

Ueber die Veränderlichkeit des Sternes DM. +21°2140.

Von Herrn Dr. E. Becker.

In dem über die Beobachtung der Zone +20° bis +25° Decl. erstatteten Bericht (Vierteljahrsschrift der Astr. Ges. 16. Jahrg. 4. Heft) habe ich angeführt, dass der Stern DM. 21°2140 (9^m0 9^h51^m58^s1 +21°56'7 1855.0) als der Veränderlichkeit verdächtig bezeichnet werden müsse. Diese Vermuthung gründete sich darauf, dass der Stern im grossen Meridiankreis nicht aufgefunden werden konnte, während doch die gute Uebereinstimmung, welche zufolge einer gütigen Mittheilung des Herrn Prof. Schönfeld die Bonner Originalbeobachtungen zeigen, die Annahme eines Versehens in der Durchmusterung in hohem Grade unwahrscheinlich macht. — Der Stern ist in Bonn 1855 April 20 und 1856 Jan. 14, beide Male von Herrn Prof. Schönfeld beobachtet und als 9^m notirt worden. Dagegen fehlt derselbe in einer dritten Zone, welche bis 21°52' reicht und als Zone 833 von Herrn Prof. Krueger 1856 Febr. 4 beobachtet worden ist. Der letztere Umstand ist aber weder nach der einen, noch der andern Richtung von Bedeutung, da die eigentliche Grenze dieser Zone 22°0' war und auch manche andere Sterne südlich von diesem Parallel darin nicht vorkommen. In diesen Tagen ist es mir nun gelungen, den Stern in ganz derselben, nur um die Präcession veränderten Position, in der ich ihn früher vergeblich gesucht hatte, aufzufinden und zu beobachten und zwar schätzte ich seine Grösse

April 4 8^m7, April 5 8^m6, (wohl etwas zu hell). Da ich mich gleich nach dem ersten vergeblichen Versuche von der Richtigkeit der Position des Zonencataloges und der Einstellung überzeugt und bei späteren Beobachtungsversuchen mich eines 34^s vorangehenden etwa 4' nördlichen Sternes 6^m7 als Referenzobjectes bedient habe, so glaube ich jetzt die Veränderlichkeit des Sternes als erwiesen annehmen und ihn der Aufmerksamkeit derjenigen Astronomen, welche den Beobachtungen veränderlicher Sterne ihre Fürsorge zukommen lassen, empfehlen zu dürfen. Die Tage, an denen der Stern im grossen Meridiankreise nicht sichtbar war, sind:

1880 März 3 und 4, 1881 Febr. 21, März 14, April 4, April 12; die Position ist:

1882 April 6 9^h53^m30^s14 +21°49'24",
für 1855.0: 9 51 56.9 +21 57'3.

An den letzten drei Tagen notirte ich in der Nähe des angeblichen Ortes ein schwaches Sternchen von etwa der 12. Grösse; dasselbe ist aber mit dem in Rede stehenden Stern nicht identisch gewesen, da ich dasselbe auch jetzt (im Refractor) in seiner Nähe, etwa 3^s pr. und 1' südlich wieder gesehen habe.

Berlin 1882 April 6.

E. Becker.

The Greenwich Observations of γ Draconis, made with the Reflex Zenith Tube.

Having been engaged for some time past in observations for stellar parallax, I have been led to examine the observations of γ Draconis made at Greenwich with the Reflex Zenith Tube. Considering the very favorable position of the Greenwich Observatory for observations of this star, the simple and excellent character of the instrument employed, and also the fact that this star is well situated for a determination of its parallax from observations of declination, it seemed probable that this long series of observations would give a knowledge of the parallax of the star. It soon became evident, however, that the observations were so discordant that any result deduced from them would have but little weight. In some years, the parallax would be positive, in others, negative; but in all cases the result would be so uncertain that my work seemed useless, and was abandoned. The following are the probable errors of a single complete observation with the Reflex Zenith Tube.

Year	r_1	No. Obs.
1852	$\pm 0''.480$	49
1853	0.460	25
1854	0.742	46
1855	0.601	40

Year	r_1	No. Obs.
1856	$\pm 0''.518$	60
1857	0.446	92
1858	0.515	98
1859	0.546	78
1860	0.573	63
1861	0.530	60
1862	0.649	33

These observations are not so good, therefore, as careful meridian-circle observations.

For a single determination of declination from observations in the prime vertical, Struve found his probable error to be $\pm 0''.117$. The Washington observations of α Lyrae in the prime vertical, 1862-1867, are affected with a systematic error that destroys their value for a determination of the parallax of this star; but the probable error of a single observation is only $\pm 0''.141$. The prime vertical instrument seems to be better, therefore, than any other for determinations of absolute parallax.

