# ON THE MECHANISM OF THE EYE

By

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## II. *The Bakerian Lecture. On the Mechanism of the Eye. By* Thomas Young, *M. D. F. R. S.*

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I. In the year 1793, I had the honour of laying before the Royal Society, some observations on the faculty by which the eye accommodates itself to the perception of objects at different distances.* The opinion which I then entertained, although it had never been placed exactly in the same light, was neither so new, nor so much forgotten, as was supposed by myself, and by most of those with whom I had any intercourse on the subject. Mr. Hunter, who had long before formed a similar opinion, was still less aware of having been anticipated in it, and was engaged, at the time of his death, in an investigation of the facts relative to it; † an investigation for which, as far as physiology was concerned, he was undoubtedly well qualified. Mr. Home, with the assistance of Mr. Ramsden, whose recent loss this Society cannot but lament, continued the inquiry which Mr. Hunter had begun; and the results of his experiments appeared very satisfactorily to confute the hypothesis of the muscularity of the crystalline lens. ‡ I therefore thought it incumbent on me, to take the earliest opportunity of testifying my persuasion of the justice of Mr. Home's conclusions, which I accordingly mentioned in a Dissertation published at

* Phil. Trans. for 1793, p. 169.
† Phil. Trans. for 1794, p. 21.
‡ Phil. Trans. for 1795, p. 1.

Gottingen in 1796,* and also in an Essay presented last year to this Society.† About three months ago, I was induced to resume the subject, by perusing Dr. PORTERFIELD's paper on the internal motions of the eye;‡ and I have very unexpectedly made some observations, which I think I may venture to say, appear to be finally conclusive in favour of my former opinion, as far as that opinion attributed to the lens a power of changing its figure. At the same time, I must remark, that every person who has been engaged in experiments of this nature, will be aware of the extreme delicacy and precaution requisite, both in conducting them, and in drawing inferences from them; and will also readily allow, that no apology is necessary for the fallacies which have misled many others, as well as myself, in the application of those experiments to optical and physiological determinations.

II. Besides the inquiry respecting the accommodation of the eye to different distances, I shall have occasion to notice some other particulars relative to its functions; and I shall begin with a general consideration of the sense of vision. I shall then enumerate some dioptrical propositions subservient to my purposes, and describe an instrument for readily ascertaining the focal distance of the eye. On these foundations, I shall investigate the dimensions and refractive powers of the human eye in its quiescent state; and the form and magnitude of the picture which is delineated on the retina. I shall next inquire, how great are the changes which the eye admits, and what degree of alteration in its proportions will be necessary for these changes, on the various suppositions that are principally

* De Corporis humani Viribus conservatricibus, p. 68.

† Phil. Trans. for 1800, p. 146.

‡ Edinb. Med. Essays, Vol. IV. p. 124.

deserving of comparison. I shall proceed to relate a variety of experiments which appear to be the most proper to decide on the truth of each of these suppositions, and to examine such arguments as have been brought forwards, against the opinion which I shall endeavour to maintain; and I shall conclude with some anatomical illustrations of the capacity of the organs of various classes of animals, for the functions attributed to them.

III. Of all the external senses, the eye is generally supposed to be by far the best understood; yet so complicated and so diversified are its powers, that many of them have been hitherto uninvestigated; and on others, much laborious research has been spent in vain. It cannot indeed be denied, that we are capable of explaining the use and operation of its different parts, in a far more satisfactory and interesting manner than those of the ear, which is the only organ that can be strictly compared with it; since, in smelling, tasting, and feeling, the objects to be examined come almost unprepared into immediate contact with the extremities of the nerves; and the only difficulty is, in conceiving the nature of the effect produced by them, and its communication to the sensorium. But the eye and the ear are merely preparatory organs, calculated for transmitting the impressions of light and sound to the retina, and to the termination of the soft auditory nerve. In the eye, light is conveyed to the retina, without any change of the nature of its propagation: in the ear, it is very probable, that instead of the successive motion of different parts of the same elastic medium, the small bones transmit the vibrations of sound, as passive inelastic hard bodies, obeying the motions of the air in their whole extent at the same instant. In the eye, we judge very precisely of the direction of light, from the part of the retina on which it impinges: in the ear, we have no other criterion than the slight difference of motion in the small bones, according to the part of the tympanum on which the sound, concentrated by different reflections, first strikes; hence, the idea of direction is necessarily very indistinct, and there is no reason to suppose, that different parts of the auditory nerve are exclusively affected by sounds in different directions. Each sensitive point of the retina is capable of receiving distinct impressions, as well of the colour as of the strength of light; but it is not absolutely certain, that every part of the auditory nerve is capable of receiving the impression of each of the much greater diversity of tones that we can distinguish; although it is extremely probable, that all the different parts of the surface exposed to the fluid of the vestibule, are more or less affected by every sound, but in different degrees and succession, according to the direction and quality of the vibration. Whether or no, strictly speaking, we can hear two sounds, or see two objects, in the same instant, cannot easily be determined; but it is sufficient, that we can do both, without the intervention of any interval of time perceptible to the mind; and indeed we could form no idea of magnitude, without a comparative, and therefore nearly cotemporary perception of two or more parts of the same object. The extent of the field of perfect vision for each position of the eye, is certainly not very great; but it will appear hereafter, that its refractive powers are calculated to take in a moderately distinct view of a whole hemisphere: the sense of hearing is equally perfect in almost every direction.

## IV. DIOPTRICAL PROPOSITIONS.

### *Proposition* I. *Phenomenon.*

In all refractions, the ratio of the sine of the angle of incidence to the sine of the angle of refraction is constant. (NEWTON's Opt. I. Ax. 5. SMITH's Opt. 13. WOOD's Opt. 24.)

*Scholium* 1. We shall call it the ratio of $m$ to $m \mp 1$, and $m \mp 1, n$. In refractions out of air into water, $m = 4$ and $n = 3$, very nearly; out of air into glass, the ratio is nearly that of 3 to 2.

*Scholium* 2. According to BARROW, (*Lect. Opt.* ii. 4.) HUYGENS, EULER, (*Conject. phys. circa prop. soni et luminis. Opusc. t. ii.*) and the opinion which I lately submitted to the Royal Society, (Phil. Trans. for 1800. p. 128,) the velocity of light is the greater the rarer the medium: according to NEWTON, (Schol. Prop. 96. l. i. Princip. Prop. 10. p. 3. l. ii. Opt.) and the doctrine more generally received, the reverse. On both suppositions, it is always the same in the same medium, and varies in the ratio of the sines of the angles. This circumstance is of use in facilitating the computation of some very complicated refractions.

### *Proposition* II. *Phenomenon.*

If between two refracting mediums, a third medium, terminated by parallel surfaces, be interposed, the whole refraction will remain unchanged. (NEWTON's Opt. l. i. p. 2. Prop. 3. SMITH. r. 399. WOOD, 105.)

*Corollary.* Hence, when the refractions out of two mediums into a third are given, the refraction at the common surface of these mediums may be thus found. Let the refractions given be as $m : n$, and as $m' : n'$; then the ratio sought will be that of $m n' : m' n$. For instance, let the three mediums be glass, water, and air; then $m = 3$, $n = 2$, $m' = 4$, $n' = 3$, $m n' = 9$, and $m' n = 8$. If the ratios be 4 : 3, and 13 : 14, we have $m n' : m' n$ : : 39 : 56; and, dividing by 56 — 39, we obtain 2 . 3 and 3 . 3 for $m$ and $m + 1$, in Schol. 1, Prop. I.

*Proposition* III. *Problem.* (Plate II. Fig. 1.)

At the vertex of a given triangle (CBA), to place a given refracting surface (B), so that the incident and refracted rays may coincide with the sides of the triangle (AB and BC.)

Let the sides be called $d$ and $e$; then in the base take, next to $d$ (or AB), a portion (AE) equal to $\frac{n d}{n d + m e}$, or (AD =) $\frac{m d}{m d + n e}$; draw a line (EB, or DB) to the vertex, and the surface must be perpendicular to this line, whenever the problem is physically possible. When $e$ becomes infinite, and parallel to the base, take $\frac{n d}{m}$ or $\frac{m d}{n}$ next to $d$, for the intersection of the radius of curvature.

*Proposition* IV. *Theorem.* (Fig. 2.)

In oblique refractions at spherical surfaces, the line (AI, KL,) joining the conjugate foci (A, I; K, L;) passes through the point (G), where a perpendicular from the centre (H) falls on the line (EF), bisecting the chords (BC, BD,) cut off from the incident and refracted rays.

*Corollary* 1. Let $t$ and $u$ be the cosines of incidence and refraction, the radius being 1, and $d$ and $e$ the respective distances of the foci of incident and refracted rays; then $e = \frac{m d u u}{m d u - n d t - n t t}$.

*Corollary* 2. For a plane surface, $e = \frac{m d u u}{- n t t}$.

*Corollary* 3. For parallel rays, $d = \infty$, and $e = \frac{m u u}{m u - n t}$.

*Scholium* 1. It may be observed, that the caustic by refraction stops short at its cusp, not geometrically, but physically, the total reflection interfering.

*Corollary* 4. Call $\frac{m u u}{m u - n t}$, $b$, and $\frac{n t t}{m u - n t}$, $c$; then $e = \frac{b d}{d - c}$, and $e - b = \frac{b c}{d - c}$; or, in words, the rectangle contained by the focal lengths of parallel rays, passing and repassing any surface in the same lines, is equal to the rectangle contained by the differences between these lengths and the distances of any conjugate foci.

*Corollary* 5. For perpendicular rays, $e = \frac{m d}{d - n} = m + \frac{m n}{d - n}$; or, if the radius be $a$, $e = \frac{m a d}{d - n a}$; and if $d$ and $e$ be given to find the radius, $a = \frac{d e}{m d + n e}$.

*Corollary* 6. For rays perpendicular and parallel, $e = m$, or $e = m a$.

*Corollary* 7. For a double convex lens, neglecting the thickness, call the first radius $g$, the second $b$, and $e = \frac{n d g b}{d g + d b - n g b}$. Hence $n = \frac{d e}{d + e} \cdot \frac{g + b}{g b}$; and, for parallel rays, $e = \frac{n g b}{g + b}$, and $n = e \cdot \frac{g + b}{g b}$. If $g = b = a$, $e = \frac{n a d}{2 d - n a}$; and for parallel rays $e = \frac{n a}{2}$: calling this principal focal length $b$, $e = \frac{b d}{d - b}$, as in Cor. 4; whence we have the joint focus of two lenses; also, $b = \frac{d e}{d + e}$.

*Corollary* 8. In a sphere, $e = m a \cdot \frac{d + a}{2 d - (m - 2) a}$, for the distance from the centre, and $b = \frac{m a}{2}$.

*Scholium* 2. In all these cases, if the rays converge, $d$ must be negative. For instance, to find the joint focus of two convex, or concave lenses, the expression becomes, $e = \frac{bd}{b+d}$.

*Corollary* 9. In Cor. 3, the divisor becomes ultimately constant; and, when the inclination is small, the focus varies as $uu$.

*Corollary* 10. For parallel rays falling obliquely on a double convex, or double concave lens, of inconsiderable thickness, the radius being 1, $e = \frac{ntu}{2(mu - nt)}$; which varies ultimately as the product of the cosines, or as $\frac{m+n}{nn} t + t^2$.

*Scholium* 3. In the double convex lens, the thickness diminishes the effect of the obliquity near the axis; in the double concave, it increases it.

*Scholium* 4. No spherical surface, excepting one particular case, (WOOD, 155,) can collect an oblique pencil of rays, even to a physical point. The oblique rays which we have hitherto considered, are only such as lie in that section of the pencil which is made by a plane passing through the centre and the radiant point. They continue in this plane, notwithstanding the refraction, and therefore will not meet the rays of the collateral sections, till they arrive at the axis. The remark was made by Sir ISAAC NEWTON, and extended by Dr. SMITH, (SMITH r. 493, 494;) it appears, however, to have been too little noticed. (WOOD, 362.) The geometrical focus thus becomes a line, a circle, an oval, or other figure, according to the form of the pencil, the nature of the surface, and the place of the plane receiving the image. Some of the varieties of the focal image of a cylindrical pencil obliquely refracted are shown in Plate VI. Fig. 28.

*Corollary* 11. Hence the line joining the remoter conjugate foci, will always pass through the centre. The distance of the remoter focus of parallel rays will be expressed by $f = \frac{m}{mu - nt}$; and the least circle of aberration will be at the distance $\frac{1 + u^2 - 2u^4}{(1 + uu) \cdot (mu - nt)}$, dividing the length of aberration in the ratio of the distance of its limits from the surface. In the case of Cor. 10. $f = \frac{n}{2(mu - nt)}$.

*Corollary* 12. This proposition extends also to reflected rays; and, in that case, the line from the centre passes through the point of incidence.

### *Proposition* V. *Problem.*

To find the place and magnitude of the image of a small object, after refraction at any number of spherical surfaces.

*Construction.* (Plate II. Fig. 3.) From any point (B) in the object (AB), draw lines to (C), the centre of the first surface, and to (D), the focus of parallel rays coming in a contrary direction: from the intersection of the second line (BD) with the tangent (EF) at the vertex, draw a line (EH) parallel to the axis, and it will cut the first line (BC) in (H), the first image of the point (B). Proceed with this image as a new object, and repeat the operation for each surface, and the last point will be in the image required. For calculation, find the place of the image by Cor. 5. Prop. IV. and its magnitude will be to that of the object, as their respective distances from the centre.

*Corollary.* If a confused image be received on any given plane, its magnitude will be determined by the line drawn from the preceding image through the centre of the last surface.

*Proposition* VI. *Problem.*

To determine the law by which the refraction at a spherical surface must vary, so as to collect parallel rays to a perfect focus.

*Solution.* Let $v$ be the versed sine to the radius 1; then, at each point without the axis, $n$ remaining the same, $m$ must become $\sqrt{mm \pm 2nv}$; and all the rays will be collected in the principal focus.

*Corollary.* The same law will serve for a double convex lens, in the case of equidistant conjugate foci, substituting $n$ for $m$.

*Proposition* VII. *Problem.*

To find the principal focus of a sphere, or lens, of which the internal parts are more dense than the external.

*Solution.* In order that the focal distance may be finite, the density of a finite portion about the centre must be equable: call the radius of this portion $l$, that of the sphere being unity; let the whole refraction out of the surrounding medium into this central part, be as $m$ to $n$; take $r = \frac{\log. l}{\log. m - \log. n}$, and let the density be supposed to vary every where inversely as the power $\frac{1}{r}$ of the distance from the centre: then the principal focal distance from the centre will be $\frac{r-1}{2} \cdot \frac{m}{nl - m}$. When $r = 1$, it becomes $\frac{1}{2\,(\text{H. L}.\, m - \text{H. L}.\, n)}$. For a lens, deduct one fourth of the difference between its axis and the diameter of the sphere of which its surfaces are portions.

