should unquestionably be preferred¹⁵. For example, among my first applications of such reasoning was my conclusion that the Olbers comet will most probably next appear on the 9th of February 1887 with a probable error of 101 days. The period during which its new occurrence should be expected can thus be immediately estimated.

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

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

We may easily tolerate all that and would have a good reason to thank very much these English reviewers for teaching us how to determine the true cometary orbits etc. Indeed, we ought to be only satisfied with probability when denied the truth. Nothing else has been done nor could have been done. Yes, we have often called true what was only probable and even not the most highly probable. But, on the other hand, no one ever thought of proving the Pythagorean proposition by probabilities since it can be, and was proved certainly.

I have somewhat extensively dealt with the application of that (?) reasoning to astronomy but would have rather considered other

sciences more closely connected with everyday life. However, those sciences are not yet completely cultivated¹⁷, and, in addition, I myself am too little informed about other things and do not venture into any such investigation. Nevertheless, any person tending to contemplate will have sufficient possibilities to note that what I said about astronomy was only an example and that the same, even if in another form, is true everywhere else.

Each science which passes from experience to theory begins with observations and learns from the calculus of probability how to observe and apply the observations and, finally, how to construct *the most probable* theory. In astronomy, for example, practice is a problem of that calculus, and theory, a problem of higher mechanics¹⁸. 150 years previously it was different, no one thought of either, but what science had amounted to in those times as compared with today? A chaos of phenomena¹⁹, whereas nowadays they comprise a coherent whole whose separate parts are most closely connected by the mentioned (?) strong ties.

It is indeed instructive to consider the course which science had taken until our time. It did not at all arrive at knowledge by issuing from prior systems as it possibly was attempted in other fields²⁰. On the contrary, it had invariably asked the observations for advice and was always on guard against admitting something not following from them into its propositions. And it certainly had arrived at its aims not by leaps but by slowest and surest steps. I wish all experimental sciences to proceed by such thoughtful steps, and I hope that the calculus of probability will soon provide them such an audible proper rhythm that any deviation from the proper course will offend both eye and ear.

Notes

1. Bessel actively participated in the work of the Physical section of the Königsberg Physical – Economic Society.
2. Bessel did not mention moral certainty which was discussed by Jakob Bernoulli but introduced into science much earlier (Sheynin 2009, §§ 2.1.2, 2.2.2 and 3.2.2).
3. One of those events was apparently certain, but the other one only probable.
4. Only here did Bessel mention the *theory of probability*, in all other cases it was *calculus of probability*.
5. *Some years ago*: not less than nine (see Note 16). Then, Laplace had included the theory of probability into applied mathematics whereas his predecessors had regarded it as a branch of pure science. Again, Bessel had not mentioned Laplace's *Essai philosophique ...* of 1814.
6. Only assumed as the very first approximation and even so, not always. Then, in information theory, probability 1/2 (see below) means complete ignorance.
7. Not the number of the faces, but the appropriate ratio.
8. Yes, mathematicians, beginning with Jakob Bernoulli, and for any chance event rather than for probable events.
9. Bessel only defined the probable error in § 7. In 1816, he himself introduced it into probability theory.
10. Bessel invariably mentions the calculus of probability instead of the theory of errors. However, unlike Laplace or Gauss, he himself (1820, p. 166) picked up that second term from Lambert.
11. Those *actual counts* are extremely dubious, see Sheynin (1984, § 2) to which I am now adding that Lambert had studied that problem and Daniel Bernoulli urged him to go on with his investigation (Radelet de Grave et al 1979, p. 62). Bernoulli remarked that the possible influence of the Moon on the atmosphere can be revealed since its distance varies if only it influences the air the same way as the sea.

However, the elasticity of the air and its insignificant inertia ought to be allowed for.

12. By aids Bessel meant astronomical instruments.

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

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

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

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

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

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

19. How about Kepler?

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

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F. W. Bessel

**On measures and weights in general
and on the Prussian measure of length in particular¹.**

Über Maß und Gewicht im allgemeinen
und das Preußische Längenmaß in besonderen.

