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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1919 June 13.

Prof. A. Fowler, F.R.S., *President*, in the Chair.

Secretaries: Rev. T. E. R. Phillips, M.A.
H. S. Jones, M.A., B.Sc. (*Acting Secretary*).

The Minutes of the previous Meeting were read and confirmed.

Sixty presents were announced as having been received since the last meeting, including Stereograms of the Moon and Jupiter, and Hydrogen and Helium Images of the Orion Nebula (presented by Mr. J. H. Reynolds).

The President. Since the last meeting one who has served this Society with devoted interest for over 40 years has passed away. The Council at their meeting this afternoon passed a resolution of condolence with Mr. Wesley, our Assistant Secretary, in the loss that he has suffered in the death of Mrs. Wesley, and I am sure that all the Fellows would wish to be associated with this expression of sympathy.

A number of American astronomers, including several of our Associates, will be in London early in July on their way to an International meeting in Brussels. It is proposed by the Council to give Fellows a chance of meeting the American visitors, and, if the plans of the latter admit of their being present, a special meeting of the Society will be held on Friday, July 11, to welcome them.

I will now ask the Astronomer Royal to give us what information he can about the work of the eclipse observers on May 29 last.

The Astronomer Royal. I have not very much to say that has not been already published in *The Observatory* or *Nature*. Both parties arrived safely, and we have had telegrams from both since the eclipse. We had adopted a code for the information to be sent, and the word "splendid" sent from Brazil was next to the word indicating a perfect eclipse. Prof. Eddington broke with the code in order to tell us that he was hopeful that something could be made of the photographs he had secured through clouds. A later telegram from Davidson stated that 12 out of 13 stars had been secured on the plate taken with the Astrographic telescope. I do not know whether the missing star is the one nearest to the Sun. The images are reported to be diffuse. This is probably due to the mirror or the cœlostat in some way. The proper silvering of the mirror under the conditions prevailing before the expedition started proved very difficult. Also the cœlostats, after many years of eclipse work, now need overhauling, a work which was impossible under the labour conditions during the war. The other telescope taken to Brazil had a 4-inch object-glass of 20 feet focal length, and was kindly loaned by Father Cortie. This had a smaller field, but on the plate taken by it 7 stars are shown. The observers are staying to take plates of the field after the Sun has moved away. That may keep them in Brazil a couple of months.

The President. We are all glad to hear that the expeditions have met with so much success. On the eclipse day a very large prominence was seen on the limb of the Sun, and I will ask Mr. Stratton to give an account of Mr. Moss's paper about it.

Mr. Stratton. The prominence which I have to show you in a series of slides was first seen on the 28th, when it appeared as a long rolling cloud right down on the eastern limb. By the morning of the 29th this cloud had lifted, showing what looked like a smoke-stack at about 40° S., from the top of which a heavy cloud floated northwards about 2'·5 above the limb. Subsequent photographs secured by the spectroheliograph during the day showed the cloud spreading northward and sending out streamers which reached down to the Sun's limb at or even north of the equator. The next stage was marked by arched structure appearing between the cloud and the limb. This structure vanished and the cloud broke free from its connexions with the limb, first of all from the stack at the S. end and then from the streamers at the northern end. The last plate taken during the day showed the stack now tilting over markedly to the south and a few wisps of the cloud left. This plate was taken when the Sun was getting low and conditions were much less favourable for securing a good observation. On the plate secured on the following day the only part of the prominence that was seen was the stack at the southern end considerably reduced in size. It will be interesting in view of the solar eclipse on May 29th to see later what influence this huge prominence may have had on the outer layers of the Sun's atmosphere. Mr. Moss secured an excellent series of plates during the day of the eclipse.

The President. It is valuable to have a spectroheliograph working in this country, and we must congratulate the Solar

Physics Observatory on securing these excellent photographs. The prominence was seen at South Kensington on the afternoon of May 28th, and it was seen then to be breaking away from the limb.