*Corollary.* If the density be supposed to vary suddenly at the surface, $m$ must express the difference of the refractions at the centre and at the surface; and the focal distance, thus determined, must be diminished according to the refraction at the surface.

*Proposition* VIII. *Problem.*

To find the nearer focus of parallel rays falling obliquely on a sphere of variable density.

*Solution.* Let $r$ be as in the last proposition, $s$ the sine of incidence, $t$ the cosine, and $e$ the distance of the focus from the point of emersion. Then $e = \frac{w - t}{2 - tw}$, $w$ being $= \frac{2}{(r-1)s^{\frac{r+1}{r-1}}}$. $(aA + bB + cC + \dots) + 2aA + 6bBs^2 + 10cCs^4 + \dots$, where $a = \frac{r}{r+1}$, $b = \frac{r}{3r-1}$, $c = \frac{r}{5r-3}$, $A = 1$, $B = \frac{1}{2}A$, $C = \frac{3}{4}B$, $D = \frac{5}{6}C$. But, when $s$ is large, the latter part of the series converges somewhat slowly. The former part might be abridged if it were necessary: but, since the focus in this case is always very imperfect, it is of the less consequence to provide an easy calculation.

*General Scholium.* The two first propositions relate to well known phenomena; the third can hardly be new; the fourth approaches the nearest to MACLAURIN's construction, but is far more simple and convenient; the fifth and sixth have no difficulty; but the two last require a long demonstration. The one is abridged by a property of logarithms; the other is derived from the laws of centripetal forces, on the supposition of velocities directly as the refractive densities, correcting the series for the place of the apsis, and making the sine of incidence variable, to determine the fluxion of the angle of deviation.

V. Dr. PORTERFIELD has employed an experiment, first made by SCHEINER, to the determination of the focal distance of the eye; and has described, under the name of an optometer, a very excellent instrument, founded on the principle of the phenomenon.* But the apparatus is capable of considerable improvement; and I shall beg leave to describe an optometer, simple in its construction, and equally convenient and accurate in its application.

Let an obstacle be interposed between a radiant point (R, Plate II. Fig. 4,) and any refracting surface, or lens (CD), and let this obstacle be perforated at two points (A and B) only. Let the refracted rays be intercepted by a plane, so as to form an image on it. Then it is evident, that when this plane (EF) passes through the focus of refracted rays, the image formed on it will be a single point. But, if the plane be advanced forwards (to GH), or removed backwards (to IK), the small pencils passing through the perforations, will no longer meet in a single point, but will fall on two distinct spots of the plane (G, H; I, K;) and, in either case, form a double image of the object.

Let us now add two more radiating points, (S and T, Fig. 5,) the one nearer to the lens than the first point, the other more remote; and, when the plane which receives the images passes through the focus of rays coming from the first point, the images of the second and third points must both be double (*s s*, *t t*;) since the plane (EF) is without the focal distance of rays coming from the furthest point, and within that of rays coming from the nearest. Upon this principle, Dr. Porterfield's optometer was founded.

But, if the three points be supposed to be joined by a line, and this line to be somewhat inclined to the axis of the lens,

* Edinb. Med. Ess. Vol. IV. p. 185.

each point of the line, except the first point (R, Fig. 6,) will have a double image; and each pair of images, being contiguous to those of the neighbouring radiant points, will form with them two continued lines, and the images being more widely separated as the point which they represent is further from the first radiant point, the lines (*s t*, *s t*,) will converge on each side towards (*r*) the image of this point, and there will intersect each other.

The same happens when we look at any object through two pin holes, within the limits of the pupil. If the object be at the point of perfect vision, the image on the retina will be single; but, in every other case, the image being double, we shall appear to see a double object: and, if we look at a line pointed nearly to the eye, it will appear as two lines, crossing each other in the point of perfect vision. For this purpose, the holes may be converted into slits, which render the images nearly as distinct, at the same time that they admit more light. The number may be increased from two to four, or more, whenever particular investigations render it necessary.

The optometer may be made of a slip of card-paper, or of ivory, about eight inches in length, and one in breadth, divided longitudinally by a black line, which must not be too strong. The end of the card must be cut as is shown in Plate III. Fig 7, in order that it may be turned up, and fixed in an inclined position by means of the shoulders: or a detached piece, nearly of this form, may be applied to the optometer, as it is here engraved. A hole about half an inch square must be made in this part; and the sides so cut as to receive a slider of thick paper, with slits of different sizes, from a fortieth to a tenth of an inch in breadth, divided by spaces somewhat broader; so that each observer may choose that which best suits the aperture of his pupil.

In order to adapt the instrument to the use of presbyopic eyes, the other end must be furnished with a lens of four inches focal length; and a scale must be made near the line on each side of it, divided from one end into inches, and from the other according to the table here calculated from Cor. 7. Prop. IV, by means of which, not only diverging, but also parallel and converging rays from the lens are referred to their virtual focus. The instrument is easily applicable to the purpose of ascertaining the focal length of spectacles required for myopic or presbyopic eyes. Mr. Cary has been so good as to furnish me with the numbers and focal lengths of the glasses commonly made; and I have calculated the distances at which those numbers must be placed on the scale of the optometer, so that a presbyopic eye may be enabled to see at eight inches distance, by using the glasses of the focal length placed opposite to the nearest crossing of the lines; and a myopic eye with parallel rays, by using the glasses indicated by the number that stands opposite their furthest crossing. To facilitate the observation, I have also placed these numbers opposite that point which will be the nearest crossing to myopic eyes; but this, upon the arbitrary supposition of an equal capability of change of focus in every eye, which I must confess is often far from the truth. It cannot be expected, that every person, on the first trial, will fix precisely upon that power which best suits the defect of his sight. Few can bring their eyes at pleasure to the state of full action, or of perfect relaxation; and a power two or three degrees lower than that which is thus ascertained, will be found sufficient for ordinary purposes. I have also added to the second table, such numbers as will point out the spectacles necessary for a presbyopic eye, to see at twelve and at eighteen inches respectively: the middle series will perhaps be the most proper for placing the numbers on the scale. The optometer should be applied to each eye; and, at the time of observing, the opposite eye should not be shut, but the instrument should be screened from its view. The place of intersection may be accurately ascertained, by means of an index sliding along the scale.

The optometer is represented in Plate III. Fig. 8 and 9; and the manner in which the lines appear, in Fig. 10.

*Table* I. *For extending the scale by a lens of 4 inches focus.*

| | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 4 | 2.00 | 11 | 2.93 | 30 | 3.52 | 200 | 3.92 | −35 | 4.51 | −12 | 6.00 |
| 5 | 2.22 | 12 | 3.00 | 40 | 3.64 | ∞ | 4 00 | −30 | 4.62 | −11 | 6.29 |
| 6 | 2.40 | 13 | 3.06 | 50 | 3.70 | −200 | 4.08 | −25 | 4.76 | −10 | 6.67 |
| 7 | 2.55 | 14 | 3 11 | 60 | 3.75 | −100 | 4 17 | −20 | 5.00 | −9.5 | 6.90 |
| 8 | 2.67 | 15 | 3.16 | 70 | 3.78 | −50 | 4 35 | −15 | 5.45 | −9.0 | 7.20 |
| 9 | 2.77 | 20 | 3.33 | 80 | 3.81 | −45 | 4.39 | −14 | 5.60 | −8.5 | 7.56 |
| 10 | 2.86 | 25 | 3.45 | 100 | 3.85 | −40 | 4.44 | −13 | 5.78 | −8.0 | 8.00 |

*Table* II. *For placing the numbers indicating the focal length of convex glasses.*

| Foc. | VIII. | XII | XVIII. | Foc. | VIII. | XII. | XVIII. | Foc. | VIII. | XII. | XVIII. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 0 | 8.00 | 12 00 | 18.00 | 20 | 13.33 | 30.00 | 180.00 | 8 | ∞ | −24 00 | −14.40 |
| 40 | 10.00 | 17.14 | 32.73 | 18 | 14.40 | 36.00 | ∞ | 7 | −56.00 | −16.80 | −11.45 |
| 36 | 10.28 | 18.00 | 36.00 | 16 | 16 00 | 48.00 | −144.00 | 6 | −24.00 | −12.00 | — 9.00 |
| 30 | 10.91 | 20.00 | 45.00 | 14 | 18.67 | 84.00 | — 63.00 | 5 | −13.33 | — 8.57 | — 5.92 |
| 28 | 11 20 | 21.00 | 50.40 | 12 | 24.00 | ∞ | — 36.00 | 4.5 | −10.29 | — 7 20 | — 6.00 |
| 26 | 11.56 | 22 29 | 58.50 | 11 | 29.33 | −132 00 | — 28.29 | 4.0 | — 8.00 | — 6.00 | — 5.14 |
| 24 | 12 00 | 24 00 | 72.00 | 10 | 40 00 | — 60.00 | — 22.50 | 3.5 | — 6 22 | — 4.94 | — 4.34 |
| 22 | 12 77 | 26.40 | 99.00 | 9 | 72.00 | — 36.00 | — 18.00 | 3.0 | — 4.80 | — 4.00 | — 3.6 |

*Table* III. *For concave glasses.*

| Number. | Focus and furthest place. | Nearest place. | Number. | Focus and furthest place. | Nearest place. | Number. | Focus and furthest place. | Nearest place. |
|---|---|---|---|---|---|---|---|---|
| 0 | | 4.00 | 7 | 8 | 2.67 | 14 | 3.00 | 1.71 |
| 1 | 24 | 3.43 | 8 | 7 | 2.54 | 15 | 2.75 | 1.63 |
| 2 | 18 | 3.27 | 9 | 6 | 2.40 | 16 | 2.50 | 1.54 |
| 3 | 16 | 3.20 | 10 | 5 | 2.22 | 17 | 2.25 | 1.44 |
| 4 | 12 | 3.00 | 11 | 4.5 | 2.12 | 18 | 2.00 | 1.33 |
| 5 | 10 | 2.86 | 12 | 4.0 | 2.00 | 19 | 1.75 | 1.22 |
| 6 | 9 | 2.77 | 13 | 3.5 | 1.87 | 20 | 1.50 | 1.02 |

VI. Being convinced of the advantage of making every observation with as little assistance as possible, I have endeavoured to confine most of my experiments to my own eyes; and I shall, in general, ground my calculations on the supposition of an eye nearly similar to my own. I shall therefore first endeavour to ascertain all its dimensions, and all its faculties.

For measuring the diameters, I fix a small key on each point of a pair of compasses; and I can venture to bring the rings into immediate contact with the sclerotica. The transverse diameter is externally 98 hundredths of an inch.

To find the axis, I turn the eye as much inwards as possible, and press one of the keys close to the sclerotica, at the external angle, till it arrives at the spot where the spectrum formed by its pressure coincides with the direction of the visual axis, and, looking in a glass, I bring the other key to the cornea. The optical axis of the eye, making allowance of three hundredths for the coats, is thus found to be 91 hundredths of an inch, from the external surface of the cornea to the retina. With an eye less prominent, this method might not have succeeded.

The vertical diameter, or rather chord, of the cornea, is 45 hundredths: its versed sine 11 hundredths. To ascertain the versed sine, I looked with the right eye at the image of the left, in a small speculum held close to the nose, while the left eye was so averted that the margin of the cornea appeared as a straight line, and compared the projection of the cornea with the image of a cancellated scale held in a proper direction behind the left eye, and close to the left temple. The horizontal chord of the cornea is nearly 49 hundredths.

Hence the radius of the cornea is 31 hundredths. It may be thought that I assign too great a convexity to the cornea; but I have corrected it by a number of concurrent observations, which will be enumerated hereafter.

The eye being directed towards its image, the projection of the margin of the sclerotica is 22 hundredths from the margin of the cornea, towards the external angle, and 27 towards the internal angle of the eye: so that the cornea has an eccentricity of one fortieth of an inch, with respect to the section of the eye perpendicular to the visual axis.

The aperture of the pupil varies from 27 to 13 hundredths; at least this is its apparent size, which must be somewhat diminished, on account of the magnifying power of the cornea, perhaps to 25 and 12. When dilated, it is nearly as eccentric as the cornea; but, when most contracted, its centre coincides with the reflection of an image from an object held immediately before the eye; and this image very nearly with the centre of the whole apparent margin of the sclerotica: so that the cornea is perpendicularly intersected by the visual axis.

My eye, in a state of relaxation, collects to a focus on the retina, those rays which diverge vertically from an object at the distance of ten inches from the cornea, and the rays which diverge horizontally from an object at seven inches distance. For, if I hold the plane of the optometer vertically, the images of the line appear to cross at ten inches; if horizontally, at seven. The difference is expressed by a focal length of 23 inches. I have never experienced any inconvenience from this imperfection, nor did I ever discover it till I made these experiments; and I believe I can examine minute objects with as much accuracy as most of those whose eyes are differently formed. On mentioning it to Mr. CARY, he informed me, that he had frequently taken notice of a similar circumstance; that many persons were obliged to hold a concave glass obliquely, in order to see with distinctness, counterbalancing, by the inclination of the glass, the too great refractive power of the eye in the direction of that inclination, (Cor. 10. Prop. IV.) and finding but little assistance from spectacles of the same focal length. The difference is not in the cornea, for it exists when the effect of the cornea is removed by a method to be described hereafter. The cause is, without doubt, the obliquity of the uvea, and of the crystalline lens, which is nearly parallel to it, with respect to the visual axis: this obliquity will appear, from the dimensions already given, to be about 10 degrees. Without entering into a very accurate calculation, the difference observed is found (by the same corollary) to require an inclination of about 13 degrees; and the remaining three degrees may easily be added, by the greater obliquity of the posterior surface of the crystalline opposite the pupil. There would be no difficulty in fixing the glasses of spectacles, or the concave eye-glass of a telescope, in such a position as to remedy the defect.