In author's *Populäre Vorlesungen über wissenschaftliche Gegenstände*.
Hrsg. H. C. Schumacher. Hamburg, 1848, pp. 269 – 325.

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

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

Many of such local measures had certainly disappeared with the expansion of those ties but the great number of the remaining can be estimated by the comparison of the Italian measures of the foot in the *Annuaire* of the French Bureau des Longitudes for 1832. It took into account, not completely, the measures applied in field measurements, but not in commerce, and still, 215 measures were listed².

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

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

That process apparently went on gradually with the introduction of separate generally valid regulations about, for example, the levying of taxes. The final goal, a complete unification of the measures in all parts of a country, was already attained in most of the large European

countries whereas the other ones have been approaching it.

During its revolutionary years, France had even attempted to introduce a single measure for all the civilized nations. The intended success was not achieved but some neighbouring countries had adopted the French measure³.

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

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

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

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

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

And so, three measures are needed, those of length, of liquids and grain, and weights. Their embodiments are necessary and serve as the base for establishing any system of measures which becomes quite definite when those embodiments exclude any ambiguity, becomes invariable when resisting all the influences of time. Then it conforms to its intention the better the more accessible are its initial units.

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

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

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

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

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

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

the toise of Peru and for removing the causes of its damage. Until now, both aims have been attained.

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

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

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

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

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

And still the intended aim was not yet reached since an investigation of that yard, legally elevated to become its initial

measure, made in 1834 by Baily, revealed that an unambiguous length cannot be got from it since both points [pins] determining it were not rounded nor did they have any other regular form, but were irregular to the highest extent.

This fact was explained not by their initial condition but by the damage of the points by various use made without proper precaution. The ensuing uncertainty was obviously not large enough for preventing applications of that standard in everyday life, but I have noted above that scientific use requires complete definiteness rather than a restriction of uncertainty within narrow bounds⁵.

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

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

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

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

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

In a similar way, the multiples and the fractional parts of the are, ster, litre and gram were named. The monetary unit, the franc, weighed 5 gram, 9/10th of it silver and 1/10th, copper, was divided into

decimes and centimes. The day was just as well subjected to the decimal system: it had 10 hours, 100 minutes per hour, of 100 seconds each. The quadrant of a circle was divided into 100 grads, a grad contained 100 minutes of 100 seconds each. Even the calendar did not resist the revolution: it began with the vernal equinox and was divided into 12 months, each 30 days long, and 5 or 6 additional days⁶.

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

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

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

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

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

[5] *Each* measure is obviously equally easy and reliably applied for measurements since it only serves for establishing the *ratio* of two magnitudes of the same kind. It does not acquire any advantage even when the ratio of the measured distance between two points on the surface of the Earth to the length of the entire quadrant of the meridian

is expressed by a decimal fraction and becomes directly known.

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

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

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

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

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

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

A quadrant of a meridian was preferred to the length of a pendulum since the latter's interpretation depends on time (on the period of oscillation of the pendulum) whereas the former is a measure of length without any further connections [complications]. The thus chosen measure becomes definite after a certain meridian is named; this is necessary since we are not convinced in that all meridians of the Earth are identical whereas the new meridian arc measurements decisively resist this assumption⁷.

So which meridian we may choose not only as a measure, but as a natural measure? This can only be decided by measurements, but we never obtain any magnitude by measuring or observing it, we only get to know it approximately. Therefore, measurement does not ensure the fulfilment of even the first requirement demanded from a measure, – the exclusion of any uncertainty.

[6] When introducing a certain measure corresponding to the results of measurement, – when introducing an embodiment of those results, – and adopting it for further usage, we thus sacrifice the natural measure. We will only get hold of a natural measure by measurement after learning how thus to reach a completely definite result. This, however, is impossible since each improvement of the methods of measurement only brings about a better approximation; imperfect possibilities of our senses will never lead to perfection⁸.