Mr. Inwards. What was the line across the plate at top and bottom?

Mr. Stratton. That is due to scattered light, which is most marked when the moving slit is tangential to the limb of the screened Sun.

The President. We will thank Mr. Moss for his paper and Mr. Stratton for showing us the slides. I will now ask Mr. Gunther to say a few words about the collection of old astronomical instruments and books now at the Bodleian Library, Oxford.

Mr. Gunther. We have been having a collection of old astronomical instruments at Oxford. I showed them recently to my friend Prof. Turner, and he suggested that I should come here and talk about them. The exhibition really covers two periods. The one portion contains mostly instruments and tables of the 14th century. The will of a Merton astronomer, Simon Bredon, has recently been found stating that he had left two astrolabes, one to Merton College and the other to William Reed, another astronomer of Merton of the same date. It is almost certain that we have one of these instruments in our collection. The table of precession marked on it commences at 1350, which is the probable date of the instrument. The back of this instrument is engraved with circles designed to illustrate the epicyclic movements of the planets. I hope that Dr. Dreyer may be persuaded to take it in hand and to explain the method of using it. Then there are several old astrolabes and quadrants, including one associated by tradition with Chaucer's son, "littel Lowys." This instrument has been fitted with a tablet marked "LATITVDO LONDONIE LII," but is of earlier date. It was probably an Eastern astrolabe with Arabic characters on it which were apparently not understood: they have been partially rubbed off and the instrument re-marked.

Again, we have some old astronomical tables constructed for Oxford, including those of the Merton astronomer John Maudith, bearing date 1310; and several copies of the tables of Reed and Aschenden, as well as their works on the eclipses of 1345 and 1349, and on the conjunctions of 1365 and 1367 with certain correspondence with Lambourne, a monk of Eynsham, relating thereto. Of Richard of Wallingford, Abbot of St. Albans, the designer of the first astronomical clock, we have two treatises—the Rectangulus and the Albion. The former was an instrument with 3 or 4 rules pivoted and arranged for pointing to various heavenly bodies. The rules were left unchanged in position for some time, and then served to measure how the various bodies had changed in relative position. The Albion was a non-perfidious instrument by which all astronomical processes could be carried out at the same time—all-by-one.

The later period in the collection is represented by a collection of instruments formerly belonging to the Earl of Orrery, after whom the orrery was named. These were bequeathed to Christ Church, Oxford; such care was taken of them that their very existence was only known to the librarian and dean of the College. They date from the last quarter of the 17th and the first years of the 18th centuries. They are highly decorated, and they probably represent the best work of the English instrument workers of the period. There are several telescopes: two with vellum bodies and seven or eight draw tubes, and two others with square wooden tubes; one of these by Wilson is 9 feet long. We know that it is by Wilson as his name is scratched on the object-glass. Then there are several astronomical models made by Rowley, illustrating the solar system from the Copernican and Ptolemaic points of view. There is a copy made by T. Wright of the original orrery; the original was made by Rowley and is in the possession of the Orrery family in Ireland. A photograph of the original is in the South Kensington Museum.

The President. Dr. Gunther has given us a most interesting account. Prof. Turner, have you any remarks to offer?

Prof. Turner. If I may be personal enough to explain Mr. Gunther to the Society, I should describe him as a scientific Janus, looking both backwards and forward. I had the pleasure of his company and help in the eclipse in Egypt in 1905. There are many very interesting things in the collection at Oxford. One book makes a possible claim to the invention of the telescope long before 1609—in fact in 1571. There are also a number of very fine illustrations in the old books. Fellows might like to hear from Mr. Gunther if there is any day more convenient than another when they might come to Oxford to study the collection.

Mr. Knobel. Mr. Gunther's remarks made my mouth water. Wright's orrery, to which he has referred, figures in Moore's *Astronomy*. I have had many astrolabes at one time or another in my hands. As to the one with Arabic characters erased, the latitude plate would show at once whether it was made for northern latitudes or no. The 3-armed instrument is probably an Arabian one described in a Persian manuscript in the British Museum. The astrolabes at Oxford are not very old. I have recently had one in my hands dating from 374 A.D.—the oldest instrument I have seen.