In order to ascertain the focal distance of the lens, we must assign its probable distance from the cornea. Now the versed sine of the cornea being 11 hundredths, and the uvea being nearly flat, the anterior surface of the lens must probably be somewhat behind the chord of the cornea; but by a very inconsiderable distance, for the uvea has the substance of a thin membrane, and the lens approaches very near to it: we will therefore call this distance 12 hundredths. The axis and proportions of the lens must be estimated by comparison with anatomical observations; since they affect, in a small degree, the determination of its focal distance. M. PETIT found the axis almost always about two lines, or 18 hundredths of an inch. The radius of the anterior surface was in the greatest number 3 lines, but oftener more than less. We will suppose mine to be $3\frac{1}{4}$, or nearly $\frac{3}{10}$ of an inch. The radius of the posterior surface was most frequently $2\frac{1}{2}$ lines, or $\frac{2}{9}$ of an inch.* The optical centre will be therefore $\left(\frac{18 \times 30}{30 + 22} = \right)$ about one-tenth of an inch from the anterior surface: hence we have 22 hundredths, for the distance of the centre from the cornea. Now, taking 10 inches as the distance of the radiant point, the focus of the cornea will be 115 hundredths behind the centre of the lens. (Cor. 5. Prop. IV.) But the actual joint focus is $(91 - 22 =)$ 69 behind the centre: hence, disregarding the thickness of the lens, its principal focal distance is 173 hundredths. (Cor. 7. Prop. IV.) For its refractive power in the eye, we have (by Cor. 7. Prop. IV.) $n = 13{,}5$, and $m = 14{,}5$. Calculating upon this refractive power, with the consideration of the thickness also, we find that it requires a correction, and comes near to the ratio of 14 to 13 for the sines. It is well known that the refractive powers of the humours are equal to that of water; and, that the thickness of the cornea is too equable to produce any effect on the focal distance.

For determining the refractive power of the crystalline lens by a direct experiment, I made use of a method suggested to me by Dr. WOLLASTON. I found the refractive power of the centre of the recent human crystalline to that of water, as 21 to 20. The difference of this ratio from the ratio of 14 to 13, ascertained from calculation, is probably owing to two circumstances. The first is, that the substance of the lens being in some degree soluble in water, a portion of the aqueous fluid

* Mem. de l'Acad. de Paris, 1730. p. 6. Ed. Amst.

within its capsule penetrates after death, so as somewhat to lessen the density. When dry, the refractive power is little inferior to that of crown glass. The second circumstance is, the unequal density of the lens. The ratio of 14 to 13 is founded on the supposition of an equable density: but, the central part being the most dense, the whole acts as a lens of smaller dimensions; and it may be found by Prop. VII. that if the central portion of a sphere be supposed of uniform density, refracting as 21 to 20, to the distance of one half of the radius, and the density of the external parts to decrease gradually, and at the surface to become equal to that of the surrounding medium, the sphere thus constituted, will be equal in focal length to a uniform sphere of the same size, with a refraction of 16 to 15 nearly. And the effect will be nearly the same, if the central portion be supposed to be smaller than this, but the density to be somewhat greater at the surface than that of the surrounding medium, or to vary more rapidly externally than internally. On the whole, it is probable that the refractive power of the centre of the human crystalline, in its living state, is to that of water nearly as 18 to 17; that the water imbibed after death, reduces it to the ratio of 21 to 20; but that, on account of the unequable density of the lens, its effect in the eye is equivalent to a refraction of 14 to 13 for its whole size. Dr. WOLLASTON has ascertained the refraction out of air, into the centre of the recent crystalline of oxen and sheep, to be nearly as 143 to 100; into the centre of the crystalline of fish, and into the dried crystalline of sheep, as 152 to 100. Hence, the refraction of the crystalline of oxen in water, should be as 15 to 14: but the human crystalline, when recent, is decidedly less refractive.

These considerations will explain the inconsistency of different observations on the refractive power of the crystalline; and, in particular, how the refraction which I formerly calculated, from measuring the focal length of the lens,* is so much greater than that which is determined by other means. But, for direct experiments, Dr. WOLLASTON'S method is exceedingly accurate.

When I look at a minute lucid point, such as the image of a candle in a small concave speculum, it appears as a radiated star, as a cross, or as an unequal line, and never as a perfect point, unless I apply a concave lens inclined at a proper angle, to correct the unequal refraction of my eye. If I bring the point very near, it spreads into a surface nearly circular, and almost equably illuminated, except some faint lines, nearly in a radiating direction. For this purpose, the best image is a candle, or a small speculum, viewed through a minute lens at some little distance, or seen by reflection in a larger lens. If any pressure has been applied to the eye, such as that of the finger keeping it shut, the sight is often confused for a short time after the removal of the finger, and the image is in this case spotty or curdled. The radiating lines are probably occasioned by some slight inequalities in the surface of the lens, which is very superficially furrowed in the direction of its fibres: the curdled appearance will be explained hereafter. When the point is further removed, the image becomes evidently oval, the vertical diameter being longest, and the lines a little more distinct than before, the light being strongest in the neighbourhood of the centre; but immediately at the centre there is a darker spot, owing to such a slight depression at the vertex as is often

* Phil. Trans. for 1793. p. 174.

observable in examining the lens after death. The situation of the rays is constant, though not regular; the most conspicuous are seven or eight in number; sometimes about twenty fainter ones may be counted. Removing the point a little further, the image becomes a short vertical line; the rays that diverged horizontally being perfectly collected, while the vertical rays are still separate. In the next stage, which is the most perfect focus, the line spreads in the middle, and approaches nearly to a square, with projecting angles, but is marked with some darker lines towards the diagonals. The square then flattens into a rhombus, and the rhombus into a horizontal line unequally bright. At every greater distance, the line lengthens, and acquires also breadth, by radiations shooting out from it, but does not become a uniform surface, the central part remaining always considerably brightest, in consequence of the same flattening of the vertex which before made it fainter. Some of these figures bear a considerable analogy to the images derived from the refraction of oblique rays, (Schol. 4. Prop. IV.) and still more strongly resemble a combination of two of them in opposite directions; so as to leave no doubt, but that both surfaces of the lens are oblique to the visual axis, and co-operate in distorting the focal point. This may also be verified, by observing the image delineated by a common glass lens, when inclined to the incident rays. (See Plate VI. Fig. 28—40.)

The visual axis being fixed in any direction, I can at the same time see a luminous object placed laterally at a considerable distance from it; but in various directions the angle is very different. Upwards it extends to 50 degrees, inwards to 60, downwards to 70, and outwards to 90 degrees. These internal limits of the field of view nearly correspond with the external limits formed by the different parts of the face, when the eye is directed forwards and somewhat downwards, which is its most natural position; although the internal limits are a little more extensive than the external; and both are well calculated for enabling us to perceive the most readily, such objects as are the most likely to concern us. Dr. WOLLASTON's eye has a larger field of view, both vertically and horizontally, but nearly in the same proportions, except that it extends further upwards. It is well known, that the retina advances further forwards towards the internal angle of the eye, than towards the external angle; but upwards and downwards its extent is nearly equal, and is indeed every way greater than the limits of the field of view, even if allowance is made for the refraction of the cornea only. The sensible portion seems to coincide more nearly with the painted choroid of quadrupeds: but the whole extent of perfect vision is little more than 10 degrees; or, more strictly speaking, the imperfection begins within a degree or two of the visual axis, and at the distance of 5 or 6 degrees becomes nearly stationary, until, at a still greater distance, vision is wholly extinguished. The imperfection is partly owing to the unavoidable aberration of oblique rays, but principally to the insensibility of the retina: for, if the image of the sun itself be received on a part of the retina remote from the axis, the impression will not be sufficiently strong to form a permanent spectrum, although an object of very moderate brightness will produce this effect when directly viewed. It would probably have been inconsistent with the economy of nature, to bestow a larger share of sensibility on the retina. The optic nerve is at present very large; and the delicacy of the organ renders it, even at present, very susceptible of injury from slight irritation, and very liable to inflammatory affections; and, in order to make the sight so perfect as it is, it was necessary to confine that perfection within narrow limits. The motion of the eye has a range of about 55 degrees in every direction; so that the field of perfect vision, in succession, is by this motion extended to 110 degrees.

But the whole of the retina is of such a form as to receive the most perfect image, on every part of its surface, that the state of each refracted pencil will admit; and the varying density of the crystalline renders that state more capable of delineating such a picture, than any other imaginable contrivance could have done. To illustrate this, I have constructed a diagram, representing the successive images of a distant object filling the whole extent of view, as they would be formed by the successive refractions of the different surfaces. Taking the scale of my own eye, I am obliged to substitute, for a series of objects at any indefinitely great distance, a circle of 10 inches radius; and it is most convenient to consider only those rays which pass through the anterior vertex of the lens; since the actual centre of each pencil must be in the ray which passes through the centre of the pupil, and the short distance of the vertex of the lens from this point, will always tend to correct the unequal refraction of oblique rays. The first curve (Plate IV. Fig. 16.) is the image formed by the furthest intersection of rays refracted at the cornea; the second, the image formed by the nearest intersection; the distance between these, shows the degree of confusion in the image; and the third curve, its brightest part. Such must be the form of the image which the cornea tends to delineate in an eye deprived of the crystalline lens; nor can any external remedy properly correct the imperfection of lateral vision. The next three curves show the images formed after the refraction at the anterior surface of the lens, distinguished in the same manner; and the three following, the result of all the successive refractions. The tenth curve is a repetition of the ninth, with a slight correction near the axis, at F, where, from the breadth of the pupil, some perpendicular rays must fall. By comparing this with the eleventh, which is the form of the retina, it will appear that nothing more is wanting for their perfect coincidence, than a moderate diminution of density in the lateral parts of the lens. If the law, by which this density varies, were more accurately ascertained, its effect on the image might be calculated from the eighth proposition; but the operations would be somewhat laborious: probably the image, thus corrected, would approach very nearly to the form of the twelfth curve.

To find the place of the entrance of the optic nerve, I fix two candles at ten inches distance, retire sixteen feet, and direct my eye to a point four feet to the right or left of the middle of the space between them: they are then lost in a confused spot of light; but any inclination of the eye brings one or the other of them into the field of view. In BERNOULLI's eye, a greater deviation was required for the direction of the axis;* and the obscured part appeared to be of greater extent. From the experiment here related, the distance of the centre of the optic nerve from the visual axis is found (by Prop. V.) to be 16 hundredths of an inch; and the diameter of the most insensible part of the retina, one-thirtieth of an inch. In order to ascertain the distance of the optic nerve from the point opposite to the pupil, I took the sclerotica of the human eye, divided it into segments, from the centre of the cornea towards the optic nerve, and extended it on a plane. I then measured the longest and shortest

* Comm. Petrop. I. p. 314.

distances from the cornea to the perforation made by the nerve, and their difference was exactly one-fifth of an inch. To this we must add a fiftieth, on account of the eccentricity of the pupil in the uvea, which in the eye that I measured was not great, and the distance of the centre of the nerve from the point opposite the pupil will be 11 hundredths. Hence it appears, that the visual axis is five hundredths, or one-twentieth of an inch, further from the optic nerve than the point opposite the pupil. It is possible that this distance may be different in different eyes: in mine, the obliquity of the lens, and the eccentricity of the pupil with respect to it, will tend to throw a direct ray upon it, without much inclination of the whole eye; and it is not improbable, that the eye is also turned slightly outwards, if looking at any object before it, although the inclination is too small to be subjected to measurement.

It must also be observed, that it is very difficult to ascertain the proportions of the eye so exactly as to determine, with certainty, the size of an image on the retina; the situation, curvature, and constitution of the lens, make so material a difference in the result, that there may possibly be an error of almost one-tenth of the whole. In order, therefore, to obtain some confirmation from experiment, I placed two candles at a small distance from each other, turned the eye inwards, and applied the ring of a key so as to produce a spectrum, of which the edge coincided with the inner candle; then, fixing my eye on the outward one, I found that the spectrum advanced over two-sevenths of the distance between them. Hence, the same portion of the retina that subtended an angle of seven parts at the centre of motion of the eye, subtended an angle of five at the supposed intersection of the principal rays; (Plate III. Fig. 11.) and the distance of this intersection from the retina was 637 thousandths. This nearly corresponds with the former calculation; nor can the distance of the centre of the optic nerve from the point of most perfect vision be, on any supposition, much less than that which is here assigned. And, in the eyes of quadrupeds, the most strongly painted part of the choroid is further from the nerve than the real axis of the eye.

I have endeavoured to express in four figures, the form of every part of my eye, as nearly as I have been able to ascertain it; the first (Pl. V. Fig. 17.) is a vertical section; the second (Fig. 18.) a horizontal section; the third and fourth are front views, in different states of the pupil. (Fig. 19 and 20.)

Considering how little inconvenience is experienced from so material an inequality in the refraction of the lens as I have described, we have no reason to expect a very accurate provision for correcting the aberration of the lateral rays. But, as far as can be ascertained by the optometer, the aberration arising from figure is completely corrected; since four or more images of the same line appear to meet exactly in the same point, which they would not do if the lateral rays were materially more refracted than the rays near the axis. The figure of the surfaces is sometimes, and perhaps always, more or less hyperbolical* or elliptical: in the interior laminæ indeed, the solid angle of the margin is somewhat rounded off; but the weaker refractive power of the external parts, must greatly tend to correct the aberration arising from the too great curvature towards the margin of the disc. Had the refractive power been uniform, it might have collected the lateral rays of a direct pencil nearly as well; but it would have been less adapted to oblique pencils of

* Petit Mém. de l'Acad. 1725, p. 20.

rays; and the eye must also have been encumbered with a mass of much greater density than is now required, even for the central parts: and, if the whole lens had been smaller, it would also have admitted too little light. It is possible too, that Mr. RAMSDEN's observation,* on the advantage of having no reflecting surface, may be well-founded: but it has not been demonstrated, that less light is lost in passing through a medium of variable density, than in a sudden transition from one part of that medium to another; nor are we yet sufficiently acquainted with the cause of this reflection, to be enabled to reason satisfactorily on the subject. But, neither this gradation, nor any other provision, has the effect of rendering the eye perfectly achromatic. Dr. JURIN had remarked this, long ago,† from observing the colour bordering the image of an object seen indistinctly. Dr. WOLLASTON pointed out to me on the optometer, the red and blue appearance of the opposite internal angles of the crossing lines; and mentioned, at the same time, a very elegant experiment for proving the dispersive power of the eye. He looks through a prism at a small lucid point, which of course becomes a linear spectrum. But the eye cannot so adapt itself as to make the whole spectrum appear a line; for, if the focus be adapted to collect the red rays to a point, the blue will be too much refracted, and expand into a surface; and the reverse will happen if the eye be adapted to the blue rays; so that, in either case, the line will be seen as a triangular space. The observation is confirmed, by placing a small concave speculum in different parts of a prismatic spectrum, and ascertaining the utmost distances at which the eye can collect the rays of different colours to a focus. By these means I find, that the red rays, from a point at

* Phil. Trans. for 1795, p. 2.

† SMITH, c. 96.

