# The Journal of the British Astronomical Association.

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# REPORT OF THE ANNUAL GENERAL MEETING OF THE ASSOCIATION,

## HELD ON WEDNESDAY, OCTOBER 30, 1929, AT SION COLLEGE, VICTORIA EMBANKMENT, E.C.

Inst.-Capt. M. A. Ainslie, R.N. (retired), B.A., F.R.A.S.,
*President.*

Major A. E. Levin, T.D., F.R.A.S. } *Secretaries.*
F. J. Sellers, M.I.Mech.E., F.R.A.S. }

*Mr. Sellers* read the notice convening the Annual General Meeting and afterwards read the minutes of the last Annual General Meeting and of a Special General Meeting for the Alteration of By-laws.

*Major Levin* read the Auditor's certificate.

*The President* moved the adoption of the Report and Accounts; this was seconded by *Dr. Crommelin* and confirmed by the *Meeting.*

*The President* then read the following address:—

Following the example of my predecessors, I will commence what I have to say with a brief review of the activities of the Association during the last year.

The Council's Report is now in your hands, and you will see from it that there has, unfortunately, been a reduction in the number of our Members. This is much to be regretted, but I do not think that we need be unduly perturbed by it. We must expect these ups and downs. I have, myself, met with other evidence that the general interest in Astronomy in this country, which undoubtedly received an impetus from the solar eclipse of 1927, has again relapsed: we have, I fear, more "Gallios" than "Galileos" in our midst.

But for many reasons we should like to see our numbers as high as possible, and I would like to suggest to Members that they should, each one of them, lose no opportunity of bringing into the fold all who are interested in our science, whether active observers or not.

A glance at the Report will reveal the continued activity of the Association as a whole, and of the Observing Sections in particular: but first and foremost I would like to draw your grateful attention to the splendid generosity of Mr. Walter Goodacre, one of our former Presidents, and now for many years the able and energetic Director of the Lunar Section. He has taken a most practical step towards the encouragement of amateur observers by the gift of a sum of over £300 for the foundation of a "Walter Goodacre Gift and Medal," to be given periodically to a Member of the Association selected by the Council, in recognition of the value of his work. This gift, which is analogous to the "Jackson-Gwilt Medal and Gift" of the Royal Astronomical Society, supplies a long-felt want, and all Members, whether active observers or not, will feel a deep sense of gratitude to Mr. Goodacre for his generosity.

Mercifully the hand of death has been comparatively light on us this year, although the Obituary list is still long: we have received no such blow as fell on us last year in the death of our Founder. But I think that I may make some passing reference to a name which does not occur in the list. It is some years since Prof. Bickerton was seen at our meetings, and at the time of his death he had ceased for some time to be a Member; but many will remember the steadfast way in which he fought what might be called a "rearguard action," and although his views commended themselves to few, they certainly stimulated thought, and have had to be seriously considered.

Although there has been nothing very spectacular in the astronomical events of the past year, as affecting our Association, the Observing Sections continue what may be termed, in his Majesty's words in 1922 to the Royal Astronomical Society, their "patient and unobtrusive labours." Although it is much to be wished that more Members should engage in systematic observations in one or other of the lines of work open to them, still good work has been going on.

The Solar Section continues to provide careful observations of spots and prominences, and the formation of a Solar Section in connection with the New Zealand Astronomical Society, although not, strictly speaking, connected with our Association, gives promise of increased continuity in the records.

The Lunar Section continues its useful work under the guidance of Mr. Goodacre: and we welcome the useful work accomplished with the 24-inch Cassegrain reflector recently constructed by Mr. Tomkins. This instrument, which through the kindness of its constructor I have been privileged to inspect, is being employed in a systematic photographic study of our satellite, and we may confidently expect important advances in our knowledge as a result.

Turning to the Planetary Sections, we have the usual full report of work from the Director of the Mercury and Venus Section. These two planets are notoriously difficult to observe, and he and his Section are to be congratulated on the results they have obtained.

Mars remains for the moment out of our reach: Saturn, with becoming modesty, has retired far south of the Equator, possibly to assist in amateur work at the Antipodes: but Jupiter has been full of interest. The very remarkable series of disturbances in his southern hemisphere, and the equally remarkable series of observations of them by the Director and other members of the Section, will be fresh in your memory: they were important in themselves, and equally so as demonstrating a fact which has been questioned in some quarters: namely, that our English climate is not so hopeless as some people think in the matter of "seeing," and that what has been termed "that peculiarly British instrument, the Newtonian reflector," is unsurpassed for planetary observation: and that large apertures can be used to advantage on this side of the Atlantic. Only last night I had the pleasure of observing the planet with the 18-inch speculum belonging to the Association, with which so much good work was done many years ago by the late N. E. Green, who bequeathed it to us. In its present home at the Director's observatory at Headley it is doing splendid work, and it gave me a most magnificent view of the Jovian detail. Exceptional seeing, combined with good instruments and careful observation, have made the past apparition of Jupiter a specially notable one.

Dr. Crommelin and his Section continue to keep watch over comets: these have been scanty, but he lets nothing escape that can be netted, and although visual observers have had a rather blank season, the photographers have had some sport.

The work of the Meteor Section has proceeded steadily, and, in addition to the usual visual observations, we note some valuable photographic work by the Director, as well as by Messrs. Collinson and Waters. The design of apparatus suitable for this work is important, and it is encouraging to learn that this matter has not been neglected. We also welcome observations from a Southern observer, and echo the hope expressed by the Director that more observers will be found to "assist in a thorough research on Southern Radiants."

Auroræ and Zodiacal Light continue to be well studied, and the number of observations has increased, although the number of workers in this field remains small. Observations in this branch of amateur work, requiring as they do no instrumental equipment, should be more attractive than they seem to be: and the same may be said with regard to the observation of meteors.

Our Variable Star Section has long since established its position in the astronomical world: it is, perhaps, the department in which the activities of the Association are most striking. The Section has just published its tenth Report, and the figures quoted in the Council's Report give striking evidence of the Section's activity. "43,590 observations of 51 stars made by

48 observers": and, again, "14,000 observations of Long-period Variables made in 1928" are in themselves remarkable. There is no doubt that in this particular department, so well suited to amateur energies, we may justly be a little proud of our achievements. And in this connection the long-continued and unselfish labours of Messrs. A. N. Brown and C. L. Brook, no less than those of the Director, have greatly contributed to this result.

The photographers continue their good work: they have long since shown that quite small instruments, in amateur hands, can do useful work, and one notices their activities in the field of small-scale photography of the great nebulæ. I think, too, that I may well call your attention to Dr. Waterfield's successful work on the occasion of the Solar Eclipse of last May. Those of us who have seen his "Eclipse outfit" may well congratulate him on the good results he obtains with such simple apparatus. I am sure, too, that we shall all wish success to the work on Jupiter which is foreshadowed in the Report.

The work of the Computing Section, as evident in their splendid *Handbook,* needs no words of mine to add to its laurels. The Section has called to its aid a calculating machine designed by one of its members, an event possibly without precedent in an amateur society, and one which gives promise of immense utility in the prediction of Occultations: and Dr. Comrie's work in connection with the revision of the *Nautical Almanac* is of national, even world-wide, importance.

I have, I fear, little to say of my own Section, except to appeal once more for some of the thousand-and-one "dodges" which must be known to so many observers and might be of use to so many others. May I give just one example of my meaning? I have used high-power eyepieces for many years, and still would like to know how to clean them without risk of scratches. The slightest scratch sadly impairs the efficiency of these tiny lenses; and users of reflectors, in particular, would, I think, be grateful for such information.

To turn for a moment to our other spheres of work. The Meetings of the Association have been well attended and full of interest: and at the January Meeting we had the pleasure of welcoming Prof. H. F. Newall, F.R.S., who recently vacated his Chair of Astrophysics at Cambridge. His interesting account of his solar work will long be remembered. He was also our guest at the very successful dinner which followed that meeting. The *Journal,* under the able editorship of Mrs. Maunder, maintains its high position as one of the most important Astronomical publications of the day: and the continued usefulness of the Library, as well as of our collection of lantern slides, has been more than maintained. The Branches, both in Scotland and in Australia, have had a good year of useful activity.

We may, then, say that the Association is *alive.* If new blood is wanted, it is not to resuscitate a moribund invalid, but to give added strength to a healthy and hearty being. If I may, I would like to quote some words which I wrote some years ago in a publication well known to many of you:—

"Finally, '*Labor Ipse Voluptas.*' The amateur has been well defined by Professor Hale as 'the man who works in Astronomy because he cannot help it, because he would rather do such work than anything else in the world, and who therefore cares little for hampering conditions of any kind.' The list of such amateurs is by no means too long."

My predecessor in this Chair last year gave you an interesting and most valuable address on a subject on which he is eminently qualified to speak: the limitations of vision, considered from the standpoint of the optics and anatomy of the eye. It has occurred to me that it might not be altogether uninteresting to many of you if I were to take up the optical system which we use in our observations at a somewhat earlier stage in the journey of the light which, when it ultimately gives up its energy on the retina, is the source of our knowledge of what is happening in regions beyond the reach of our other senses. You may notice that I use the words "the Optical System which we use in our observations." It is often forgotten that the optical system involved ends, not at the eyepiece, but on the retina: the eye is a part, and a most important part, of the system. But Dr. Steavenson told you all about it last year, and so I will, on the present occasion, consider, in what I fear must be a rather sketchy manner, the vicissitudes encountered by the light received from the stars as it passes through the telescope.

Not many years ago I had the pleasure of listening to a Presidential address delivered to a Microscopical society. On that occasion the President took as his subject, "Microscopes," without further specifying the point of view he proposed to take: he excused his selection by saying that the advantage of such a choice of title was that you were not bound to say anything in particular, but that within fairly wide limits you could say anything you liked. I must say that I think his view of the matter has much to recommend it: so much so, indeed, that I propose to pay him the compliment of doing very much the same thing: for there is quite a lot to be said about telescopes—far more, indeed, than I am qualified to say, or have the time to say—far more too than you would have the patience to listen to. But in the action of a telescope, under ordinary observing conditions, things happen which you will not find described in the elementary textbooks, and it is to a few of them that I should like to draw your attention.

