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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, January 8, 1897.

Dr. A. A. COMMON, F.R.S., *President*, in the Chair.

Secretaries: Prof. H. H. TURNER, M.A., B.Sc., and E. W. MAUNDER.

THE Minutes of the previous Meeting were read and confirmed.

Mr. Maunder. Eighty presents have been received since the date of the last Meeting. Among these is vol. v., part 5, of the Astronomical Papers issued by the American Nautical Almanac Office, which contains papers by Prof. Newcomb on the Mass of Jupiter, and another on the Orbit of Polyhymnia. Also a catalogue of 16,748 southern stars deduced from observations by Lieut. Gilliss, in his well-known expedition, presented by the U.S. Naval Observatory. The 'Nautical Almanac' for 1900, presented by the Lords Commissioners of the Admiralty. Mr. J. C. Clancey, who is at present a Candidate for Fellowship of this Society, has presented five works on Land-Surveying, of which he is the author; the Italian Government have kindly sent us the sixth volume of the edition of the works of Galileo, which they are now preparing. We have also received from Prof. K. D. Naegamvala some photographic reproductions of a photograph of the nebula M 8 Sagittarii, and from Mr. J. Lunt a photograph of the partial phase of the solar eclipse of 1896, Aug. 9, taken at Vadsö.

Rev. A. L. Cortie read a paper on "A Method of determining the Approximate Heliographic Latitude and Longitude of Sun-spots and Faculæ on Drawings of the Sun." A set of eight diagrams had been prepared on transparent paper, each showing the limb of the Sun represented as a circle of approximately 10 inches diameter, and having solar meridians of longitude and circles of latitude drawn upon them. These diagrams were drawn consistently with eight different angles that the axis of the Sun makes with the celestial equator; for inclinations other than those for which

the diagrams are drawn, interpolation is necessary. Mr. Cortie had used these transparent diagrams to read off the latitudes and longitudes of spots shown on drawings of the Sun made at Stonyhurst, and compared his results with the positions of the same spots as shown in the Greenwich volumes. The comparison showed that the determined longitudes were almost identical, but that the latitudes were not so accordant, there being differences in some cases amounting to half a degree.

Mr. Maunder. Some years ago Dr. Thomson brought out a series of skeleton charts showing orthographical projections of the circles of latitude and longitude on the Sun as seen from the Earth when in different relative positions to the Sun. These were made to the scale in which a solar diameter would be 8 inches, and I used these charts to measure the positions of the spots on a number of photographs, and on comparing these results with those given by the more accurate measurement and reduction of the same spots made at Greenwich, I found that the projection results were correct to about half a degree. I think that from this it is evident that Mr. Cortie's projections must be more accurately drawn than Dr. Thomson's, and that his drawings must be extremely carefully done for the average difference to be so small as that which he has shown to be the result of the comparison. It is clear that for a rough estimation of the positions of spots the projection method is very easy and very fairly accurate.

I have here two photographs of the great spot which has just come upon the Sun, which it may interest the Society to see. The first photograph was taken on January 6, when the definition was very bad, and the picture shows very little more than the immense size of the group. That on Jan. 7 is much better. Although the maximum is now more than 3 years past, this particular group compares favourably as to size with all but two or three which we saw at the very height of the maximum. Its total area is about 2800 million square miles; the length is about 16 degrees of solar longitude.

The President. What is about the size of the Earth as compared with this spot?

Mr. Maunder. The diameter of the Earth is about one-sixth of the diameter of the great leading spot of the group. We were only able to get one photograph on Jan. 7, but the definition is better. The most remarkable feature shown on this day is the intense blackness of the nucleus and its large area. It is unusual to see so large an umbra as there is in this spot. I am hoping that the passage of this spot across the meridian may be accompanied by great magnetic disturbance, because it is fairly compact in form. Extended streams do not give so much disturbance. Probably it will depend upon whether the spot, which has begun to break up, reaches the central meridian sufficiently compact. I may say that about the time it came on the east limb there was a very considerable oscillation of the magnets.

Mr. Newbegin. When does it come on the central meridian?

Mr. Maunder. The entire length of the spot takes more than a day to pass any point, but, roughly speaking, it will be from noon on Saturday to noon on Sunday.

