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ON THE FOCUS OF A LENS.

BY J. W. GORDON.

*Read before the Optical Society on January 19th,
1910.*

At the monthly meeting of your Society, which was held in March, 1909, a paper was read by Mr. Selby, in which the writer was good enough to take notice of a previous paper of mine, and to point out that it had been written without reference to Lommel's paper on the Diffraction Phenomena of a Circular Opening, in the Proceedings of the Royal Bavarian Academy for 1886. Mr. Selby inferred, as indeed the fact was, that I had not seen Lommel's paper, and somewhat summarily dismissed my conclusions as being incompatible with Lommel's results.

Being naturally impressed by Mr. Selby's criticisms, I took the earliest opportunity of reading Lommel's paper, with the result that I have reached a conclusion so very unlike Mr. Selby's that I am prompted to ask the Society to hear me in answer in the hope that I can satisfactorily show that Lommel's paper, far from disposing of the question of the phenomenal focal plane, has in fact no bearing on that question, but does incidentally, by its deficiency in this and in some other respects, show that it is abundantly necessary that the question should be raised and fully discussed.

Let me briefly state the points in issue. It has, so far as I am aware, been assumed without proof, and without so much as a demand for proof, by all writers upon optics, that the visible image formed by a lens is formed in that plane in which the aplanatic rays of light from

a point in the object meet after passing the aperture. The assumption was originally a perfectly natural one. The earliest theoretical writers upon optics dealt exclusively with pencils of aplanatic rays, and gave themselves no concern about the incidental phenomena of diffraction. That state of things, however, has passed away. Nobody who desires to write or speak with accuracy at the present day thinks of dealing with problems of definition or resolution, that is to say, with certain essential problems of image formation, without taking diffraction into account. So soon as we thus enlarge the scope of our enquiry it becomes a wholly unwarrantable assumption that the image which we see is formed in that plane in which pencils of aplanatic rays come to a point. We know perfectly well that the visible image contains a large component of diffracted light; that it is, in fact, a compromise between the punctiform image which aplanatic light might be supposed to produce and the blurred sketch which might be expected to result if all the aplanatic light were suppressed. In performing the operation of focussing we always proceed upon the method of trial and error, and we settle upon that focal adjustment which gives us the best visible result without regard to theoretical considerations of any kind. Now, if it be a fact that the antipoints formed by diffraction are more compressed in a plane lying adjacent to but somewhat behind the aplanatic focal plane, then there is a great probability that the phenomenal focal plane occupies more or less the same position, and is to be sought, not exactly where the aplanatic rays meet, but where the antipoints are most compressed. Whether this is so is the problem which I have ventured to raise for discussion, and my immediate object in seeking the hospitality of the Society this evening is to point out that upon the solution of that problem Lommel's results throw no light whatever. I think, however, that I shall make

no great additional demand upon your time, and shall perhaps add something to the interest of the discussion if I take the opportunity, incidentally, of suggesting those lines of argument which seem to me to point in the direction which I have indicated.

It will, of course, be understood that the interest of this discussion is not directly practical, although it leads to practical conclusions of the very highest importance. Those conclusions are, however, reached indirectly. Upon the basis of the accepted theory as to the focal plane certain calculations have been made touching the necessary and impassable limits of resolving power in optical instruments, and we have even been told, with great show of authority, that those limits have actually been reached by existing instruments. That, if unassailable, is a very momentous proposition, but if those calculations have been made for the wrong plane, then those inferences are wholly untrustworthy, and we may still live in the hope of seeing further improvements, not merely in mechanical details, but also in the optical design of our high power instruments. Thus, although the immediate result of the enquiry can only be a theoretical proposition its ultimate significance is greater than that of any other question at present occupying the attention of the students of optics.

Coming now to Lommel's paper I observe that it is prefaced by an introductory paragraph in which our author sets out the object which he has in view. This, which is expressed with admirable terseness, may best be quoted.

After referring to the work done by Fresnel, Poisson, Scherard, Airy, Abria, Knochenhauer, and himself (in an earlier paper) in the way of investigating the law of light intensity in the diffraction pattern due to a circular aperture and to a circular screen, he says:—"There thus exist at present on the one hand certain results obtained for the middle of the diffraction image

and, on the other hand, a complete theoretical knowledge of the phenomena in the Fraunhofer case, but independent of one another and without any systematic connection. To fill this gap in the theory and to deduce from the wave theory the whole infinite multiplicity of the phenomena, finally, to test the theoretical results experimentally is the scope of the present paper."

It will be observed that Lommel does not here propose to add to our knowledge—which, indeed, he describes as being already theoretically complete—of Fraunhofer diffraction; that is to say, of the distribution of light in the focal plane. But this is the exact field of the investigation which I propose, and it will accordingly be found that Professor Lommel has, like all his predecessors, taken for granted the very point which I desire to have tested. The simplest way of bringing out this point will be by explaining what he has accomplished, and that may best be done by passing in review the first section of the paper, a section which he devotes to the determination and development of his integral.

It is not very useful to deal in the way of a read paper with mere mathematical formulas. I propose, therefore, instead of following Lommel step by step to lead up to the formula which he adopts in such a way that its significance will be apparent the instant it is disclosed, and no discussion of the formula itself will be necessary. Let me, then, ask you to assume that we are interested in the solution of a problem different in detail from Lommel's, but presenting to a large extent the same subject matter of calculation. Imagine that you are standing beneath a dome, as it might be, say, the dome of St. Paul's, but assume that it is exactly hemispherical in form and constructed entirely of ground glass. Assume, further, that it is lighted not by direct sunlight, but by the diffused light from a bright but cloudy sky, and that the problem is to ascertain how much light such a dome would send to the observer's eye.

An obvious answer suggests itself. Two things will have to be considered, (1) the area of the dome, (2) the brightness of its illumination.

Now, the second of these may for present purposes be at once dismissed from consideration. Of course, the amount of light will be greater at noonday than at twilight, but that variation is easily allowed for. We simply have to multiply by some factor representing the intensity of the external light the expression, whatever it is, which we arrive at by calculating the area of the dome. Since this simple arithmetical operation can be performed at any time, we will not bother further about it this evening.