Fortunately we are not quite in the same position, when we consider the action of a telescope, as we are when we deal with the sister instrument, the microscope. In the case of the microscope we are confronted with the difficulty that we are wholly unable to verify our observations: the images which we have to interpret are those of objects which are far too small to be seen in any other way: their very existence would not be known but for the microscope: so we are entirely dependent on theory, and the matter is made still more difficult for us by the fact that the dimensions of many, if not most, of the objects observed with a high-power microscope are comparable with the wavelength of light, and, indeed, are often considerably smaller.

Thus the comfortable assumptions of the elementary textbooks on Geometrical Optics, and their view of a " pencil " of light as consisting of what are called " rays," travelling in a succession of straight lines from a point in the object to a point in the image, has to be abandoned. We have to start afresh from the wave-theory, and talk about " disturbances in the ether," and so on, with a keen eye to the effects of diffraction: added to which trouble we have to take into account the fact that we are not dealing with self-luminous objects, and are not justified, without careful theoretical consideration, in expecting our objects to behave as such. Even to-day experts are not agreed as to the mode of formation of the microscopical image.

I have dwelt thus far on the sister instrument—I might almost say the daughter instrument, for there are good reasons for believing that the early microscopes were simply slight modifications of the early telescopes, adapting them for viewing near objects—because I want to impress on you the necessity for accurate theory if we are to form correct notions as to the action of any optical instrument. The conception of " rays " of light will not take us very far: telescopic images, just as much as microscopic, demand a consideration of the nature of light itself—the wave theory, in fact—for their complete explanation. But it is a comforting thought that we are able, to a great extent, to verify our observations. We can, for example, make a critical examination of such an object as a newspaper placed on the further side of a field: we can ascertain, if not from the title, at any rate from the nature of the visible structure, that it is, say, the *Daily Telegraph* and not the *Daily Herald:* and we can then go up to the paper and verify our interpretation. Possibly in this instance we may find, on studying the " visible markings " on the paper that there are many things there which are previously unknown to us, and whose interpretation is not easy on any theory whatever: but we can, at any rate, convince ourselves that our instrument, if it does not tell us the whole truth, nevertheless does not—as sometimes the microscope does—tell us anything but the truth, and it is not straining analogy too far if we infer that the instrument as it stands gives us an accurate rendering of any object within its capacity. And as to the illumination, we are mostly, with the telescope, dealing with self-luminous objects, and when they are not so, as in the case of Moon and Planets, theory indicates that they may be taken as such: for diffraction by the structure of the object, if it exists, is quite imperceptible, while the light from any point in the object uniformly fills the aperture of our instrument, and thus an important condition is satisfied.

But let me go back to a phrase I used just now—" within its capacity ": for this is where theory becomes of the first importance. If you follow out the explanation given in Geometrical Optics of the action of a lens in forming an image, you will notice that it is assumed that each so-called " ray " of light issuing from a point on the object finds its destiny in a corresponding point in the image: and that, subject to errors

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due to faulty design and construction of the lens, as well as to certain other " aberrations," the image is a point-for-point representation of the object. For many purposes this may be considered to be the case: we can, for example, compute such quantities as the magnifying power, the extent of the field of view, and so on, on this simple assumption: and anyone wishing to design, say, a Cassegrain Reflector, will find this idea of " rays " quite adequate for his purpose. But when we try to explain the actual appearance of the image, and to estimate the ability of a telescope to exhibit fine detail, we have to abandon this simple idea, just as we have in the case of the microscope, and to take into account the " wave-nature " of light, and its consequences.

You know that a luminous point sends out a continuous stream of waves which are carried by a medium which is called the " ether," whatever that may be; its exact nature need not concern us. At any moment the wave issuing from the point occupies a spherical surface whose centre is the luminous point, and when the wave has travelled far enough it practically becomes plane: this is the case with the light entering our telescopes. As to what the exact nature of the wave may be, we need not inquire: it is sufficient to say that as the wave passes any point, the " ether " at that point is thrown into rapidly alternating opposite conditions; we might call them " crests " and " troughs." The interval in time between two successive " crests " passing the point is called the period of the wave, and the distance, measured along the direction of propagation, between two successive " crests " at the same instant of time is called the wave-length. For visible light the average wave-length may be taken as about 1/47,500 of an inch: as you know, colour is the subjective impression on our eyes of wave-length: blue light consists of shorter waves than red. For the present, however, it is convenient to assume that the light entering our telescope is all of one wave-length or colour: monochromatic, in fact.

Suppose now that a wave-train of light is advancing in the direction of the axis of our telescope: the waves will all be at right angles to the axis, and, just before they strike the object glass, plane: just after, they will have been converted by the object glass into spherical waves, whose centre—assuming that the corrections of the object glass for the particular wave-length are perfect—will be the focal point. According to Geometrical Optics, all the light will be focussed accurately on the focal point: we shall have a mathematical point-image. This, if it were so, would be very nice, as we could in that case magnify a sufficiently bright object as much as we please, and the resolving power would be unlimited; but unfortunately Geometrical Optics fail us, for when we examine the image with a fairly high-powered eyepiece, we at once see that instead of being a mathematical point the image consists of a bright disc of quite appreciable size, surrounded by one or more bright rings concentric with it. Obviously this fact sets a limit to the power of our telescope to show separately two stars which are very close together in the sky: for if these "spurious discs," as they are called, overlap, no eyepiece, however powerful, can make them look separate. We have got to take what the object glass gives us, and, as we shall see presently, this depends on its diameter.

It is, then evidently of interest and importance to determine the size of the spurious disc. This is a matter calling for the use of some fair mathematical skill: but it so happens that in the rather unpractical case of a square object glass the treatment is quite easy, and I will try to show you how the result is arrived at in this particular case. Let us suppose that we have a telescope with a square object glass directed to a star in the Zenith, the image of the star being in the centre of the field of view, and therefore on the axis of the instrument, figure 1. The light from the star will, just outside the object glass, form a plane wave at right angles to the axis, and at all points on this wave-front the ether-disturbance will be in the same direction and of the same amount. Geometrical optics indicate that the disturbances on this wave-front will all be concentrated on a point $F$ (the "principal focus" of the object glass) on the axis. But we must not make the assumption that this represents the true state of things: we have to apply theory to determine what happens at points in the neighbourhood of $F$, before we can form any idea as to what the image is really like: we are not entitled to say that there will be no light except at $F$.

Suppose, then, we take a point $P$, near $F$, and in the focal plane, and that the angle betwen the line joining it to the centre of the object glass, makes an angle $\theta$ with the axis. If any disturbances reach it, Geometrical Optics indicate that they must at some previous instant of time have all been situated on an inclined plane which you see in the figure, inclined at the same angle $\theta$ to the horizontal wave-front, and cutting the wave-front along a line at right angles to the axis. This inclined plane is *not* a wave-front: for it is clear that the disturbances all over this slope will not be the same, but will depend on the distance we go up the plane. At the lower edge of the plane the light from the star will have travelled beyond the assumed wave-front, and the disturbance will be in a more advanced phase: along the top edge, it will be less advanced. The total effect at $P$ will be the sum of all these disturbances: and we have to see whether we can find out something about it. In general the effect at $P$ will not be zero: but if we divide up the slope into many horizontal strips of (small) equal width, we can in general find, for any strip, another along which the light disturbance is of the same intensity but of opposite phase: and this will take place when the phases of the disturbances in the two strips differ by half a wave-length. We can, then, as a rule, get rid of some of the effect at $P$ by taking as many pairs of strips as possible which satisfy this condition, and letting them cancel out: but in general there will be a certain number of strips left which have no mate, and whose effect will therefore reach $P$: so that $P$, in general, will not be a point of darkness, which of course means that the image of the star will not be a mathematical point.

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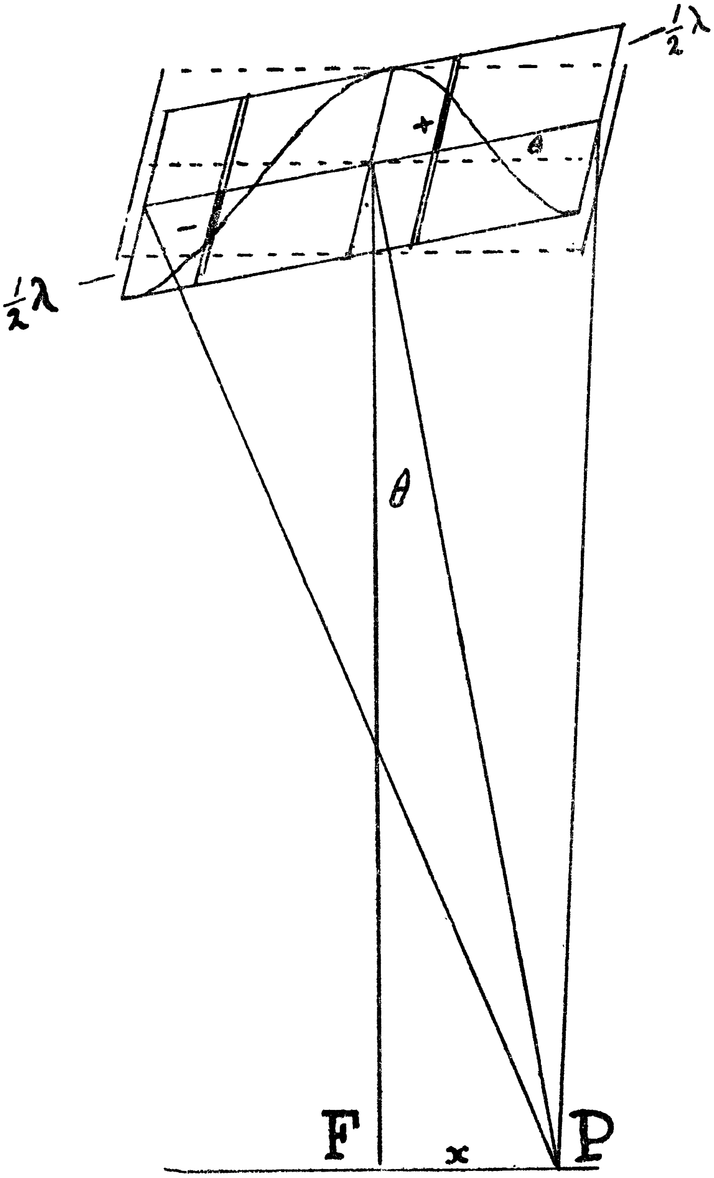

FIG. 1.