12 inches distance, are as much refracted as white or yellow light at 11. The difference is equal to the refraction of a lens 132 inches in focus. But the aberration of the red rays in a lens of crown glass, of equal mean refractive power with the eye, would be equivalent to the effect of a lens 44 inches in focus. If, therefore, we can depend upon this calculation, the dispersive power of the eye collectively, is one-third of the dispersive power of crown glass, at an equal angle of deviation. I cannot observe much aberration in the violet rays. This may be, in part, owing to their faintness; but yet I think their aberration must be less than that of the red rays. I believe it was Mr. RAMSDEN's opinion, that since the separation of coloured rays is only observed where there is a sudden change of density, such a body as the lens, of a density gradually varying, would have no effect whatever in separating the rays of different colours. If this hypothesis should appear to be well-founded, we must attribute the whole dispersion to the aqueous humour; and its dispersive power will be half that of crown glass, at the same deviation. But we have an instance, in the atmosphere, of a very gradual change of density; and yet Mr. GILPIN informs me, that the stars, when near the horizon, appear very evidently coloured. At a more favourable season of the year, it would not be difficult to ascertain, by means of the optometer, the dispersive power of the eye, and of its different parts, with greater accuracy than by the experiment here related. Had the dispersive power of the whole eye been equal to that of flint glass, the distances of perfect vision would have varied from 12 inches to 7 for different rays, in the same state of the mean refractive powers.

VII. The faculty of accommodating the eye to various distances, appears to exist in very different degrees in different individuals. The shortest distance of perfect vision in my eye, is 26 tenths of an inch for horizontal, and 29 for vertical rays. This power is equivalent to the addition of a lens of 4 inches focus. Dr. Wollaston can see at 7 inches, and with converging rays; the difference answering to 6 inches focal length. Mr. Abernethy has perfect vision from 3 inches to 30, or a power equal to that of a lens $3\frac{1}{3}$ inches in focus. A young lady of my acquaintance can see at 2 inches and at 4; the difference being equivalent to 4 inches focus. A middle aged lady at 3 and at 4; the power of accommodation being only equal to the effect of a lens of 12 inches focus. In general, I have reason to think, that the faculty diminishes in some degree, as persons advance in life; but some also of a middle age appear to possess it in a very small degree. I shall take the range of my own eye, as being probably about the medium, and inquire what changes will be necessary in order to produce it; whether we suppose the radius of the cornea to be diminished, or the distance of the lens from the retina to be increased, or these two causes to act conjointly, or the figure of the lens itself to undergo an alteration.

1. We have calculated, that when the eye is in a state of relaxation, the refraction of the cornea is such as to collect rays diverging from a point ten inches distant, to a focus at the distance of $13\frac{2}{3}$ tenths. In order that it may bring to the same focus, rays diverging from a point distant 29 tenths, we find (by Cor. 5, Prop. IV.) that its radius must be diminished from 31 to 25 hundredths, or very nearly in the ratio of five to four.

2. Supposing the change from perfect vision at ten inches to 29 tenths, to be effected by a removal of the retina to a greater

distance from the lens, this will require, (by the same Corollary,) an elongation of 135 thousandths, or more than one-seventh of the diameter of the eye. In Mr. ABERNETHY's eye, an elongation of 17 hundredths, or more than one-sixth, is requisite.

3. If the radius of the cornea be diminished one-sixteenth, or to 29 hundredths, the eye must at the same time be elongated 97 thousandths, or about one-ninth of its diameter.

4. Supposing the crystalline lens to change its form; if it became a sphere, its diameter would be 28 hundredths, and, its anterior surface retaining its situation, the eye would have perfect vision at the distance of an inch and a half. (Cor. 5 and 8, Prop. IV.) This is more than double the actual change. But it is impossible to determine precisely how great an alteration of form is necessary, without ascertaining the nature of the curves into which its surfaces may be changed. If it were always a spheroid more or less oblate, the focal length of each surface would vary inversely as the square of the axis: but, if the surfaces became, from spherical, portions of hyperbolic conoids, or of oblong spheroids, or changed from more obtuse to more acute figures of this kind, the focal length would vary more rapidly. Disregarding the elongation of the axis, and supposing the curvature of each surface to be changed proportionally, the radius of the anterior must become about 24, and that of the posterior 17 hundredths.

VIII. I shall now proceed to inquire, which of these changes takes place in nature; and I shall begin with a relation of experiments made in order to ascertain the curvature of the cornea in all circumstances.

The method described in Mr. HOME's Croonian Lecture for

1795,* appears to be far preferable to the apparatus of the preceding year: † for a difference in the distance of two images seen in the cornea, would be far greater, and more conspicuous, than a change of its prominency, and far less liable to be disturbed by accidental causes. It is nearly, and perhaps totally impossible to change the focus of the eye, without some motion of its axis. The eyes sympathize perfectly with each other; and the change of focus is almost inseparable from a change of the relative situation of the optic axes; so much, that if I direct both my eyes at an object beyond their furthest focus, I cannot avoid bringing that focus a little nearer: while one axis moves, it is not easy to keep the other perfectly at rest; and it is not impossible, that a change in the proportions of some eyes, may render a slight alteration of the position of the axis absolutely necessary. These considerations may partly explain the trifling difference in the place of the cornea that was observed in 1794. It appears that the experiments of 1795 were made with considerable accuracy, and no doubt with excellent instruments; and their failing to ascertain the existence of any change, induced Mr. HOME and Mr. RAMSDEN to abandon, in great measure, the opinion which suggested them, and to suppose, that a change of the cornea produces only one-third of the effect. Dr. OLBERS of Bremen, who in the year 1780 published a most elaborate dissertation on the internal changes of the eye, ‡ which he lately presented to the Royal Society, had been equally unsuccessful in his attempts to measure this change of the cornea, at the same time that his opinion was in favour of its existence.

* Phil. Trans. for 1796, p. 2.

† Phil. Trans. for 1795, p. 13.

‡ De Oculi Mutationibus internis. Gotting. 1780. 4°.

Room was however still left for a repetition of the experiments; and I began with an apparatus nearly resembling that which Mr. HOME has described. I had an excellent achromatic microscope, made by Mr. RAMSDEN for my friend Mr. JOHN ELLIS, of five inches focal length, magnifying about 20 times. To this I adapted a cancellated micrometer, in the focus of the eye not employed in looking through the microscope: it was a large card, divided by horizontal and vertical lines into fortieths of an inch. When the image in the microscope was compared with this scale, care was taken to place the head so that the relative motion of the images on the micrometer, caused by the unsteadiness of the optic axis, should always be in the direction of the horizontal lines, and that there could be no error, from this motion, in the dimensions of the image taken vertically. I placed two candles so as to exhibit images in a vertical position in the eye of Mr. KÖNIG, who had the goodness to assist me; and, having brought them into the field of the microscope, where they occupied 35 of the small divisions, I desired him to fix his eye on objects at different distances in the same direction: but I could not perceive the least variation in the distance of the images.

Finding a considerable difficulty in a proper adjustment of the microscope, and being able to depend on my naked eye in measuring distances, without an error of one 500th of an inch, I determined to make a similar experiment without any magnifying power. I constructed a divided eye-glass of two portions of a lens, so small, that they passed between two images reflected from my own eye; and, looking in a glass, I brought the apparent places of the images to coincide, and then made the change requisite for viewing nearer objects: but the images still coincided. Neither could I observe any change in the images reflected from the other eye, where they could be viewed with greater convenience, as they did not interfere with the eye-glass. But, not being at that time aware of the perfect sympathy of the eyes, I thought it most certain to confine my observation to the one with which I saw. I must remark that, by a little habit, I have acquired a very ready command over the accommodation of my eye, so as to be able to view an object with attention, without adjusting my eye to its distance.

I also stretched two threads, a little inclined to each other, across a ring, and divided them by spots of ink into equal spaces. I then fixed the ring, applied my eye close behind it, and placed two candles in proper situations before me, and a third on one side, to illuminate the threads. Then, setting a small looking-glass, first at four inches distance, and next at two, I looked at the images reflected in it, and observed at what part of the threads they exactly reached across in each case; and with the same result as before.

I next fixed the cancellated micrometer at a proper distance, illuminated it strongly, and viewed it through a pin-hole, by which means it became distinct in every state of the eye; and, looking with the other eye into a small glass, I compared the image with the micrometer, in the manner already described. I then changed the focal distance of the eye, so that the lucid points appeared to spread into surfaces, from being too remote for perfect vision; and I noted on the scale, the distance of their centres; but that distance was invariable.

Lastly, I drew a diagonal scale, with a diamond, on a looking-glass, (Plate III. Fig. 12.) and brought the images into contact with the lines of the scale. Then, since the image of the eye occupies on the surface of a glass half its real dimensions; at whatever distance it is viewed, its true size is always double the measure thus obtained. I illuminated the glass strongly, and made a perforation in a narrow slip of black card, which I held between the images; and was thus enabled to compare them with the scale, although their apparent distance was double that of the scale. I viewed them in all states of the eye; but I could perceive no variation in the interval between them.

The sufficiency of these methods may be thus demonstrated. Make a pressure along the edge of the upper eyelid with any small cylinder, for instance a pencil, and the optometer will show that the focus of horizontal rays is a little elongated, while that of vertical rays is shortened; an effect which can only be owing to a change of curvature in the cornea. Not only the apparatus here described, but even the eye unassisted, will be capable of discovering a considerable change in the images reflected from the cornea, although the change be much smaller than that which is requisite for the accommodation of the eye to different distances. On the whole, I cannot hesitate to conclude, that if the radius of the cornea were diminished but one-twentieth, the change would be very readily perceptible by some of the experiments related; and the whole alteration of the eye requires one-fifth.

But a much more accurate and decisive experiment remains. I take out of a small botanical microscope, a double convex lens, of eight-tenths radius and focal distance, fixed in a socket one-fifth of an inch in depth; securing its edges with wax, I drop into it a little water, nearly cold, till it is three-fourths full, and then apply it to my eye, so that the cornea enters half way into the socket, and is every where in contact with the water. (Plate III. Fig. 13.) My eye immediately becomes presbyopic, and the refractive power of the lens, which is reduced by the water to a focal length of about 16 tenths, (Cor. 5. Prop. IV.) is not sufficient to supply the place of the cornea, rendered inefficacious by the intervention of the water; but the addition of another lens, of five inches and a half focus, restores my eye to its natural state, and somewhat more. I then apply the optometer, and I find the same inequality in the horizontal and vertical refractions as without the water; and I have, in both directions, a power of accommodation equivalent to a focal length of four inches, as before. At first sight indeed, the accommodation appears to be somewhat less, and only able to bring the eye from the state fitted for parallel rays to a focus at five inches distance; and this made me once imagine, that the cornea might have some slight effect in the natural state; but, considering that the artificial cornea was about a tenth of an inch before the place of the natural cornea, I calculated the effect of this difference, and found it exactly sufficient to account for the diminution of the range of vision. I cannot ascertain the distance of the glass lens from the cornea to the hundredth of an inch; but the error cannot be much greater, and it may be on either side.

After this, it is almost necessary to apologize for having stated the former experiments; but, in so delicate a subject, we cannot have too great a variety of concurring evidence.

IX. Having satisfied myself that the cornea is not concerned in the accommodation of the eye, my next object was to inquire if any alteration in the length of its axis could be discovered; for this appeared to be the only possible alternative: and, considering that such a change must amount to one-seventh of the diameter of the eye, I flattered myself with the expectation of submitting it to measurement. Now, if the axis of the eye were elongated one-seventh, its transverse diameter must be diminished one-fourteenth, and the semi-diameter would be shortened a thirtieth of an inch.

I therefore placed two candles so that when the eye was turned inwards, and directed towards its own image in a glass, the light reflected from one of the candles by the sclerotica appeared upon its external margin, so as to define it distinctly by a bright line; and the image of the other candle was seen in the centre of the cornea. I then applied the double eye-glass, and the scale of the looking-glass, in the manner already described; but neither of them indicated any diminution of the distance, when the focal length of the eye was changed.

Another test, and a much more delicate one, was the application of the ring of a key at the external angle, when the eye was turned as much inwards as possible, and confined at the same time by a strong oval iron ring, pressed against it at the internal angle. The key was forced in as far as the sensibility of the integuments would admit, and was wedged, by a moderate pressure, between the eye and the bone. In this situation, the phantom caused by the pressure extended within the field of perfect vision, and was very accurately defined; nor did it, as I formerly imagined, by any means prevent a distinct perception of the objects actually seen in that direction; and a straight line coming within the field of this oval phantom, appeared somewhat inflected towards its centre; (Plate III. Fig. 14.) a distortion easily understood by considering the effect of the pressure on the form of the retina. Supposing now, the distance between the key and the iron ring to have been, as it really was, invariable, the elongation of the eye must have been either totally or very nearly prevented; and, instead of an increase of the length of the eye's axis, the oval spot caused by the pressure would have spread over a space at least ten times as large as the most sensible part of the retina. But no such circumstance took place: the power of accommodation was as extensive as ever; and there was no perceptible change, either in the size or in the figure of the oval spot.

Again, since the rays which pass through the centre of the pupil, or rather the anterior vertex of the lens, may, as already observed, be considered as delineating the image; and, since the divergence of these rays with respect to each other, is but little affected by the refraction of the lens, they may still be said to diverge from the centre of the pupil; and the image of a given object on the retina must be very considerably enlarged, by the removal of the retina to a greater distance from the pupil and lens. (Cor. Prop. V*.) To ascertain the real magnitude of the image with accuracy, is not so easy as it at first sight appears; but, besides the experiment last related, which might be employed as an argument to this purpose, there are two other methods of estimating it. The first is too hazardous to be of much use; but, with proper precautions, it may be attempted. I fix my eye on a brass circle placed in the rays of the sun, and, after some time, remove it to the cancellated micrometer; then, changing the focus of my eye, while the micrometer remains at a given distance, I endeavour to discover whether there is any difference in the apparent magnitude of the spectrum on the scale; but I can discern none. I have not insisted on the attempt; especially as I have not been able to make the

* This Corollary should stand thus. "If a confused image be received on any given plane, it will be necessary, in order to determine its magnitude, to advert to the aperture admitting the rays. If the aperture be supposed to be infinitely small, it may be considered as a radiant point, in order to find the direction of the emergent rays."

spectrum distinct enough without inconvenience; and no light is sufficiently strong to cause a permanent impression on any part of the retina remote from the visual axis. I therefore had recourse to another experiment. I placed two candles so as exactly to answer to the extent of the termination of the optic nerve, and, marking accurately the point to which my eye was directed, I made the utmost change in its focal length; expecting that, if there were any elongation of the axis, the external candle would appear to recede outwards upon the visible space. (Plate III. Fig. 15.) But this did not happen; the apparent place of the obscure part was precisely the same as before. I will not undertake to say, that I could have observed a very minute difference either way: but I am persuaded, that I should have discovered an alteration of less than a tenth part of the whole.