Washington 1882 March 4.

A. Hall.

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ASTRONOMISCHE NACHRICHTEN.

N^o 2434.

Schreiben von Dr. Gould, Director der Sternwarte in Cordoba, an den Herausgeber. Ref.

No. 2411 of the Nachrichten has just been received, with the remarks of Mr. Tebbutt suggesting that the object which I observed 1881 June 11, supposing it to be the comet, was in reality the fixed star λ Eridani, and that the unknown object, with which it was compared was the star Bradley 718, or No. 1592 in the catalogue of the British Association.

It is not surprising that any plausible hypothesis should be resorted to, in order to explain the singular, and to me yet inexplicable phenomenon in question. And it is true that the relative position of the two stars is nearly identical with that of the objects measured. Yet there are sundry difficulties in the way of this ingenious supposition; one of them being the diminution of the distance in declination by 10" in less than six minutes; which, if this distance did in fact remain unchanged, would imply a degree of roughness in the measurements which I should be disinclined to acknowledge, and such a change in the differential refraction, for two objects differing by only 5' cannot seriously be imagined.

I had seen the suggestion published in much fuller detail in the »Observatory« but it had not occurred to me to reply, as I supposed the facts to have been given in my letter with sufficient distinctness. But now that I see that you accept the suggestion as a probable one, it is clearly my duty to record the details of the observation yet more fully. The facts remain very distinct in my own memory and that of others.

The appearance of the comet precluded the slightest doubt as to its identity. The tail itself could not be seen with the telescope, it is true, but the large, diffuse, and very elongated head, much brighter and more definite on the advancing side, were sufficient to enable the veriest tyro to recognize it as a comet.

My position was necessarily at the top of a high observing chair, from which I did not descend during the whole course of the observation. Mr. Thome took charge both of reading the circles, and of recording the transits as I dictated them. As soon as the comet could be seen, I placed it on the middle declination-thread, clamped the telescope, and noted the comet's transit across the middle of the field. This was recorded, and the circles then read. These gave the position communicated to you, and published in Astr. Nachr. No. 2384.

No comparison-star could be hoped for, unless some very bright one should be in the immediate vicinity; for the zenith distance was nearly 80° when I first detected the object, and the daylight was so strong that no artificial illumination of the field was needed, even after the comet

had set. In searching, I loosed the right-ascension clamp, and moved the instrument to and fro in right-ascension. No star appearing, and there being no time to lose, I set the comet on the most northerly thread by the slow-motion screw, and swept again unsuccessfully. The southern declination thread was then brought upon the comet, still by use of the slow-motion screw, and without unclamping the instrument; and I was about to make a new sweep when I saw the object in question at the upper part of the field on the left, while the comet was on the right, below. The four published comparisons were then made, but while I was in the act of pointing the micrometer-thread upon the comet, for a fifth, it disappeared below my horizon.

No jar of the instrument had taken place. Had this been so, I should not have taken the responsibility of so strange a statement as that which I have placed on record, for its intrinsic improbability made me very hesitant in publishing it. The field of the telescope was fully under control from the beginning, the declination-clamp remaining tight throughout. No one who saw the comet could have entertained the idea that any amount of blurring could have given such an aspect to a fixed star, even though it were many times brighter than λ Eridani.

The object used for comparison was somewhat fainter than the comet, much smaller, and, though very much blurred, was round and symmetrical. Although I have no explanation to suggest, and least of all, to maintain, — I made some experiments on subsequent evenings, under similar circumstances, by looking at known stars of different magnitudes when close to the horizon, and through different kinds of haze. And as I have never seen one offer a similar appearance, I can only suppose another comet to have been in the field. That it was not a companion-comet is manifest, not only from the relative motion, and from an examination made the next day, but still more from the abundant scrutiny in the northern hemisphere, which could not have failed to detect any companion. That it was not a fixed star, was evident from the beginning.

Next as regards the brilliancy. I do not believe that a star of the sixth magnitude would have been visible under the circumstances, but this is of course a matter of personal opinion. The estimates of Bradley 718, made for the Uranom. Argent. (Eridanus, No. 291) extended from 1871 to 1877, and varied only between 6^m.4 and 6^m.7. At what date Mr. Stone estimated it as 5^m I cannot tell, as I have not yet succeeded in obtaining a copy of his catalogue.

But there has been yet another question raised, in criticism of my statement. In No. 2404 Dr. Fabritius maintains that I had greatly over-estimated the comet's brightness