There is left the area of the dome to be considered, and this problem is complicated by the consideration that all the light which reaches the spectator must reach him through the circular opening at the bottom of the dome. You cannot increase the amount of light which the dome will yield by merely augmenting its area so long as it is seen by projection through a limited aperture. You must therefore be content to go warily. It cannot, however, be wrong to assume the dome to be divided up into windows, and having calculated what amount of light each window will transmit to take the sum of the light so calculated for all the windows as being the illumination due to the dome.

A Window Pane Analogy.

There is an obvious way of arranging windows in such a structure. Those members of the frame which I may call the vertical members would naturally converge to a point in the vertex of the dome, whereas the other parts of the frame would be arranged as rings, probably equally distant from one another; thus dividing the whole area up into a multitude of trapezoidal panes. An expression for the dimensions of any one of these planes is easily found. Let us write (a) for the radius of the dome, and taking one of the panes in the lowest row, that is to

say, those skirting the edge of the dome, for our unit let us assign (f) for the breadth of the frame, and (p) for the height of the frame, then obviously, the area of that pane will be ($f \times p$). Taking that for datum, if we select any other pane anywhere in the dome, and if we write ψ for the angle between the vertex of the dome and this selected pane; its polar distance, as astronomers would say; then the dimensions of this pane will be given by the following expressions. Its vertical height will, as in the other case, $= p$, its breadth will be $(\sin \psi f)$. Now, the amount of light which will enter through these two panes will be proportional to these two areas respectively. But it does not follow that the amount of light which reaches an observer on the floor of the building will be similarly proportional to those two areas. On the contrary, it is pretty certain that the lower pane would be more foreshortened than the upper, and consequently the area of its projection or, what I may call, its aperture value, will be less than its real area to an extent determined by this foreshortening. Without for the moment going into that matter, we may take it for granted that the measure of that foreshortening can be expressed as the cosine of a certain angle, which we may write down $(\cos z)$ and, therefore, for the amount of light to be radiated to a given point upon the floor from any window in the dome we must write $(\sin \psi f \cos z p)$ and for the sum of all the panes which we may therefore write,

$$\frac{2 \pi a}{f} \sum_{\psi = (0 \ z_1)}^{\psi = (\pi/2 \ z_2)} (\sin \psi f \cos z p)$$

an expression which will give us the amount of light radiated in the direction of our spectator by the dome.

There is, however, another consideration which must be taken into account. Although the light radiated in the direction of the spectator

has an initial value represented by the above expression, its value when it reaches him will depend partly upon his distance from the dome, that is to say, the farther off he is the smaller will be the proportion of the total light which the dome transmits that will reach his eye. It is well known that the falling off of light intensity due to this cause varies inversely with the square of the distance, and if we take the light transmitted to a distance equal to the radius of the dome to be that represented by unity, and if, moreover, we write (d) for the distance between any given pane and the spectator, then the amount of light which would reach our spectator would be measured by

$$\frac{a^2}{d^2} \left(\sin \psi f \cos z p \right) \text{ and the total light will be } \\ \frac{2 \pi a^3}{f} \sum_{(o \quad z_1)}^{(\pi/2 \quad z_2)} \left(\frac{1}{d^2} \sin \psi f \cos z p \right)$$

So far our problem has been analogous to that with which Lommel is concerned, because down to this point our glass dome has not differed in any material respect from a wave front. But here we must observe a distinction. A dome lighted in the way here suggested would give us a steady average illumination, in which there would be no distinguishable phase. A wave front, on the contrary, if it coincided with the dome, would give out light constantly varying in character, and varying so much that it would be alternately positive and negative light. We must therefore modify the conditions of our problem to correspond, and we may find a sufficiently close analogy by the help of those electric signs which have become so formidable a feature of the modern street. Everybody is familiar with those unpleasant advertisements in which changing colours chase one another through the lettering of some uninteresting legend. Now let me suppose that our dome is lighted by elec-

tric lamps which change colour in this way, and that the scheme of lighting may be described thus:—Assume two colours, say, for example, blue and yellow, which are to alternate and chase one another in radiating zones from the vertex towards the edge of the dome. If, under these conditions, you wanted to know what was the colour of the compound light that at any moment would be radiated from your dome upon the spectator, you would have to consider not only the area of every pane but also the particular colour of the light which at a given moment it was radiating. You would require, therefore, to go over the foregoing calculation once more, supplying a co-efficient which would tell you whether at the given instant your electric lamp was radiating blue light or yellow light. Consider, then, what form this factor would take. All the coloured zones start from the vertex, and if we assume that blue and yellow follow one another in true periodic succession, we can, by a very simple calculation, tell what at any moment is the colour of the light being radiated from the *vertex* of the dome. Let us assume that the colours change once in some interval of time which we may express by writing it = T . Now, suppose that we know the exact time at which the illumination began and that, measured from that moment, the time at which our observation is made = t , then t/T will give us the number of alternations in the colour at the vertex of the dome. We may, therefore, write $t/T = c$ and then, according as c is odd or even, the colour will be blue or yellow. That tells us all about the coloration at the vertex of the dome, but if we want to know the colour of a pane which is situated elsewhere in the dome we must, obviously, make allowance for the time which the colour takes to travel from the vertex to that selected pane. This allowance is easily arranged for. We may write λ = the distance that the colour traverses in the time T , and if we write \triangle for the distance, measured along

the vertical member of the frame, at which our selected pane stands from the vertex, then Δ/λ will indicate the extent to which this pane is behind the vertex in the matter of colour. Thus we obtain the following expression for the colour of the given pane

$$c_1 = \left(\frac{t}{T} - \frac{\Delta}{\lambda} \right)$$

and according as (c_1) is odd or even so the colour of the given pane will be blue or yellow. The expression which we have so far found would give us exactly what we want, provided the expression within the bracket always gave us an integral number. But obviously this is a condition which will not, in fact, be satisfied, and therefore we shall have values of (c_1) which are neither odd nor even but, on the contrary, are very unhandy fractional quantities not to be classified in the required way. We need, therefore, to seek for some more suitable expression for the colour, and there is a well-known mathematical formula which satisfies the case.