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

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

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

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

The other cause manifested itself during measurement of the length of the pendulum in Königsberg. It occurred that the previously applied theory of the influence of the surrounding air overlooked an essential circumstance so that it was decided that that influence was twice less than actually⁹.

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

Bearing in mind these remarks, it is easy to imagine the consequences of an immediate adoption by some country of the Huygens proposal to choose the length of a simple seconds pendulum

as the unit of measure and to base on it a decisive revolution of its system of measures. That length would have been measured as perfectly as the art and aids of the time allowed it, and the derived magnitude fixed as the measure. Not long afterwards, after the discovery that the length of the pendulum increases with the distance of the place of observation from the equator (by about $2\frac{1}{4}$ lines after moving from the equator to a pole)¹⁰, it will be noted that a measurement was only valid for that place and that the established measure did not possess the previously attributed property of being independent from the place of measurement.

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

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

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

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

The latest arc measurement in East Prussia made it probable that the actual figure of the Earth is to a regular surface approximately as the irregular surface of flowing water is to the surface of an even and calm water. Separate deviations are therefore small and perhaps not exceed a few miles¹¹.

This nature of the figure of the Earth means that an arc measurement can only determine the curvature of a place of a body

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

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

[8] The introduction of the meter led to the measurement of a great arc of the Paris meridian from Formentera¹² to Dunkerque more than 1/8 of its quadrant. This arc was advantageously situated: its middle latitude was almost 45° so that the derived length of a degree was very near to the eighth of the length of a mean degree or to 1/90 of the quadrant and almost independent from the flattening of the Earth. Its length was 57008.22 toises. Multiplied by 90, it provided the length of the quadrant and of the meter: 1/10,000,000th of that was consequently 3 feet 11.296 lines, or 443.296 lines of the toise of Peru. This length was declared the legal length of the meter. A platinum bar was produced for its embodiment; at the temperature of melting ice its end surfaces should have marked that length, the length of the declared standard meter.

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

At present, we already have ten arc measurements, and all of them have an equal right in deriving the size and figure of the Earth¹³. I have found out their most probable result: the mean degree of the quadrant of a meridian is 57011.453 toises, about 3 1/4 toises longer than legally established. It follows that the length of an entire quadrant which we ought to regard now as its most probable value is not 10,000,000, but 10,000,000 and 565 metres. Its unavoidable variation, when keeping to the initial definition, i. e., to the meter being a 1/10,000,000th part of a quadrant, will lead to internal contradiction: the fraction whose denominator differs from its numerator will still be unity. We should therefore abandon the initial definition and assume that the meter is established not by the length of a quadrant, but by its ratio to the toise. For a quadrant to be once more 10,000,000 metres long, the meter ought to be lengthened by 1/40 of the line.

However, for the new value of the meter to attain a weight greater than it had initially, we will have to sacrifice the unsuccessful idea of a natural measure. Indeed, it is impossible to doubt that each future arc measurement will again lead to another value of the meter. The uncertainty remaining in its length after all the now existing arc measurements are coordinated is about the same as that, which follows from the change of the previous definition of the meter. It will decrease with the increase of the number of those measurements but

no increase will be sufficient for it to disappear.

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

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

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

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

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

[10] For introducing a *real* natural measure we ought to refer anyone who requires a true measure not to its embodiment, but to nature itself. However, apart from today's impossibility of following this advice, its unavoidable consequence is that differing measures will become the bases for each new measurement and the errors of the measurement proper will be combined with the variations in the derivation of the measure. When desiring to illustrate this conclusion by a definite example of a measure only defined by its relation to a quadrant of a meridian rather than by its embodiment, we may imagine, for example, two measurements of the length of a simple seconds pendulum, one made at the time when the meter was

introduced, and the other one, nowadays. Even if they completely coincide, they actually still essentially differ by about 1/40 of the line. And later measurements, when the most probable length of a quadrant will be different again, and when complete coincidence still takes place, the length of the seconds pendulum will ever differ.

