III.—*Some Remarks on the Apertometer.*

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(*Read* 14*th January*, 1880.)

Several papers have recently been published by Professor Hamilton L. Smith, Dr. Woodward, Mr. Wenham, and Mr. R. Hitchcock,† relating to the measurement of apertures, which touch from different points of view the use and performance of the apertometer. In order to remove certain difficulties which have been met with in the application of my method, and some objections which have been made to it, I beg to give here some further explanations as to the arrangement of the apparatus and the principles on which it is based. From the discussions, I infer that some essential points in my method of measurement have not received due attention, owing perhaps to the very brief—though exhaustive—explanation in Mr. Zeiss' description of the instrument.‡

In measuring the apertures of an objective by observing the limits of its telescopic field of vision, "the real area of field in the microscopic action of the lens, or the central part of this area, must be made to act as the area of aperture in telescopic vision." Or, the same condition expressed in another way: the diaphragm, through which in this telescopic observation the image-forming pencils have access to the eye of the observer, must be in such a position as to be conjugate to the same focus in front of the objective, from which the rays start in microscopic observation.

With the view of rendering clear the meaning of this condition as well as its necessity, I may refer to a general proposition which, though derived from the A B C of optics, applies to many important questions in the theory of optical instruments.

Let L be any system of lenses which takes in and transmits rays from different objects, and O and O′ two limited areas in planes perpendicular to the axis, which in position and figure are in the relation of an object to its image in respect to L, i. e. which are situated at conjugate points of the axis and are conjugate one to another as to their linear extension, then all the rays entering the system L through the area O will leave the system through the area O′; and no single ray can emerge from L through the area O′ which has not entered L through the area O, from whatever objects the rays considered may start, or towards whatever points they may proceed. (It is the same thing, whether O is considered to be the object and O′ the image or *vice versâ*.)

* The original paper is written by Professor Abbe in English.

† 'Am. Quart. Micr. Journ.,' i. (1879) pp. 194, 272, 280, 284.

‡ This Journal. i. (1878) p. 19.

This proposition is directly derived from the elementary notion of conjugate points in optics: that all rays diverging in different directions from one point are gathered in, or pass through, the other conjugate point after their passage through the system, setting aside spherical aberration.

Now let O′ be the clear opening of a diaphragm in any position *at the back* of an objective L, and O the real or virtual image of this opening as it is projected by the system to the other side. By virtue of the proposition above, this diaphragm will exclude from the passage through the system all rays which have not entered through the area of its conjugate image in front, and will admit all rays which have entered through this area. Therefore *any diaphragm at the back of an object-glass acts in every respect exactly like a similar diaphragm at the conjugate focus in front,* the clear diameter of which is to the clear diameter of the former one in the ratio of the linear amplification at the pair of conjugate foci in question.

In applying my apertometric method, there is in every case such a diaphragm behind the objective limiting the pencils from external objects (from the index-pointers, for instance, on the glass disk) in their passage to the observer's eye. It may be either an eye-hole at the end of the tube, or the pupil of the eye itself; and when an auxiliary Microscope is used, this limiting diaphragm may be either a stop belonging to its object-lens, or the brasswork of the lens itself if there is no such stop. Therefore all the pencils of light by which the telescopic image becomes visible to the eye of the observer must in their *entrance* to the objective have passed the area of the image conjugate to this diaphragm in respect to the objective, or to the objective and the auxiliary lens together. In consequence of this, the conjugate focus, where the image of the said diaphragm would be formed, will represent in every case the apex of the angle which is to be measured as the angle of aperture; this point therefore, *and no other*, must be made to coincide with the focus of the objective in its ordinary microscopic performance, *and* also with the centre of the divided circle of the measuring apparatus.

If an objective is focussed to the centre of the measuring circle by means of an eye-piece and a long tube, or, as is recommended by Professor H. L. Smith,* without any eye-piece, a point very near to the front principal focus of the objective—in the latter case even inside this focus—is made to coincide with the centre. If now, as Professor Smith does, the telescopic image is observed by the naked eye through an eye-hole at the end of a tube of perhaps 4 inches length, the conjugate image of this eye-hole, i.e. the apex of the angle-defining pencils, must obviously occur at a less

* This Journal, ii. (1879) p. 776.

or greater distance beyond the centre, according to the focal length of the objective in question; and the distance may be very considerable with a low-power objective. The excentricity of the apex will not permit a correct evaluation of the angle in every case; but if, as in Professor Smith's apparatus, the rays have to pass to air from this excentrical point by a spherical surface, their divergence of course must be considerably increased by the refraction at this surface. With the same apparatus Professor Smith will obtain the true angular aperture of a low-power objective, either by focussing the objective with an eye-piece on his 4-inch tube, or observing the telescopic image by an eye-hole at a considerable distance from the objective.