Mr. Maw. Can Mr. Maunder tell us how this group compares as to size with the great group in Feb. 1892? I saw the spot last year, and it struck me as an extensive group, and I am interested in seeing this photograph to-night.

Mr. Maunder. It is almost precisely four-fifths of the area of that of Feb. 1892 as it appeared on the day of maximum, Feb. 10.

Mr. Maw. How long had you begun to observe it before it developed to that size?

Mr. Maunder. We only saw it on Wednesday. We had had cloudy weather before that, so we did not see any sign of it.

Mr. Maw. Had you seen it in the previous rotation?

Mr. Maunder. I have not looked for it; if it was there it must have been very much smaller. It is easily visible to the naked eye.

Prof. Turner. I wish to express the very great interest I take in Father Cortie's attempts to reduce numerical labour. I think figures have been regarded as cheap so long that we have been using them rather too freely in astronomy. I would urge him strongly to continue his investigation because I do not think it would be just to assume from a single set of experiments, such as he has described, that he can always ensure accuracy. The methods used at Greenwich were adopted after very careful consideration and after a great deal of talk, and a debt of gratitude is due to the present Astronomer Royal for having organized this method of measuring Sun-spots, which no one had taken up at all at the time. Personally I can testify to the amount of trouble he took to make this method exact. About the time I was appointed Chief Assistant at Greenwich one of the things on his mind was the question as to whether these methods were accurate or not. I hope that Father Cortie will improve upon them and adopt some method of saving labour, because there is a great deal of labour involved in measuring these photographs. One remark by Father Cortie struck me, namely, that the method at Greenwich was about the most perfect in use. I do not want to criticize the grammar, but I think that a proper description would be that most figures are employed upon it, and possibly perfection is not to be attained by employing as many figures as possible. Again, it seems to me that he sometimes uses two decimal places and sometimes one. I would urge upon him that he should select the decimal place which he will adopt as his criterion of accuracy, and retain it throughout uniformly. This is one of the points which professional astronomers are always urging upon amateurs. I believe mistakes are liable to be made for want of uniformity of system.

The President. It will be remembered that at the last Meeting, when we heard Sir George Baden-Powell's account of his successful expedition to Nova Zemlya, I mentioned that possibly at this

Meeting we might have some account of the less successful, but perhaps as interesting, expedition to Japan, which was undertaken by the Astronomer Royal and others, and also of that to Vadsö, whither I went with a numerous party. Prof. Turner will give you a short account of the Japan Expedition, and I shall call upon Mr. Hinks to show a few slides which will give you some idea of the chief points of interest in our expedition.

Prof. Turner. I happened to be in possession of a slide—the only slide that we have brought back from our expedition— to illustrate this paper, which is a joint paper of the Astronomer Royal, Capt. Hills, and myself. This slide is the production of a Japanese photographer in Yokohama. A gentleman representing 'Harper's Magazine' photographed us when the Sun was very bright, and we had a slide made by a Japanese photographer, which you now see on the screen. This photograph was taken the day before the eclipse. Saturday was fine, and Monday was very fine, but Sunday was not favourable for seeing the eclipse, for it was very cloudy. We were told that the chances were very much against us, but from the actual experience of three weeks on the spot, we did not think that it was more than an even chance whether we should see the eclipse or not, and a day or two before the eclipse the chances of fine weather were good. This really shows what amount of trust is to be placed in deductions from meteorological observations. The choice of a station is determined by previously-observed meteorological conditions, and if you make what appears to be a favourable choice, one always seems to be contradicted by the events at the time. When we got to Yokohama, it was reported in the papers that it had been fine at Kushiro and Shibetcha, but these were exactly the two places that meteorological reports warned us against. In the report we have put down the exact figures taken out by the Japanese meteorologists from their records before we got there. I believe Prof. Todd had asked them to collect records of 3 previous years as data to select the probably best station, and they got out a lot of figures showing that these two places which we were warned against were likely to be unfavourable. This paper is merely an account of what we did in getting ready to which the Astronomer Royal and Capt. Hills referred briefly at the November Meeting. We made out that it was possible to focus the telescopes with great accuracy on the mirror without resorting to star observations. I did get some star observations myself. Then there is a small note on the polariscopic observations.