Mr. Gunther. I think that we have this very astrolabe in our collection. It belongs to Mr. Lewis Evans, and was specially loaned to us. As to the Chaucer astrolabe, the latitude plate shows signs of being much newer than the body of the instrument.

Dr. Dreyer. There is one object in the collection that I should like to mention—some old manuscript tables of the 14th century.

I have been studying these MSS. in the last few months, and I hope to give some details to the Society at the November meeting. There was considerable astronomical activity in Oxford about the year 1350, but the interest was then entirely in the future and never in the past.

Mr. Parr. Might I ask whether the instruments are mostly on the Galilean principle? I have seen many of the instruments in the Galileo collection at Rome, and have been greatly interested in Mr. Gunther's account of the Oxford collection.

Mr. Gunther. The Galilean principle is only illustrated by one small perspective glass $4\frac{1}{2}$ inches long, of English make, with ivory mounting, in a vellum tube. The others are on Campani's principle. With regard to the point raised by Prof. Turner, unfortunately the exhibition must close next week. I hope that we may get the instruments more permanently together at a later date in a place where they can be more easily seen than in their present homes scattered through different colleges.

The President. We are much indebted to Mr. Gunther for coming here to-day, and I offer him our thanks. I will now call on Mr. Reynolds to read his paper on "The Distribution of Hydrogen and Nebulium in the Orion Nebula."

Mr. Reynolds. The photographs that I am about to show you have been taken with screens which cut off certain portions of the spectrum. Special attention has been paid to selecting the region of the green nebular rays at $\lambda\lambda$ 5007, 4959, $H\gamma$, δ and ϵ of the hydrogen series, and $H\alpha$ separately. Keeler was the first to work along these lines (*Ap. J.* ix. 133). Later Hartmann worked on the region of the strong nebular line at λ 3727. He showed that this radiation extended far beyond the Huygenian region, but his plate dealing with $\lambda\lambda$ 5007 and 4959 transmitted $H\beta$ almost without loss, and therefore is not reliable evidence of the distribution of nebulium. He attempted to isolate the hydrogen radiations also, but was not successful. One screen I used for this was an æsculine screen, which cuts off all radiations below λ 3900. This gave $H\gamma$, $H\delta$ and $H\epsilon$, the much weaker nebulium radiation at λ 4363, and some faint helium lines. I used Fath's published plate to give the relative intensities of the different lines in the nebula. Another plate was taken through a screen transmitting only the region $\lambda\lambda$ 4700–5500. This included $\lambda\lambda$ 5007, 4959, and $H\beta$, but the light from $H\beta$ was cut down to 35 per cent., and the curve of sensitivity was in the direction of still further reducing it. It is very difficult to cut out the $H\beta$ line entirely, but there seems to be no reason why a screen should not be produced which will only transmit a negligible amount. The plate on the screen shows two comparable photographs secured on good nights. The image with the brighter centre is the nebulium screened plate, and it shows nothing of the large extensions seen in the unscreened plate. Nebulium is then highly concentrated in the centre of the nebula. Again, the screened plate shows none of the separate nebulosity which can be seen round *c* Orionis on the unscreened plate. With the æsculine screen giving the hydrogen radiations $H\gamma$, $H\delta$, $H\epsilon$, the nebulosity round this separate star is conspicuous, and with an hour's exposure the hydrogen image extends generally as far as Hartmann found for the λ 3727 radiation; the general structure for the outlying regions is the same as Hartmann's λ 3727, but there are definite differences in brightness. The photograph in the red depends on $H\alpha$ only, and shows the nebula in the region S.E. of the Huygenian region, also a condensation round θ Orionis. It is very faint compared with the other hydrogen radiations. I am hopeful that some of this work will be started later at Helwan under the more steady atmospheric conditions available there.