PLATE I.

*To face page 9.*]

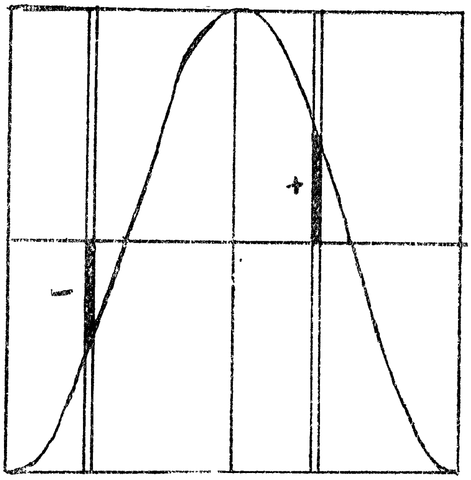

FIG. 2.

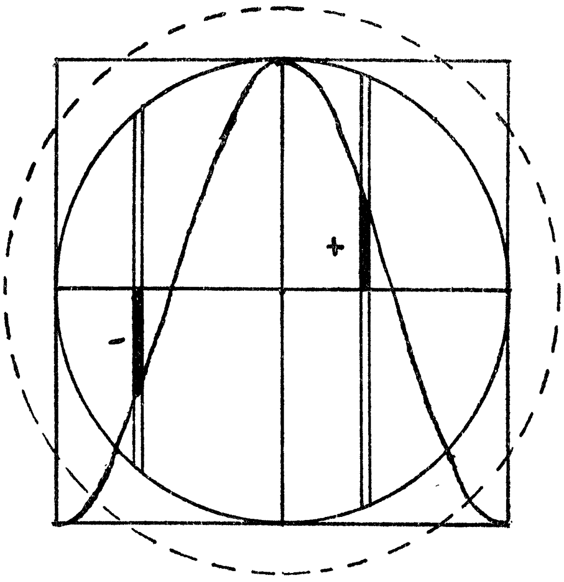

FIG. 3.

PLATE II.

But if we alter the position of $P$, we alter the angle $\theta$, and it is clear that we shall alter the distance between two strips which have, together, no effect at $P$.

Now a little consideration will indicate that if we incline the plane so that, as we go from the lower edge to the middle, or from the middle line to the top, the light-disturbance undergoes a change of phase of half a wave-length, we can entirely fill the plane with strips which mutually cancel each other: figure 1 shows this condition. I have drawn the curve which indicates the phase of the disturbance so that there is a trough at the bottom of the slope, a crest half-way up, and again a trough at the top: and figure 2 shows the same thing in plan, and also the shape of the curve which gives the condition of things at any moment in the period of the light. You will notice that it is a *sine* curve. The total disturbance at any moment in any strip depends on two things: on the area of the strip, which for a square object glass is the same for all strips, and on the disturbance at any point on the strip, which depends on the phase and, as shown in figure 2, is proportional to the distance to which the curve has risen or fallen with reference to its mean line. The disturbances for the pair of strips under consideration are shown by the dark lines marked + and −.

Now to fill our plane completely with self-cancelling pairs of strips, and thus to get darkness at $P$, we must have the plane so inclined that we get one, two, three, etc., complete wave-lengths' difference between the light at the top and that at the bottom: the square in figure 2 must contain a whole number of complete curves. In figure 2 I have drawn it so that it contains one: when there is a trough along the bottom edge, the next trough has just reached the top. As shown, then, there will be darkness at $P$: and although I have assumed, for drawing the figure, a crest at the middle, there was no necessity to do this, and any other arrangement would have given the same result.

Now in the case considered—a whole wave-length difference in phase between top and bottom—it is clear from figure 1 that the distance $FP$ bears to the focal length of the object glass (which I will call $f$) the same ratio that half a wave-length bears to half the side of the square, or a whole wave-length to the Aperture. Calling the wave-length $\lambda$, and the Aperture $A$, we have, for darkness at $P$,

$$\frac{FP}{f} = \frac{\lambda}{A}$$

$$\text{or, } FP = \frac{\lambda f}{A}$$

and since we can get the same result by making the phase-difference any number of whole wave-lengths, we get points of darkness at distances $FP$ equal to

$$\frac{2\,\lambda f}{A},\quad \frac{3\,\lambda f}{A},$$

and so on: everywhere else in the focal plane there will be *some* light, although it can be shown that after we have passed the first two points of darkness, except for very bright stars—and often after the first point—the illumination is inappreciable.

So far, for the case of a square object glass, the matter is fairly simple: but when we come to the usual state of things—a circular object glass—things become much more complicated. In figure 3 I have drawn the circle inscribed in the square to represent the object glass, and you will see that a different state of things prevails. So far as the *phase* and amount of the disturbance at any point in a strip is concerned, we still have a perfect balance: but the strips are now of unequal length, and therefore of unequal area; there is a balance for phase, but not for the total amount of light-disturbance in each strip. We shall not have darkness at *P*. In fact, a glance at figure 3 shows that the crests will over-balance the troughs of the waves, and to restore the balance we should have to have a few more strips beyond the top and bottom of the slope: we shall have to make the object glass somewhat larger. How much larger is a matter for careful calculation, and we shall find ourselves involved in some rather severe mathematics. On working it out, we find that to keep *P* (the darkness-point) in its place, we shall have to make the diameter of the object glass rather more than a fifth greater than the side of the square: the ratio is 1·22 to one, which happens to be the proportion between the diameters of a penny and a halfpenny. Or, alternatively, if we keep the diameter of the object glass equal to a side of the square, we shall have to push *P* a little further away, in the proportion just mentioned: the formula for the distance of *P* from the axis is now

$$\frac{1\cdot22\,\lambda f}{A},$$

and since we have symmetry every way we shall have a bright circular disc centred at *F*, surrounded by a dark ring of radius given by this formula.

You will notice that the radius of the disc is proportional to $f/A$, the ratio of the focal length to the aperture. It follows that all object glasses and mirrors for which this ratio is the same will give dark rings of the same size, whatever their aperture. Refractors, for example, usually have a ratio of about 15 to 1: a little arithmetic shows, that with average-wave-length, the radius of the dark ring is actually about 1/2640 of an inch. (May I say in passing that it is rather wonderful that the operations of grinding and polishing are capable of producing surfaces of sufficient accuracy to condense the light into this tiny space.)

What the angular value of this will be is a question of focal length entirely, and here we can form some idea as to why a telescope of large aperture has a greater resolving power than one of small. Let us take a special case. Suppose two refracting telescopes, one of 2 inches aperture and 30 inches focal length, and the other of 10 inches aperture and 150 inches focal length: in each the focal ratio ($f/A$) is 15 to 1. Suppose that we are

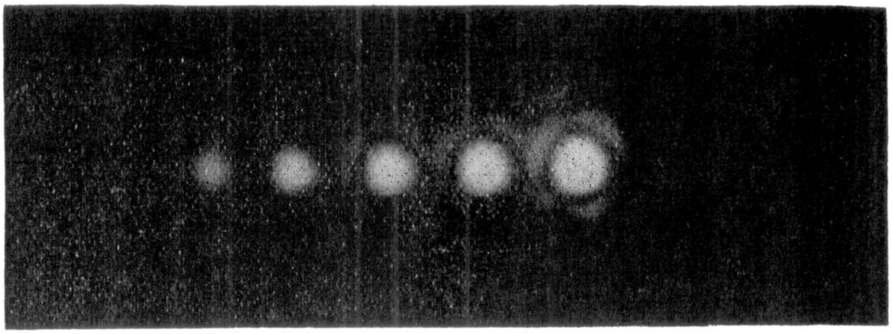

Fig. 4.