If we give to the magnitude $\left(\frac{t}{T} - \frac{\Delta}{\lambda} \right)$

an angular value, we shall have a series of magnitudes which will be alternately positive and

negative. Thus $\sin \left\{ \left(\frac{t}{T} - \frac{\Delta}{\lambda} \right) 2\pi \right\}$ is a cycli-

cal expression which will exactly meet our requirements. It rises to a maximum, comes down to zero, proceeds to a minimum, returns to zero, goes to the maximum again, and is alternately positive and negative, always between the limits $+1$ and -1 . We may therefore adopt this as a criterion for distinguishing between our blue and our yellow light, and if we assume that our blue and yellow lights are complementary, and will exactly cancel one another when they are present in equal force, yielding then a colourless

light, we shall be able to obtain by means of this expression a quantitative determination of the extent to which the blue and yellow lights will enter into the illumination of the floor. Combining this result with that already obtained, we get the following expression :—

$$\text{Light} = \frac{2 \pi a^3}{f} \sum_{(o \quad z_1)}^{(\pi/2 \quad z_2)} \left\{ \frac{1}{d^2} \sin \left(\frac{t}{T} - \frac{\Delta}{\lambda} \right) 2\pi \right. \\ \left. \sin \psi f \cos z p \right\} \dots \dots (1)$$

A somewhat simpler expression may be obtained if, instead of the full light intensity, which is the resultant of an indefinite number of displacements in one plane, we measure one of those displacements only. We then get an expression involving only first powers instead of the squares a^2 and d^2 . Still, however, an expression in the form above stated represents the light radiated by a polygonal surface. If we want to pass from this to a spherical surface we must, of course, assume that each pane is divided up into infinitesimally small panes, and that in the process the opaque frame is obliterated. We may then, using the notation of the differential calculus, write $f = d\phi$; $p = d\psi$. In these expressions ϕ is, of course, the angle of azimuth, and ψ , as already defined, the polar distance of the given pane. Substituting these values for f and p in the above expression, and making use of the notation of the calculus, we obtain the following expression for the light which would be received from a hemisphere of ground glass under the conditions specified, the light being measured by the amplitude of its displacement at a given point.

$$\text{Amplitude} = a \int_0^{2\pi} \int_{(o \quad z_1)}^{(\pi/2 \quad z_2)} \frac{1}{d} \sin \left(\frac{t}{T} - \frac{\Delta}{\lambda} \right) 2\pi \sin \psi d\phi \cos z d\psi \dots (2)$$

It will be observed that the operation denoted by $\int_0^{2\pi} \sin \psi \, d\phi$ in this expression is equivalent

to that denoted by $2\pi a/f \sin \psi \, f$ of (1). Bearing this in mind, and substituting the amplitude factor a/d for the intensity factor a^2/d^2 , the reader will perceive that equation (2) is directly deducible in the way indicated from equation (1). It may be useful to point out further—in elucidation of this expression—that the angle z is a function of ϕ and ψ , the exact form of which is of no importance in the present discussion, and that the quantity \triangle , as already indicated, is equal to $a \psi$.

If now we compare this expression with the expression which Professor Lommel starts to integrate, we shall see at a glance what is the scope of his undertaking. He writes under his first Section, which bears the headline, "The determination and development of the integral":—"The disturbance produced at the point $\epsilon \eta$ of the screen by any given part of the spherical wave is then expressed by the double integral

$$a \int \int \frac{1}{d} \sin 2\pi \left(\frac{t}{\tau} - \frac{d}{\lambda} \right) \sin \psi \, d\phi \, d\psi$$

when the integration is extended to all effective parts of the spherical wave.

"(2) In the case of a circular opening having its centre upon the polar axis, and lying in a plane parallel to that of the screen the effective part of the spherical wave is defined by the boundary of the circular opening, and the amplitude of vibration at the point $\epsilon \eta$ is expressed by

$$a \int_0^{2\pi} \int_0^{\psi_1} \frac{1}{d} \sin 2\pi \left(\frac{t}{\tau} - \frac{d}{\lambda} \right) \sin \psi \, d\phi \, d\psi \quad \dots \quad (3)$$

where ψ is the angle which the ray passing from

the edge of the opening to the radiant point makes with the axis." The rest of the paper is taken up with the integration of this expression.

Now, understanding that the same symbols represent the same factors in equations (2) and (3), and comparing this last with the expression which we just obtained, it will be noticed that there are three points of difference. First of all, in place of \triangle Lommel uses the distance d ; next he substitutes for $\cos z$ the value unity, and in the third place he makes the integration in respect of the angle ψ to a limit indicated by ψ_1 , and not, as in our expression, over the entire quadrant. What is the significance of these changes?

First, as to the substitution of d for \triangle . This can be very simply explained. \triangle , in our expression, is the distance which any coloured ring has travelled at the moment of observation from the vertex of the hemisphere. That distance it is which determines the colour of the light at the given moment. That distance, however, has no significance for Professor Lommel. He is not concerned with changes of colour, but with variations of phase, and the phase does not vary at any given instant over the surface of his wave front. He therefore is able to write $\triangle=0$, and to eliminate this from his integration. On the other hand the difference of phase with which rays from different points upon his wave front arrive at the point where the amplitude is to be calculated is determined by differences in the distance d , that is to say, the variable distance of a point on the wave front from the point of observation, or to recur to our original illustration, the various distances of different parts of the dome from the spectator on the floor. Consequently, this spectator distance d must, in Lommel's formula, take the place in our formula of the polar distance \triangle . The change does not in any way affect the nature of the calculation, but only the calculated result.

Next, to take the difference of the limits over which the integration is to go. Professor Lommel does, in fact, when he comes to deal with the problem of assigning a value to his integral, limit himself expressly to very small values of the angle ψ_1 . This simplifies the problem, perhaps it would be better to say, brings it within the compass of calculation. It is very important to realise how small this angle is. Professor Lommel stipulates that it shall be taken so small that the sine and arc may be substituted interchangeably for one another (page 237), that is to say, Professor Lommel is, in substance, dealing with a plane wave front. This is one of the reasons why I said at the outset of this paper that it does not appear to me that Professor Lommel's results have any bearing on the question which I have raised. The problem of the position of the focal plane can only arise when we are dealing with beams of light having a considerable divergence angle. And here, perhaps, I may interject the observation that in the case of the microscope—with which I am chiefly concerned—those angles are very considerable indeed.

The third point of difference is the substitution of the value $\cos 0$ for $\cos z$. This is legitimate on the assumption that the point of observation is taken not very far from the centre of the sphere of which the wave front forms a segment. My impression, as the result of a somewhat cursory reading of Lommel's paper, is that he does not expressly lay down this condition, but, whether or no, he does in fact observe it. His calculations are not made for distances extending to very many wave lengths from the focal point and hence, within the limits which he practically observes, it is quite legitimate to treat this angle z as having a zero value.