The President. The ideal method would be to use a spectroheliograph for this work, but he would be a brave man who would take it on with the present instrumental facilities. Meanwhile, Mr. Reynolds is using the screen method with great success, and we all wish him continued succees with the work in Egypt.

Mr. Stratton. Have you made measures of the stellar images on the different screened plates to see how the differences in the nebular images compare with the stellar images?

Mr. Reynolds. Not yet.

Prof. Lindemann. Is $H\alpha$ stronger at the centre than at the edges? If so, that would indicate a lower temperature—a somewhat unexpected result.

Mr. Jeans. This is probably not a temperature effect, and temperature considerations may lead us off the track altogether.

Prof. Lindemann. I think that all questions of distribution of energy in the spectrum may be explained in terms of temperature. I doubt whether it is possible that there can be electric effects in nebulæ.

The President. We will express our thanks to Mr. Reynolds, and I will now ask Mr. Jones for his paper on "Results obtained with the Cookson Floating Zenith Telescope."

Mr. H. S. Jones. It will be remembered by most of the Fellows that the Cookson Floating Telescope was originally loaned by the Cambridge Observatory to the Royal Observatory for a period of seven years. This period has now expired, and a summary of the results obtained from the seven years' observations is given in this paper. The greater portion of the paper deals with the method of reduction, instrumental corrections, etc., and as this part is of a somewhat technical nature I will make no reference to it this afternoon, but will proceed at once to some remarks about the results.

I will first deal with the question of latitude-variation. The observing programme was primarily designed for the determination of the aberration, but the variation of latitude is obtained as a bye-product. This part of the work has latterly assumed a greater importance owing to the partial disorganisation of the

International Service. Although the programme of observation would have been somewhat different had it been desired simply and solely to determine latitude-variation, yet the actual programme has answered very well. The values obtained direct from the observations are shown on the screen, with a smooth curve drawn through them. It is at once apparent that only at one or two points is there any considerable deviation from the smooth curve. The observations, on the whole, are so smooth that one feels that considerable reliance can be placed on them.

I have made some preliminary comparisons of the values obtained at Greenwich with the International values, as far as published. On the whole, the comparison tends to confirm the tentative conclusion put forward in my paper containing a preliminary discussion of three years' observations with this telescope, viz.:—that whilst there are systematic differences between the Cookson and International values, whether the z term is included in the latter or not, the best agreement is obtained when a fraction only of the z term is included. In this connection it is interesting to note that Dr. Wanach, of the International Bureau, has concluded, in a comparatively recent paper, that whilst the x and y of the International results give the true components of the polar motion, yet the latitude-variation deduced for a given station from the International x, y and z values may be as much as $0''\cdot10$ in error. He adds that for the purpose of applying corrections to meridian observations, the latitude-variation should be determined at the actual observatory. It certainly seems probable that the Cookson values represent the latitude-changes at Greenwich much better than do the International. This is in accord with the view that the z term arises from local meteorological causes.

Coming now to the question of the aberration, it is at once apparent that its determination is a problem of far greater difficulty than the determination of latitude-variation. Provided sufficient plates can be obtained to enable accurate group-corrections to be deduced and accidental errors to be averaged out, the latter is straightforward. For the aberration, however, each plate must be weighted proportionally to its aberration factor. In the summer months, when observations must necessarily be obtained near midnight, the factor is small and the plates carry least weight. The winter plates carry much greater weight, and these are not only liable to be interfered with by long spells of bad weather, but their average quality is less than that of the summer plates. Mr. Witchell and Mr. Acton, who have measured almost all the Cookson plates, estimate that stars are obtainable in the summer of about half a magnitude fainter than are obtainable in the winter. Now one difficulty with the Cookson telescope is that the range of available magnitudes is very limited. Stars brighter than $5^{m}\cdot0$ (photographic) give trails so broad that they are measurable with difficulty; on the other hand, stars fainter than $6^m \cdot 5$ give faint trails which are lost if the sky is poor. In the winter far more plates have reduced weight through one or two star-trails not showing.