It may be inquired if no change in the magnitude of the image is to be expected on any other supposition; and it will appear to be possible, that the changes of curvature may be so adapted, that the magnitude of the confused image may remain perfectly constant. Indeed, to calculate from the dimensions which we have hitherto used, it would be expected that the image should be diminished about one-sixtieth, by the utmost increase of the convexity of the lens. But the whole depends on the situation of the refracting surfaces, and the respective increase of their curvature, which, on account of the variable density of the lens, can scarcely be estimated with sufficient accuracy. Had the pupil been placed before the cornea, the magnitude of the image must, on any supposition, have been very variable: at present, this inconvenience is avoided by the situation of the pupil; so that we have here an additional instance of the perfection of this admirable organ.

From the experiments related, it appears to be highly improbable that any material change in the length of the axis actually takes place; and it is almost impossible to conceive by what power such a change could be effected. The straight muscles, with the adipose substance lying under them, would certainly, when acting independently of the socket, tend to flatten the eye: for, since their contraction would necessarily lessen the circumference or superficies of the mass that they contain, and round off all its prominences, their attachment about the nerve and the anterior part of the eye must therefore be brought nearer together. (Plate V. Fig. 21, 22.) Dr. OLBERS compares the muscles and the eye to a cone, of which the sides are protruded, and would by contraction be brought into a straight line. But this would require a force to preserve the cornea as a fixed point, at a given distance from the origin of the muscles; a force which certainly does not exist. In the natural situation of the visual axis, the orbit being conical, the eye might be somewhat lengthened, although irregularly, by being forced further into it; but, when turned towards either side, the same action would rather shorten its axis; nor is there any thing about the human eye that could supply its place. In quadrupeds, the oblique muscles are wider than in man; and in many situations might assist in the effect. Indeed a portion of the orbicular muscle of the globe is attached so near to the nerve, that it might also co-operate in the action: and I have no reason to doubt the accuracy of Dr. OLBERS, who states, that he effected a considerable elongation, by tying threads to the muscles, in the eyes of hogs and of calves; yet he does not say in what position the axis was fixed; and the flaccidity of the eye after death might render such a change very easy as would be impossible in a living eye. Dr. OLBERS also mentions an observation of Professor WRISBERG, on the eye of a man whom he believed to be destitute of the power of accommodation in his life-time, and whom he found, after death, to have wanted one or more of the muscles: but this want of accommodation was not at all accurately ascertained. I measured, in the human eye, the distance of the attachment of the inferior oblique muscle from the insertion of the nerve: it was one-fifth of an inch; and from the centre of vision not a tenth of an inch; so that, although the oblique muscles do in some positions nearly form a part of a great circle round the eye, their action would be more fitted to flatten than to elongate it. We have therefore reason to agree with WINSLOW, in attributing to them the office of helping to support the eye on that side where the bones are most deficient: they seem also well calculated to prevent its being drawn too much backwards by the action of the straight muscles. And, even if there were no difficulty in supposing the muscles to elongate the eye in every position, yet at least some small difference would be expected in the extent of the change, when the eye is in different situations, at an interval of more than a right angle from each other; but the optometer shews that there is none.

Dr. HOSACK alleges that he was able, by making a pressure on the eye, to accommodate it to a nearer object: * it does not appear that he made use of very accurate means of ascertaining the fact; but, if such an effect took place, the cause must have been an inflection of the cornea.

It is unnecessary to dwell on the opinion which supposes a joint operation, of changes in the curvature of the cornea and

* Phil. Trans. for 1794, p. 212.

in the length of the axis. This opinion had derived very great respectability, from the most ingenious and elegant manner in which Dr. Olbers had treated it, and from being the last result of the investigation of Mr. Home and Mr. Ramsden. But either of the series of experiments which have been related, appears to be sufficient to confute it.

X. It now remains to inquire into the pretensions of the crystalline lens to the power of altering the focal length of the eye. The grand objection to the efficacy of a change of figure in the lens, was derived from the experiments in which those who have been deprived of it have appeared to possess the faculty of accommodation.

My friend Mr. Ware, convinced as he was of the neatness and accuracy of the experiments related in the Croonian Lecture for 1795, yet could not still help imagining, from the obvious advantage all his patients found, after the extraction of the lens, in using two kinds of spectacles, that there must, in such cases, be a deficiency in that faculty. This circumstance, combined with a consideration of the directions very judiciously given by Dr. Porterfield, for ascertaining the point in question, first made me wish to repeat the experiments upon various individuals, and with the instrument which I have above described as an improvement of Dr. Porterfield's optometer: and I must here acknowledge my great obligation to Mr. Ware, for the readiness and liberality with which he introduced me to such of his numerous patients as he thought most likely to furnish a satisfactory determination. It is unnecessary to enumerate every particular experiment; but the universal result is, contrary to the expectation with which I entered on the inquiry, that in an eye deprived of the crystalline lens, the actual focal distance is totally unchangeable. This will appear from a selection of the most decisive observations.

1. Mr. R. can read at four inches and at six only, with the same glass. He saw the double lines meeting at three inches, and always at the same point; but the cornea was somewhat irregularly prominent, and his vision not very distinct; nor had I, at the time I saw him, a convenient apparatus.

I afterwards provided a small optometer, with a lens of less than two inches focus, adding a series of letters, not in alphabetical order, and projected into such a form as to be most legible at a small inclination. The excess of the magnifying power had the advantage of making the lines more divergent, and their crossing more conspicuous; and the letters served for more readily naming the distance of the intersection, and, at the same time, for judging of the extent of the power of distinguishing objects too near or too remote for perfect vision. (Plate V. Fig. 23.)

2. Mr. J. had not an eye very proper for the experiment; but he appeared to distinguish the letters at 2 $\frac{1}{2}$ inches, and at less than an inch. This at first persuaded me, that he must have a power of changing the focal distance: but I afterwards recollected that he had withdrawn his eye considerably, to look at the nearer letters, and had also partly closed his eyelids, no doubt contracting at the same time the aperture of the pupil; an action which, even in a perfect eye, always accompanies the change of focus. The slider was not applied.

3. Miss H. a young lady of about twenty, had a very narrow pupil, and I had not an opportunity of trying the small optometer: but, when she once saw an object double through the slits, no exertion could make it appear single at the same dis-

tance. She used for distant objects a glass of 4 $\frac{1}{2}$ inches focus; with this she could read as far off as 12 inches, and as near as five: for nearer objects she added another of equal focus, and could then read at 7 inches, and at 2 $\frac{1}{2}$.

4. HANSON, a carpenter, aged 63, had a cataract extracted a few years since from one eye: the pupil was clear and large, and he saw well to work with a lens of 2 $\frac{3}{8}$ inches focus; and could read at 8 and at 15 inches, but most conveniently at 11. With the same glass, the lines of the optometer appeared always to meet at 11 inches; but he could not perceive that they crossed, the line being too strong, and the intersection too distant. The experiment was afterwards repeated with the small optometer: he read the letters from 2 to 3 inches; but the intersection was always at 2 $\frac{1}{2}$ inches. He now fully understood the circumstances that were to be noticed, and saw the crossing with perfect distinctness: at one time, he said it was a tenth of an inch nearer; but I observed that he had removed his eye two or three tenths from the glass, a circumstance which accounted for this small difference.

5. Notwithstanding HANSON's age, I consider him as a very fair subject for the experiment. But a still more unexceptionable eye was that of Mrs. MABERLY. She is about 30, and had the crystalline of both eyes extracted a few years since, but sees best with her right. She walks without glasses; and, with the assistance of a lens of about *four* inches focus, can read and work with ease. She could distinguish the letters of the small optometer from an inch to 2 $\frac{1}{2}$ inches; but the intersection was invariably at the same point, about 19 tenths of an inch distant. A portion of the capsule is stretched across the pupil, and causes her to see remote objects double, when without her

glasses; nor can she, by any exertion, bring the two images nearer together, although the exertion makes them more distinct, no doubt by contracting the pupil. The experiment with the optometer was conducted, in the presence of Mr. WARE, with patience and perseverance; nor was any opinion given to make her report partial.

Considering the difficulty of finding an eye perfectly suitable for the experiments, these proofs may be deemed tolerably satisfactory. But, since one positive argument will counterbalance many negative ones, provided it be equally grounded on fact, it becomes necessary to inquire into the competency of the evidence employed to ascertain the power of accommodation attributed, in the Croonian Lecture for 1794, to the eye of BENJAMIN CLERK. And it appears, that the distinction long since very properly made by Dr. JURIN, between distinct vision and perfect vision, will readily explain away the whole of that evidence.

It is obvious that vision may be made distinct to any given extent, by means of an aperture sufficiently small, provided at the same time, that a sufficient quantity of light be left, while the refractive powers of the eye remain unchanged. And it is remarkable, that in those experiments, when the comparison with the perfect eye was made, the aperture of the imperfect eye only was very considerably reduced. BENJAMIN CLERK, with an aperture of $\frac{3}{40}$ of an inch, could read with the same glass at $1\frac{7}{8}$ inch, and at 7 inches.* With an equal aperture, I can read at $1\frac{1}{2}$ inch and at 30 inches: and I can retain the state of perfect relaxation, and read with the same aperture at $2\frac{1}{4}$ inches; and this is as great a difference as was observed in

* Phil. Trans. for 1795. p. 9.

BENJAMIN CLERK's eye. It is also a fact of no small importance, that Sir HENRY ENGLEFIELD was much astonished, as well as the other observers, at the accuracy with which the man's eye was adjusted to the same distance, in the repeated trials that were made with it.* This circumstance alone makes it highly probable, that its perfect vision was confined within very narrow limits.

Hitherto I have endeavoured to shew the inconveniences attending other suppositions, and to remove the objections to the opinion of an internal change of the figure of the lens. I shall now state two experiments, which, in the first place, come very near to a mathematical demonstration of the existence of such a change, and, in the second, explain in great measure its origin, and the manner in which it is effected.

I have already described the appearances of the imperfect image of a minute point at different distances from the eye, in a state of relaxation. For the present purpose, I will only repeat, that if the point is beyond the furthest focal distance of the eye, it assumes that appearance which is generally described by the name of a star, the central part being considerably the brightest. (Plate VI. Fig. 36—39.) But, when the focal distance of the eye is shortened, the imperfect image is of course enlarged; and, besides this necessary consequence, the light is also very differently distributed; the central part becomes faint, and the margin strongly illuminated, so as to have almost the appearance of an oval ring. (Fig. 41.) If I apply the slider of the optometer, the shadows of the slits, while the eye is relaxed, are perfectly straight, dividing the oval either way into parallel segments: (Fig 42, 44.) but, when the accom-

* Phil. Trans. for 1795. p. 8.

modation takes place, they immediately become curved, and the more so the further they are from the centre of the image, to which their concavity is directed. (Fig. 43, 45.) If the point be brought much within the focal distance, the change of the eye will increase the illumination of the centre, at the expense of the margin. The same appearances are equally observable, when the effect of the cornea is removed by immersion in water; and the only imaginable way of accounting for the diversity, is to suppose the central parts of the lens to acquire a greater degree of curvature than the marginal parts. If the refraction of the lens remained the same, it is absolutely impossible that any change of the distance of the retina should produce a curvature in those shadows, which, in the relaxed state of the eye, are found to be in all parts straight; and, that neither the form nor the relative situation of the cornea is concerned, appears from the application of water already mentioned.

The truth of this explanation is fully confirmed by the optometer. When I look through four narrow slits, without exertion, the lines always appear to meet in one point: but, when I make the intersection approach me, the two outer lines meet considerably beyond the inner ones, and the two lines of the same side cross each other at a still greater distance. (Plate V. Fig. 24.)

The experiment will not succeed with every eye; nor can it be expected that such an imperfection should be universal: but one case is sufficient to establish the argument, even if no other were found. I do not however doubt, that in those who have a large pupil, the aberration may be very frequently observable. In Dr. Wollaston's eye, the diversity of appearance is imperceptible; but Mr. König described the intersections exactly as they appear to me, although he had received no hint of what I had observed. The lateral refraction is the most easily ascertained, by substituting for the slits a tapering piece of card, so as to cover all the central parts of the pupil, and thus determining the nearest crossing of the shadows transmitted through the marginal parts only. When the furthest intersection was at 38, I could bring it to 22 parts with two narrow slits; but with the tapered card only to 29. From these data we may determine pretty nearly, into what form the lens must be changed, supposing both the surfaces to undergo proportional alterations of curvature, and taking for granted the dimensions already laid down: for, from the lateral aberration thus given, we may find (by Prop. III.) the subtangents at about one-tenth of an inch from the axis; and the radius of curvature at each vertex, is already determined to be about 21 and 15 hundredths of an inch. Hence the anterior surface must be a portion of a hyperboloid, of which the greater axis is about 50; and the posterior surface will be nearly parabolical. In this manner the change will be effected, without any diminution of the transverse diameter of the lens. The elongation of its axis will not exceed the fiftieth of an inch; and, on the supposition with which we set out, the protrusion will be chiefly at the posterior vertex. The form of the lens thus changed will be nearly that of Plate V. Fig. 26; the relaxed state being nearly as represented in Fig. 25. Should, however, the rigidity of the internal parts, or any other considerations, render it convenient to suppose the anterior surface more changed, it would still have room, without interfering with the uvea; or it might even force the uvea a little forwards, without any visible alteration of the external appearance of the eye.

From this investigation of the change of the figure of the lens, it appears that the action which I formerly attributed to the external coats, cannot afford an explanation of the phenomenon. The necessary effect of such an action would be, to produce a figure approaching to that of an oblate spheroid; and, to say nothing of the inconvenience attending a diminution of the diameter of the lens, the lateral refraction would be much more increased than the central; nor would the slight change of density, at an equal distance from the axis, be at all equivalent to the increase of curvature: we must therefore suppose some different mode of action in the power producing the change. Now, whether we call the lens a muscle or not, it seems demonstrable, that such a change of figure takes place as can be produced by no external cause; and we may at least illustrate it by a comparison with the usual action of muscular fibres. A muscle never contracts, without at the same time swelling laterally, and it is of no consequence which of the effects we consider as primary. I was induced, by an occasional opacity, to give the name of membranous tendons to the radiations from the centre of the lens; but, on a more accurate examination, nothing really analogous to tendon can be discovered. And, if it were supposed that the parts next the axis were throughout of a tendinous, and therefore unchangeable nature, the contraction must be principally effected by the lateral parts of the fibres; so that the coats would become thicker towards the margin, by their contraction, while the general alteration of form would require them to be thinner; and there would be a contrariety in the actions of the various parts. But, if we compare the central parts of each surface to the belly of the muscle, there is no difficulty in conceiving their thickness to be immediately increased, and to produce an immediate elongation of the axis, and an increase of the central curvature; while the lateral parts co-operate more or less, according to their distance from the centre, and in different individuals in somewhat different proportions. On this supposition, we have no longer any difficulty in attributing a power of change to the crystalline of fishes. M. PETIT, in a great number of observations, uniformly found the lens of fishes more or less flattened: but, even if it were not, a slight extension of the lateral part of the superficial fibres would allow those softer coats to become thicker at each vertex, and to form the whole lens into a spheroid somewhat oblong; and here, the lens being the only agent in refraction, a less alteration than in other animals would be sufficient. It is also worthy of inquiry, whether the state of contraction may not immediately add to the refractive power. According to the old experiment, by which Dr. GODDARD attempted to show that muscles become more dense as they contract, such an effect might naturally be expected That experiment is, however, very indecisive, and the opinion is indeed generally exploded, but perhaps too hastily; and whoever shall ascertain the existence or non-existence of such a condensation, will render essential service to physiology in general.