We now see the scope of Professor Lommel's undertaking, so far as he deals with calculated results. He is dealing with a wave front of infinitesimally small angular magnitude, and,

considering the diffraction pattern in a region which is either the focal point for aplanatic rays or a region lying closely adjacent to that point. It follows that, so far as calculation is concerned, he does not touch the problem of the phenomenal focal plane for strongly convergent beams of light. He adds to his paper certain experimental results, and speaks of these as strikingly confirmatory of the results to which his calculations lead. From what has been already said, the Fellows of the Society will be prepared to understand that Professor Lommel does not in fact deal with beams of focussed light. His observations were made upon direct sunlight. The apparatus with which he worked is thus described on page 298.

Lommel's Apparatus.

"In order to compute the phenomena theoretically, and so be able to compare them with the results of observation, it is necessary to make use of homogeneous light of a definite wave length. There was produced by means of a clockwork heliostat, slit, achromatic lens, and flint glass prism, a clean sharp solar spectrum, and this was thrown upon a screen furnished with a small round aperture $\frac{1}{2}$ mm. in diameter. This small aperture was advanced to the several Fraunhofer lines by moving the screen in a straight line until the dark line occupied the middle of the aperture, and thus light in the immediate neighbourhood of the dark line on either side of it passed through the aperture. This little aperture thus served as a nearly punctiform source of homogeneous light for the diffraction apparatus which was situated at a suitable distance behind it.

"In the whole research the distance ($a = 2120$ mm.) of the light source from the diffraction opening was retained unaltered, and only the distance, b , between the opening and the micrometer varied.

"Moreover, the diffracting opening was the

same in all the observations. Its diameter, measured by means of a glass micrometer and a microscope, came out 0.56 mm. Thus its radius was 0.28 mm.

"The measurements were taken over the Fraunhofer lines, C, D, E and F. For the lines B and G the phenomena were not sufficiently bright for secure measurements."

It will thus be seen that the light which suffers diffraction is only the narrow pencil which, after passing the first aperture, $\frac{1}{2}$ mm. in diameter, is transmitted at a distance of about seven feet by another aperture of substantially the same size. Such a beam of light would consist, like Lommel's calculated beam, of substantially plane wave fronts.

It thus appears that both the calculated and the observed results are very remote from the question of diffraction phenomena produced by strongly convergent beams of light and, speaking for myself, with all the will in the world to derive enlightenment upon the question under discussion from Professor Lommel's paper, I must confess that I have been unable to extract from it so much as a single grain of relevant information.

The New Line of Argument.

I hope, however, that I shall not be thought pertinacious if I venture to suggest, for the consideration of the Optical Society, one of those lines of argument which seem to me to point irresistibly to the conclusion that the minimum extent of the false disc must lie at a position slightly beyond the aplanatic focal plane. I confine myself to a single line of argument because a complete discussion of the matter would involve something like a small treatise and, having regard to the necessary limits of space and time, I select that line of argument which appears to me most easily compressible into a small compass. In a paper read before the Royal Microscopical Society, in December, 1904,

which it would be tedious here to recapitulate, I pointed out that all the light which reaches the focal plane outside the focal point must pass through the conical surface which forms the boundary of a focussed beam of light. Now, we know just as much about the condition of that boundary as we do about the condition of the wave fronts which are contained within it, and therefore it must be just as legitimate to start our investigation over that surface as over the

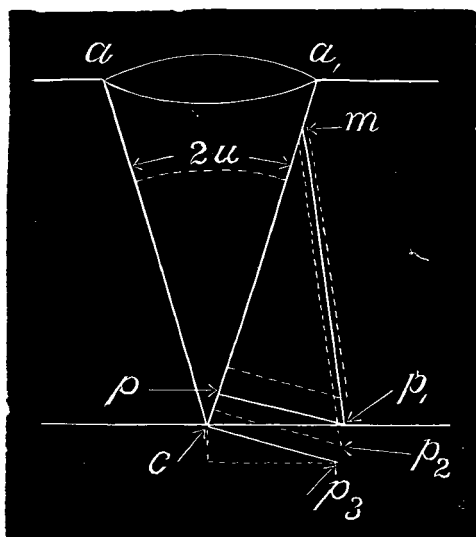


FIG. 1.

surface of the wave fronts. I endeavoured to perform the necessary operation, and although I did not succeed in obtaining an expression from which I was able to get a numerical result, certain interesting conclusions of a general character at once presented themselves as the outcome of investigating the matter along those lines. The following diagram (fig. 1) will serve to illustrate the conclusion reached.

Consider under what conditions light dif-

fracted from the surface of the convergent beam $a a_1 c$ can reach the point p_1 upon the focal plane. Draw $p \dots p_1$ perpendicular to the edge $a_1 \dots c$. It is at once obvious that no light of any kind can be radiated upon the point P_1 from any part of the conical surface which lies below the level of point P , for all the wave fronts below that point would have to radiate backward any light which they might radiate to the point P_1 . We may therefore look upon the horizontal plane through the point P as being the lower limit of the surface of the focussed beam which radiates diffracted light upon the point p_1 . Another conclusion easily established is that we can find a point m such that the light reaching the point p_1 from that point and from the whole area above it on the surface of the focussed beam will be negligibly small by reason of the fact that light from the successive zones upon this part of the surface of the beam interferes so as almost to produce darkness, and that the small residual illumination comes from a surface subject to strong foreshortening, and consequently having very small aperture value. We thus get a definite area consisting of a series of zones, not, in fact, a very extensive series, upon the surface of the beam from which alone light can effectively be radiated to the point p_1 , and it is to be observed that there are both upper and lower limits to the region so defined.