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

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

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

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

But still, those regulations require weighing by an embodying weight which is incomparably more expedient than referring to the explanation which accords with the business at hand not better than the introduced meter accorded with a quadrant. Neither do I doubt that, when, for example, a repeated and very precise weighing of a given volume of densest water provides a weight *differing* from the embodied weight, the appearing doubt will be resolved by preferring the latter¹⁴. In this case an interpretation referring to volume and water will be useless since anyway both interpretations more or less essentially contradict each other.

On this occasion I remark that the weighing of a given volume of densest water with a relative reliability of 1/10,000 is not at all easy and is probably not yet attained¹⁵. In addition, concerning the use of water it is possible to make a remark similar to that stated above about the introduction of a length provided by nature for measuring lengths. And a restoration of a lost weight is as easy and reliable when turning to volume and water as it is when arbitrarily (?) weighing it by water.

Before I finally leave the problem of natural measure, I ought to say

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

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

[11] The derivation of the length of a terrestrial arc first requires a measurement of a line [of a base] on the surface of the Earth which is the only operation in which a measure of length itself is applied. That line becomes a side of a triangle whose angles are measured by proper instruments and the other sides of the triangle are trigonometrically calculated. A second triangle is adjoined to the first one and its elements become known in a similar way, then a third triangle is added etc¹⁶.

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

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

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

Most reliable the measure is again derived as long as the end points of the base are still preserved so that it can be measured anew. Less reliable, when those points have disappeared and we are therefore compelled to measure once more (?) another side of the triangulation. Indeed, in this case the uncertainty of the angle measurements are added to that of the base measurement. Still less reliable, when every point of the triangulation has disappeared and only the computed length of a degree is left. The uncertainty of the astronomical observations is then also added. Although becoming ever smaller, it will always remain larger than the uncertainty of the other measurements¹⁷. Finally, least reliable, when only the length of a

quadrant is left since it is only derived, and will be derived, from a combination of various arc measurements under the presumption of a regular figure of the meridian which is known to be only approximately true.

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

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

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

Finally, I regard essential that the establishment of a measure be accompanied by discovering means for producing its copies as perfectly and as easily as possible¹⁸. The fulfilment of these three requirements by each established measure with superb rigour, especially in the case of the measures of length and weight should be achieved if the art of investigating measures, without restricting it just to everyday needs, is to be put in order and preserved.

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

The length of 3 foot and its division into 36 inches and the division of the last inch into 12 lines was marked on that bar by strokes. Two of them, located on one of the wide sides of the bar, perpendicularly intersected two parallel lines about 0.4 lines apart which ran along the entire length of the bar. The strokes marking inches were silver pins and those marking lines were on inlaid plates. The bar and its three

copies to be preserved at appropriate places were produced by Pistor.

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

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

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

That influence on the bar with the same properties as our has, is so great that a playing card inserted between it and the plane on which it lies can already change the distance between its extreme strokes by many thousandths of a line. Even the bending caused by the bar's own weight when it rests on two points essentially changes this distance. My calculations showed that, when the bar rests on its ends, it shortens by $\frac{6}{12}$ thousandths of a line; that this shortening becomes smaller as the distance between the ends of the bar and its supports lengthened; and that the shortening disappears and becomes a lengthening when that distance is $\frac{7}{4}$ inches¹⁹.

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

Such uncertainty is peculiar to any similarly produced standard. It can be avoided if the definition of a measure depends on the distance not between two points or strokes marked on its surface, but between its end planes. It will then not be difficult to produce such a rigid bar that neither its own weight nor an unintentionally preserved bending will actually change the distance between its end planes as measured

along its unchanged middle line.