In applying the method of telescopic observation by means of an auxiliary Microscope (acting as a terrestrial eye-piece) for enabling more accurate observation in the case of object-glasses of short focal length, any 2-inch to 4-inch lens may be used. But if the objective has been focussed previously to the centre of the divided circle by an eye-piece on the 10-inch tube, and afterwards an ordinary 2-inch or 4-inch lens is put to the draw-tube without any further precautions, the result will be incorrect. The clear aperture of the auxiliary lens representing now the critical diaphragm spoken of above, and this diaphragm being at the distance of a few inches only from the objective, the conjugate focus to it, i.e. the apex of the aperture angle will be at a considerable distance from the ordinary focus of the objective, and from the centre of the measuring apparatus, at least in the case of an objective of low or moderate power. Moreover, as a 2-inch or 4-inch lens generally has a clear aperture of 0·5 inch or more, the area of entrance of the image-forming pencils to the objective (which is in this case the image of the auxiliary lens projected in front of the objective) will be a considerable part of the focal length of the objective, much too large for admitting a good definition of objects situated so far from the aplanatic focus. For these reasons a correct application of the method requires a suitable stop behind the auxiliary lens, as expressly stated in the original description of the apertometer. The position and the diameter of this stop must be regulated in such a way, that its clear area in respect to the auxiliary lens corresponds to, or is optically conjugate with, a *small* central part of the field of the eye-piece applied in the previous focussing of the objective. According to this condition, the correct place of this stop may be easily calculated from the focal length of the auxiliary lens and the length of tube, for which apertures have to be measured. This place will be always somewhat within the posterior principal focus of the auxiliary lens.

Reducing the clear opening of the diaphragm, in the case of an

auxiliary lens, to a small part of its focal length, or applying a small eye-hole in the case of an observation by the naked eye, the area of entrance of the image-forming pencils may be diminished to an extremely small diameter, owing to the inverse action of the amplification of the objective. With a $\frac{1}{12}$-inch objective, for example, there is no lack of light, and no other inconvenience, if this entrance-area is reduced to a diameter of one-thousandth of an inch. *The pencils crossing in the apex at the conjugate focus of the objective may be made in this way to define the angle of aperture practically like single rays.*

The foregoing remarks should explain all the discrepancies which have appeared to some observers in using the apertometer, and they will be applicable more or less to every method of measuring apertures.

The irregularities observed by Professor H. L. Smith with low-power objectives will disappear at once, as soon as he focusses the objective *not* by the naked eye but by an eye-piece, and takes care that the position of the diaphragm in this eye-piece corresponds virtually (i. e. the action of the field-lens considered) to the position into which the eye-hole at the end of this tube is brought afterwards. Repeating his measurements in this way with the apparatus figured on p. 775 of his paper, he will find the correct angle of the lowest powers as of the highest, but, of course, the angle corresponding to a length of tube of 4 inches, according to the description of his apparatus. In the case of a low-power objective, this angle may be sometimes considerably less, sometimes considerably larger, than the aperture of the same objective on the 10-inch tube, both cases being equally possible, though not equally frequent. Professor Smith's own method of observation is subject to exactly the same conditions for obtaining correct results, the position and the *diameter* of the illuminating area (tissue-paper or ground glass) at the end of his tube requiring special precautions. Both methods will yield similar results with low or high powers, if both are applied with the same precautions. But for both methods Professor Smith will do well to exclude another source of errors in his apparatus by taking away the magnifying lens at the end of the swinging arm *between* the centre *g* and the eye-hole *f*, applying this lens *outside* the eye-hole, if he cannot dispense with it.

A stop with a small circular hole at the centre of Professor Smith's apparatus, as suggested by Dr. Woodward in his paper, will not correct by itself a faulty adjustment of the objective or the diaphragm, and will be quite unnecessary if the adjustment is made correctly. But in the case of a coarsely incorrect adjustment the observation would be practically impossible; the limits of the aperture would be invisible as long as the conjugate area to the critical diaphragm does not approximately coincide with the circular hole.