Mr. Hinks then showed on the screen some slides to illustrate the President's report on the Vadsö expedition.

The President. From the excellent way in which Mr. Hinks has on the spur of the moment given a description of these photographs you may judge what an admirable man he was to have on the expedition. On our voyage I was up every morning for a week at 3 o'clock on the deck of the 'Norse King,' and at this hour the

sky was perfect all the way to Vadsö, but when we got within a short distance of Bodö it did strike me that we ought to divide our forces, and that it would be a good thing to leave somebody behind there. Had we followed the principle laid down by Prof. Turner, namely, to disregard meteorological observations, one of us should have stopped there. However, no one did stay at Bodö, although Mr. Hinks offered to do so if called upon. We had made arrangements before leaving London and we had then come to the conclusion that, owing to the low altitude of the Sun at Bodö, it would be better not to occupy that place, but to go on to Vadsö, where the meteorological probabilities were more favourable. I have very little to add to the account Mr. Hinks has given—it is extremely difficult to make much of an unsuccessful expedition; but I may say we had a very pleasant time, and that was at least some compensation for the disappointment. I hope, notwithstanding what Prof. Turner says, that the meteorological conditions at the next eclipse will allow us to get some excellent results, and I look forward to that as a consolation for the disappointment we experienced at the last one.

Rev. W. Sidgreaves. I had intended this evening to read a complete paper on the spectrum of β Lyræ. It is, however, not yet quite ready for presentation to the Society, although the bulk of the work has been ready for three or four months. But the President has consented to my giving a description of its contents this evening, as there are very few papers set down for reading.

The data on which my paper is founded consist of, first, the tabulated relative wave-lengths of the more prominent lines from about 90 photographs of the spectrum arranged in the order of their time intervals from the preceding chief minimum of the star's magnitude; and, secondly, a table of mean wave-lengths for each day of the period, along with a map showing the relative positions of the lines on each of the same days. The order of decreasing wave-lengths has been followed throughout; and I much regret that many astro-physicists have recently decided to follow the reverse order of the pianoforte keyboard in the order of the spectrum lines.

The discussion has proved to be a long one. There are many perplexing changes, and exceptions to otherwise general rules. Many hypotheses to account for these have been started, only to be run down. But there is one amongst these which seems to hold together fairly well, and is interesting as offering the sort of confirmation, which can be drawn from the tables, of the conclusion arrived at by Belopolsky and Vogel—viz., that the division of the bright lines is the effect of superposition of their dark companions. A table of curves has been plotted for the bright lines showing the measured wave-lengths of their centres, with reference to dark Hγ at 4340, for each day of the light period. All these curves are of the same general form, having their highest and lowest ordinates at the first and second maxima respectively; and in all,

save one, the difference, or extreme change of wave-length, is very great, and the changes at the two minima are very rapid. The exceptional curve showed comparatively small displacements, but enough to prove that, on the supposition of separate stars for different displacements, the origin of the line was not on the dark line star any more than on the bright line star. Its extreme change was from 4476 to 4470. But there is another curve which starts at the middle of the period at λ 4469, the line being found only on the plates belonging to the 8th and subsequent days; and it was observed that if at the time when this line came into view the other at λ 4474 had disappeared, we should have had a single curve between the extremes 4476 and 4461, of precisely the same form as the rest. In this way it becomes probable that the pair of lines seen during the latter half of the period are parts of the same radiation divided by its fellow-absorption, in agreement with Mr. McClean's remark upon the same line in last month's Mon. Not. The position of the true centre of this line for each day would be given very closely by the mean of those of the parts. The curve thus obtained for the last half of the period can then be produced backwards by applying a constant correction to the position of the outside part of the whole broad line; and the result is a curve of comparatively small changes of relative position, in accord with Vogel's conclusion in favour of the smaller relative displacements. The inference is that all the other curves need the same corrections, and that perfect uniformity is to be obtained by bringing these to the form of the exceptional one.