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

These advantages of an endpoint measure leave no doubt in that the still necessary definite establishment of the Prussian foot should be attempted on such grounds rather than by a later assertion concerning a measure restricted by strokes. And it is necessary to continue to follow the legal intention of having the foot equal to 139.13 lines of the French foot²⁰.

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

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

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

Great could have been the caution exercised in producing that measure, but it can be essentially increased during measurement. It is necessary to compare repeatedly and as precisely as possible the length of the bar expressed in the French measure with that standard. A series of such measurements showed that the bar was 417.38939 French lines long, by 0.00061 of those lines or by 0.00063 Prussian lines shorter than intended. Actually, it is really indifferent whether to choose the established value of the Prussian foot or the still unknown value which will be a few ten thousandths shorter or longer. The length of the bar can therefore be declared exactly equal to 3 Prussian foot.

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

The Standard of the Prussian Unit of Length, 1837

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

The Royal Act of 10 March 1839 recognized it as the only one possessing that property²¹. And thus the Prussian foot was declared definitely and unambiguously. In accord with the above, its ratio to the French foot is

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

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

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

The determination of the ratio of the two measures can be considered satisfactory indeed, but we must not forget that the applied French measure was not the toise of Peru, but its copy produced by Fortin in Paris and owned by the Königsberg observatory. Arago and Zahrtmann compared it with its original after which it acquired the greatest possible authenticity. The same length represented by that copy of the toise had been the basis for the measurement of the length of the pendulum in Königsberg, Gùldenstern²² and Berlin as also for the arc measurement in Eastern Prussia [No. 322/135].

Two more equally authentic copies of the toise of Peru are kept in the rich collection of instruments of the state councillor Schumacher in Altona. I have compared them with the previously mentioned by

means of the same Baumann appliance and found out that one of them also produced by Fortin was 0.0025 of a line longer, and the other one produced by Gambey 0.0049 of a line shorter.

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

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

An authentic copy of the Prussian measure ought to be a bar of lithe cast steel of which that measure was produced as well. Both have the same thickness and the same or almost the same length. Instead of the sapphire end planes fastened to the measure a copy is fitted with end planes of tempered steel²³. After being firmly attached to the bar, they are ground and polished smoothly and are exactly perpendicular to the bar's axis. To prevent dust and rusting these end planes are covered by brass cylindrical caps pushed on the cylindrical ends of the bar which can be screwed or unscrewed from it.

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

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

This inscription will make it an authentic copy of the Prussian measure. For officially recognizing this fact it will be necessary to apply to the Royal Commission on Standards in Berlin and submit the original comparison as stated on the inscription on the bar. The price of authentication is 60 Prussian talers.

[16] For estimating the advantage promised by these rules I ought to go into some detail about the comparison of a copy with the standard. It is done by means of an appliance equipped with two very delicate micrometers fixed on a mahogany prop together with a

Repold water level-probe²⁴. The standard and the copy are brought in turn between those micrometers. Both bars are laid side by side on a trolley which can only move perpendicular to the line of the micrometers and only between two points, when the axis of either bar is brought on that line. The movement is stopped by a shock against the edges of the two screws each of which is situated in the intended position at each placement of the bars.

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

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

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

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

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

In my opinion, it should impede an unfavourable influence of negligence or carelessness, and I had therefore thought of abandoning the immersion of the bars in a liquid and of discovering another means. Obviously, it is now essential to produce copies of the same material and size as the standard and to process them the same way. Failing that, it will be impossible to keep both bars incessantly at the same temperature in spite of external disturbances and the never ceasing fluctuations of the temperature of the surrounding air.