With regard to the manner in which apertures are indicated by the apertometer—what Dr. Woodward calls the "arbitrary scale of my own invention"—and the objections made against it by different writers, the following explanations may be useful:—

The application of such a scale, instead of a simple angular division of the glass disk, would be unwise if this scale were intended as a device for making out the angle of an objective in crown glass, or even balsam angles or air angles. But the idea from which this part of the arrangement has arisen has no such aim, as Mr. J. W. Stephenson * and Mr. R. Hitchcock † have clearly explained. My intention has been to introduce practically as a matter of simple and direct observation a new expression of aperture, quite apart from the angles, which may be conveniently applied by microscopists in all scientific discussions pertaining to apertures.

In my opinion, apertures, until recently, have not been measured in any strict sense of the word. *Apertures cannot be measured by degrees.* Angles being measured by degrees, the angular aperture i. e. the *angle of* aperture can be likewise, of course. But the angle by itself is no measure of aperture—at least, nobody has attempted to prove such an hypothesis. An angle is a mere indication or statement of an aperture, just as a number of degrees on the thermometer may exactly indicate a quantity of heat, under given circumstances, but does not measure it. Just so the angle of divergence of the gold-leaves in an electroscope indicates or determines an electric charge, but does not measure it. There is not one point of view from which apertures can be compared *quantitatively* by means of the angles, because there is not one function in the performance of the Microscope in respect to which twice the angle represents the double effect, and three times the angle the triple effect, except when the angles do not exceed a few degrees. Neither the quantity of light, nor the resolving power, nor any other performance connected with the aperture, is increased in the ratio of 1:2 if the angle (air or balsam or interior angle) is increased, say, from 60° to 120°. Moreover, apertures cannot be directly compared as to greater or less, by the angles, as long as they do not relate to exactly the same medium. For example, no comparison by means of degrees can be made between a dry lens and an immersion lens, except by reducing the angles to an entirely arbitrary common medium which has no actual existence either for both objectives or, at least, for one of them.

None of these defects of the angular expression of aperture can be overcome by introducing, as Dr. Woodward suggests, the "first interior angle" of the objective. The adoption of this plan, on the contrary, would appear to me to be a serious change for the worse.

* This Journal, i. (1878) p. 51.

† 'Am. Quart. Journ.,' i. (1879) p. 286.

It withdraws the basis even of an indisputable indication of aperture, making the angle to depend on two practically inaccessible elements of very uncertain character—the refractive index and the external curvature of the front lens. Crown glass is a variable thing. In the crown fronts, for instance, of Mr. Zeiss' different objectives, the refractive index of the ray D varies from 1·501 to 1·544, i. e. in the range of 3 per cent. of the average value, and nobody, except the maker, can know what may be the index in any given objective without dismounting it.

But there are many low-power object-glasses the front lens of which is made of flint. Ought an interior angle of, say, 37° in such a flint front of 1·62 refractive index to be considered as denoting less aperture and less resolving power than 40° "interior angle" in a crown front of 1·50? Moreover, what would be the signification of "first interior angle" if the front lens has not a plane external surface, but a curved one? I have at my disposal a dry $\frac{1}{8}$ objective, made for a special purpose, the numerical aperture of which is 0·91 (air angle = 132°); but the first interior angle, within the crown front of 1·513 refractive index, is 87°, owing to a concave surface of the front lens. If this objective were to be compared with other glasses of ordinary construction by the interior angle, and if this angle, in fact, had the prominent signification which Dr. Woodward proposes, the glass spoken of would range among immersion lenses, which, of course, neither Dr. Woodward nor anybody else will concede.

I may briefly signalize the principal conclusions by which, in my opinion, the product $n \,.\, \sin w = a$ (a, numerical aperture; w, semi-angle of aperture; n, refractive index of the *external* medium to which the angle relates) can be demonstrated as the true and general measure of aperture.

1. The expression of aperture, as a *quantity*, must be based on the evaluation of the number, or quantity, of rays collected by the objective from one point of the object and transmitted to one point of the image.

No ray can exist in the wide-angled cone of light, emanating from the object, which is not contained in the contracted cone of light going from the objective to the microscopic image, *et vice versâ*. As soon as this latter cone of light is reduced to a narrow-angled pencil, owing to the distance of the apex, it is unquestionable, from well-established optical principles, that double or triple the angle represents double or triple the number of rays contained in one section plane through the axis. Therefore the capacity of an objective of collecting rays, i. e. its aperture, is proportionate to the angle of the narrow-angled pencil on the side of the image, other circumstances being equal.