Dr. PEMBERTON, in the year 1719, first systematically discussed the opinion of the muscularity of the crystalline lens.* He referred to LEEUWENHOEK's microscopical observations; but he so overwhelmed his subject with intricate calculations, that few have attempted to develope it: and he grounded the

* De Facultate Oculi qua ad diversas Rerum distantias se accommodat. L. B. 1719. Ap. Hall. Disp. Anat. IV. p. 301.

whole on an experiment borrowed from BARROW, which with me has totally failed; and I cannot but agree with Dr. OLBERS in the remark, that it is easier to confute him than to understand him. He argued for a partial change of the figure of the lens; and perhaps the opinion was more just than the reasons adduced for its support. LOBE', or rather ALBINUS,* decidedly favours a similar theory; and suggests the analogy of the lens to the muscular parts of pellucid animals, in which even the best microscopes can discover no fibres. CAMPER also mentions the hypothesis with considerable approbation.† Professor REIL published, in 1793, a Dissertation on the Structure of the Lens; and, in a subsequent paper, annexed to the translation of my former Essay in Professor GREN's Journal, § he discussed the question of its muscularity. I regret that I have not now an opportunity of referring to this publication; but I do not recollect that Professor REIL's objections are different from those which I have already noticed.

Considering the sympathy of the crystalline lens with the uvea, and the delicate nature of the change of its figure, there is little reason to expect that any artificial stimulus would be more successful in exciting a contractive action in the lens, than it has hitherto been in the uvea; much less would that contraction be visible without art. Soon after Mr. HUNTER's death, I pursued the experiment which he had suggested, for ascertaining how far such a contraction might be observable. My apparatus (Plate V. Fig. 27.) was executed by Mr. JONES. It consisted of a wooden vessel blacked within, which was to be

* De quibusdam Oculi Partibus, L. B. 1746. Ap. Hall. Disp. Anat. IV. p. 301.

† De Oculo Humano. L. B. 1742. Ap. Hall. Disp. Anat. VII. 2. p. 108, 109.

§ 1794. p. 352, 354.

filled with cool, and then with warmer water: a plane speculum was placed under it; a perforation in the bottom was filled with a plate of glass; proper rings were fixed for the reception of the lens, or of the whole eye, and also wires for transmitting electricity: above these, a piece of ground and painted glass, for receiving the image, was supported by a bracket, which moved by a pivot, in connection with a scale divided into fiftieths of an inch. With this apparatus I made some experiments, assisted by Mr. WILKINSON, whose residence was near a slaughter-house: but we could obtain, by this method, no satisfactory evidence of the change; nor was our expectation much disappointed. I understand also, that another member of this Society was equally unsuccessful, in attempting to produce a conspicuous change in the lens by electricity.

XI. In man and in the most common quadrupeds, the structure of the lens is nearly similar. The number of radiations is of little consequence; but I find that in the human crystalline there are ten on each side, (Plate VI. Fig. 46.) not three, as I once, from a hasty observation, concluded.* Those who find any difficulty in discovering the fibres, must have a sight very ill adapted to microscopical researches. I have laboured with the most obstinate perseverance to trace nerves into the lens, and I have sometimes imagined that I had succeeded; but I cannot positively go further than to state my full conviction of their existence, and of the precipitancy of those who have absolutely denied it. The long nerves, which are very conspicuous between the choroid and sclerotic coats, divide each into two, three, or more branches, at the spot where the ciliary zone begins, and seem indeed to furnish the choroid with some fine

* De Corp. Hum. Vir. Cons. p. 68.

filaments at the same place. The branches often re-unite, with a slight protuberance, that scarcely deserves the name of a ganglion: here they are tied down, and mixed with the hard whitish-brown membrane that covers the compact spongy substance, in which the vessels of the ciliary processes anastomose and subdivide. (Plate VI. Fig. 47.) The quantity of the nerves which proceeds to the iris, appears to be considerably smaller than that which arrives at the place of division: hence there can be little doubt that the division is calculated to supply the lens with some minute branches; and it is not improbable, from the appearance of the parts, that some fibres may pass to the cornea; although it might more naturally be expected, that the tunica conjunctiva would be supplied from without. But the subdivisions which probably pass to the lens, enter immediately into a mixture of ligamentous substance and of a tough brownish membrane; and I have not hitherto been able to develope them. Perhaps animals may be found in which this substance is of a different nature; and I do not despair that, with the assistance of injections, for more readily distinguishing the blood vessels, it may still be possible to trace them in quadrupeds. Our inability to discover them, is scarcely an argument against their existence: they must naturally be delicate and transparent; and we have an instance, in the cornea, of considerable sensibility, where no nerve has yet been traced. The capsule adheres to the ciliary substance, and the lens to the capsule, principally in two or three points; but I confess, I have not been able to observe that these points are exactly opposite to the trunks of nerves; so that, probably, the adhesion is chiefly caused by those vessels which are sometimes seen passing to the capsule in injected eyes. We may, however, discover ramifications from some of these points, upon and within the substance of the lens, (Plate VI. Fig. 48.) generally following a direction near to that of the fibres, and sometimes proceeding from a point opposite to one of the radiating lines of the same surface. But the principal vessels of the lens appear to be derived from the central artery, by two or three branches at some little distance from the posterior vertex; which I conceive to be the cause of the frequent adhesion of a portion of a cataract to the capsule, about this point: they follow nearly the course of the radiations, and then of the fibres; but there is often a superficial subdivision of one of the radii, at the spot where one of them enters. The vessels coming from the choroid appear principally to supply a substance, hitherto unobserved, which fills up the marginal part of the capsule of the crystalline, in the form of a thin zone, and makes a slight elevation, visible even through the capsule. (Fig. 49—51.) It consists of coarser fibres than the lens, but in a direction nearly similar; they are often intermixed with small globules. In some animals, the margin of the zone is crenated, especially behind, where it is shorter: this is observable in the partridge; and, in the same bird, the whole surface of the lens is seen to be covered with points, or rather globules, arranged in regular lines, (Plate VII. Fig. 52.) so as to have somewhat the appearance of a honeycomb, but towards the vertex less uniformly disposed. This regularity is a sufficient proof that there could be no optical deception in the appearance; although it requires a good microscope to discover it distinctly: but the zone may be easily peeled off under water, and hardened in spirits. Its use is uncertain; but it may possibly secrete the liquid of the crystalline; and it as much deserves the name of a gland, as the greater part of the substances usually so denominated. In peeling it off, I have very distinctly observed ramifications, which were passing through it into the lens; (Plate VI. Fig. 50.) and indeed it is not at all difficult to detect the vessels connecting the margin of the lens with its capsule; and it is surprising that M. PETIT should have doubted of their existence. I have not yet clearly discerned this crystalline gland in the human eye; but I infer the existence of something similar to the globules, from the spotted appearance of the image of a lucid point already mentioned; for which I can no otherwise account, than by attributing it to a derangement of these particles, produced by the external force, and to an unequal impression made by them on the surface of the lens.

In birds and in fishes, the fibres of the crystalline radiate equally, becoming finer as they approach the vertex, till they are lost in a uniform substance, of the same degree of firmness, which appears to be perforated in the centre by a blood vessel. (Plate VII. Fig. 53.) In quadrupeds, the fibres at their angular meeting are certainly not continued, as LEEUWENHOEK imagined, across the line of division; but there does not appear to be any dissimilar substance interposed between them, except that very minute trunks of vessels often mark that line. But, since the whole mass of the lens, as far as it is moveable, is probably endued with a power of changing its figure, there is no need of any strength of union, or place of attachment, for the fibres, since the motion meets with little or no resistance. Every common muscle, as soon as its contraction ceases, returns to its natural form, even without the assistance of an antagonist; and the lens itself, when taken out of the eye, in its capsule, has elasticity enough to reassume its proper figure, on the removal of a force that has compressed it. The capsule is highly elastic; and, since it is laterally fixed to the ciliary zone, it must co-operate in restoring the lens to its flattest form. If it be inquired, why the lens is not capable of becoming less convex, as well as more so, it may be answered, that the lateral parts have probably little contractive power; and, if they had more, they would have no room to increase the size of the disc, which they must do, in order to shorten the axis; and the parts about the axis have no fibres so arranged as to shorten it by their own contraction.

I consider myself as being partly repaid for the labour lost in search of the nerves of the lens, by having acquired a more accurate conception of the nature and situation of the ciliary substance. It had already been observed, that in the hare and in the wolf, the ciliary processes are not attached to the capsule of the lens; and if by the ciliary processes we understand those filaments which are seen detached after tearing away the capsule, and consist of ramifying vessels, the observation is equally true of the common quadrupeds, and I will venture to say, of the human eye.* Perhaps this remark has been made by others, but the circumstance is not generally understood. It is so difficult to obtain a distinct view of these bodies, undisturbed, that I am partly indebted to accident, for having been undeceived respecting them: but, having once made the observation, I have learnt to show it in an unquestionable manner. I remove the posterior hemisphere of the sclerotica, or somewhat more, and also as much as possible of the vitreous humour, introduce the point of a pair of scissors

* Vid. Hall. Physiol. V. p. 432. et DUVERNEY, ibi citat.

into the capsule, turn out the lens, and cut off the greater part of the posterior portion of the capsule, and of the rest of the vitreous humour. I next dissect the choroid and uvea from the sclerotica; and, dividing the anterior part of the capsule into segments from its centre, I turn them back upon the ciliary zone. The ciliary processes then appear, covered with their pigment, and perfectly distinct both from the capsule and from the uvea; (Plate VII. Fig. 54.) and the surface of the capsule is seen shining, and evidently natural, close to the base of these substances. I do not deny that the separation between the uvea and the processes, extends somewhat further back than the separation between the processes and the capsule; but the difference is inconsiderable, and, in the calf, does not amount to above half the length of the detached part. The appearance of the processes is wholly irreconcileable with muscularity; and their being considered as muscles attached to the capsule, is therefore doubly inadmissible. Their lateral union with the capsule, commences at the base of their posterior smooth surface, and is continued nearly to the point where they are more intimately united with the termination of the uvea; so that, however this portion of the base of the processes were disposed to contract, it would be much too short to produce any sensible effect. What their use may be, cannot easily be determined: if it were necessary to have any peculiar organs for secretion, we might call them glands, for the percolation of the aqueous humour; but there is no reason to think them requisite for this purpose.

The marsupium nigrum of birds, and the horse-shoe-like appearance of the choroid of fishes, are two substances which have sometimes, with equal injustice, been termed muscular. All the apparent fibres of the marsupium nigrum are, as

HALLER had very truly asserted, merely duplicatures of a membrane, which, when its ends are cut off, may easily be unfolded under the microscope, with the assistance of a fine hair pencil, so as to leave no longer any suspicion of a muscular texture. The experiment related by Mr. HOME,* can scarcely be deemed a very strong argument for attributing to this substance a faculty which its appearance so little authorises us to expect in it. The red substance in the choroid of fishes, (Plate VII. Fig. 55.) is more capable of deceiving the observer; its colour gives it some little pretension, and I began to examine it with a prepossession in favour of its muscular nature. But, when we recollect the general colour of the muscles of fishes, the consideration of its redness will no longer have any weight. Stripped of the membrane which loosely covers its internal surface, (Fig. 56.) it seems to have transverse divisions, somewhat resembling those of muscles, and to terminate in a manner somewhat similar; (Fig. 57.) but, when viewed in a microscope, the transverse divisions appear to be cracks, and the whole mass is evidently of a uniform texture, without the least fibrous appearance; and, if a particle of any kind of muscle is compared with it, the contrast becomes very striking. Besides, it is fixed down, throughout its extent, to the posterior lamina of the choroid, and has no attachment capable of directing its effect; to say nothing of the difficulty of conceiving what that effect could be. Its use must remain, in common with that of many other parts of the animal frame, entirely concealed from our curiosity.

The bony scales of the eyes of birds, which were long ago described in the Philosophical Transactions by Mr. RANBY, †

* Phil. Trans. for 1796. p. 18.

† Phil. Trans. Vol. XXXIII. p. 223. Abr. Vol. VII. p. 435.

and by Mr. WARREN*, afterwards in two excellent Memoirs of M. PETIT on the eye of the turkey and of the owl,† and lately by Mr. PIERCE SMITH,‡ and Mr. HOME,§ can, on any supposition, have but little concern in the accommodation of the eye to different distances: they rather seem to be necessary for the protection of that organ, large and prominent as it is, and unsupported by any strength in the orbit, against the various accidents to which the mode of life and rapid motion of those animals must expose it; and they are much less liable to fracture than an entire bony ring of the same thickness would have been. The marsupium nigrum appears to be intended to assist in giving strength to the eye, to prevent any change in the place of the lens by external force: it is so situated as to intercept but little light, and that little is principally what would have fallen on the insertion of the optic nerve; and it seems to be too firmly tied to the lens, even to admit any considerable elongation of the axis of the eye, although it certainly would not impede a protrusion of the cornea.

With respect to the eyes of insects, an observation of POUPART deserves to be repeated here. He remarks, that the eye of the libellula is hollow; that it communicates with an air-vessel placed longitudinally in the trunk of the body; and that it is capable of being inflated from this cavity: he supposes that the insect is provided with this apparatus, in order for the accommodation of its eye to the perception of objects at different distances.|| I have not yet had an opportunity of examining

* Phil.Trans. Vol. XXXIV. p. 113. Abr. Vol. VII. p. 437.

† Mem. de l'Acad. 1735. p. 163. 1736, p. 166. Ed. Amst.

‡ Phil. Trans. for 1795. p. 263.

§ Phil. Trans. for 1796. p. 14.

|| Phil. Trans. Vol. XXII. p. 673. Abr. II. p. 762.

the eye of the libellula; but there is no difficulty in supposing that the means of producing the change of the refractive powers of the eye, may be, in different classes of animals, as diversified as their habits, and the general conformation of their organs.