Mr. Maunder. With regard to the point that Father Sidgreaves has raised, I think the evidence (such as we have in our possession at present) indicates that stars with the wide hydrogen lines are stars of low density, and that stars with spectra like that of the Sun are stars with considerably greater density. That shows nothing with regard to their mass—it is a question of density, not mass. It seems to me there is another explanation. The photosphere in stars does not necessarily form at the same relative level in all stars. It seems to me if we could conceive that it formed in the Sirian stars at a lower level with regard to the chromosphere than in the solar stars, we should have a sufficient reason for the greater breadth and prominence of the hydrogen lines. I was extremely pleased to hear Father Sidgreaves's opinion as to the place in which the red end of the spectrum should be drawn, because, like himself, I am a heretic on that point. The 'Astrophysical Journal' has laid down the law that the red end should be to the right, but I think the whole analogy is the opposite way. I showed some slides of the spectrum of the helium here some time ago, and in those slides the red end was put to the left, and this arrangement was criticized; but it was necessary it should be so, because I was dealing with the question of series of lines, and it seems to me we can only deal with a series of lines by beginning with the left end, in the same

way as we begin on the left-hand side of the page of a book. There is also the analogy of the piano. I shall be very glad to see a reversal of the decision of the 'Astrophysical Journal.'

Mr. Newall. I think they are re-opening the question.

Mr. McClean. The absorption-lines appear to remain constant in relative position, and the bright emission-lines do the same, while the two systems shift with regard to each other. The amount of shift may be judged by means of the two absorption-lines at wave-lengths 4472 and 4482 respectively. The corresponding bright line at 4472 shifts at least through a range of that extent corresponding to a relative variation of velocity of the two components of the system in the line of sight of 400 miles a second. This is about the accepted amount.

The delineation of the spectrum from left to right in accordance with the increase of wave-lengths was adopted by Kirchhoff, Thalen, and Angström, also by Huggins, Lockyer, Cornu, Duner, Draper, Pickering, Rowland, and others.

Prof. Alex. Herschel. On the subject of the arrangement of spectrum-lines referred to by Mr. Maunder, Mr. McClean, and Father Sidgreaves, I think that for the proper representation of the spectrum-scale the small wave-lengths ought, as in the high keys of a pianoforte, to be placed on the right. With regard to the changeable appearances presented by the hydrogen lines, I think that more experience of them than I possess is evidently needed to explain them, and Father Sidgreaves's descriptions and interpretations of them therefore appeared to me to be very admirably clear and interesting.

Mr. Maunder then showed a photograph of M 8 Sagittarii, which he said was a copy of the drawing by Sir John Herschel in his volume of 'Cape Observations,' together with a series of photographs of the same nebula taken by Prof. K. D. Naegamvala. The original negative was too thin to show much detail except under the most careful examination, but by successive copying the details were brought out very distinctly.

The President. These are extremely interesting photographs, and there is no doubt that it is possible to bring out in detail by photography things that one cannot see with the naked eye. Many years ago I tried producing copies of pictures, and by taking photographic copies on wet collodion the details were brought out in a remarkable way. This is a point well worthy of the attention of those who want to illustrate their papers by photographs on the screen.

The Ordinary Meeting then closed, and the Fellows present resolved themselves into a Special General Meeting of the Society to consider an alteration proposed by the Council, of Bye-laws 69–74, which form Section XVI., and relate to Awarding the Medal, the alteration consisting of the addition of the word "Gold" before the word "Medal."

The object of this alteration (it had been stated in a circular previously distributed to the Fellows) was to make it possible to award a Silver or Bronze Medal, in addition to the Gold Medal. The Council desired now to award a Bronze Medal, at intervals, in accordance with the provisions of the Mrs. Jackson-Gwilt gift.

On the proposal being submitted to the Meeting by the *President*, *Mr. H. Seward* asked whether the proposed alteration would not place the Society in a similar difficulty as that which the alteration itself was intended to remove, for it might happen that just as now the Council wished to award a Bronze Medal, they might at some time wish to award two Gold Medals, or to award the Gold Medal in a way other than that authorized by the Bye-laws.

