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ON SOME IMPROVEMENTS IN THE
METHODS OF TRIAL CASE TESTING AT
THE NATIONAL PHYSICAL LABORATORY,
WITH A DISCUSSION OF CERTAIN THEO-
RETICAL QUESTIONS RELATING TO THE
FOCUS OF A LENS.

By F. J. SELBY, M.A.

Lecture delivered on March 18th, 1909.

By way of preface to my remarks to-night, I should like to say a few words as to the origins of this paper, and in explanation of its title. Some three weeks since, our Secretary informed me that no paper had yet been offered for the March meeting, and asked whether I knew of anything at the Laboratory which might be of interest for a short demonstration. A few days afterwards a member of this Society visited the Laboratory for the purpose of seeing the trial case testing bench to get some ideas for the construction of a similar bench in his own works. I may remark, in passing, that it is gratifying to know that the work done by the Laboratory in connection with trial cases is already proving useful, and I may venture to add that there seems every reason to think that its usefulness will be greatly increased in the near future. While showing the bench to my visitor I was led to the conclusion that it would plainly be helpful to say a little more, as to the methods employed, than I had said in my previous paper, and to clear up one or two points which were perhaps not before sufficiently emphasised. On considering the matter in connection with the Secretary's enquiry, I remem-

bered also that I had undertaken, last summer, to open a discussion on "the focus" of a lens, and that this had been postponed on account of the presentation of the Trial Case Report at the July meeting. It occurred to me that since a paper was wanted I might very well combine the two subjects. In some respects they are widely separated; one is an essentially practical matter, the other I shall at least *approach* from the purely theoretical point of view. But, nevertheless, they are closely connected; most of what I have to say as to trial-case testing relates to finding the focus of a lens, and anyone who is concerned with the practical problem of finding the focus of an ordinary commercial trial-case lens cannot fail to be interested in the light which is thrown upon that problem by the theoretical discussion of the physical questions connected with the focus of a "perfect" lens.

I wish to commence, then, by making a few observations with reference to the subject matter of my previous paper, read on May 16, 1907. I will speak first of all on the question of the accuracy obtainable. I think that anyone who reads the last paragraph on p. 141 in the "Transactions" for 1906-7, will conclude from what I say there that the accuracy possible may be set down generally as about $\frac{1}{2}$ per cent. It is not possible to be exactly definite except at considerable length. The accuracy naturally differs for different powers, and sometimes the error is due mainly to focussing, sometimes the difficulty is the accurate measurement of a length. For positive lenses the error will generally be less than $\frac{1}{2}$ per cent., for negative lenses this error will sometimes be exceeded. But I gather that there has been an impression in the minds of those practically interested that the accuracy possible was there greatly over-stated. I wish to say most emphatically, in order to reassure those who might find the method useful, that this impression is quite erroneous. The

accuracy possible is not over-stated. It is perfectly possible to obtain results in general to within $\frac{1}{2}$ per cent.; indeed, I have no hesitation in saying that with the improvements I could now introduce, after the experience I have had, in the apparatus—even the National Physical Laboratory bench is not yet perfect—it would not be at all difficult to reduce the error in general to below $\frac{1}{4}$ per cent., without the necessity of repeated observations.

I think that if this impression has existed in the minds of those interested, that the accuracy stated cannot be reached, it may have been due to a point made in the discussion by Mr. Chalmers, which is of considerable practical importance, from one point of view, to which I will return in a moment, but which is of no practical importance at all as regards the actual finding of the focus of a lens on the bench. Mr. Chalmers stated that if the diameter of the lens be limited to 5 mm.—taken as the diameter of the eye pupil—then, for a 1 D lens, on either side of the true focus there is a range of at least 20 mm., and probably 40 mm., within which the definition is not visibly worse than at the true focus, in other words, you cannot tell where the focus is more nearly than to say that it is somewhere within a range of 8 cm., and that in setting for the focus you might be 4 cm. out, corresponding to a 4 per cent. error. But, as I said at the time, in testing a 1 D lens one does not limit the diameter used to 5 mm. In practice, the whole diameter of the lens is employed (most of the lenses I have tried have been good enough for this), viz., a diameter of 38 mm., seven times the diameter Mr. Chalmers mentioned, and giving 49 times the accuracy, or an error not exceeding $1/49$ of 4 cm., or, say, 1 mm. With auto-collimation even this would be halved. I do not say this accuracy can be reached. It assumes a perfect lens, and the spherical aberration alone in a lens of this diameter and power approaches .1

per cent.; and want of accuracy in the surface is generally responsible for more. But certainly Mr. Chalmers' point raises no practical difficulty.