As far as the aberration constant itself is concerned, the correction resulting from the whole series of observations is $-0''\cdot028$, giving for the aberration constant $20''\cdot442$. The corrections resulting from each single year's observations were derived and from the discordances between them the probable error was estimated as $\pm 0''\cdot013$. The deduced aberration constant is therefore $20''\cdot442 \pm 0''\cdot013$, corresponding to a solar parallax of $8''\cdot815 \pm 0''\cdot006$. The discordances between the results of the several years' observations were larger than had been anticipated, and the individual corrections range from about $-0''\cdot07$ to $+0''\cdot05$. For this reason and for another to which I shall shortly refer, too much emphasis should not be laid on the correction obtained, though, since five years gave a negative correction and only two a positive, the result certainly points to a small negative correction.

In the paper, I have discussed in some detail the various sources of error which enter into the result. Of these, the smallest is the error of measurement. The one which chiefly concerns us now is that which Prof. Eddington called the "error of the night," whose existence has been fully established by the later observations. If the probable error of one plate is determined from all the plate-residuals, after allowing for latitude-variation and group-correction, it is found to be $\pm 0''\cdot078$. On the other hand, if the probable error is deduced from the differences of residuals of strictly consecutive plates it is found to be $\pm 0''\cdot059$. There is, therefore, an error of amount $\pm 0''\cdot051$, which affects consecutive plates equally. This is the largest of all the sources of error. The matter is at present under investigation, but some information is thrown on it in the following way: on the screen are shown plate-residuals plotted against wind direction. Two features are at once evident on looking at the screen. First, the residuals tend to be negative for S.W. winds and positive for N.E. winds, the means in the two cases being numerically about $0''\cdot050$. Secondly, the dispersion of the residuals is much greater for the N.E. winds than for the S.W. This may possibly be associated with the fact, well-known to all the Greenwich observers, that the poorest definition is usually obtained with winds from an easterly quarter. Since wind-direction is generally the same for two consecutive plates, this phenomenon is of importance in connection with the "error of the night." Its physical explanation has not as yet been discovered. I have not attempted yet to ascertain whether there is any systematic effect on the aberration; any such effect would depend on two factors, the relative seasonal frequenices of various winds and the relative proportion of clear nights associated with different winds. I incline at present to the opinion that there is little systematic effect, but that the accidental effect may be large. Thus, if only a few plates are obtained in one interval and the prevailing wind is, say, S.W. for them all, there would tend to be a negative-residual for this interval and a rather wide departure from the smooth latitude curve. In this way, some of the actual discrepancies can be accounted for. Further, if, say, 4 plates have residuals of $\pm 0''\cdot 200$ and are obtained at a time when the aberration factor is $\pm 20''$, these plates would account for the whole of the correction deduced from one year's observations. It is partly because the accidental errors may be so large in comparison with the smallness of the quantity one is seeking for, that I did not desire to put too great an emphasis on the actual aberration correction obtained. Unless the accidental errors can be in some way reduced, I do not see how to overcome this difficulty other than by continuing the observations for a very lengthy period.

It is intended to continue the observations for a further seven years, as the period of loan of the telescope has been extended. The latitude-variation will be determined, and it will be of interest to see what value is obtained for the constant of aberration. With this further experience we shall be able better to decide whether we can hope to determine the aberration-constant with an accuracy exceeding the best previous determinations. At present, I feel rather doubtful whether it is not better to attempt merely to obtain the latitude-variation.

The President. It is evident that these results have not been obtained without a great amount of labour. The conclusions have been presented in a manner that carries confidence. Would the Astromomer Royal or Prof. Sampson like to speak about this work?