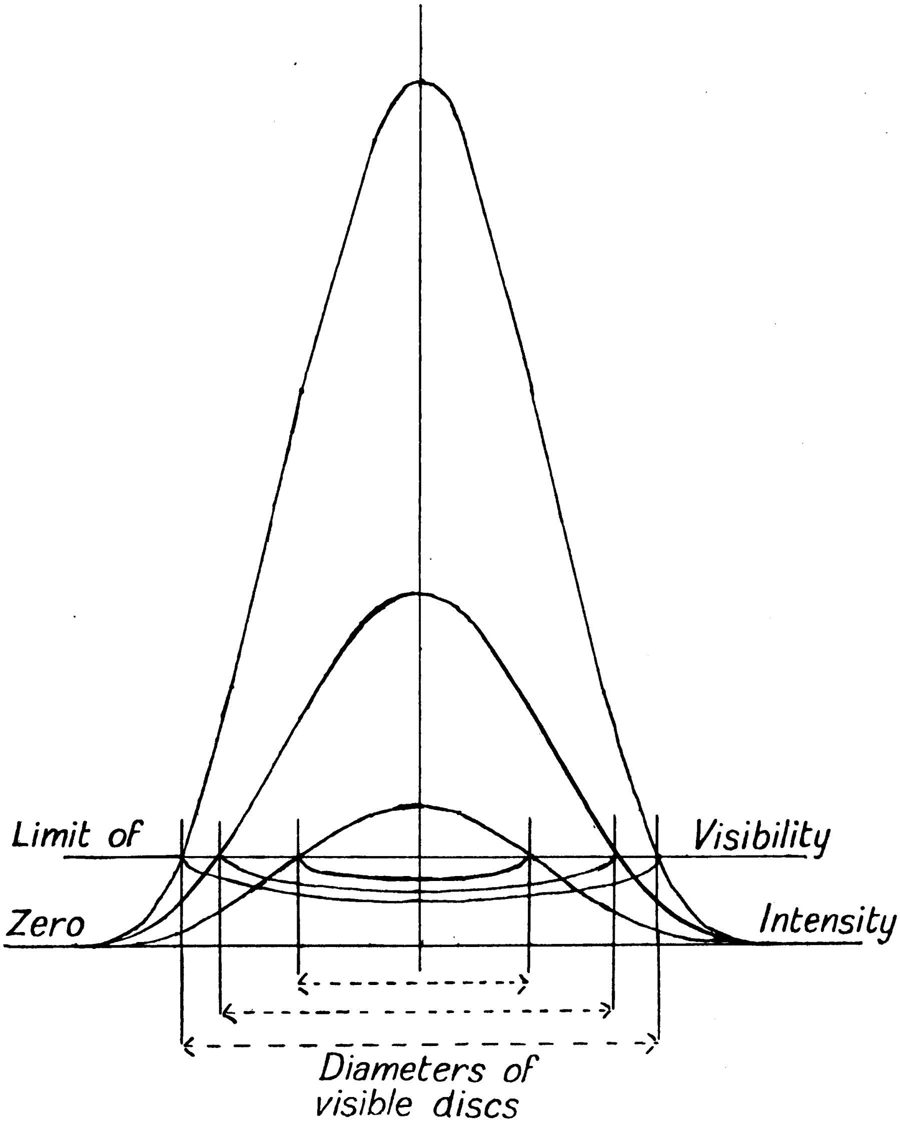

Fig. 5.

Plate III.

examining a double star whose components are 1″ apart in the sky. In the 2-inch, the actual (linear) distance between the centres of the images will be 1/6900 of an inch: in the 10-inch, 1/1375. But in each case the diameter of the dark ring will be 1/1300, and—to anticipate a little— the diameter of the visible disc of the star will be about half this, or 1/2600. In the 2-inch the discs will overlap considerably, and no eyepiece, however powerful, will separate them: in the 10-inch they will be well separated, even with a moderate power in the eyepiece.

We may now consider the visible disc of the star: and it must be remembered that with even the largest telescope at present available the real disc of a star is totally invisible: its diameter is in every case smaller than the "spurious disc" which we see in the telescope. The size of this spurious disc, as seen by the eye, depends on several things. In the first place, the intensity of the light at the focus is greatest in the centre, and falls off as we go outwards, becoming zero at the dark ring. After a certain distance the light in the disc will be too feeble to be perceived: and how far from the axis we have to go before this occurs depends on the brightness of the star, as well as on various physiological circumstances in the eye, with which I do not propose to deal.

I will show you, on the screen (figure 4), some photographs of actual star-discs, taken with a telescope of very small aperture and very long focal length. In order to make the star-discs on the plate sufficiently large, it was necessary to use an effective focal length of about 40 feet, and an aperture as small as 1/10 of an inch: a ratio of focal length to aperture of 4800 to 1. You will notice two things: first, that the star-discs are not sharply defined, but have a fuzzy edge: secondly, that they become smaller as the brightness of the star diminishes. The difference between any two successive images represents a drop of exactly one magnitude, or a light ratio of 2½ to 1. Obviously then the resolving power of a telescope will, on the whole, be greater for faint stars than for bright, as the discs, being smaller, will not be so liable to overlap. As to the fuzzy edge of the discs, the effect of this is, probably, mainly physiological: it would depend on the sensitiveness of the eye—or of different parts of the retina—to light of diminishing intensity.

The reason for this diminution of the diameter of the visible disc with diminution of the brightness of the star will, I think, be evident from figure 5. Here I have drawn the intensity curves from the axis to the dark ring for three stars differing by a whole magnitude; and I have drawn a line parallel to that of zero intensity to represent the least intensity which the eye can perceive. This latter line is of course quite arbitrary, as is also the height of the curves. But you see that the circles in which the curves cut the line of limit of visibility diminish in diameter as the height of the curve decreases: faint stars, then, have smaller discs than brighter, and the diminution from one magnitude to the next below is more marked as the brightness decreases.

A third feature of the image of the star, which, unfortunately, I have not been able to show in the slide, is, that since the size of disc and dark ring depends on the wave-length—is, in fact, proportional to it—the disc and ring formed by light near the red end of the spectrum is considerably larger than that formed by light from the violet end. You can easily verify this for yourselves by looking at a suitable artificial star—the sun, for example, reflected in a wireless valve—through an aperture of about 1/100 or 1/150 of an inch in diameter. The disc, which will be very perceptible if the aperture is sufficiently small, will be seen to have a red edge, and the diffraction ring surrounding it will be seen to be, in reality, an annular spectrum with the violet inside and the red outside. This colour effect has nothing whatever to do with the object glass or its colour correction: it is equally produced by a reflector, which of course is perfectly achromatic. It is not very easy, however, to see the colour on an actual star, as the aperture has to be very much reduced before the disc is large enough to show the effect, and there is not much light available.

But this inequality of the diameter of the disc in light of different colours has an effect on the resolving power of the telescope: if we could use only light from the extreme red end of the visible spectrum, we should have the diameter of the disc (assumed for the moment to be half that of the dark ring) about 1/1750 of an inch in diameter: while with light from the extreme violet it would be about 1/3500 of an inch. This, of course, is not very practical, since we cannot use monochromatic light of these extreme wave-lengths: but under actual conditions the theoretical effect is still rather remarkable: taking the effective visual wave-lengths of stars of types $B_0$ and $M_0$ as being in the same ratio as the effective photographic wave-lengths as given by Davidson and Martin—which may, however, be an unsafe assumption—we ought to have the resolving power of a telescope something like 11 per cent. greater with a $B_0$ pair of stars than with an $M_0$: whether double star observers have observed this effect I am not aware. It would mean that if an $M_0$ pair could just be resolved with a 10-inch, it would only require a 9-inch to deal with a $B_0$ pair of the same angular separation.

The question of the separating power of a telescope is one which has given rise to a certain amount of misconception. We often hear of the "theoretical" resolving power, as if it were some exact quantity which could be calculated apart from observation: and there is a well-known formula usually attributed to Dawes which states that the least separable angular distance between two stars is

$$\frac{4''{\cdot}56}{A}$$

$A$ being the aperture of the object glass or speculum in inches. This formula has a mathematical appearance, perhaps due to the two places of decimals: but it is worth while to inquire what basis it has in theory.

*To face page* 13.]

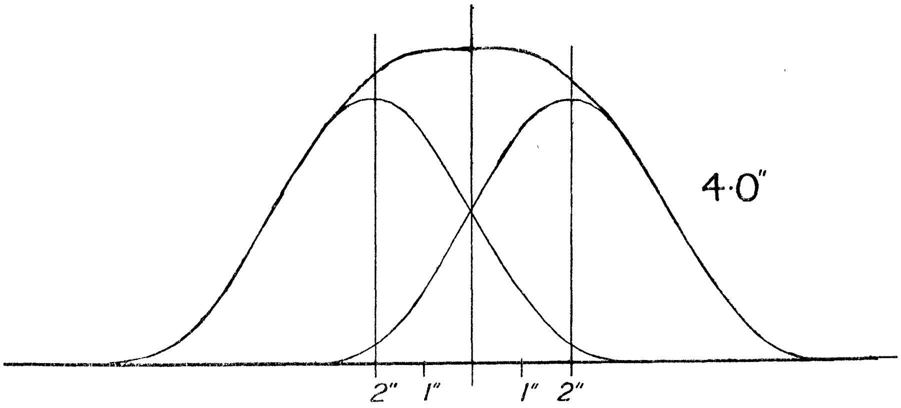

FIG. 6.

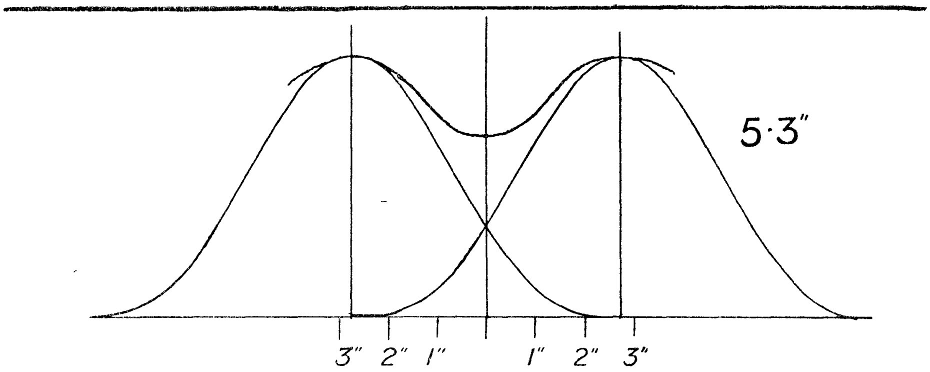

FIG. 7.

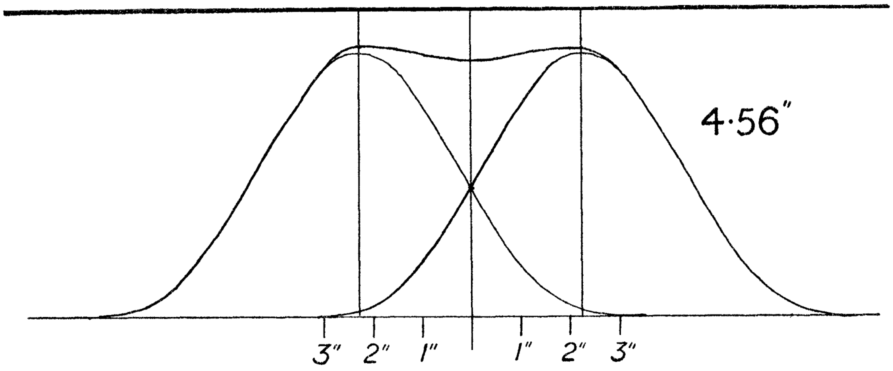

FIG. 8.

