

ASTRONOMISCHE NACHRICHTEN.

N^o. 84.

Schreiben des Herrn Professors und Ritters *Bessel* an den Herausgeber.

Königsberg 1825. August 1.

When I had the pleasure of being with you in Alton, you shewed me those numbers of the Philosophical Magazine, which contain a severe critique on the Greenwich Observations for the year 1821. This censure surprized me, as I have always been accustomed to consider the collection of Observations made at the Greenwich Observatory as peculiarly valuable, and as a rich store of Astronomical truths: you likewise were of the same opinion, and after reading both papers (vol. 64 Page 367 and 451) our views, respecting the small importance of the errors found fault with, agreed so completely, that they were in truth one and the same.

You now call on me for an elucidation of the subject of this controversy for the readers of the „Astronomischen Nachrichten“. To those who know the Greenwich Observations, and have seen the attack made on them, every explanation will be superfluous; but as it may be presumed that the latter will come into the hands of other readers — also into the hands of some who are not astronomical readers — I willingly comply with your request, and shall endeavour to state the view, which we in common entertain, of this subject. With you, I feel that a call is made to secure as far as possible, that this censure of an Institution to which astronomy is indebted for much in its progress, may not be regarded as more important than it is. That it cannot be very important, appears already from the fact, that Mr. *Olufsen* has found no difficulty, to calculate, from the Greenwich Observations for 1822, the declinations of the Fundamental stars; an account of which is contained in the „Astronomischen Nachrichten.“

The greater number of the errors animadverted on by the writer in the Philosophical Magazine are accidental errors in writing down the observations. Errors of this nature are certainly not pleasant, and it would be desirable if they could be entirely avoided, but, as every

collection of observations which we have hitherto, contains such errors, it would appear that they are unavoidable; and nothing remains, but either to correct them before they are sent to the press and while they are being printed, or to leave the correction to the person who afterwards calculates the observations. I have preferred the first method in my observations; that is, errors in writing of whole minutes in time or degrees, and in general all such as admit of being corrected in an unquestionable way, I have rectified when they occurred without farther remark. Still some may prefer an exact impression of the original observations as they are made in the Observatory, in as much as the reader thereby receives all precisely as it is put down, and can judge for himself, how far a conjecture, to be made on account of any error, is admissible or not. Mr. *Pond* has preferred this last method; for it is not to be supposed that he did not perceive part of the errors, as he is himself accustomed to calculate the most of his observations, and to furnish the catalogues of the stars founded on the calculations, for the same volumes which contain the observations themselves.

The first class of errors mentioned in the Philosophical Magazine, contains those instances in which the Mean taken from the indications of the two Microscopes A and B does not coincide with the column in which this Mean is given. As in all these instances there is an obvious oversight, it may sometimes be difficult to decide where this exists, whether in the columns A and B, or in that of the Mean; still the comparison of the immediately preceding or subsequent observations of the same star, will in most cases instantly shew which of the three numbers is given incorrectly; this is the case in most of the nine examples produced in the Philosophical Magazine.

The second class contains differences between statements which are repeated of the same Observation.

These are slips of the pen which have crept into the copy of the observations sent to the press, but not errors in reading off from the Microscope itself. In most cases these errors may be corrected by comparing both instruments, as happens in several of the instances cited in the *Philosophical Magazine*; sometimes they occur in whole degrees, or in decades of minutes and are then, as to what is essential, of no consequence; for example, in the observation of Procyon on the 23^d Febr 1821 and in β Cephei of the 8th Decbr. where the polar distances are incorrectly written to the amount of 30° and 5°.

Of a different nature are the discrepancies which the writer in the *Philosophical Magazine* ranks under the other classes. The sixth class contains the intervals of the wires of the Transit as they are derived from different observations of the same star. Frequently the differences here arise from errors of the pen; for example, in Capella on the 7th February, where the fourth wire is incorrectly written to the amount of 5"; in Sirius on the 5th Febr. where the last wire is incorrectly written to the amount of 40"; frequently they are the consequence of accidental errors in observing. In the former case they are of no sort of importance, since the truth presents itself at the first glance; in the other the observation is certainly not accurate, but this lies in the nature of all observations, and it were ridiculous should one require an astronomer to perform what cannot be performed. All collections of observations furnish inaccuracies of this sort, and should one be made known which was exempt from them, the falsification of the statements contained in it, might be maintained with confidence. The accuracy of the astronomer is discovered not in the full agreement of the tenth parts of the seconds but in the magnitude of their mean or probable error. That this far exceeds the limit assigned by the means possessed at Greenwich, it might be difficult for the reviewer to prove.

The errors of the fifth class, which contains the differences of the polar distances observed with 2 and 6 Microscopes, appear to me to be censured quite without reason; they are either unimportant errors of the pen (as in γ Draconis 28 Jan.) or accidental and small errors in observing, mixed with the variations in the position of the stars, and with those of refraction; or lastly variations in the polar point of the instrument. For the last of these the observer cannot be responsible; had the reviewer pointed out a new method of erecting the in-

struments, to that they might undergo no variation, he would have deserved the thanks of astronomers; but all experience hitherto shews, that every precaution in securing the pillars does not protect absolutely but only to a certain degree. The Erection of the instruments at Greenwich, has often, and through a long period shewn itself to be excellent, at other times it has not been free from variation, as may be seen from the Table in Nr. 73 of the „*Astronomischen Nachrichten*," which contains the polar points of the instrument. The differences between the last days of July and the first of August 1821 proceed from a cause of this sort, and cannot possibly be considered as accidental errors in observing; and, let me add, they are not to any extent injurious when they can be determined by a series of observations of the polar star, so complete as that at Greenwich. The accidental irregularities of the polar distances which remain after the necessary regard to the variations of the polar point, cannot, any more than those of the intervals of the wires, be alleged as any proof against the accuracy of the Greenwich observer; as appears from the inquiry in Nr. 73 of the „*Astronomischen Nachrichten*."