It is, however, of practical importance. It does show that small differences of 2 or 3 per cent. in the power, owing to the physical properties of light, cannot possibly make any difference to the person wearing a spectacle lens, and it shows that an error in power of, say, .03 D in a 1 D lens is of no consequence in practice.

The above argument has been stated for a 1 D lens. For higher powers it holds *à fortiori*. But I need not labour the point any longer. Any one interested is perfectly welcome to come and try for himself on the N.P.L. bench, and I have no doubt he will go away convinced that one can obtain the accuracy stated.

I show, however, a table giving the results of measurements made on the bench by a number of different observers. These are readings of the focus for a 1 D lens taken by the auto-collimation method. A.H.B. is a junior in the Laboratory, entirely without experience in taking scientific observations, whether optical or other, and his readings show the widest variation; the others are all practised observers, though not all have before taken readings of this kind.

Next, as to the ways of obtaining the highest possible accuracy. The arrangement of the apparatus, when a collimator is employed, is shown in figs. 1 and 2 of the previous paper. But it is much better to use a plane mirror and auto-collimation as diagrammatically represented in figs. 1a, 1b. This has the immense advantage that one is working always at unit magnification, and it gives double the accuracy obtained by the collimator method at its best. I stated in 1907 that I always used auto-collimation for the positive lenses, but I said it could not be employed for the negatives, as the field of view obtained was too small for convenient work. I

Readings, by auto-collimation method, of different observers for the focus of a lens of power + 1 D (nominal), (1) at full aperture, 3.8 cms., (2) at an aperture of 1 cm. Zero arbitrary.

Observer	Readings.		Means.	
	Aperture 3.8 cm.	Aperture 1 cm.	Aperture 3.8 cm.	Aperture 1 cm.
	meter	meter	meter	meter
	1.0080	1.0084		
T.S.	77	098		
	94	068	1 0083	1.0087
	85	088		
	87	100		
	1.0099	1.0055		
F.E.S.	89	058		
	89	080	1.0093	1.0078
	90	115		
	96	082		
	1.0081	1.0090		
R.T.G.	80	095		
	90	050	1.0084	1.0069
	87	062		
	80	049		
	1.0090	1.0050		
A.H.B.	90	091		
	95	130	1.0087	1.0087
	75	090		
	86	076		
	1.0089	1.0091		
C.C.P.	85	089		
	89	085	1.0086	1.0096
	86	094		
	81	120		
Mean of all readings			1.0087	1.0083

Greatest difference of any single reading at full aperture from the mean of all the readings = .0012, *i.e.*, no single observation was in error by more than $\frac{1}{8}$ of one per cent.

Zero reading (corresponding to position of principal point) = .0175 meter.

Focal length (from the mean of all the readings at full aperture) = .991₂ meter.

Power = 1.008₉ D.

wish to correct that statement. There is a considerable element of truth in it; the field of view in a good many cases is very small, and if the lens is not accurately centered this does cause trouble, especially when one has to pass rapidly from one power to another. But if the manufacturer of lenses wants to use such a bench for testing purposes the trouble is negligible. He will test a large number of lenses all of the same power at one time, and he

FIG. 1a.

will then find no practical difficulty at all in the use of auto-collimation after a small experience in the use of the bench. I would correct my former statement by saying that I now use auto-collimation for positive and negative lenses alike for all powers down to .5 D.

Let us examine a little more closely into the question of the field of view. Take first the case of a positive lens under test, a collimator being used. This is diagrammatically represented in fig. 2. The positive lens and observing microscope together form a telescope which is focussed on the cross wires of the collimator. If the collimator is some distance away, its object glass will be seen practically in focus as well as the cross wires, and the boundary of the image of the object glass is the limit of the field of view. The more distant the collimator the smaller is the field, and when it is far away the portion of the collimator cross wires seen is only a very small part near the centre. If the collimator is brought nearer to the positive lens

FIG. 1b.