PLATE IV.

Several of the books give the statement that two stars are usually considered to be separated when the centre of the disc of one falls on the dark ring of the other. I will not trouble you with the arithmetic, simple though it is, but merely say that with 47,500 waves to the inch this corresponds to the formula

$$\frac{5''\cdot3}{A}$$

and I at once hear my double star friends saying that this will not do, and that they habitually measure double stars considerably closer than this: in which I entirely agree with them.

Not long ago, however, I came across an idea which was new to me, and which—as far as I know—was first suggested by Professor A. W. Porter, F.R.S., in a paper read before the Royal Miscroscopical Society in 1908. I have not seen the original paper, but he himself quotes it in a much later paper, published in 1920.

His idea is briefly this: that we may consider that we have arrived at the limit of resolution of two equally bright luminous points when, as we pass from the centre of one disc to the centre of the other, a diminution of light at the half-way point just ceases to be perceptible: or, if you like, at the half-way point we must be losing the light of one star as fast as we are gaining the light of the other. In figure 6, I show the intensity curves drawn so as to intersect at the points where, for an appreciable distance, they are virtually straight lines: that is, at what is called their "points of inflection." Since the curves are identical for two equally bright stars, you see that on either side of the point of intersection one curve rises as fast as the other falls: so the condition just mentioned is satisfied.

The point of inflection can be found mathematically if we know the law of diminution of intensity of the curve from centre to dark ring, and the figure has been computed from this. The total intensity, at any point in the combined discs, is obtained by adding the ordinates of the two curves, and is shown in the upper curve: you will see that for a considerable distance on either side of the half-way point there is no appreciable alteration in the total intensity of the light, so that Prof. Porter's limit would be just about the separation shown: you will notice that this is 4″ for in drawing the curves I have assumed an aperture of 1 inch. The "Porter limit," then, for two equally bright stars, is in the neighbourhood of

$$\frac{4''\cdot0.}{A}$$

Figure 7 shows the condition when the stars are 5″·5 apart, and the centre of the disc of one falls on the dark ring of the other. Evidently here the diminution of light at the half-way point is considerable, and we should see the stars separately even if they were considerably closer: and figure 8 illustrates the "Dawes limit" of $4''\cdot56/A$. Here you see that the diminution of light, though not great, is quite appreciable: a normal eye would probably easily recognise the darkening and see the two stars separately. But, as Prof. Porter points out in his later paper, "the question of resolving power is not an exact branch of science: the human element enters: and in consequence no exact statement can be made." Certainly the formulation of an exact law giving the resolving power for a pair of stars of any unequal brightness would be a rather complicated problem, if I may judge from the amount of computation I had to do to arrive at the point of intersection for two equal stars.

Before leaving the subject of resolving power I should like to say a few words about that of a reflector. I will quote from Webb's *Celestial Objects*, Vol. I, p. 7:—

"Reflectors somewhat surpass Refractors in this respect, as theoretically they ought to do: but they are apt to be more troubled by rings and flares, and by scattered light." I fear that users of reflectors have only too much reason to endorse the latter part of the sentence. In figure 9 I have drawn on a greatly enlarged scale, the portions of the curves for the two types of instrument, in the neighbourhood of the dark ring and a little beyond it; the reflector curve is computed for the average ease in which the minor axis of the flat mirror is one-fifth of the diameter of the speculum. The figures below give angular distances from the axis for an aperture of one inch: for any other aperture we must divide the figures given by the aperture in inches.

The curves show two things: first, that the resolving power of the reflector is about 5 per cent. greater than that of the refractor, the disc and ring being smaller in this proportion: and, second, that since the gradient of the reflector curve beyond the dark ring is the steeper, the first bright ring surrounding the disc is brighter in the reflector than in the refractor: the presence of the flat causes this effect, to which Webb calls attention in the words just quoted. With a larger flat the resolving power is further increased: but the bright ring is still brighter.

It is, as a rule, a matter for regret that interference, which is at the root of the formation of the spurious disc and dark ring, should set a limit to the performance of our telescopes by blurring the images of stars: but there is an old proverb "*fas est ab hoste doceri*," which may be freely translated "it's a nuisance, but let's see if we can't learn something from it"; and it was never more applicable than in the present case. For this very property of interference may be made to give us priceless and long-sought-for information. In 1868 Fizeau—whose name you will remember in connection with the determination of the velocity of light—suggested a method, which was successfully applied by Michelson in 1891, of measuring the angular separation of two stars, even if no existing telescope could show them separately. And the method has done even more: it has at last lifted the veil which hid from our eyes the actual angular diameters of stars: and we are now able—though as yet in only a very few cases—to measure this by direct

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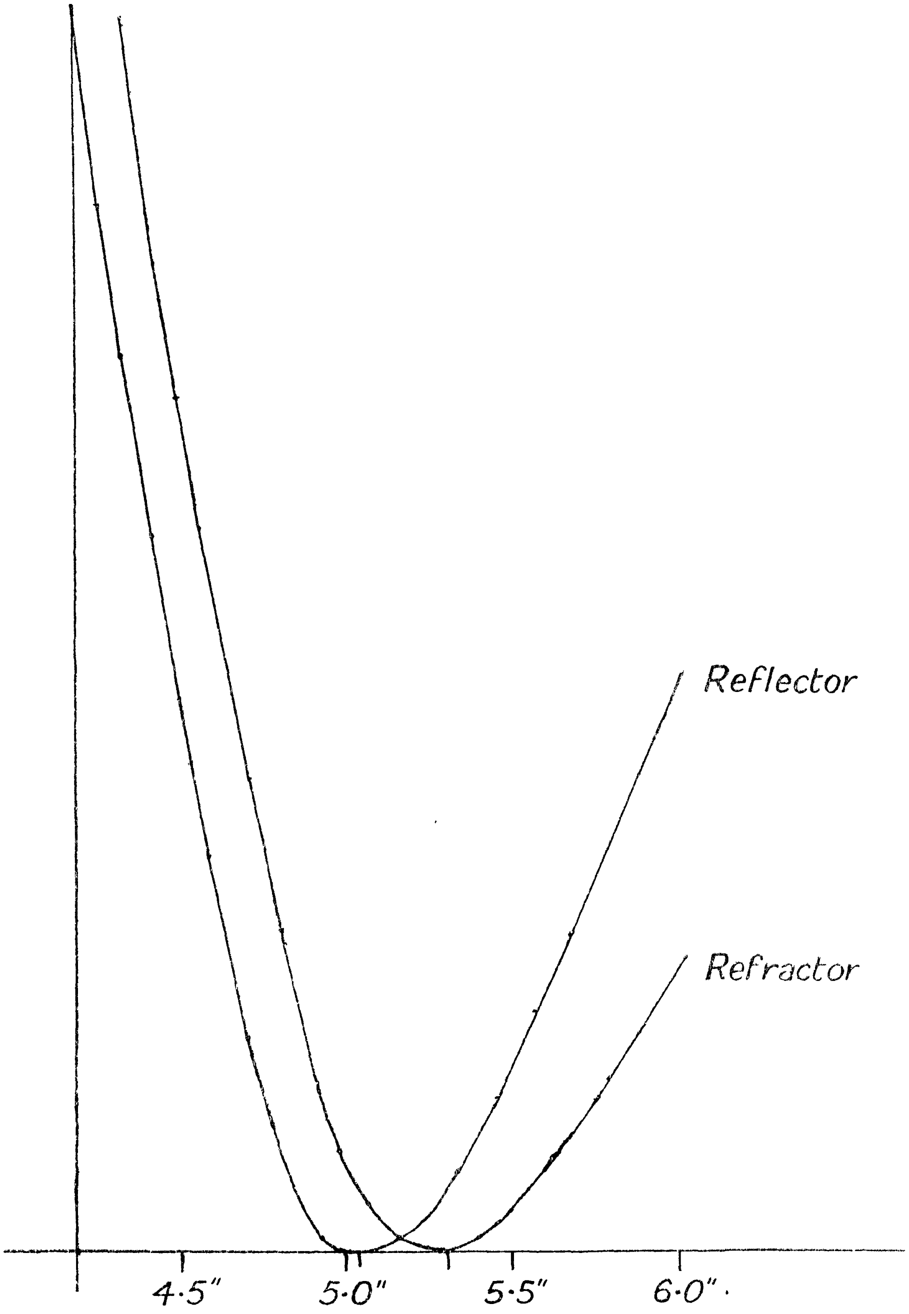

Fig. 9.

Plate V.