I beg leave to correct here an observation in my former paper, relative to the faint lateral radiations, which I supposed to proceed from the margin of the iris.* I find, on further examination, that they are occasioned by reflections from the eye-lashes.

XII. I shall now finally recapitulate the principal objects and results of the investigation which I have taken the liberty of detailing so fully to the Royal Society. First, the determination of the refractive power of a variable medium, and its application to the constitution of the crystalline lens. Secondly, the construction of an instrument for ascertaining, upon inspection, the exact focal distance of every eye, and the remedy for its imperfections. Thirdly, to show the accurate adjustment of every part of the eye, for seeing with distinctness the greatest possible extent of objects at the same instant. Fourthly, to measure the collective dispersion of coloured rays in the eye. Fifthly, by immerging the eye in water, to demonstrate that its accommodation does not depend on any change in the curvature of the cornea. Sixthly, by confining the eye at the extremities of its axis, to prove that no material alteration of its length can take place. Seventhly, to examine what inference can be drawn from the experiments hitherto made on persons deprived of the lens; to pursue the inquiry, on the principles suggested by Dr. PORTERFIELD; and to confirm his opinion of the utter inabi-

* Phil. Trans. for 1793. p. 178.

lity of such persons to change the refractive state of the organ. Eighthly, to deduce, from the aberration of the lateral rays, a decisive argument in favour of a change in the figure of the crystalline; to ascertain, from the quantity of this aberration, the form into which the lens appears to be thrown in my own eye, and the mode by which the change must be produced in that of every other person. And I flatter myself, that I shall not be deemed too precipitate, in denominating this series of experiments satisfactorily demonstrative.

## CORRECTIONS.

Page 28, line 11, Prop. III. *after e, insert* the base being unity.

Page 30, line 8, Cor. 10. *for* $ntu$, *read* $ntt$; line 9, *for* product &c. *read* square of the cosine of incidence.

Page 31, line 5, Cor. 11. *for* $1 + u^2 - 2u^4$, *read* $2muu$.

Page 31. Prop. V. Cor. See the note in p. 60.

Page 33. Prop. VIII. By a mistake of a sign, the eighth proposition is rendered erroneous; no use having been made of that proposition, it has been inserted without proper revision. It ought to stand thus, with its demonstration:

### Proposition VIII. Problem.

To find the path of a ray of light falling obliquely on a sphere, of a refractive density varying as any power of the distance from the centre.

The refractive density, in the sense of these propositions, varies as the ratio of the sines, and as the velocity of light in the medium. (Schol. 2. Prop. I.) Let the velocity at the distance $x$ be $x^{-\frac{1}{r}}$; then, considering the refractive force as a species of attraction, we have, in Prop. 41. l. 1. Princip. $\sqrt{ABFD} = x^{-\frac{1}{r}}$, $Q = s$, the sine of incidence, the radius being unity, $Z = s x^{-1}$, $Dc = \dfrac{s}{2xx\sqrt{x^{-\frac{2}{r}} - s^2 x^{-2}}}$ $= \frac{1}{2} s x^{\frac{1}{r}-2} \cdot \overline{1 - s^2 x^{\frac{2}{r}-2}}|^{-\frac{1}{2}}$, and the fluxion of the area described by the radius $= -\frac{1}{2} s x^{\frac{1}{r}-2} \dot{x} \cdot \overline{1 - s^2 x^{\frac{2}{r}-2}}|^{-\frac{1}{2}}$. Let the sine of the inclination to the radius at each point be called $y$; then $y = s\,x^{\frac{1}{r}-1}$, $\dot{y} = \frac{1-r}{r}\,s\,x^{\frac{1}{r}-2}\,\dot{x}$, and the fluxion of the area $= \frac{r}{2r-2}\,\dot{y}\,.\,\overline{1-yy}|^{-\frac{1}{2}}$, of which the fluent is $\frac{r}{2r-2}\,Y$, $y$ being the sine of the arc Y; and the angle corresponding is $\frac{r}{r-1}\,Y$. The value of that angle being found for any two values of $x$ or $y$, the difference is the intervening angle described by the radius. This angle is therefore always to the difference of the inclinations as $r$ to $r-1$, and the deviation is to that difference as 1 to $r-1$.

*Corollary.* Hence, in the passage to the apsis, and the return to the surface, the deviation is always proportionate to the arc cut off by the incident ray produced: therefore such a sphere could never collect parallel rays to any focus, the lateral density being too small towards the surface.

Page 33, line 20, *for* but the two last &c. *read* the seventh may either be deduced from the eighth, or may be demonstrated independently of it.

Page 42, line 18, *after* internally, *insert* Or, if a lens of equal mean dimensions, and equal focal length, with the crystalline, be supposed to consist of two segments of the external portion of such a sphere, the refractive density at the centre of this lens must be as 18 to 17.

Page 47, line 12, *for* calculated &c. *read* estimated by means of the eighth proposition; and probably.

Page 53, line 24, *for* 24, *read* 21; line 25, *for* 17, *read* 15.

Page 61, line 21, *for* sixtieth, *read* fortieth.

EXPLANATION OF THE FIGURES.

Plate II. Fig. 1. See Page 28. Prop. III.

Fig. 2. See Page 28. Prop. IV.

Fig. 3. See Page 31. Prop. V.

Fig. 4—6. Relating to the optometer. See Page 34.

Plate III. Fig. 7. The form of the ends of the optometer, when made of card. The apertures in the shoulders are for holding a lens: the square ends turn under, and are fastened together.

Fig. 8. The scale of the optometer. The middle line is divided, from the lower end, into inches. The next column shows the number of a concave lens requisite for a short-sighted eye; by looking through the slider and observing the number opposite to which the intersection appears when most remote. By observing the place of apparent intersection when nearest, the number requisite will be found in the other column, provided that the eye have the average power of accommodation. At the other end, the middle line is graduated for extending the scale of inches by means of a lens four inches in focus; the negative numbers implying that such rays as proceed from them are made to converge towards a point on the other side of the lens. The other column shows the focal length of convex glasses required by those eyes to which the intersection appears, when nearest, opposite the respective places of the numbers.

Fig. 9. A side view of the optometer, half its size.

Fig. 10. The appearance of the lines through the slider.

Fig. 11. Method of measuring the magnitude of an image on the retina. See Page 48.

Fig. 12. Diagonal scale drawn on a looking-glass.

Fig. 13. The method of applying a lens with water to the cornea.

Fig. 14. The appearance of a spectrum occasioned by pressure; and the inflection of straight lines seen within the limits of the spectrum.

Fig. 15. An illustration of the enlargement of the image, which would be the consequence of an elongation of the eye: the images of the candles which, in one instance, fall on the insertion of the nerve, falling, in the other instance, beyond it.

Plate IV. Fig. 16. The successive forms of the image of a large distant object, as it would be delineated by each refractive surface in the eye; to show how that form at last coincides with the retina. E G is the distance between the foci of horizontal and vertical rays in my eye.

Plate V. Fig. 17. Vertical section of my right eye, seen from without; twice the natural size.

Fig. 18. Horizontal section, seen from above.

Fig. 19. Front view of my left eye when the pupil is contracted; of the natural size.

Fig. 20. The same view when the pupil is dilated.

Fig. 21. Outline of the eye and its straight muscles when at rest.

Fig. 22. Change of figure which would be the consequence of the action of those muscles upon the eye, and upon the adipose substance behind it.

Fig. 23. Scale of the small optometer.

Fig. 24. Appearance of four images of a line seen by my eye when its focus is shortest.

Fig. 25. Outline of the lens when relaxed; from a comparison of M. PETIT's measures with the phenomena of my own eye, and on the supposition that it is found in a relaxed state after death.

Fig. 26. Outline of the lens sufficiently changed to produce the shortest focal distance.

Fig. 27. Apparatus for ascertaining the focal length of the lens in water.

Plate VI. Fig. 28. Various forms of the image depicted by a cylindrical pencil of rays obliquely refracted by a spherical surface, when received on planes at distances progressively greater.

Fig. 29. Image of a minute lucid object held very near to my eye.

Fig. 30. The same appearance when the eye has been rubbed.

Fig. 31—37. Different forms of the image of a lucid point at greater and greater distances; the most perfect focus being like Fig. 33, but much smaller.

Fig. 38. Image of a very remote point seen by my right eye.

Fig. 39. Image of a remote point seen by my left eye; being more obtuse at one end, probably from a less obliquity of the posterior surface of the crystalline lens.

Fig. 40. Combination of two figures similar to the fifth variety of Fig. 28; to imitate Fig. 38.

Fig. 41. Appearance of a distant lucid point when the eye is adapted to a very near object.

Fig, 42, 44. Shadow of parallel wires in the image of a distant point, when the eye is relaxed.

Fig. 43, 45. The same shadows rendered curved by a change in the figure of the crystalline lens.

Fig. 46. The order of the fibres of the human crystalline.

Fig. 47. The division of the nerves at the ciliary zone; the sclerotica being removed. One of the nerves of the uvea is seen passing forwards and subdividing. From the calf.

Fig. 48. Ramifications from the margin of the crystalline lens.

Fig. 49. The zone of the crystalline faintly seen through the capsule.

Fig. 50. The zone raised from its situation, with the ramifications passing through it into the lens.

Fig. 51. The zone of the crystalline detached.

Plate VII. Fig. 52. The crenated zone, and the globules regularly arranged on the crystalline of the partridge.

Fig. 53. The order of the fibres in the lens of birds and fishes.

Fig. 54. The segments of the capsule of the crystalline turned back, to show the detached ciliary processes. From the calf.

Fig. 55. Part of the choroid of the cod-fish, with its red substance. The central artery hangs loose from the insertion of the nerve.

Fig. 56. The membrane covering this substance internally, raised by the blow-pipe.

Fig. 57. The appearance of the red substance, after the removal of the membrane.