Now, the point p_1 is, in the diagram, shown as a point in what I have called the focal plane for aplanatic rays, and it may quite well be assumed to be the dark ring which forms the boundary of the false disc. It is obvious from the diagram that if we selected a point p_2 below that focal plane we could define an area, marked out by dotted lines in the diagram, upon the surface of the cone, which would stand in the same geometrical relation to the point p_2 as that in which the radiant surface $m \dots p_1$ stands to the point p_1 . General considerations, which do not

involve the discussion of the diagram at all, show that if the point p_1 in the focal plane for aplanatic rays represents a ring of zero illumination the point p_2 must similarly represent a ring of zero illumination in the sub-focal plane in which it lies. If we now select another point in the position marked p_3 in the diagram, the distance of which below the focal plane for aplanatic rays is equal to $\frac{1}{2} \sin 2u r$; $2u$ being the angular aperture of the beam, and r being the radius of the ring defined by the point p_1 in the aplanatic focal plane; in that case, we should find a dark ring forming the boundary of a false disc, which would receive the entire radiation from the whole beam of light, which dark ring would pass through the point p_3 , and it is clear from the diagram that its axial distance is dimensioned to the axial distance of the point p_1 in the proportion $\cos^2 u : 1$

This conclusion does not stand alone. It is possible to carry the point p_1 above the aplanatic focal plane as well as below it, and in that case we still obtain a ring, now of increasing dimensions but of zero illumination, and thus arrive theoretically at the conclusion that the false disc above the aplanatic focal plane must open out into a luminous ring bounded as before by a circle of zero illumination at its outer edge, but of increasing dimensions in accordance with the increased sectional diameter of the beam itself.

Now, this theoretical result is obviously one that can be very easily verified. Any system that will show the boundary of the false disc at all must be capable of showing its expansion into a bright ring surrounding the lower end of the focussed beam of light, and the expansion of this ring may be made to take place upon so obvious a scale that there can be no instrumental difficulty in observing it. Having reached this result as a theoretical conclusion I proceeded to test it experimentally. In fig. 2 I am able to present to the Society a photo-

graphic reproduction of a series of sections of a focussed beam of light, the sections being obtained by focussing the camera upon successive

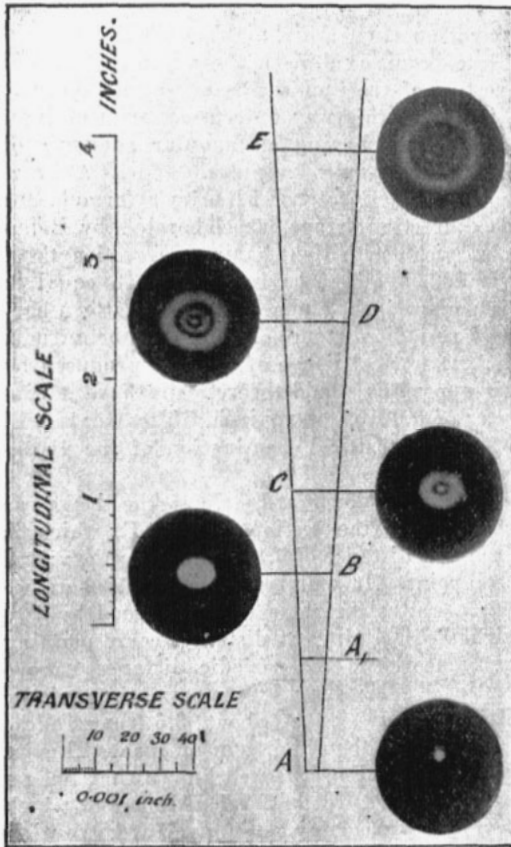


FIG. 2.

planes in the course of the beam. The visibly focussed image is shown at A, being the image of an artificial star having a circular aperture of 1/100 of an inch in diameter, illuminated

from behind by the light of a Welsbach mantle. This artificial star was placed at a distance of 187 inches in front of a telescopic combination of a telephoto lens, with a well-corrected photographic lens and the field lens of an ocular. By computation the equivalent focal length of this telescopic combination was 25.6 inches. The aperture of the incident beam was cut down to $\frac{1}{4}$ inch. In this way a focussed beam of light was formed, having an angular aperture of about twenty-seven minutes. Such a beam should in conformity with Airy's formula for the diffraction fringe, as elaborated by Helmholtz, yield, with light having a wave length of $1/50,000$, a fringe having a breadth equal to .0024 of an inch, and a diffraction pattern having a periodic distance of twice that magnitude. The geometrical image of the star produced by this apparatus would theoretically have a diameter of 0.00159 of an inch. These data will suffice for a critical examination of the photograph.

Proceeding downwards along the beam we come first to the section marked E, which is taken at a distance of 5.1 inches from the visual focal point. It will be observed that here we have a central bright spot surrounded by two bright Fresnel rings at distances corresponding pretty nearly to the periodic distances above mentioned. Measured by the scale, the periodic distance comes out at about 3.6 thousandths instead of 4.8 thousandths of an inch. This we should expect. For it is well known that images vary directly as to the focal distances at which they are formed from the apertures forming them, and it results from Helmholtz's investigation that diffraction patterns are subject to the same law of relative magnitudes as images. Now this diffraction pattern in the plane at E is formed at about $20\frac{1}{2}$ inches from the aperture, whereas the periodic distance 0.0043 in. is calculated for a focal distance of 25.6 in. We should therefore expect to find the scale of the

periodic structure reduced in this plane to about 4-5ths of its value in the plane for which the value 0.0048 in. was calculated. We find it reduced in ratio of 36 to 48. This is a coincidence which must to some extent be lucky, for it is, assuredly, well within the limit of the experimental error.

Errors of Centration

It will, no doubt, be remarked that the beam, in this section of it, shows as being not quite symmetrical. The explanation of that is obscure, but I surmise that it may be found probably in some want of accurate centering of my lenses. From the description above given of the apparatus with which these photographs were produced it will be seen that several lenses separately mounted had to be employed, and as, in fact, my optical bench was of a very rudimentary character, I had great difficulty with the centring.

Coming next to the section at D (distant 3.7 inches from the visual focus) it will be observed that the second bright ring, counting from the centre, seems to have disappeared, the central spot and first ring remaining in place and the outermost ring having contracted into the space vacated by the second. What, in fact, has happened, however, is that all the rings have contracted equally. The central bright spot of E has disappeared altogether; the first bright ring of E has condensed into the central bright spot of D; the second bright ring of E has contracted into the first bright ring of D, and the outermost ring of all has contracted in the manner which is evident upon the face of the photograph. All these contractions have proceeded quite gradually in the space from E to D, and the reason why in the lower plane the central pattern of the upper plane is so exactly reproduced is that the distance from E to D is a periodic distance measured along the axis of the beam.

Passing over another periodic distance we

come to the plane C (distant 2.3 inches from the visual focus) where we find the typical pattern of E and D again reproduced *mutatis mutandis*. In view of what has been already said it would be superfluous to discuss this section in detail, but it is not superfluous perhaps to point out how precisely the periodic distance C . . . D is equal to the periodic distance of D . . . E.