On a theorem announced long ago by Professor Helmholtz and

myself, the angle of the emerging pencil (a narrow-angled pencil considered only) does not vary with the angle of the wide-angled entering pencil or with the angle of aperture, but does vary with the *sine* of the semi-angle, and if different working media are considered at the same time, with the product $a = n . \sin w$—aplanatic foci supposed. If there are any two objectives different in focal length and different in working media, for example a dry $\frac{1}{4}$ and an immersion $\frac{1}{12}$, which yield equal values of this number a, the pencils forming the microscopic image will be equal in angle as soon as equal amplification of the images is attained in any way whatever (by projecting the images to different distances or by interposing other lenses); and if the value of a for one objective is greater in any ratio, the angle of the image-forming pencil will be greater in the same ratio.* For this reason the said product must be considered as the true quantitative expression of aperture.

2. In addition to, partially in consequence of, these statements, it may be strictly proved that all principal functions of microscopic vision depend upon the expression of numerical aperture and cannot be indicated in a general and exhaustive manner apart from it.

The illuminating power of an objective, i. e. the brightness of image attainable with any definite amplification, is *proportionate* to the square of a, where a may relate to the whole aperture or to that part of it which is made active by the illuminating pencil.

The depth of focus is inversely proportionate to a, any particular amplification considered.

The resolving power, defined by the minimum distance δ of separable elements in regular structures, is expressed by the equations

$$\delta = \frac{\lambda}{a} \text{ and } \delta = \frac{1}{2}\frac{\lambda}{a}$$

(λ denoting the wave-length of the image-forming rays); the former relating to the case of a strictly axial pencil, the latter to the case of the utmost oblique incidence of the illuminating pencil.

3. The practical usefulness of this expression of aperture is shown by the following considerations:—

The numerical value is an *absolute* measure, in the strict sense of metrological science, by defining apertures without regard to changeable or accessory elements (as, for instance, the refractive index of the working medium) and comparing them with a natural standard unit. *This unit of aperture is the capacity of an objective of collecting the whole hemisphere of rays emanating from one luminous point within a medium of the refractive index* 1·00, and is

* From the proposition referred to above may be derived a new method of measuring apertures, quite different, in principle and in process, from the methods hitherto applied, which will be described hereafter.

readily represented by any objective yielding $n \cdot \sin w = 1$. This would be an immersion lens, the water or balsam angle of which is double the critical angle within water or balsam. But any definite part of the unit can be represented by a dry objective,—for example, the half of the unit by an objective of 60° air angle.

By the value of a any two objectives performing with different media are directly compared, without needing the introduction of a merely hypothetical angle (as, for instance, the balsam angle of a dry lens) which has no real existence in the performance of the system.

The *numerical value* affords at once a clear and exhaustive expression of the relation between any two objectives in respect to all significant functions of the optical performance as far as they depend on aperture; while by adhering to the *angles* the judgment would be greatly misled. Comparing two dry lenses of 110° and 140° angular aperture in air, the increase of aperture in the latter seems, according to the angles, equal to 27 per cent. Comparing the true measures (num. ap.) 0·82 and 0·94, the real difference is reduced to not quite 15 per cent. Again, taking the case of an objective of 1·40 numerical aperture on the homogeneous-immersion system, its balsam angle (138°) looks remote from the ultimate limit of 180°, and there appears ample range for further increase of aperture and resolving power. But, in fact, the aperture is brought within 7–8 per cent. of the ultimate limit which in every objective is the refractive index of the least refractive medium between the object and the first convex surface of the system. As long as there is not used a substance of much higher refractive power than crown glass as a mounting medium, working medium, and front lens, the utmost increase of the angle of the entering cone of rays would increase very slightly the angle of the image-forming pencil on the other side; and as from nothing, nothing can come, the advance in performance would remain practically inappreciable.

From these explanations it will, I hope, be understood that the application of the numerical scale in the apertometer is not a mere fancy, but a deliberate attempt to get rid of the serious defects appertaining to the method at present in use.

I add a few remarks relative to the degree of accuracy attained by the apertometer, as I cannot agree with Dr. Woodward in the objections he makes to the construction of the apparatus on this point.