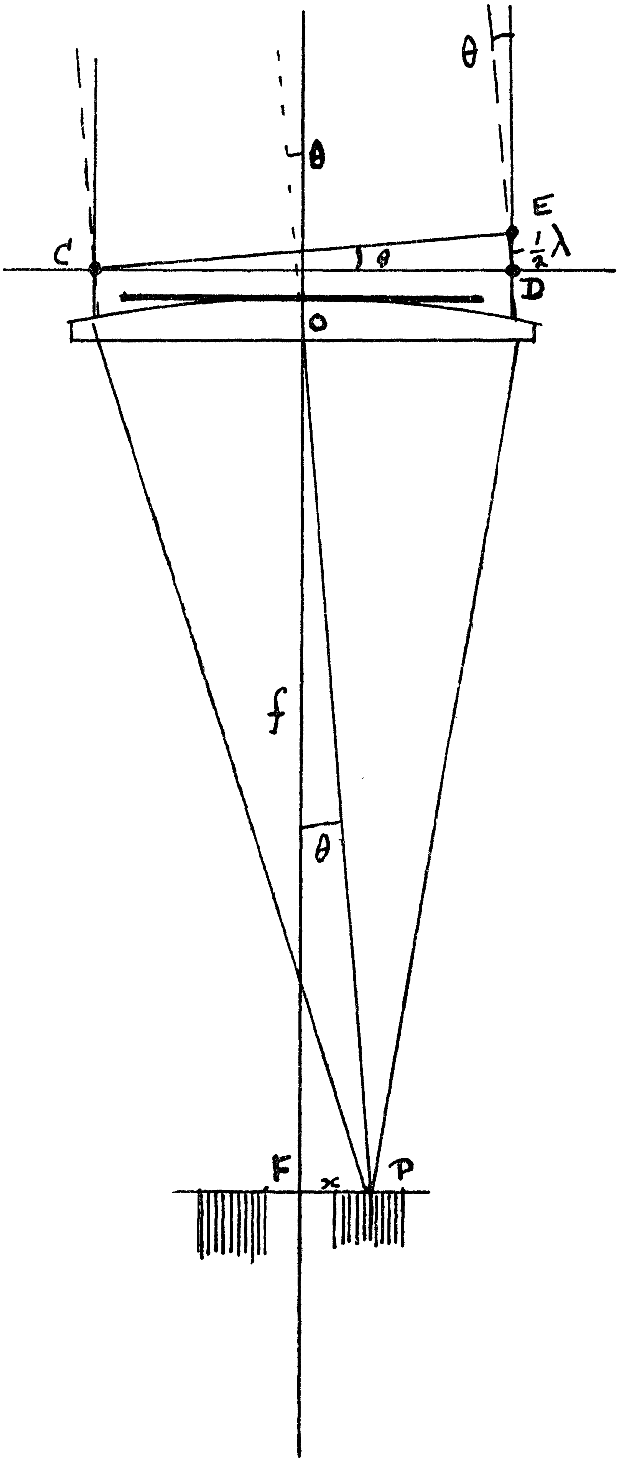

Fig. 10.

Plate VI.

observation. You will all have heard of the Interferometer which has, in the hands of the Mt. Wilson observers, given such remarkable results—remarkable both in themselves and as confirming theory—in the direct measurement of such huge stars as Antares, Betelgeuse, *o* Ceti, and a few others. Although the results of these observations have been known for some time, it is not altogether easy to find, outside the pages of scientific journals, any simple explanation of the method: and I propose, therefore, if you will allow me, to spend a few minutes in an attempt to make clear to you what the observation is, and how and why it gives the results that it does.

We know that the smaller the angular separation of two stars, the larger is the aperture required to resolve it: and in such resolution the outside zone of the objective is chiefly concerned. This being the case, cannot we make an object glass that only possesses this outside zone? and the answer is that if we admit the light only through two small apertures at the ends of a diameter of the object glass, we are able, although we cannot observe the two stars separately, to obtain information as to their separation which, for many purposes, is sufficient for our requirements.

The diagram I am now going to show you (figure 10) will, I hope, help to make this matter clear. You see that here I have covered up the whole aperture of the object glass, with the exception of two small openings at the extremities of a diameter, through which the light is allowed to pass. Each of these apertures will act as a complete small object glass, and form at the focus a complete system of disc and rings in the normal way: and since each of these images will be formed at the focus of the whole object glass, they will coincide, and we shall have their combined light. Of course, since the apertures are much smaller than the whole object glass, we shall have a disc and rings of considerable size, much larger than those formed by the object glass as a whole: but this, so long as the apertures are large enough to give enough light, is rather an advantage than otherwise: we shall have a large screen on which we can look for any further effects.

Now let us look into the distribution of light on this screen. Take, as before, a point *P* not far from the centre of the disc, and suppose its distance from the axis to be " $x$ ": angular distance from the axis, as before, " $\theta$." The whole argument then proceeds, with slight modification, just as in the case of finding the diameter of the dark ring for a square object glass. The disturbance at *P* must at some previous instant, just before it entered the object glass, have occupied a plane inclined to the plane of the wave-front at the same angle " $\theta$ " as the line from *P* to the centre of the object glass is inclined to the axis.

Now let us suppose (and here is the modification of the former argument) that this plane cuts the wave-front at one of the apertures, and is a *half* (not a whole) wave-length behind it at the other: then the two disturbances meeting at *P* will be " out of step " to the extent of half a wave-length: if one is a crest, the other will be a trough, and vice versa. They will thus exactly balance out, and we shall have darkness at *P*. If we make the position of *P* such that the disturbance is out of step by a whole wave-length, we shall have the light from one aperture added to that from the other, and *P* will be a point of "brightness." As we go further afield it is not hard to see that we shall have a succession of alternate brightness and darkness: a set of fringes similar to, though not so sharply defined as, those in figure 11. Here again, since the spacing of the fringes evidently depends on the wave-length of the light, that is, on its colour, we shall have colour effects similar to, and even more pronounced than, those seen in the spurious disc of a star.

I think you will be interested in an actual photograph of the fringes (figure 12), formed by two apertures about 0·07 inch in diameter, and half an inch apart: though not perhaps very sharp, they are pretty evident, though their intensity, owing to the colour effect just mentioned, falls off towards the margin of the disc. Had I been able to use monochromatic light, the fringes would have been much sharper.

Now we may consider the spacing and width of the fringes. In figure 10 you will see that the distance of the centre of the first dark fringe from the axis has the same ratio to the focal length as a half wave-length has to the distance between the apertures: each of these ratios is the "circular measure" of the angle $\theta$. If then $x$ is the distance from the axis of the centre of the first dark fringe on either side, we have $x/f = \lambda/2D$, so that $x = \lambda f/2D$. The dark fringes may be taken to be as wide as the bright ones, so that we may say that the width of any fringe is $\lambda f/2D$.

We may now consider how these fringes may be made to give us information concerning the angular separation of two stars. If we are looking at two stars close together in the sky, and we place the line joining the apertures parallel to that joining the stars, we shall have two sets of fringes, one from each star, and they will be superimposed one on the other.

The operation of measuring the separation of the two stars consists simply in so adjusting the distance between the apertures that the fringes disappear: when this is the case, the separation of the stars can be at once deduced from the distance between the apertures.

Now the two sets of fringes are identical in width and spacing: so that to make them disappear we must so place one set relatively to the other that a bright fringe of one falls on a dark fringe of the other, and vice versa. Unless we do this, a combined set of fringes will be visible, the dark fringes of one set partly overlapping those of the other.

The actual relative displacement of the two sets, however, is fixed by the angular separation of the stars: for the angle between corresponding fringes of either set is obviously equal to this. To make the fringes disappear, then, we must either adjust their width by altering the distance between the apertures, or, as an alternative, alter the direction in which the fringes themselves lie with respect to the line from star to star, until they fit.

FIG. 11.

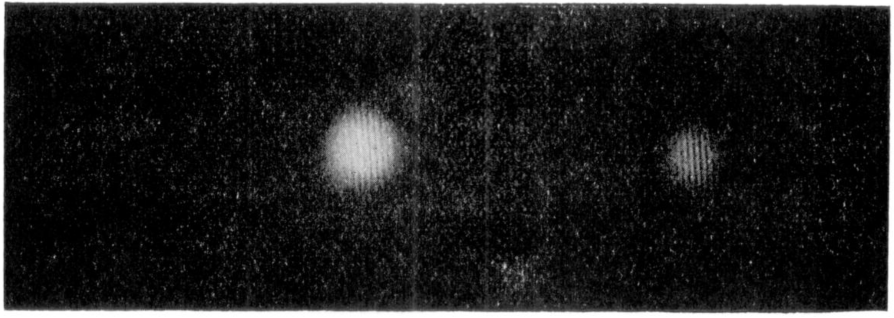

FIG. 12.

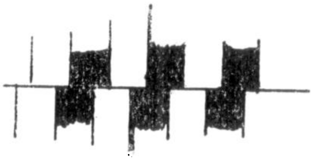

FIG. 13.

FIG. 14.

FIG. 13A.

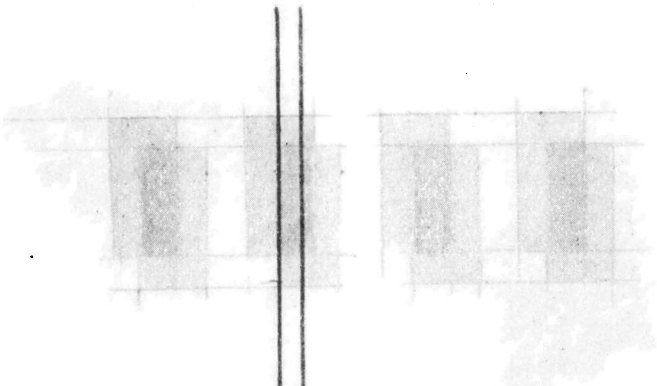

FIG. 14A.

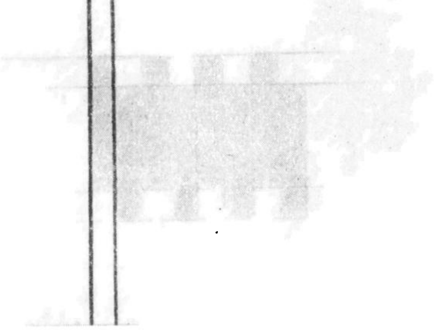

PLATE VII.

Figures 13 and 14 illustrate this point, but in these figures one set of fringes is shown below the other, instead of superposed on it. (See also figures 13A and 14A.)

In each figure the relative displacement of the fringes is the same; but in figures 13 and 13A the fringes are too wide, and the two sets do not cancel each other: we must therefore increase the separation of the apertures, and reduce the width and spacing of the fringes, until the dark fringes of one set exactly cover the bright fringes of the other, and no fringes are visible. (Figures 14 and 14A.)

When this is the case, the angular separation of the two stars will be, in circular measure, $x/f$, which equals $\lambda/2D$: to express this in seconds, we multiply by 206265, the number of seconds in the unit of circular measure.