The fourth class contains the differences between the times of transits observed with the Transit instrument, and with the circle. But as the last instrument from its construction neither is used nor can be used as a Transit, so it would have possessed no interest, although such observations, as might serve to the accurate ascertainment of its position to the meridian, had been made with the utmost completeness. That here and there the time of a Transit has been given, is to be thankfully acknowledged, as we are thereby assured, that the plane of the instrument never differs from the meridian, so far, that on that account an error of the polar distances could arise; but these specifications have no farther aim. Neither *Bradley* nor *Maskelyne* ever mention the times of transits observed with the Mural Quadrant, although this instrument, for the preservation of its plane, gives less security than the meridian circle. No intelligent astronomer can find fault, that the axis of the latter is not constantly rectified, to bring its plane still nearer to the Meridian; and it would certainly be no improvement of the Greenwich observations, were they, in this respect, arranged conformably to the opinion of the reviewer.

Other errors which are noticed, such as that here and there the name of a star, the hour or minute of its transit etc. are incorrectly written, are of no essential

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importance; besides how difficult it is entirely to avoid such errors will appear from the fact that I have remarked 1400 of them in *Bradley's* observations. The remark that the observations in the Greenwich observatory are usually concluded about midnight, will then only have any weight when it has been shewn that something essential was thereby neglected, which, as far as I can judge, is not the case. The observations in the Greenwich observatory relate chiefly to the sun, the fundamental stars, the moon and oppositions of the planets; it is easy to convince ones self that these possess an unusual completeness. Had the writer in the *Philosophical Magazine* pointed out other observations, which, without injuring those now mentioned, might be united with them, he would undoubtedly have made an agreeable communication to the *Astronomer Royal*. Every observation cannot be made in one observatory, and if our wishes are fulfilled in one respect, they necessarily remain unsatisfied in others; nor can observations without a plan have any interest, and I think the reviewer himself will not wish for them when he attends more closely to the consequences of such a method. He who is not yet content with the great copiousness of the Greenwich observations, would do well himself to try to conduct others more numerous; he will then at least acknowledge that the pains and perseverance which are required to perform

as much as is done at present, is all; and more than may be fairly expected from a single observatory.

The third class, of errors which relate to the indications of the Meteorological Instruments, I have as yet not mentioned; because I am likewise of opinion that it is here essential to observe the same accuracy, which is attempted to be attained in the statements of the astronomical instruments. But not only the accurate reading off from the Barometer and Thermometer is required, but it is as necessary, and even more necessary, to determine accurately the errors of these instruments; the truth of this remark may be perceived from this, that several Thermometers which were represented as good and accurate; exhibited errors of $1^{\circ},5$: when they were examined according to the method which I have given in the VIII division of my observations. Errors of this magnitude, would be sufficient to remove the very small difference, which still exists between the declinations of the fundamental stars, as observed in Greenwich and Königsberg (*Astr. Nachr.* Nr. 73.). Should I therefore have any thing farther to desire from the Greenwich observations, it would be, satisfactory information respecting the meteorological instruments, and the place where the exterior Thermometer is suspended.

B e s s e l.

Auszug aus einem Schreiben des Herrn Professors *Encke* an den Herausgeber.
Seeberg 1825. August 21.

Von dem *Ponsschen* Cometen habe ich leider nur eine einzige Beobachtung erhalten können. Im July haben eifrige Nachforschungen mir nichts gezeigt, vielleicht weil ich ein zu schwaches Fernrohr angewandt habe. Indessen glaubte ich ein stärkeres nicht nehmen zu müssen, weil nach den bisherigen Erfahrungen Gegenstände, die für das gebrauchte Fernrohr zu schwach sind, überhaupt auch nicht beobachtungsfähig seyn mögen. Im August war und ist das Wetter so sehr ungünstig, daß ich erst heute wieder einige Hoffnung habe. Der Comet war übrigens am 15ten August so lichtstark und bestimmt in seiner Form, daß er sich vortreflich beobachten liefs, und

ich die erhaltene Position auf $\pm 20''$ wenigstens für sicher halte.

	Seeb. Zeit.	AR.	δ
	$\overset{h}{\text{Aug. 15}} \quad \overset{m}{14} \quad \overset{s}{26} \quad 58''$	$\overset{^{\circ}}{106} \quad \overset{'}{29} \quad \overset{''}{38},7$	$\overset{^{\circ}}{30} \quad \overset{'}{51} \quad \overset{''}{56},5$
der Fehler der Ephemeride ist	$- 1 \, 45,2$		$+ 49,1$

Beide Fehler deuten darauf hin, wenn man anders die übrigen Elemente als sicher genug annehmen darf, daß das Perihel noch um etwa 0,025 Tag früher angesetzt werden muß. Für die Zulässigkeit einer Tangentialkraft, wird dieser Durchgang in Verbindung mit 1819 und 1822 allein doch nicht viel entscheiden können.

J. F. E n c k e.