The next photograph was taken at a shorter interval than the periodic distance, and was, in fact, produced with the object of obtaining a section showing the black central dot. This, of course, results from the condensation of one of the dark rings, and it will easily be understood that the dark dot in B results, in fact, from the condensation of the dark ring in C. It would of course be expected, and rightly expected, that this black dot would be formed at half a periodic distance from the bright dot, and, in fact, the plane B does occupy that position. The black dot, however, does not make so good a figure as the white dot, for the simple reason that it has been eaten into by halation.

Analysis of the Outer Ring.

We are now brought face to face with a question which down to this point it has been possible to evade, namely, what is the constitution of the outermost ring? According to the systematic arrangement of Fresnel rings we should expect to find a Fresnel ring surrounding this black dot of the B section, but the ring which actually surrounds it is disproportionately large. If the aperture here contained only one fully developed dot and one ring it would have diameter of a period and a half, that is to say, of about a little over $7/1000$ of an inch. In fact, however, the mean diameter of the B section is $10\frac{1}{2}/1000$ so that we obviously have something more than a Fresnel ring surrounding the black dot. Independently of this consideration we know that there must be something in addi-

tion to the Fresnel ring, that is to say, there must be a diffraction ring encircling the beam and perhaps, therefore, we may take it that what we see in section B is a mutilated black dot, an indistinguishable Fresnel ring, and surrounding the whole a diffraction ring. If the outermost Fresnel ring and the diffraction ring could be considered to be entirely independent of one another we should infer that the breadth of the beam at this point ought to be two and a half times the periodic length of the diffraction system. The long diameter of the ellipse itself satisfies this condition almost exactly. The shorter diameter, however, falls appreciably short of it. Speaking for myself I find it difficult to offer any observations upon this measurement, for I am not sufficiently informed as to the connection between the outermost Fresnel ring and the circumjacent diffraction ring, to know whether the two adjacent edges will overlap, and to form any opinion, therefore, as to whether the longer or the lesser axis more truly represents the actual state of things in section B.

The Visual Focal Point.

The next section, of which I have a photograph, is at the visual focal point A. I may here be allowed to explain that when these photographs were taken I had no notion about the periodic distance in length, and no means, therefore, of identifying the theoretical focal plane for aplanatic rays. In fact, I had not perceived, until I came to put these photographs together systematically for the purpose of illustrating this paper, that there was any means of ascertaining precisely where that plane is to be found. When, however, these photographs are assembled as they are in fig. 2 it seems pretty obvious that the focal plane for aplanatic rays must lie at a point about midway between A and B, which point would lie at the periodic distance from the plane C and would, in fact, be the focal plane for the outermost of the Fresnel

rings. Unfortunately, through inadvertence at the time I failed to obtain a photograph of this most interesting section. But it will, nevertheless, be of interest to consider its dimensions as indicated by the diagram. We know exactly what the calculated dimensions of an image formed in this focal plane for aplanatic rays would be. The geometrical image of the artificial star would have a diameter of .0016 of an inch. The Airy diffraction ring would have a breadth of .00237 of an inch. Taking this last mentioned dimension twice and adding the breadth of the geometrical image we get .0063 for the calculated diameter of the image of the star. Applying the scale at the plane marked A_1 we find the diameter to be 0.008. It will be borne in mind that the diagram indicates the greater axis of the ellipse and that in the plane B the two axes differ by $1/500$ of an inch. If we assume a difference of half this amount between the two axes in the plane A_1 we should then have 0.0075 for the mean diameter of the image formed here. Having regard to the imperfect corrections of the system and to the somewhat elaborate calculations by which these numerical results have been reached, I think it may be taken that that is as close an approximation of the experimental to the theoretical results as could at all be looked for.

If now I have successfully identified the focal plane for aplanatic rays it follows that the visual image is in fact formed in that plane in which the diffraction ring condenses upon itself and not in the theoretical focal plane. If this conclusion is sound it follows also that all the calculations as to the *minimum visible* and the ultimate limit of resolving power hitherto made must be revised. Those even which have been calculated for beams of very small divergence angle, probably exaggerate by something like 50 per cent. to 100 per cent., the difficulty of rendering small objects visible. Those calculations, or, rather, inferences, which have been

made concerning the resolving power of beams of wide angle will have to go by the board.

MR. SELBY, in opening the discussion, said it was a matter for self-congratulation to him that he was in some degree responsible for Mr. Gordon's appearance before them that evening, in that on a previous occasion he (Mr. Selby) had ventured to criticise Mr. Gordon's conclusions. He regretted, however, Mr. Gordon should have thought it necessary to devote so much attention to the matter he (Mr. Selby) had then brought forward. That matter was in itself intrinsically interesting, as were all Lommel's results; but there were so many other points on which those who were interested would have liked some further information from Mr. Gordon, the question as to what, in the first instance, led him to his conclusions, whether his reasons were theoretical or experimental; the question as to the exact position he would assign to the focus of a lens; in what respects he regarded the ordinary theory as at fault—in short, what he would have desired was a full and complete statement of Mr. Gordon's views with regard to the focus of a lens, not merely a reply to the criticisms based on the conclusions of Lommel. But since Mr. Gordon had devoted so much attention to those results of Lommel it was necessary, to a certain extent, to follow him, and to say a few words in defence of the position he (Mr. Selby) had taken up. He would like, in the first place, to read a few extracts taken from Lommel's paper. It was not possible to follow Mr. Gordon through all the details of his calculations. The substance of his remarks was that Lommel's results only applied to a plane wave, or only to a portion of a spherical wave, so small that, for all practical purposes, it must be regarded as plane. (Mr. Selby then read the extracts referred to.) If he had fallen into error in supposing that Lommel was speaking of a spherical wave, and not a plane wave, it was clear from these extracts, that Lommel himself had also

fallen into that error. Lommel's results were a very close approximation for an angle up to 10° on either side of the axis of the lens, that is, an angle of 20° altogether, a considerable angular aperture. Mr. Gordon had further remarked that Lommel's results did not apply at all in the case of an image formed by a microscope objective, where, he said, the angle was very much greater than would come under Lommel's formula. That statement showed some confusion of ideas. The case chosen was pre-eminently one in which Lommel's results did apply, the angle of the image forming pencil in the microscope being quite small. The formula did not attempt to take account of aberration in the lens, nor were they concerned with aberrations on the definition of the focus. If, then, the microscope lens were free from aberration, giving an aplanatic image of a point, Lommel's results did very accurately apply. Mr. Selby proceeded further to discuss Mr. Gordon's reasoning based on the consideration of the secondary wavelets from a corneal surface in place of the spherical wavefront, and pointed out that this procedure was beset with considerable difficulties, and needed much justification, which had not been attempted. Attention was drawn also to the misleading character of the figure from which Mr. Gordon's conclusions were derived. If this figure were received with due regard to the relative dimensions of the lens and the first dark ring in the image it would appear that Mr. Gordon was treating the major portion of lens as inoperative.