In the instrument used by J. A. Anderson in 1920 for the measurement of close pairs, the second method—that of altering the direction of the fringes—was employed: his instrument could be adapted to any telescope. A screen, pierced with two parallel slits, was mounted in a tube near the eyepiece, this tube being capable of rotation around the axis of the telescope. (Figure 17.) The *slits* being initially placed parallel to the line joining the stars, the tube was then rotated through an angle $\phi$ so as to make the fringes disappear. The effective separation of the apertures is now $D \sin\phi$ : and here $D$ is not the distance between the slits, but that between their projections on the object glass.

Position angles can also be measured by this instrument: for when the slits are parallel to the line joining the stars (or the line joining the apertures at right angles to it) there will be no relative displacement of the fringes, and they will have their maximum intensity: similarly, if two directions are found for the slits, in which the fringes disappear, the line joining the two stars evidently bisects the angle between these directions.

Although with this instrument we can measure considerably smaller separations than we could with the telescope used in the ordinary way, the separation $D$ is still limited to the diameter of the object glass, and it has nothing like the capacity of the arrangement of mirrors presently to be described: and its scope is further limited by the fact that we only have available the light which can pass through the slits, which of course is very much less than that which would be admitted by the whole operture of the object glass.

But for stars which are sufficiently bright, the advantage of the method is considerable. Take, for example, the case of a 10-inch object glass, and let $D$ be 9 inches. The angular separation for which the fringes will vanish is given by the equation

$$x/f = \lambda/18:$$

from which we find that the least angular separation of the two stars that can be measured is 0″·241. The "Dawes limit" gives 0″·456 as the least separation visible with the whole object glass. We have, in fact, nearly doubled the resolving power of the telescope.

But we can make *D* as large as we please by employing a system of mirrors. That used at Mt. Wilson is shown in figure 15, and I am also showing you a photograph of the actual instrument. The principle is simple: on each side we have a pair of parallel mirrors, so adjusted that the images formed at the focus of the object glass (in this case, the 100-inch mirror) are coincident: and any shift in the position of a star will, since after reflection at two parallel mirrors the direction of the incident light is unchanged, be faithfully reproduced by an equal shift of the image at the focus.

We want so to adjust things that the half-wave-length difference between the two apertures shall be obtained: and however small "$\theta$" may be, we have only to increase the separation of the mirrors until the required difference is obtained.

In the actual instrument, which for solidity and stability is mounted on the tube of the 100-inch reflector, the mirrors are mounted on a steel beam placed across the upper end of the tube. The separation of the outer mirrors can thus be made as great as 20 feet, and the capacity of the instrument to measure a sufficiently bright double star is about that of a telescope of 40-feet aperture. And here I should like to remind you that the aperture of the 100-inch itself has nothing to do with the matter: the great telescope is merely a convenient mounting for the interferometer beam, and the separation of the outer mirrors is the essential thing.

The results obtained with the interferometer in the measurement of close double stars have been striking. Anderson's instrument, described just now, was used by him in 1920 to measure Capella, the result being $0''\cdot045$: previously all that was known was that Capella was a spectroscopic binary, although you may remember that the Greenwich 28-inch object glass was believed to show elongation of the disc.

Later, $\kappa$ Ursæ Majoris was found to be a binary of separation $0''\cdot083$, just after Aitken had found that it was a visual binary: and in 1925 Pease, with the 20-foot interferometer, resolved $\zeta$ Ursæ Majoris, obtaining a separation varying from $0''\cdot013$ to $0''\cdot011$, with a change of position angle of 45° in four days: in each of these instances agreement with the spectroscopic orbit was excellent. Further, in each case the measures, combined with the spectroscopic observations, gave values of the parallaxes agreeing well with those obtained in other ways.

If this were all that the interferometer had done, it would be a considerable achievement: but, as I indicated just now, it has done far more. Surprisingly few close double stars have as yet been found which are accessible to the instrument without being at the same time within the capacity of ordinary measurement: but the measurement of the apparent diameters of stars can be effected by no other instrument.

The problem is more difficult, but not essentially different. If a point of light in the sky is shifted, the fringes will shift, as we have seen: suppose that for an actual star we substitute a small source of light of the same total intensity, and that this is carried about over the now dark disc of the star. The fringes

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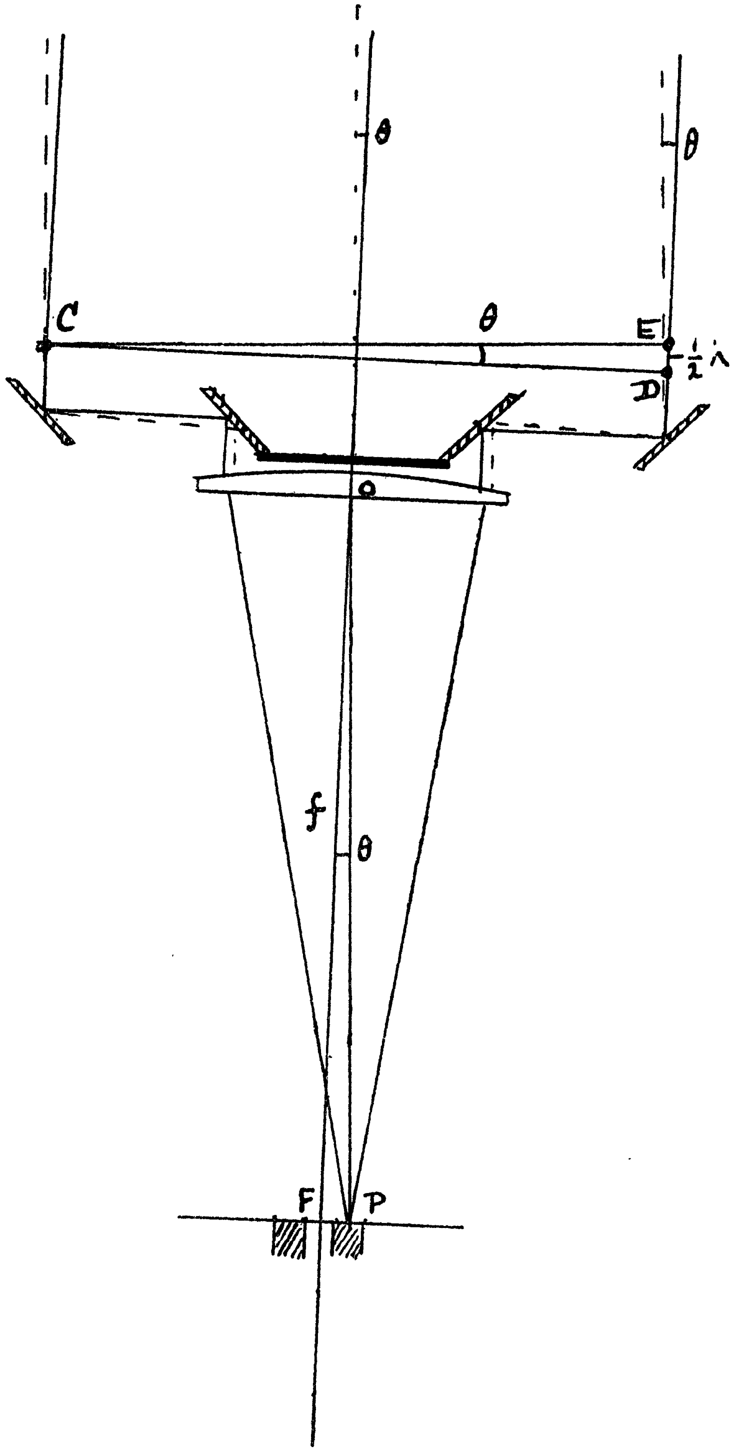

FIG. 15.

PLATE VIII.

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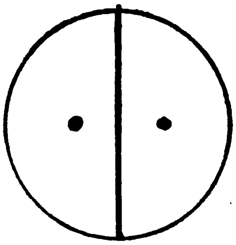

FIG. 16

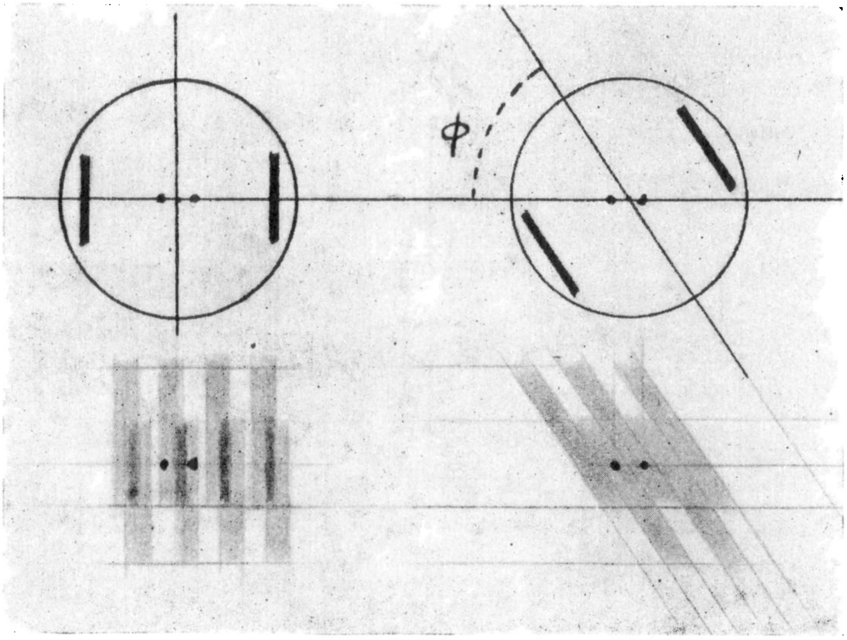

FIG. 17.