Dr. Woodward is mistaken in supposing errors to be introduced by the reflecting surface of the glass disk. This subject was carefully examined before I adopted the arrangement in question. The angle included by any two symmetrical lines of the division

has a *maximum* value if the apex coincides, by reflexion, with the exact centre of the circle. It is thus seen that deviations of the apex within the upper surface of the disk will change the angle by quantities of "second order" only; and such deviations could be increased up to 2 mm. without any appreciable fault remaining in the *middle* of both readings. The single readings may differ one from the other by some degrees, but the mean will yield the correct value; and as the observer *must* always make both readings, and take the mean, the difference is absolutely unimportant. For my own use I often apply a disk without any central mark; the silvered glass with the circular hole on the disks made by Mr. Zeiss being intended principally not for centring, but for preventing measurement with incorrect adjustment of the objective.

There is no other mechanical condition for the correct performance of the disk, but that the line of intersection between the inclined surface and the upper surface of the disk must pass through the exact centre of the circular division; the angle does not need 45° exactly. Just the same condition must be fulfilled with Dr. Woodward's apparatus; and as it is not more difficult to grind an inclined face than it is to grind a plane surface, it follows that the convenience of manipulation afforded by the reflecting surface in my arrangement is not obtained by a sacrifice of accuracy.

In engraving circular divisions of unequal intervals by means of a dividing machine, according to a calculated table, there is no greater source of error, provided the maker is not a dunce; and if the index of the glass plate has been correctly measured, the reliability of such a scale will not be inferior to that of a division into degrees. Whether the scale has been made on the base of the right refractive index may be ascertained by the observer by precisely the same method which Dr. Woodward suggests for observing the index of a disk divided into degrees. He may ascertain that any immersion lens with an aperture exceeding unity, yields exactly the unit by the mean of both readings, when it is applied to the apertometer as a dry lens; and this, indeed, is the method used by Mr. Zeiss for testing every plate before sending it away, in order to prevent accidental use of a wrong crown. In order to have the front lens separated from the disk by an infinitely thin film of air only, and to prevent the projecting brasswork of the objective ($\frac{1}{8}$) from stopping-off the utmost oblique rays, a drop of soft balsam is placed on the disk, and after it has dried superficially the objective is slightly pressed down upon it.

The divisions of the scale on the apertometer disk go from 0·05 to 0·05 of the numerical unit. The intervals being equal to 2 mm. on the average, no observer will find any difficulty in esti-

mating at a glance the fifths of an interval. The possible error of this estimation will hardly exceed half the unit of the second decimal, since nobody with a moderate amount of care would read ·03 if the point in question is decidedly nearer to ·02 than to ·03. The error of measurement will be limited, therefore, to about ½ per cent. of the unit, corresponding to half a degree, or 30′ in angle, as far as the actual reading is concerned.

An exactness of *reading* to this extent is evidently more than sufficient. An unavoidable amount of uncertainty resulting from the nature of the object, and many other sources of slight error, will always limit the real exactness of *observation* beyond 1 per cent. of the unit, different observers and different methods of equal reliability being supposed. In low powers slight deviations in the length of tube; in high powers slight alterations of the cover-adjustment, will admit of much greater difference than the error of reading will introduce. It should be observed that in high-angled objectives the aperture has not the same value for different colours, owing to the difference of focal length (or of amplification), even in objectives, which are perfectly achromatic in the ordinary sense. In the case of very large angles, the aperture, angular or numerical, will be greater for the blue rays than for the red, generally by more than 1 per cent. Last, not least, there is no possible interest, either practical or scientific, appertaining to single degrees, or half-degrees, of aperture angles; for no microscopist in the world will be able to make out any difference in the performance of objectives, as long as the numerical apertures do not differ by *several* per cent., other circumstances being equal.

For these reasons I consider all attempts at very accurate measurements of this kind to be useless.

There has been made another objection, from quite a different point of view, by Mr. Wenham,* who declares the apertometer to measure the angle of field, instead of, or in addition to, the angle of aperture. I hope Mr. Wenham will abandon this objection after having considered the dioptrical proposition spoken of early in this paper, and its bearing on the subject. By reducing to a pin-hole the critical diaphragm in apertometric observation he will be able to confine the entering pencils to one-hundredth of the ordinary field of vision of the object-glass. But then he will at once perceive that a greater or less diameter of the entrance-area has no influence at all on the outcome of the observation, not even if this area should be much greater than the field of vision in the ordinary microscopic use of the objective, provided the diaphragm be in its correct position. Too large a field taken as entrance-area in telescopic vision will deteriorate definition and for this reason

* 'Am. Quar. Micr. Journ.,' i. (1879) p. 280.

will make the true position of the index-pointers uncertain, but will add nothing substantially to the reading of aperture.