FIG. 2.

under test the field of view increases in size, and at the same time its boundary—the edge of the collimator lens—goes out of focus (this is of no importance). For maximum field the collimator must be as close as possible to the lens under test, though, if the collimator lens is larger than the lens under test nothing is gained within a certain distance.

The auto-collimation case is represented in fig. 3. We have now a “virtual” collimator instead of a real one, and all that has been said above applies. The diameter of the object glass of the virtual collimator is the same as that of the lens under test; and to get the maximum field of view we must have the auto-collimating mirror as near to the lens as possible. Since for lenses of different powers the carriage with the lens holder must be in different positions on the bench, the suggestion is, of course, an obvious one, that the best place for the mirror is the back of the lens holder carriage, to which it should be attached. It is then always close behind the lens.

Now examine the case of the negative lens tested by the auto-collimation method (fig. 4). It is at once obvious from the figure that the field of view gets smaller the further the negative lens is separated from the auxiliary positive. It is, therefore, desirable to have the latter fairly large, and to arrange the conditions of test so that the negative is as close as possible to the auxiliary lens. In the shorter focus lenses, however, this statement must be modified owing to the fact that if too great a diameter of the negative lens is employed spherical aberration begins to produce an appreciable effect. But I think I have now said enough to enable any one working with the bench to arrange his apparatus to the best advantage.

A word may be said as to the auxiliary lens. I employ two of about 50 and 16 cms. focal length, respectively. It is desirable that these should be as free as possible from spherical and

chromatic aberration. The latter does not matter so much on my bench, since I use prac-

Fig. 3.

Fig. 4.

tically monochromatic light (mercury lamp); but it is convenient to be able to use light only

approximately monochromatic. The ordinary telescope objective is not the best lens for the purpose as it is used under conditions which do not approximate to those for which it is designed. A double object glass could be specially designed for the purpose which would be entirely satisfactory, or a photographic lens, with specially good centre corrections of the type necessary for telephoto work, can be used. Except for the highest accuracy, however, an ordinary telescope objective is sufficiently good.

FIG. 5.

In the case of the high-power lenses the chief difficulty is to measure the distance between the focus of the lens and the surface (or centre) with sufficient accuracy. For a 20 D lens, *i.e.*, of 5 cms. focal length, an error of $\frac{1}{2}$ mm. in the measurement of the focal length is an error of 1 per cent. in the power. To secure accuracy the lens holder carriage must run very truly, and should have a centre drive to avoid tilting. It is further very convenient to have an accurate scale running parallel to the bench with an index line (on glass) so hinged and set that it

can lie in contact with the scale. Such an arrangement was suggested by Mr. Simmance and employed on a bench constructed by Messrs. Alex. Wright and Company for the Photometry Department at the National Physical Laboratory, and is shown in the accompanying photograph (fig. 5).

For auto-collimation work a strong light is required which is thrown on to the cross wires from behind by an unsilvered parallel glass reflector at an angle of 45° to the line of the

FIG. 6.

bench. The observing microscope, with a 3-inch objective, is behind this reflector; the magnification of the eyepiece may conveniently be about 4 or 5, but can be varied if desired.

For the cross wires I would suggest that lines ruled on glass be used. If a glass, ruled as shown in fig. 6, be employed (the circles faint) the radii of the successive circles differing by, say, 2 mm., then, if the clip or arrangement for holding the lens can be trusted to fix the lens (of accurate diameter) accurately in a definite

position—which is a matter of no difficulty—the displacement of the centre of the cross wires in the image will give twice the error of centering, and this error can be read simultaneously with the setting for focus. This is very convenient when testing a number of lenses of the same focal length.

Mechanical arrangements for rapid testing might very easily be suggested, but I do not wish to enter upon merely mechanical problems this evening.