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

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

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

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

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

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

[18] After, owing to their proper construction and ordered efforts,

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

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

Notes

1. Bessel also published a paper of the same name [No. 344].
2. Italy only became a single country in 1870, but 215 measures of the foot, even 40 years previously, is difficult to imagine.
3. The goal of an international metric convention signed in Paris in 1875 was to ensure a unity of measures and to develop the metric system.
4. The yard was subdivided into 36 inches and its $1/900^{\text{th}}$ part approximately equalled 1 *mm*.
5. But can complete definiteness be ever attained?
6. Instead of a simple indication of a leap year the new definition was not clearly connected with the year's number. The calendar was thus deprived of its decisive advantage. However, the new calendar was soon abandoned. F. W. B. Abandoned in France and never introduced elsewhere. O. S.
7. Bessel [No.254/138] briefly mentioned Gauss' pertinent reasoning.
8. There are other causes as well, for example, the influence of external conditions, unavoidable in spite of Bessel's statement to the contrary a bit below.
9. See Bessel [No. 254/138].
10. First noticed by Richer in 1672, but Huygens hardly did not understand that that should be expected.
11. Zakatov (1950, § 49) indicated that in Helmert's opinion there are no general deviations of the geoid from a rotational ellipsoid, that somewhat earlier F. A. Sludsky had formulated a contrary statement, and that, finally, the existence of *great waves* of the geoid has been proved. It was J. B. Listing who only introduced the term *geoid* in 1873.
12. A city in an island in the Mediterranean Sea.

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

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

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

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

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

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

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

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

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

18. This requirement seems self-contradictory.

19. See [No. 317/119].

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

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

22. Gldenstein, a castle near Oldenburg in Holstein, Denmark.

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

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

Brief Information about Those Mentioned

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

Baily Francis, 1774 – 1844, astronomer

Bird John, 1709 – 1776, astronomer, constructor of instruments
 Borda Jean Charle, 1733 – 1799, physicist, geodesist
 Everest Sir George, 1790 – 1866, geodesist, geographer
 Fortin Jean Nicolas, 1750 – 1831, constructor of instruments
 Gambey Henri-Prudence, 1787 – 1847, inventor, manufacturer of
 precise instruments
 Kater Henry, 1777 – 1835, physicist, metrologist, astronomer
 Lambton William, died in 1823, geodesist
 Listing Johann Benedict, 1808 – 1882, mathematician, physicist
 Mudge William, 1762 – 1820, geodesist
 Pistor Carl Philipp Heinrich, 1778 – 1847, mechanician, inventor
 Repsold Adolf, 1806 – 1871, constructor of instruments
 Richer Jean, 1630 – 1696, astronomer
 Roy William, 1726 – 1790, geodesist
 Sludsky Fedor Alekseevich, 1841 – 1897, mechanician, geodesist

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VI

Joh. A. Repsold

Friedrich Wilhelm Bessel

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[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 written shortly before his death and first published in his correspondence with Olbers (Erman 1852, pp. IX – XXX).

[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, Since Dec. 1804, through Olbers' mediation, 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 praiseworthy known Gauss¹ 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 Correspondenz* (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 Schröter² 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 him³ (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 Gefken⁴ 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 death⁵. 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 Amalie⁶, 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 education⁷ 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 him⁸, 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 Piazzini, has a level and an excellently fastened Bleifaden. No verniers, but an external micrometer with which angles can be measured unbelievably 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 times⁹, 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. 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 found 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 antidote¹⁰ 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 Göttingen whereas Gauss, in the morning of that same day went to Lauenburg. 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 what 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 am¹¹ 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 life¹²; 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 and 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, 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 theory¹³.

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 possible¹⁴.

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 patten 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] 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 axis¹⁵ 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*¹⁶ 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 pendulum¹⁷ 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 observations¹⁸, 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. 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 this¹⁹. (See [ii, § 4].)

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 Schumacher²⁰ 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 correspondence²¹. 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 begin²², 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*²³.

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*²⁴. *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 Munich²⁶ 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 [Mariánské-Lázně, 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 resistance²⁷), 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]. 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 observations²⁸, 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 G黚denstein, 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 K6nigsberg, 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 it²⁹, 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 another³⁰.

After protracted negotiations for and against Bessel's visit to Gldenstein 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*³¹.

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]³².

[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*³³.