Rev. W. Sidgreaves objected to the alteration of the Bye-laws, since he objected to the awarding of any distinction other than the Gold Medal. He thought that it would be more expedient to use the Jackson-Gwilt bequest to make grants of money to persons engaged in researches which required pecuniary resources, not always within the command of the person making the research.

On a show of hands being called for, the alteration proposed by the Council was carried unanimously.

The following papers were announced and partly read :—

F. C. Penrose. "A Method of clearing a Lunar Distance."

W. Schur. "Determination of the Diameter and Compression of the Planet Mars, from observations with the Repsold Heliometer of the Royal Observatory, Göttingen."

Isaac Roberts. "On the Comparison of Reflector and Portrait-lens Photographs."

R. T. A. Innes. "Note on the Magnitude of η Argûs, 1896."

S. W. Burnham. "Orbit of 44 Boötis $\frac{H}{\delta}$ I. 15 = Sh. 193 = Σ 1909."

Royal Observatory, Greenwich. "Observations of Occultations of Stars by the Moon, and of Phenomena of Jupiter's Satellites made in the year 1896."

Rev. A. L. Cortie. "Heliographic Co-ordinates of Sun-spots and Faculæ on the Stonyhurst Drawings."

Sir R. S. Ball. "Note on Mr. A. Y. G. Campbell's paper on the Variation of Uncanonical Arbitrary Constants, with an application to the Planetary Theory."

H. H. Turner. "On Differential Refraction to Terms of Higher Order than the First."

Radcliffe Observatory, Oxford. "Observations of Comet 1896 I. (Perrine-Lamp)."

G. Carslake Thompson & H. W. L. Tanner. "Notes on Meteors observed at Penarth, Glamorgan, on 1896, November 14."

The following gentlemen were elected Fellows of the Society :—

William Johnston, Saint Nicholas House, Westgate Street,

Gloucester; *George Edward Lumsden*, Assistant Secretary of the Province of Ontario, 57 Elm Avenue, Toronto; *Major Percy Alexander MacMahon, R.A., F.R.S.*, Artillery College, Woolwich, and 52 Shaftesbury Avenue, W.; *Louis C. H. Maston*, San Francisco, Cal., U.S.A.; *Beauchamp Prideaux Selby, J.P.*, Pawston, Cornhill-on-Tweed, Northumberland; *J. E. de Villiers*, Government Surveyor, Cape Town; *John Watson*, Norman Villa, Glossop Road, Sheffield.

The following Candidate was proposed for election as Fellow of the Society:—

John Harold Herbert, Civil Engineer, Mapperley Road, Nottingham (proposed by A. A. Common).

THE BRITISH ASTRONOMICAL ASSOCIATION.

The second ordinary Meeting of the seventh Session of the British Astronomical Association was held at University College, Gower Street, on Wednesday, December 30th, 1896, the President, *Mr. N. E. Green, F.R.A.S.*, in the Chair.

Seven new Members were elected, and the names of twelve Candidates for Membership were read and suspended.

Mr. Walter F. Gale, of Sydney, N.S.W., contributed a paper on "The Brightness and Colour of the Planet Mercury," which was read by the Secretary. The writer said that the eastern elongation of September last afforded a favourable opportunity for determining the stellar magnitude of Mercury, Spica and Arcturus being well placed as comparison stars. Mercury reached its greatest brightness about Aug. 29 and was then perceptibly brighter than Arcturus; its colour appeared an orange-yellow, the same as that of Arcturus, but not quite of so intense a tint.

Two papers by *Mr. Francis McDonald*, of Sydney, entitled, "The Zodiacal Light, 1896," and "A Peculiar Atmospheric Phenomenon," were also read by *Mr. Schooling*.

Mr. E. W. Maunder read a paper on "The Level of Sun-spots," an abstract of which will be found on page 93.

Mr. G. M. Seabroke remarked that Mr. Maunder's theory seemed to agree with his own observations, but he was rather at a loss to understand how the dust could aid appreciably in producing the darkness of the spots. He apprehended that an increased thickness of 20 miles in the extent of the dust layer would have a very slight effect. Then assuming there was a slight depression, that depression would be filled up by the reversing layer of gases, which would flow into it and produce the effect of the widening of the lines which was generally seen in a sun-spot. The theory that a spot was a