Cylindrical lenses are tested by the auto-collimation method in the same way as spheri-

FIG. 7.

cal lenses, but the axis of the cylinder must be set parallel to one of the cross lines. In practice this setting and that for focus are made simultaneously, one with each hand. Auto-collimation again halves the error of setting for axis, which is made very accurately by seeing that the image of the cross line is truly parallel to the line itself, the two being, of course, side by side. On the N.P.L. bench the cylindrical lens is itself rotated; it is, of course, possible to rotate the cross wires (at the observing end) instead, using a divided circle for reading the angle, and the reading could then more readily be taken. I am proposing to introduce

this arrangement on the Laboratory bench. With the procedure suggested for measuring centre and axis error, the only motion of the lens holder necessary is then a motion along the bench, and no rotation, with the somewhat complicated mechanism for effecting it from the observing end, is needed.

I might have said a few words, also, with regard to the testing of prisms and tinted glasses, but from fear of extending my paper to too great a length, I will now turn to the theoretical questions which arise in connection with the focus of a lens. I wish especially to speak of what I may call the natural depth of focus, which depends on the wave-length of light. There is an artificial depth of focus which depends on bad figuring of the lens; that is to say, there is a certain range for any given lens within which the definition is equally good—or equally bad—and this range may appreciably exceed the theoretical range for a perfect lens.

In my remarks on the focus of a lens I wish it to be understood that I assume a point source at a finite or infinite distance. The wave front for waves proceeding from such a source is spherical, and after passage through the lens I assume that the wave front is again spherical; in other words, I assume a "perfect" lens, or, in practice, a lens of small aperture ratio. For brevity I shall use the term focus as applying to the focus conjugate to this point source whether it be at a finite or infinite distance, and for convenience I shall limit my remarks to a positive lens and a real focus. The plane through the focus perpendicular to the axis, I shall call the focal plane; it should, perhaps, more correctly, be called the image plane.

The definition of the focus which I assume is that it is the point at which all the secondary waves (after emergence of the light from the lens) arrive in the same phase. There is only one such point—there is only one centre of a

sphere—and it is the point of maximum energy, of maximum light intensity. At no other point whatever behind the lens can we get so great an illumination; at no other point do all the light waves reinforce each other so as to give the maximum amplitude of vibration.

The image of the point source, however, which is seen in the focal plane is not a point; as is well known it consists of a central bright disc surrounded by alternately dark and light rings.

FIG. 8.

The disc is not uniformly bright; the brightness falls off gradually, but rapidly, to the first dark ring. This is shown in the familiar intensity curve representing the distribution of light energy in the focal plane (fig. 7). The first minimum of intensity is at a distance $1.22 \lambda \cdot f/a$ from the axis, where a is the diameter of the lens, and this minimum is zero. It is well to note also that of the whole light which passes through the lens 83.9 per cent. is concentrated in the central disc, the remainder being distributed in the surrounding bright rings.

Now, in the discussion on my previous paper Mr. Chalmers pointed out that for some distance on either side of the true focal plane no visible change takes place in the appearance of the central disc. Its diameter remains perceptibly constant; the total amount of light distributed over the disc (which is perhaps more important than the intensity at its centre) does not de-

crease sufficiently to be noticeable. Lord Rayleigh has discussed the matter fully, and has shown theoretically and experimentally that the definition is not visibly impaired, if, instead of the true focus, we examine a point on the axis the secondary waves arriving at which do not differ in phase by more than a quarter of a wave-length. This is illustrated in fig. 8. If F be the true focus, then AF and PF are equal. If G, G' be points such that $PG - AG = AG' - PG' = \lambda/4$, then, so far as can be detected by the eye the definition is equally good within the range GG' .

The distance GG' can readily be expressed in terms of the aperture and focal length (focal distance). We find that $FG = 2f^2/a^2 \cdot \lambda^*$ or $GG' = 4f^2/a^2 \cdot \lambda$, where a is the aperture.

If we apply this formula to the case discussed by Mr. Chalmers, to which I have already referred, we have to put $f = 1,000$ mm. $a = 5$ mm., and taking $\lambda = .5\mu$, or $.0005$ mm., we get $FG = 40$ mm., so that there is a range of 40 mm. on either side of the focus within which the definition may be regarded as equally good.