Prof. Sampson. I will speak first, because I want to say candidly that I am disappointed in these results, and I should like to give the Astronomer Royal a chance to reply. I well recollect Prof. Eddington giving us the results of his preliminary work. There were outstanding discrepancies which were to be cleared away, discordances due to instrumental trouble. The value for the aberration-constant deduced in this first paper was $20''\cdot 47$, and I remember that Sir David Gill said that the theoretical value of the aberration-constant needed no further support. I do not think that this new value of the aberration-constant can be upheld, nor do I gather that Mr. Jones wishes to press it.

The Astronomer Royal. Prof. Sampson does not, I think, quite realise our point of view. We do not think that the outstanding discrepancies are in any way instrumental in origin, but meteorological. The Kimura term in the latitude-variation is probably due to an inclination in the surfaces of equal density, which leads to a slight displacement of the zenith. I do not think that the difference in the values for the aberration-constant or the solar parallax is so large as Prof. Sampson implies. Mr. Hinks obtained from his determination of the solar parallax from Eros a value $8''\cdot806 \pm 0''\cdot004$. Our new value is $8''\cdot815 \pm 0''\cdot006$. The difference between the two values is less than the sum of their probable errors. It is really very difficult to reach a closer degree of accuracy. I personally think very highly of the instrument and think that it reflects the greatest credit on Mr. Cookson and on those who helped him to design it. It leaves very few systematic instrumental errors to correct, avoiding in particular some of those which accompany an instrument of the Durham type. It might perhaps be improved by replacing the triplet object-glass by a doublet with a consequent increase in the range of magnitude and number of available stars.

Prof. Turner. Do you get a different value for the aberration-constant from observations taken with south-westerly and easterly winds?

The Astronomer Royal. The difference of results under these two conditions of wind would not affect the results, unless you had westerly winds in the evening and easterly winds in the morning of the same night. Seasonal changes affect latitude variation rather than aberration determination.

Prof. Turner. Were the values different with different strength of wind?

Mr. Jones. On the whole, the largest residuals came with the lightest winds. I am sorry if I gave Prof. Sampson the impression that the effect was due to an effect of wind on the flotation of the telescope. If this were the case, the maximum effects would occur with N. and S. winds. This is not the case: the explanation might be sought rather in irregularities in the atmosphere which form and remain unbroken in the absence of wind. With a strong wind, the atmosphere possibly gets thoroughly mixed up. The effect is of the same nature as the one found by Doolittle when working at Bethlehem with two different telescopes. He got large residuals on the same date with both instruments. The cause is probably meteorological. With regard to the telescope, I feel that it has been designed in every particular to meet the difficulties that can arise. I do not believe that there is a better zenith-telescope in existence at the present day. Seven years' experience does not suggest any improvement other than the substitution of a doublet for a triplet lens. It must be remembered, however, that our programme of stars is slightly different from that of Cookson; we use a smaller field, and no longer need the triplet lens that he required in order to obtain a large flat field. With regard to the determination of the aberration-constant, it must be remembered that we used all the plates that have been measured including the difficult ones, which it might perhaps have been better not to measure at all. In our next programme we have rejected the weakest and brightest stars, and we are giving more latitude to those who are measuring the plates, as to whether a plate shall be rejected or measured.

The President. I will return your thanks to Mr. Jones for his paper. We have a paper on "Star-Discs" by Prof. Conrady, and I will ask him to read it to us.