The size of the field has practically no influence by the method in question.

As to the observation on the oil-immersion $\frac{1}{8}$, to which Mr. Wenham alludes in proof of his assertion, its fallacy will be obvious on a moment's reflection. When a microscopist observes any object of say 7 mm. in diameter, and wants to observe its whole extension within one field of vision, he certainly will take care that the stop in his eye-piece does not confine the field to 6, or 4, or 3 mm. Now the telescopic image of distant objects, delineated on the back of an oil-immersion $\frac{1}{8}$, must extend to upwards of 7 mm., as is evident at once to everybody from the diameter of the back lens; and for observing the *limits* of this telescopic image the whole must, of course, be within the field of the auxiliary Microscope. Why did Mr. Wenham expect then to see the limits of the aperture whilst using eye-pieces, the stops of which confine the field of vision of the auxiliary Microscope to perhaps 3, or 4, or 6 mm.?

A few words on the origin of the apertometer. Dr. Woodward, in his paper "Description of a New Apertometer,"* speaks of my arrangement and every part of my method as a "modification" of an apparatus described by Mr. R. B. Tolles in 1873. I am aware of Mr. Tolles' priority in the description of the semicircular glass disk, and I highly appreciate his merits in the propagation of sound ideas about the aperture subject; but Dr. Woodward will allow me to observe that two principal features of my arrangement, the observation of the telescopic field of vision of an objective and the numerical indication of the aperture, have obviously no connection at all with a glass disk. In fact, I have applied this method of observation with the naked eye and with an auxiliary Microscope, since 1870, in measuring air angles by means of a divided rule fixed below the stage of a vertical stand at a definite distance (100 mm.) from the focal point of the objective, black disks moved along this rule being used as indicators for marking the limits of the telescopic field on the scale; an arrangement which I use even now with objectives of moderate air angle, the rule being divided after the numerical scale. Shortly afterwards, when I felt the necessity of extending measurement to apertures approaching or exceeding the maximum air angle, I interposed a semicircular lens of well-known refractive index, centred in the stage-hole of the stand, between the objective and the scale, in order to prevent the angular extension of the cone of rays in its passage to air and the total reflexion of the oblique pencils—a device essentially identical with the arrangement described and

* 'Am. Quart. Micr. Journ.,' i. (1879) p. 284.

figured by Professor H. L. Smith in the paper quoted above. But I soon abandoned this method as being inconvenient, because a very slight deviation of the focus or apex from the centre of the lens must introduce a perceptible error, owing to the refraction of the spherical surface, unless the lens were very large. Since 1871, for measuring great apertures I have used a *rectangular* plate of crown glass 100 mm. in length and 60 mm. in breadth, three edges ground, and one, on the long side of the rectangle, polished to an angle of 45°, for allowing the plate to be applied on the stage of an ordinary Microscope. The scale of numerical aperture was engraved along the three perpendicular edges according to previous computation, and index-pointers used as now. In this shape (which is briefly described and figured in Nägeli and Schwendener, 'Das Mikroskop,' 2nd ed. p. 170) the apparatus has been used for a long time by Mr. Zeiss and myself; many microscopists have seen, and some of them have the appliance. I explained and demonstrated its use at the meeting of the Gesellschaft für Medicin und Naturwissenschaft of Jena, November 1, 1872. On the basis of measurements made by this rectangular plate Mr. Zeiss in his catalogue of August 1872 stated his immersion objectives to yield an aperture of 108 degrees water angle, thus exceeding the maximum air angle by several degrees.

The only part of the apparatus, besides the name, which is of more recent origin, is the circular shape of the glass disk, which was adopted by Mr. Zeiss when he began to make the apertometer for sale.

The description of my arrangement has been delayed for so long a time because I proposed to explain the method of aperture-measurement and allied methods for measuring focal lengths, amplifications, &c., in connection with a more exhaustive discussion of the aperture theme. Now, as priority results from literary publicity only, the application of the glass disk for measuring apertures belongs to Mr. Tolles, of course. But as to those parts of my arrangement which have not been described by others, the foregoing remarks will show them to be independent of Mr. Tolles' or any other apparatus.