But it is of interest to go further than this and examine more closely the intensity pattern in planes parallel to the focal plane. This has been done very completely by Lommel, in a paper referred to by Lord Rayleigh in his article on the Wave Theory of Light in the *Encyclopædia Britannica* (Vol. 24), and Lommel verified thoroughly by experiment the results he arrived at by theory, so that their accuracy is unquestioned. I may remark here that Mr. J. W. Gordon read a paper before the Optical Convention in 1905, in which he made certain suggestions as to the focus of a lens, which he repeated, in the course of a discussion, before this Society in February of last year. Mr. Gordon was, unfortunately, plainly unac-

* In general, if δ be the difference of path ($PG - AG$) between the axial and edge rays to the point G , then $FG = 8f^2/a^2 \delta$.

quainted with Lommel's paper. The suggestions made in Mr. Gordon's paper of 1905 are clearly and unmistakably negatived by Lommel's results—published some 10 years earlier. This will, I think, be obvious from the curves which I have to show.

FIG. 9.

Before, however, I show Lommel's intensity curves for extra-focal planes, I wish to give

some results for the variation in light intensity along the axis as we pass away from the focus. In stating the expression for the light intensity it is convenient to write it in terms of the difference of path for the rays from the centre and edge of the lens. If we call this difference δ , then the intensity of illumination at any point on the axis of the lens is given by—

$$I = \left(\frac{\sin \frac{\pi \delta}{\lambda}}{\frac{\pi \delta}{\lambda}} \right)^2$$

Where λ is the wave-length and the intensity at the focus is taken as unity.

If $\delta = \lambda/8$	$I = .950$
$\delta = \lambda/4$	$I = .811$
$\delta = 3\lambda/8$	$I = .615$
$\delta = \lambda/2$	$I = .405$

FIG. 10.

If $\delta =$ any multiple of λ , $I = 0$.

$\delta = \lambda/4$ corresponds to Lord Rayleigh's limit for the focal range, and I wish particularly to call your attention to the fact that at this distance from the focus the light intensity on the axis has already fallen by nearly 20 per cent. from its value at the focus. The change in the light intensity can hardly fail to become noticeable if we depart from the focus beyond this point.

Let us now examine Lommel's curves (fig. 9). These give the distribution of light intensity in

planes parallel to the focal plane. It is again convenient to express the position of the planes in terms of δ , and the fig. shows the curves for the planes corresponding to $\delta=0$ (the focal plane itself) $\delta=\lambda/8$, $\delta=\lambda/4$, $\delta=\lambda/2$. The first three of these curves, up to the first minimum, are of exactly similar character, and it should be noted that the distance of the first minimum from the optic axis is the same in each. There is thus clearly progressive, though slow, deterioration in the definition as we pass away from the focus. I have drawn the curve for $\delta=\lambda/8$, because it is in this plane that Mr. Gordon, in his Optical

FIG. 11.

Convention paper, contends that the best definition is obtained. Actually, the definition will, of course, be only very slightly worse than in the focal plane; but it is plain that everything is against Mr. Gordon's contention. There are three possibilities which might conceivably give rise to better definition in an extra focal plane—(1) greater intensity of light on the axis, (2) greater total amount of light within the first dark ring, (3) greater rapidity in the drop from brightness to darkness. The central disc

might be almost uniformly illuminated, with a sudden drop to darkness at the edge. However, none of these cases actually arises and the focal plane remains the plane of best definition.

The above argument is for a point source. The case of a line source or an "edge" might be similarly considered, but it is sufficiently clear that no different result is to be expected, and I cannot now enter into these cases, nor into the entirely distinct question of the effect of imperfection in the lens.

In the preface to his *Grammar of Science* Professor Karl Pearson writes of his own book :
 " . . . it is essentially intended to arouse and stimulate the reader's own thought rather

FIG. 12.

than to inculcate doctrine; this result is often best achieved by the assertion and contradiction which excite the reader to independent enquiry." I am grateful to Mr. Gordon's "assertion" for stimulating me to examine into Lommel's results, which are of exceptional interest, and of which I have only been able to show a mere fragment this evening. If, in discussing Mr. Gordon's "assertion," I have supplied the "contradiction," I trust he will take it from me

that my highest aim is to excite independent enquiry.

The additional illustrations show (fig. 10) the complete optical bench for testing trial lenses as constructed by Messrs. R. and J. Beck, (fig. 11) the travelling carriage with rotating head, and (fig. 12) the arrangement for testing a negative lens by the auto-collimation method, using an auxiliary positive lens.