In November 1834 the Bessel family happily celebrated the wedding of his eldest daughter Marie who married the Berlin professor Adolf Erman³⁴.

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 ready³⁵.

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 observations³⁶. 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 $3 \cdot 139.13035$ lines [1 line = 1/10 or 1/12 of an inch] as compared with the demanded length of $3 \cdot 139.13$

lines³⁷.

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 conviction³⁸.

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 rod³⁹, 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 remark?). 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 ii, Note 16] 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 comet⁴⁰, 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 months⁴¹:

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-willed⁴².

[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 5½ 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.

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.

[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 thalers⁴³. 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 festivities⁴⁴. 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 ones⁴⁵.

Then followed considerations about the bending of the limb [of the sector? (Teilkreis)].

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.* 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*⁴⁶. 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)⁴⁷. *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 clocks⁴⁸ 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. After 1801 Gauss became one of the first if not the very first mathematician in

the whole world. O. S.

2. 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 freely 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 Sternaussmesser 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 Schröter 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 Schrader 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.

3. No explanation provided. O. S.

4. Gefken was mentioned in Note 2. O. S.

5. Bessel distinctly saw everything situated at distances of 10 inches and farther [No. 82/17]. J. A. R.

6. Amalie's letters show that in common parlance Bessel was called *Fritz*. J. A. R.
7. 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. O. S.
8. Bessel apparently visited his parents as well. O. S.
9. 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.
10. So rabies had been somehow prevented even before Pasteur. O. S.
11. According to Bessel's still preserved passport dated 10 April 1810, his *height* was 1.68 m. J. A. R.
12. Bessel [No. 378/184] hoped that the Jews will be soon granted full civil rights. Did his hope square with the views of the Catholic or protestant Church? O. S.
13. 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*. O. S.
14. At that time Bessel was 37 years old. O. S.
15. The Earth's axis of rotation is inclined by $65\frac{1}{2}$ degrees to the plane of the ecliptic and describes a cone whose axis is perpendicular to that plane (Blazko 1947, p. 118). O. S.
16. Here and many times below the author grammatically changed the quoted passages. Bessel certainly did not use the third person when writing about himself. O. S.
17. The author several times mentions the seconds pendulum without specifying the appropriate latitude. O. S.
18. I can only refer to Bessel's contribution [No. 344]. O. S.
19. 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.
20. Schumacher lived in Altona (now, a district of Hamburg). O. S.
21. 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. O. S.
22. A similar statement was contained in a letter B – S of 12 May 1825. O. S.
23. Elsewhere Repsold (1918, pp. 24 – 25) quoted a similar letter B – S of 1828. O. S.
24. 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. O. S.
25. This contradicts the previous Note. O. S.
26. 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.
27. See [No. 254/138]. O. S.

28. Later Bessel (B – G, 1 Nov. 1845) stated that his catalogue will be useful up to 1850. O. S.
29. See Encke's opposite wish in § 15. O. S.
30. 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. O. S.
31. 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. O. S.
32. Why was it necessary to repeat the measurement of the base? O. S.
33. 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). O. S.
34. In 1832 Bessel published a paper [No. 261/83] describing Erman's scientific journey to Siberia and Kamchatka. O. S.
35. This is not altogether correct, see end of § 11. O. S.
36. On the use of the comparator see end of § 22. O. S.
37. The number of the significant digits is doubtful. O. S.
38. 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 [ii, § 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.
39. The author did not use this term previously. O. S.
40. Three comets rather than one were named after Pons. O. S.
41. Where are these letters? O. S.
42. This explanation seems hollow. O. S.
43. 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. O. S.
44. 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. O. S.
45. 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.
46. This is repeated from § 28. O. S.
47. Bessel fell ill in December, and on 2 December he was still ill! O. S.
48. I was unable to ascertain this information. O. S.

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

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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 for 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 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 45^{th} 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 stunted 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 δ *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 concurrence – 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.

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).

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.

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.

VII

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 ([vi, § 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*.

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