PLATE IX.

will shift, slightly it is true, but perceptibly with a sufficiently high power, as the light source changes its position. If we suppose that in an instant of time the light source can take up every possible position on the disc, we shall see the fringes blurred by its motion: in other words, the light from various parts of the disc of a star produces a blurred system of fringes, and it may be shown mathematically that a distance between the apertures can be found at which the fringes will blur themselves out of existence. Calculation shows that this separation of the apertures is that for which the fringes would disappear for a double star, if its separation were about 2/5 of the actual angular diameter of the star (figure 16): and this leads to the formula

$$1{\cdot}22\,\frac{\lambda}{D}$$

for the angular diameter of a star reckoned in "circular measure," which must be multiplied by 206,265 to express it in seconds. If, for example, the distance $D$ between the apertures for which the fringes disappear is 15 feet, or 180 inches, we find, on working it out, that the apparent angular diameter of the star is 1·22 times 206,265, divided by 180 times 47,500; which comes out to $0''{\cdot}0295$. (This applies to a star disc of uniform brightness: for limb-darkening similar to that of the Sun, we shall have to write 1·43 instead of 1·22.)

The wonderful results obtained in this way are well known to all of you: how the apparent diameter of Betelgeux varies from $0''{\cdot}034$ to $0''{\cdot}047$: how Antares and $\alpha$ Herculis are larger than the orbit of Mars and how $o$ Ceti shows the largest apparent diameter of all, $0''{\cdot}056$. Nor need I remind you of the remarkable way in which these measures have confirmed the theoretical values computed from the effective temperatures obtained spectroscopically. What is not, I think, generally realised is that the method was put into practice as long ago as 1891, when Michelson and Hamy measured the apparent diameters of the four great satellites of Jupiter in this way. The principle of the instrument has long been known: but it is only recently, and in large measure through the use of the 100-inch to supply a mounting for it, that it has been successfully applied to these minute angles.

Nor is the capacity of the interferometer altogether limited by the available separation of the mirrors: it is possible to plot a curve showing the degree of visibility of the fringes for any given separation of the mirrors, and, by extrapolating with the help of this curve, to predict the separation at which they would disappear, and thus obtain a very fair estimate of the actual angular diameter. Further, an instrument of much greater capacity has for some time been under construction, with mirrors of 18 inches aperture and a possible separation of 50 feet. When this instrument comes into use we may expect considerable additions to our knowledge.

*Dr. W. H. Steavenson*: I would like to propose a hearty vote of thanks to the President for the splendid address to which we have just listened. I have at least three reasons for being glad of this opportunity: First, the President is an old personal friend of mine; secondly, he has dealt with astronomical optics, which to me has always been a particularly fascinating subject; and thirdly, it is specially valuable to the Association as being the only one of our Presidential Addresses dealing with such a subject. Captain Ainslie has to-day shown himself to be very much at home in the theoretical side of his subject, and we do not forget that many years ago he established for himself a reputation as a skilful constructor of reflecting telescopes. I have had many opportunities of appreciating the excellence of the 9-inch mirror which he now uses.

Often it happens that those who make instruments do not themselves use them, and their interest flags as soon as the instrument is made. Captain Ainslie is one of the rare exceptions. The delight of observing is greatly enhanced when the observer can feel that he is the maker of his own instrument, and I congratulate Captain Ainslie on the satisfaction he must feel whenever he uses the telescope that he has made, whether in the course of his regular observations of Jupiter or upon such important and exciting occasions as that on which he observed a star through the outer ring of Saturn.

I am sure we all wish to thank our President for the treat he has given us this afternoon.

*Major Hepburn*: It is a pleasure to me to perform the duty of seconding the vote of thanks to Captain Ainslie for his address. It is a curious coincidence the we should have two Presidents in succession, each of whom is able to treat as an expert of the two components of the optical train, the eye and the instrument, and we are to be congratulated on the opportunity of listening to two such addresses dealing with a subject of such interest to us as observers. Dawes was an amateur astronomer, as we are, and we have all heard of "Dawes' limit," but we have certainly never had it so clearly explained before. I beg to second the vote of thanks to the President for his address.

*The President*: I thank you sincerely for listening to my address to you, which has been a labour of love, and I tried to give something worthy of your attention. I thank Dr. Steavenson and Major Hepburn for their expressions.

*Mr. Longbottom* proposed and *Mr. Richter* seconded the re-election of the Auditor, Mr. Suttill.

*Major Levin* read the certificate of the Scrutineers of the Ballot, and the names of the Directors of the Observing Sections, who were confirmed in their office.

*The President*: I will prolong this business for a few moments, for we have here a case of an officer retiring from one office and taking up another. Major Levin has been your Secretary and is now a Vice-President. I wish to thank Major Levin for the extreme assistance and support that I have received from him during the last year. The President depends on the Secretary to keep him in the straight and narrow path, and Major Levin has succeeded in guiding me safely throughout the Meetings.

*Major Levin*: I thank you for your very kind appreciation.

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## THE ORDINARY GENERAL MEETING.

Instr.-Capt. M. A. Ainslie, R.N., B.A., F.R.A.S., *President.*

Major A. E. Levin, T.D., F.R.A.S. (Acting) } *Secretaries.*
F. J. Sellers, M.I.Mech.E., F.R.A.S. }

*Mr. Sellers* read the minutes of the last Ordinary General Meeting which were confirmed.

*The Rev. T. E. R. Phillips*: I beg to propose a vote of thanks to the retiring Officers. It is a great pleasure to mention the Vice-Presidents, Mr. Hollis and Mr. Tomkins, and the four Members of Council, Dr. Comrie, the Rev. M. Davidson, Mr. Harold Thomson and Mr. H. H. Waters; all these have rendered very excellent service; they are all men of sound judgment and of sound common sense. The Association has derived much profit from the counsel of such men. Their retirement is inevitable under the Association's Bye-laws, but we hope to see them serving again on the Council in due course.

*Dr. A. C. D. Crommelin*: I have much pleasure in seconding this vote of thanks. The work of the Association depends in great measure on the Council, but the debates in the Council are concerned with business rather than pleasure. In the Meetings of the Association, papers are read which are of great interest and slides are shown; upstairs we have simply business to transact. Some of the gentlemen mentioned by Mr. Phillips have had to take long journeys in order to attend, and others are men of business who have been willing to give their time and attention.

*Mr. Holborn*: I ask a vote of thanks from you to give to Mr. Mobsby and Mr. Perrin, the scrutineers of the ballot. I do not think that the thanks for performing this service to the Association ought to be merely formal.

*Mr. MacDonald*: I have no desire at all to be a scrutineer of the ballot; therefore it gives me great pleasure to second a vote of thanks to two *other* men who have acted as scrutineers.

*Mr. Addey*: I propose a vote of thanks to Mr. Roy Suttill. The work of an auditor is as dry as that done by the Council.

*Mr. McNeil*: I would like to second that vote.

*Mr. Sellers* read the list of presents which included: "Hints on Reflecting and Refracting Telescopes and their Accessories," by W. H. Thornthwaite, F.R.A.S. (Alabaster, Passmore & Sons, 1880); "Silvered Glass Reflecting Telescopes and Specula," by John A. Brashear (Best & Co., 1882); "The Michelson Echelon Diffraction Grating," by Prof. Michelson (1901); "History and Description of Tebbutt's Observatory, Windsor, N.S. Wales," by John Tebbutt (Joseph Cook & Co., 1887); "Tables for the Reduction of the Barometer," by Warren de la Rue (London, 1877); "The Teaching of Geometry in Schools," Report of the Mathematical Association (London, 1923); "A New Solution of Kepler's Problem," by James Ivory (Edinburgh); "The Phonic

Chronometer," by A. B. Wood (London, 1924); all from *Frank Robbins;* " Real Paths of Meteors Observed in 1928," by A. King, from the *Author;* " The Nautical Almanac and Astronomical Ephemeris for the year 1931," from *Dr. L. J. Comrie;* " Hammond's Improved Planisphere," from *Dr. L. J. Comrie;* " Astronomy," by A. R. Hinks (London, 1928), from *Patrick Folkard;* " Applied Optics and Optical Design. Part I," by A. E. Conrady (Oxford University Press, 1929), from the *Author;* " A Voyage in Space," by H. H. Turner (The Sheldon Press, 1927), from *Miss Hawk in memory of J. G. Hawk.*

*Mr. Sellers* read the list of seven candidates proposed for election, and the election of three new Members by the Council was confirmed.

*The President*: For the few minutes that remain to us I will ask our fellow Member from the States, Mr. James Stokley, to address us.

*Mr. Stokley*: It gives me great pleasure to say a few words before this Association, which I joined through the missionary efforts of Dr. Comrie during his sojourn in the United States. In the States we have no such active amateur society as this; I wish we had. When Dr. Steavenson visited us, he attended the meeting of the American Variable Star Society in Washington which is doing a good work in one of the fields covered by the B.A.A., last spring. In the last year or two there has been an awakening of interest in amateur telescope making, brought about by Mr. R. W. Porter, of Springfield, Vermont, and Mr. A. G. Ingalls, of the *Scientific American;* many have constructed telescopes and we hope that now they will use them for the advancement of the science.

Our telescope of greatest interest is the 200-inch, being constructed in California under the guidance of Dr. G. E. Hale. It will belong to the California Institute of Technology, at Pasadena. This instrument is actually under construction, and one type of mounting has been tentatively adopted. The disc will be the largest yet made, and it is to be of quartz, though hitherto quartz discs have not been made greater than 22 inches. The idea is to use a base of white quartz, not necessarily homogeneous, and on it to cast a veneer of perfectly fused quartz; in this way it may be possible in the next few years to make this great mirror.

The location of the telescope is still uncertain. Mount Wilson is one suggestion, but others have been mentioned, such as one in Arizona. Probably, however, it will be placed in Southern California and not far from Pasadena, so that it may be available for use both by the Mount Wilson Observatory and by the Institute, and where also the workshops are available. Scientists of the Observatory and Institute have always worked with the greatest co-operation, and so the great instrument will be equally available for both institutions.

*The President*: The December Meeting, which is due to be held on Christmas Day, will be held instead on New Year's Day.

The Meeting adjourned at 7 p.m.