Prof. Conrady. About 85 years ago Airy determined the theoretical form of a star-image according to the undulatory theory of light. He assumed an object-glass perfectly corrected and an image in perfect focus. He obtained the well-known result that the image consisted of a central disc surrounded by a series of concentric rings; the rings fall away very rapidly in brightness: compared with the brightest portion of the disc as unity, successive values for the rings were $\frac{1}{57}$, $\frac{1}{240}$, &c. Now Airy's disc is an unattainable ideal. Aberration and focal error are always present to a greater or less extent, and the problem in optical design is to find the limits within which these quantities may vary without spoiling the star-image too badly. The problem has generally been dealt with in the past by methods of elaborate mathematical analysis. I have recently taken it up more from the arithmetical standpoint, and have already obtained many results of interest. I considered, first, the perfect instrument imperfectly focussed. I went beyond the sharp focus to a point where the marginal rays meet the axial ray with a difference of phase $=\frac{1}{4}\lambda$, the well-known limit given by Lord Rayleigh, and I calculated the spurious disc. The central portion of the disc followed Airy's light-curve, but with a loss of about $\frac{1}{5}$. The lost light was spread in a faint penumbra round the central disc, which remained effectively of the same diameter. This explains why an achromatic telescope can give decent images in spite of the fact that the secondary spectrum causes considerable variation in the foci of different colours. The chief effect of the variation in focus is to lessen the brightness of a star-image whilst leaving its diameter unaltered. The definition is practically unaffected. If now we move out to a point where the difference of phase is $\frac{1}{2}\lambda$, the central disc drops in brightness to $\frac{4}{10}$ of the original value, but still remains of the same diameter. Only the penumbra is widened and strengthened.

Next, I considered spherical aberration. The wave-distortion of the marginal portion varies with the fourth power of the distance from the optical axis. With $\frac{1}{4}\lambda$ of distortion we get a loss of $\frac{1}{5}$ of the light in the central disc, but the diameter remains unaltered. If we take the focus of the marginal rays as an alternative focus, we get almost the same result as for the paraxial rays. The form of the equation expressing the distortion suggests that we should get a far better result between the focus for the axial and marginal rays. In fact, at the midway point we get, in the case of $\frac{1}{4}\lambda$ of spherical aberration, a result which is practically indistinguishable from Airy's perfect disc. I have applied the same methods of integration to coma. The distortion of the waves can be derived from the sine condition; the geometrical image consists of a figure made up of a 60° angle closed by a circle. For small amounts of coma such as may be found in good instruments—and I am only concerned with them—the image is concentrated at $\frac{2}{9}$ of the length of the geometrical coma figure and becomes once more practically a perfect Airy disc. There is here total disagreement with the geometrical result, and luckily for all concerned the difference is in favour of the makers and users of optical instruments.

The President. Unfortunately we have no time to discuss this important communication. The paper will be read with interest by Fellows. I will ask you to return your thanks to Prof. Conrady, and we must now proceed to the ballot for new Fellows.

The following papers were announced and partly read:—

Rev. J. G. Hagen. "On the Light Curves of Long-Period Variables."

Miss E. Bellamy. "On a curious Instance of Opposite Proper Motions."

H. S. Jones. "Results obtained from Seven Years' Observations made with the Cookson Floating Zenith Telescope at the Royal Observatory, Greenwich."

J. H. Reynolds. "The Distribution of Hydrogen and Nebulium in the Orion Nebula."

Rev. A. L. Cortie. "The Spectrum of Nova Aquilæ, 1918 August 23 to October 23."

W. Moss. "On the Eruptive Prominence of 1919 May 29." Communicated by the Director of the Solar Physics Observatory, Cambridge.

H. H. Turner. "The Stellar Magnitude Scales of the Astrographic Catalogue, Fourteenth Note: the Tacubaya Magnitudes (−16°) and the Cape Magnitudes (−43°)."

A. E. Conrady. "Star-Discs."

Ignatius Nicholas Dracopoli, F.R.G.S., Captain in the Special Reserve of Officers, c/o J. Whittall & Co., 9 Fenchurch Street, E.C. 3, was balloted for and duly elected a Fellow of the Society.

Dr. Vesto Melvin Slipher, Director of the Lowell Observatory, Flagstaff, Arizona, U.S.A., was balloted for and duly elected an Associate of the Society.

The following candidates were proposed for election as Fellows of the Society:—

L. J. Comrie, M.A., Auckland, New Zealand: temporary address 32 The Avenue, Bruce Grove, N. 17; and *Prof. J. S. Plaskett*, Director of the Dominion Astronomical Observatory, Victoria, British Columbia.