Comments by Dr. Clay.

DR. CLAY said he desired, with Mr. Selby, to say how much they had all been interested in what Mr. Gordon had put before them. The subject was one of theoretical interest only, as it would not in practice very much matter whether the focal point was to be removed a small distance from the position usually assigned

to it. He would like to emphasise what Mr. Selby had said about the diffraction cone in the case of the micro-objective.

He did not quite follow why Mr. Gordon wished to substitute a much more complicated method of obtaining the distribution of light at the focal point by considering the light from the whole of the cone, for the usual method of compounding the light from the wave surface where it issued from the diaphragm. If Mr. Gordon was right in his contention that to obtain the resultant focus one must deal with the *volume* distribution of light in the case of a formation of an image by a lens, it would be equally true in any case of interference; for instance, in the simple interference of light that obtains with a bi-prism, the light would have to be added up from the whole volume: but in this case, as everyone knows, the pattern obtained agrees perfectly with that which should theoretically be obtained by dealing with the light from the two points only.

As regards Mr. Gordon's photographs, Dr. Clay thought the results were not at variance with the ordinarily accepted theory. It seemed to him that he was really dealing with a very similar problem to that of the diffraction of light through a circular aperture. It was well known that in this case if the beam of light which issues from the aperture is examined by an eye-piece, and that eye-piece is slowly brought from a distance towards the aperture, a series of points are found at which there will be (for any one colour) a bright spot in the centre of the field. The positions of this spot are approximately those at which the aperture will contain an odd number of Huyghen's zones. It would be found that the distances from bright spot to bright spot were not equal to one another, but that they get less as the eye-piece approaches the aperture. This was exactly what Mr. Gordon had found in his experiment. Dr. Clay thought that if Mr. Gordon would work out the

positions in which the cone of light as it issued from the diaphragm in this experiment contained an odd number of Huyghen's zones, he would find that the positions agreed with those obtained experimentally.

Experimental Tests.

MR. T. SMITH (Secretary) said he had been very much interested in what they had heard from Mr. Gordon and from the other two speakers. There were one or two things that he had wanted to say, but he was afraid, as the hour was getting so late, that he must curtail his remarks as much as possible. In the first place he very much regretted that Mr. Chalmers had been prevented from attending through indisposition. He understood that Mr. Chalmers had hoped to bring some apparatus down and give a little demonstration by which he had hoped to convert Mr. Gordon himself to the usually accepted theory. He (Mr. Smith) thought that the problem by which Mr. Gordon had illustrated the integral obtained by Lommel was not precisely analogous to the actual problem under discussion. For instance, Mr. Gordon had assumed that different portions of the wave front, as represented by the outline of the dome, were in different phases at any particular instant of time, whereas in the actual wave front of light the phase at a particular instant was the same everywhere. Mr. Gordon suggested that Lommel's results were vitiated because higher terms than the second in cosine z were neglected. If they considered Mr. Gordon's illustration they would see that the chief effect due to obliquity was equivalent to a reduction of the aperture of the dome, and thus there was nothing in Mr. Gordon's analogy to bear out the objection. He (Mr. Smith) considered that Mr. Gordon had not made out his case against Lommel, whose results were quite in agreement with experimental work carried out at the National Physical Laboratory.

MR. GORDON, in replying, said:—He first wished to thank them very much for the kindly welcome they had extended to him, and to say how very sensible he was of the great kindness with which he had been treated by Mr. Selby, and also by other gentlemen present, like Dr. Clay, who felt themselves interested in maintaining the accepted theory of the focal plane. He also felt how much the meeting had suffered by the absence of Mr. Chalmers, whom he, for his own part, had greatly hoped to see there. He would not venture to occupy more than a very few minutes, and must therefore leave many parts of the question undiscussed.

He was sure, for instance, that Mr. Selby would appreciate that it was for that reason he begged to be excused from answering his question as to where exactly the focal plane should be defined. To deal with that would occupy more time than they would care for him to take up, and more time than he was capable of usefully taking up just then. Similarly, with regard to the quotations which Mr. Selby had made from Lommel's paper, he would just say one word on that matter. He did not mean to suggest at all that Lommel did not range beyond the plane wave front, nor did he mean to suggest that Lommel's result applied only to what could be strictly called a plane wave front—in fact, he thought he had guarded himself against slipping into that error, but probably he did slip into it notwithstanding his desire to avoid it.

Case of the Telescope Objective.

He had, however, supplied the correction to anything of that sort by saying that Lommel's calculations were found to apply with remarkable accuracy to the case of the ordinary telescope objective; and that had been used against him by both Mr. Selby and Dr. Clay, because the microscope objective had a still smaller angle behind the objective. The telescope objective

had very often as big an angle as 10° , whereas the microscope never had a backward angle of any sensibly greater magnitude. That was perfectly true but there was a distinction between the two things, and notwithstanding what both Mr. Selby and Dr. Clay had said, he must claim the liberty to maintain against them both that the important distinction was, that the front angle in the microscope objective was very wide. Mr. Selby had dealt with that, and he, Mr. Gordon, understood that he commanded Dr. Clay's suffrage in doing so, but Mr. Selby dealt with that in this way. He said it was only the backward angle which had to be considered, that the forward angle did not matter, because the light from that impinged upon the lens and was not emergent from it. He confessed he (the speaker) should have thought so too, but for a paper which started him upon the whole of that enquiry—Helmholtz's paper, written in 1874, on the question of the resolving power of the microscope. Helmholtz there demonstrated a very remarkable proposition having a direct bearing upon that point, which was the answer to what Mr. Selby had said. The proposition was that in considering the case of the microscope, and the diffraction of the microscope, they need not bother about the diffraction which occurred behind the objective because the diffraction was the same in all parts of the microscope, that is to say, the diffraction was produced by the smallest aperture, wherever that aperture was. Usually the smallest aperture in the microscope was the image of the light source, and therefore the diffraction produced by the microscope usually resulted from the peculiar shape and the position of the image of the light source. If they threw the image of the light source actually into the focal plane, so that its repeated image fell in the focus of the instrument, then it gave rise to no diffraction. But if the image of the light source fell anywhere else in the microscope, it invariably set up diffraction,

because in that case it was always the smallest aperture in the instrument. That aperture was met with in several points of the instrument. They met with it either in the image plane or somewhere near the image plane, and they met with it again behind the ocular. And the main point, and the great point, which Helmholtz demonstrated in that paper was that diffraction could be calculated from that aperture wherever it was met with—it was always the same. The diffraction was the same if the diffracting aperture was found in the Ramsden disc, where they had a plane wave front, or if they found it in the principal focal plane behind the objective where they had a very deeply convex wave front; and, so far as the practical calculation of diffraction from curved wave fronts was concerned that problem, although it was by the Airy and Lommel method, as he thought they were all agreed, an insoluble problem, that is to say, it could not be solved by the process of integration, yet by Helmholtz's expedient the calculation could quite easily be made. Coming back to Lommel's paper, the limit of Lommel's results, which were the same as those of Airy, did not extend beyond something like 10° of divergence and of 20° of angular aperture. But the microscope objective had an angle of 120° of angular aperture and even more than that in some cases. Nobody suggested that Lommel's results applied to such apertures. Now, the important point was that the resolving power of a microscope was limited by the resolving power of the worst part of it; and consequently it would not do to say that we would only think of the diffraction which was produced by the back divergence angle.

Reversibility of the Microscope.

If then we had bad diffraction in front we could not get rid of that—the bad diffraction which happened in front of the lens. We should always be saddled with it, and the consequence was that unless the calculation was applied in

front of the lens as well as behind the lens, it did not tell us what we want to know. He might put the point in another way. The microscope, like every other optical instrument, was reversible. They could turn it the wrong way round and produce a small image from a big object. The power of the microscope to give you a well resolved small image under such conditions was exactly the same as its power to give a well resolved large image of a small object. Therefore the calculation of the diffraction in the case of that extremely wide angled pencil was a matter of capital importance. That was his answer to Mr. Selby's objection. With regard to Dr. Clay's objections, he thought that an interesting question was involved. Dr. Clay had suggested that the distances of bright and black dot images from one another in his (Mr. Gordon's) photograph would work out to the same as to the distances in the diagram which Dr. Clay had sketched upon the blackboard, and that the nearer they got to the source of light the greater would be the distance of the successive bright points. He entirely subscribed to Dr. Clay's theoretical proposition, but he thought that if Dr. Clay would do him the honour to look at the photograph which illustrated the paper he would see that that proposition did not in the least explain the photograph; and for the reason that the photographed images were all at so great relative distances from the source of light that the variations in their distances from one another due to the small increase in approximation of one against another to the source of light was something quite imperceptible.

Photographic Evidence.

In point of fact the distances between successive bright and dark spots were, as on Dr. Clay's rule they ought to be, measurably equal, but the observed focal plane was distant from the nearest dark spot not by a space only a little greater than this periodic distance—it was twice

as far away as on this theory it ought to be. He thought he was competent to say—he would not like to contradict Dr. Clay—but he had looked at it very critically, and had measured it with all the accuracy he could command; and it was capable of quite appreciable accuracy—he ventured to say that Dr. Clay would agree with him when he looked at the photograph and measured it, that the position of the observed focus had nothing to do with the law which Dr. Clay had referred to. The discrepancy between the displacement of the visual focus from its accepted position, as compared with the nearest black dot image, was a question of 100 per cent.—not of a fraction of 1 per cent., as it would be according to the theory which Dr. Clay had suggested. With regard to Mr. Smith's criticism of the diagram, that it was all wrong because the phases of light transmitted by a transparent dome were not the same at one moment through the whole of the area of the dome, as in the case of a wave front they were over the whole area of the wave front. That reminded him that he had missed a point, which was no doubt owing to his having broken away from his manuscript, and his answer to Mr. Smith was that it was perfectly true that the phases actually for a wave front were the same over the whole area at one and the same point of time, but as seen by a distant spectator they were apparently different, because of the different distances of the optical paths from various parts of the wave front to the spectator's eye. They had, therefore, speaking of *apparent* phases, a phenomenon exactly like that illustrated by the dome in the diagram—the arrangement was not the same, but the apparent phases were different in the wave front, just as the real phases were different in the case of the dome.

Conclusion.

There was, moreover, one other objection of Mr. Smith's which he might answer. He stated that his (Mr. Gordon's) cosine z , only affected a

portion, that is to say, the perspective dimension of a pane in the dome. It was the perspective dimensions he (Mr. Gordon) had been speaking about. The perspective dimensions of the wave front would also be affected by that cosine z . If they took a large section of a sphere, and if they did not stand at the centre, that cosine z must be taken into account. To get rid of it they must either take a small section of a sphere, or they must stand at the centre of the sphere, and in either of those cases the cosine z became $= 1$. What he wished to emphasise was that the extracts read by Mr. Smith from Lommel's paper were not at all incompatible with that proposition. Lommel was tacit about it; as a matter of fact, the formula which he used was one which was limited to a very small spherical area, and he was therefore perfectly right in treating his cosine z as being equal to one, but that was on the condition that his results should be applied only to beams of a very small angular magnitude. If anyone cared to see it he had there a mercury globule giving a source of light which was in effect an artificial star. If they would examine that they would, by throwing it out of focus, be able to see the Fraunhofer ring, and one or perhaps two of the Fresnel rings very clearly.

NOTE, added February, 1912:—

Mr. Gordon desires to have it stated that since the reading of this paper he has succeeded in carrying through the integration illustrated by the diagram of fig. 1, and has by that means established on a theoretical basis the results referred to in this paper as demonstrated by experiment. He finds, however, that the system of rings illustrated in fig. 2, which occupy the aperture of a focused pencil of light, cannot be properly described as "Fresnel rings," or computed by the Fresnel-Airy mode of calculation. He desires this paper, therefore, to be read as if he had spoken of them as diffraction phenomena pure and simple, without any reference to the producing cause.