*Fig. 1.*

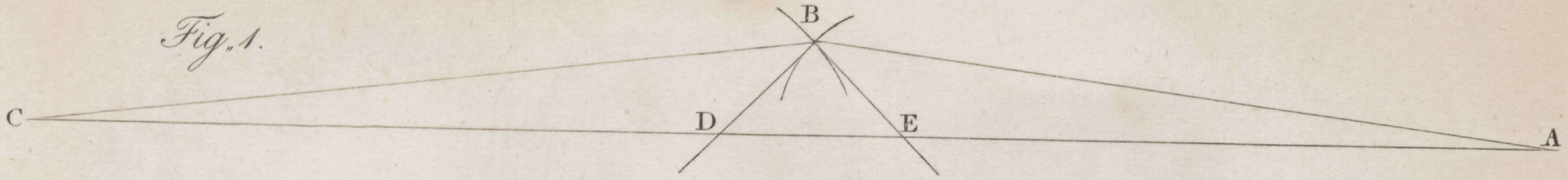



*Fig. 2.*

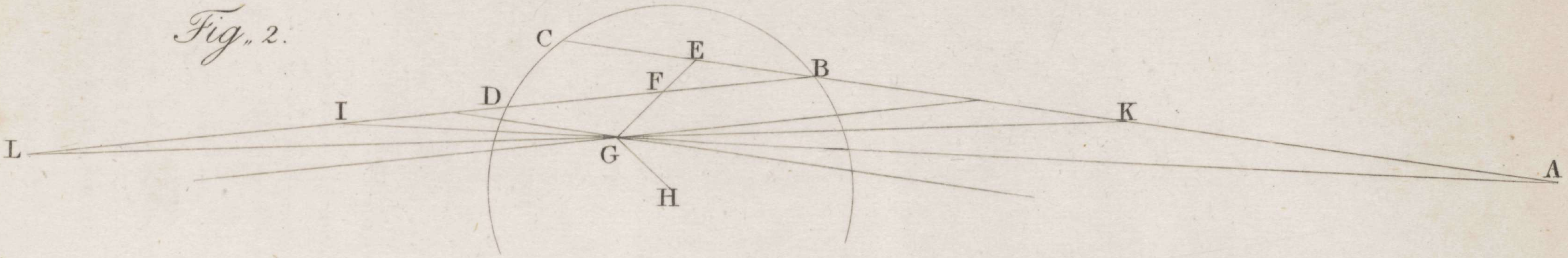



*Fig. 3.*

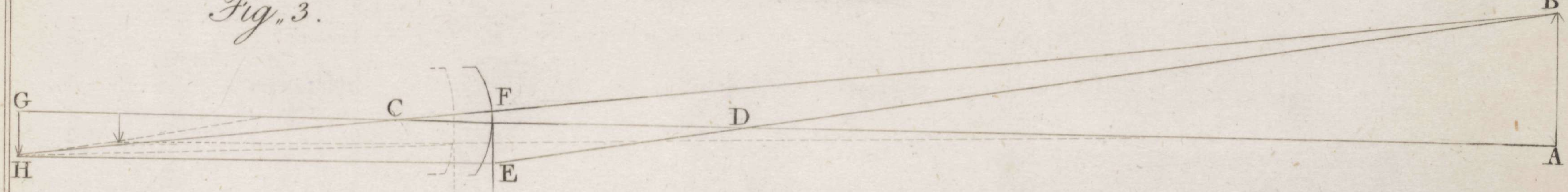



*Fig. 4.*

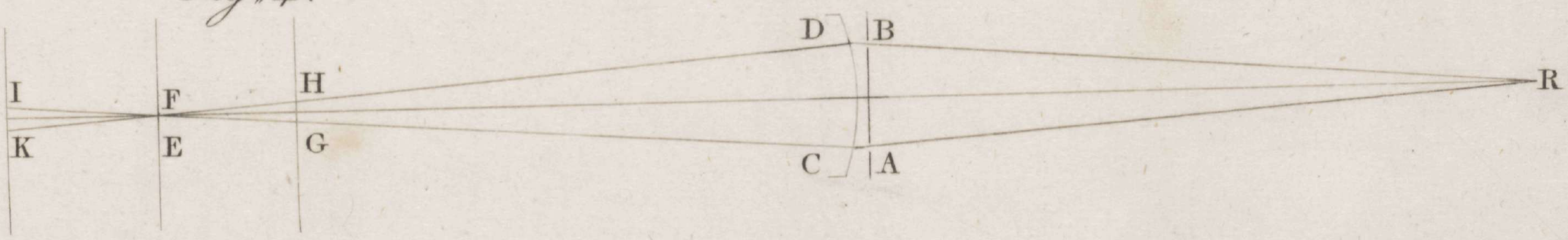



*Fig. 5.*

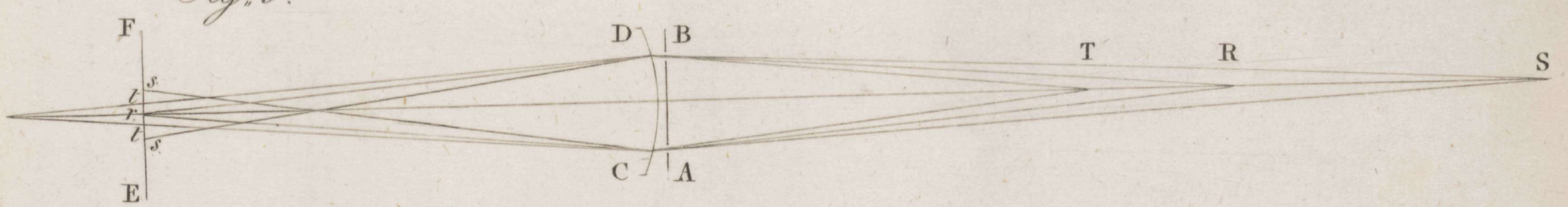
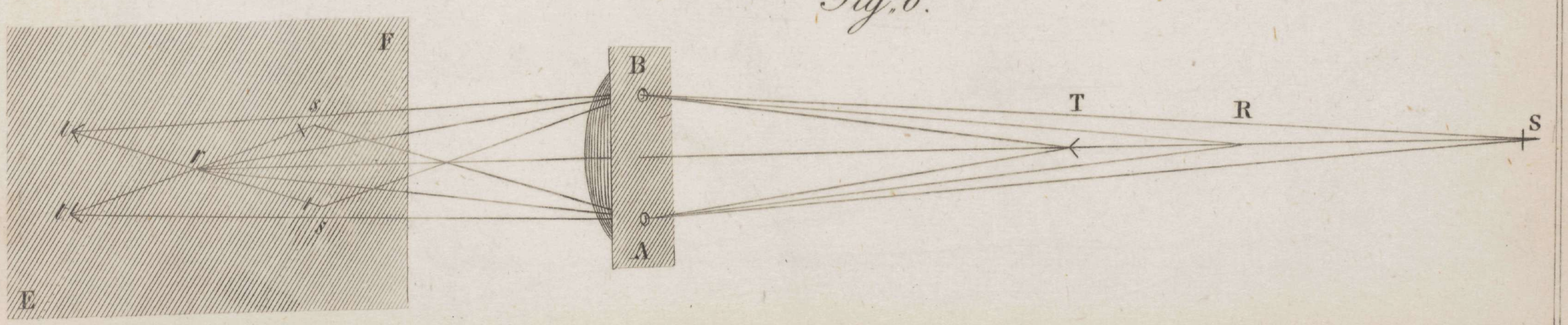



*Fig. 6.*



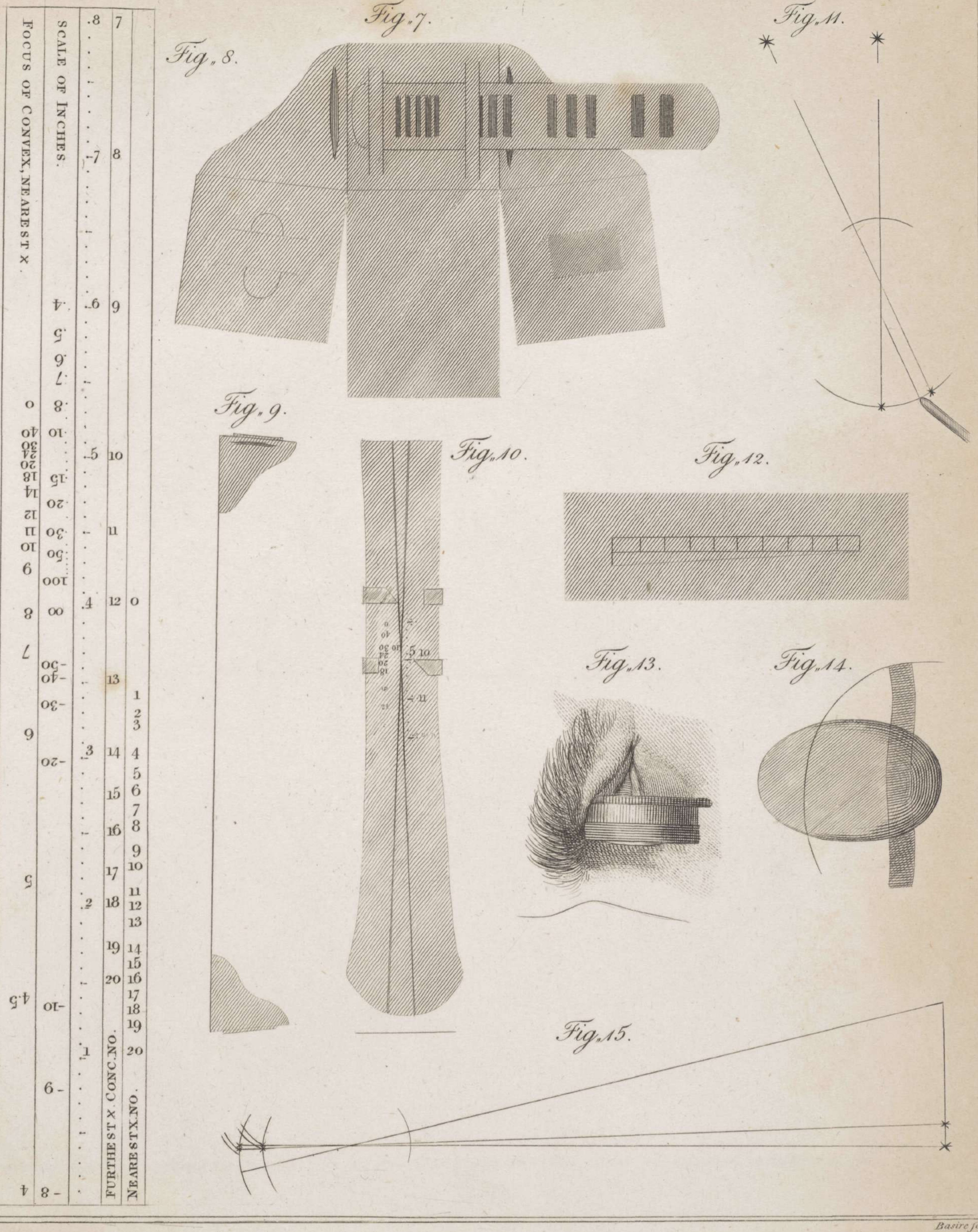


Fig. 7.
Fig. 8.
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SCALE OF INCHES.
FOCUS OF CONVEX, NEAREST X.
FURTHEST X. CONC. NO.
NEAREST X. NO.

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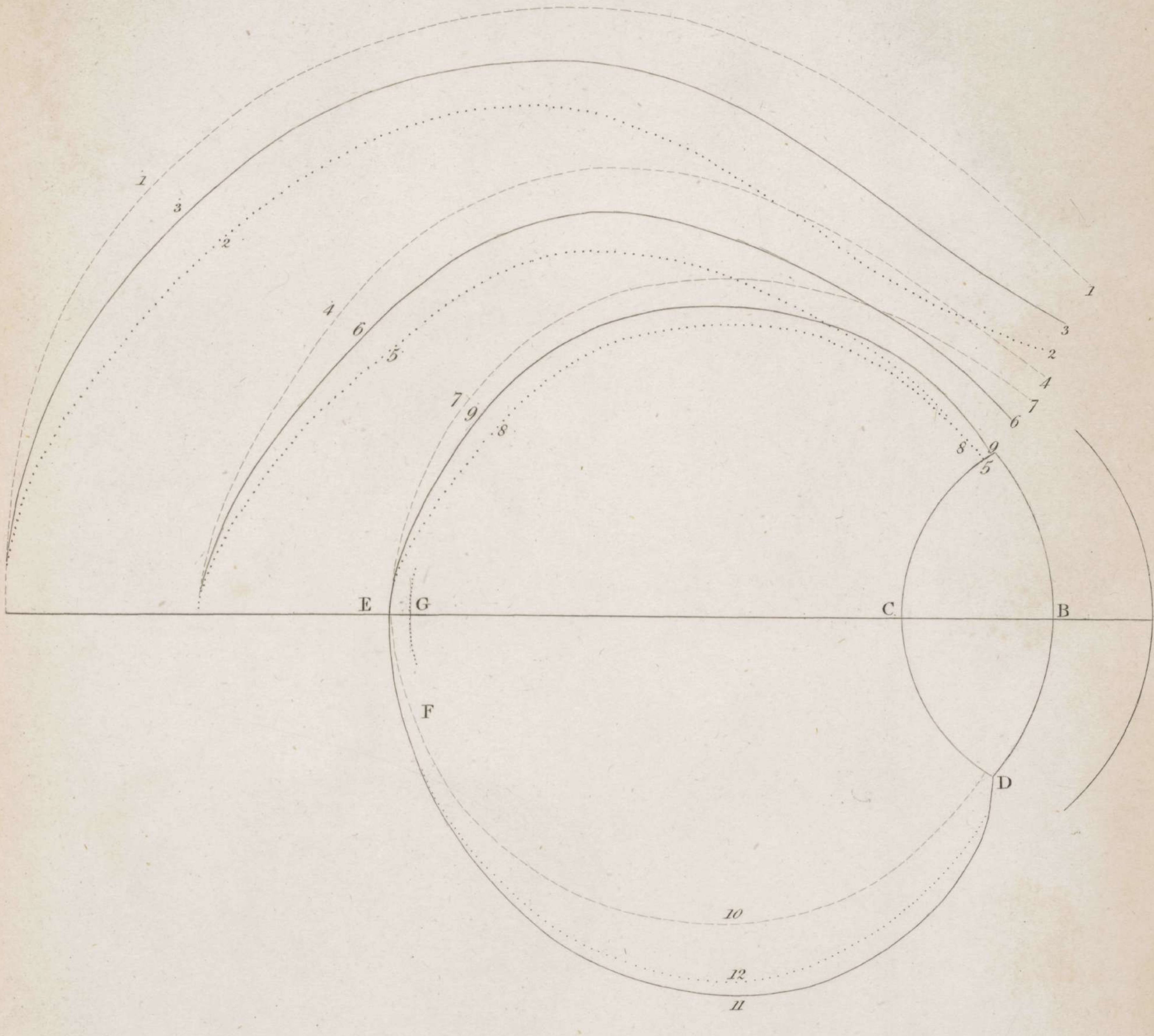
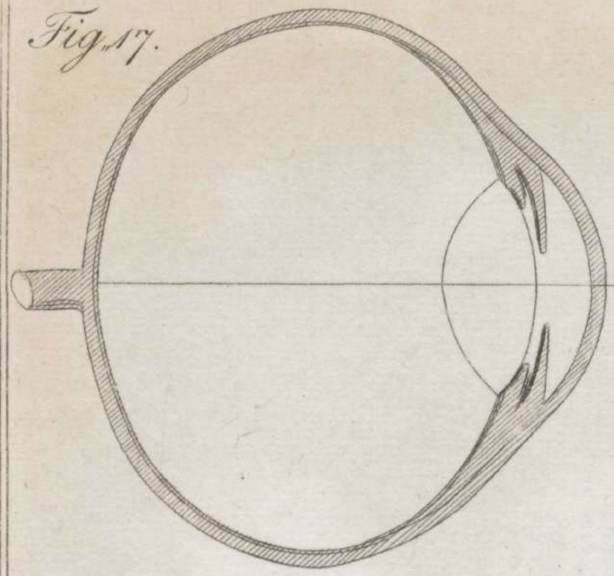



Fig. 16.

Fig. 17.



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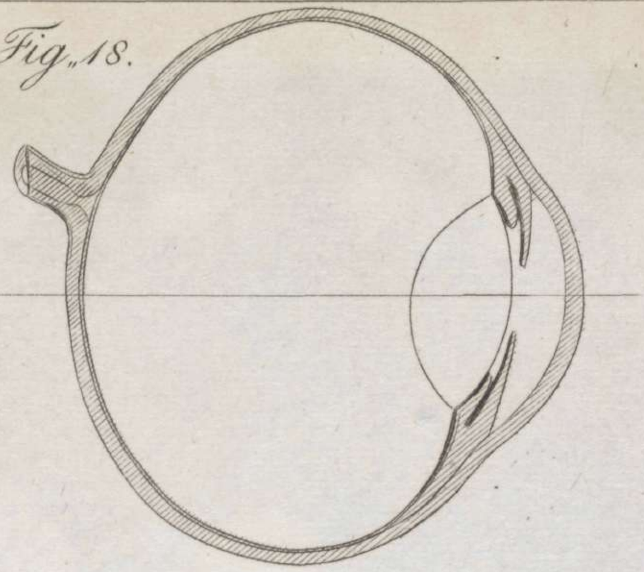

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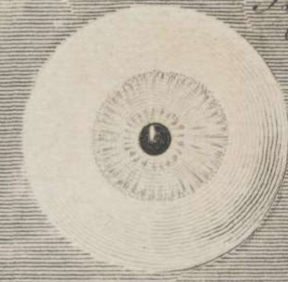

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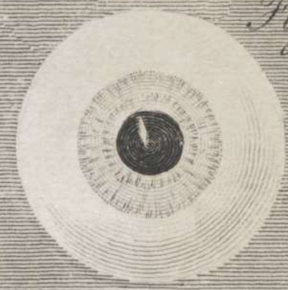

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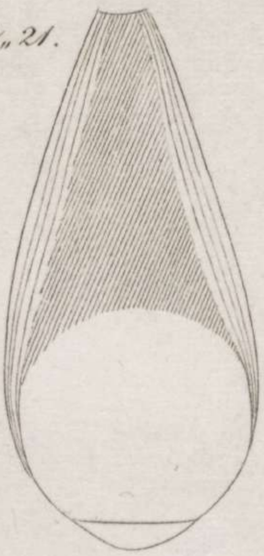

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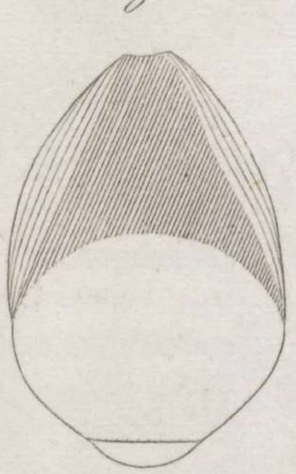
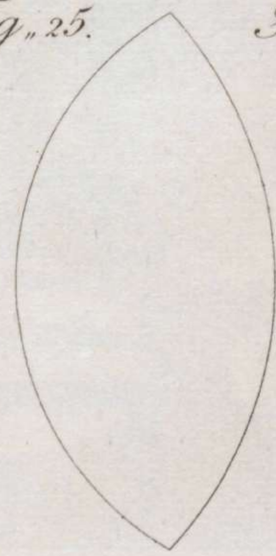


Fig. 25.



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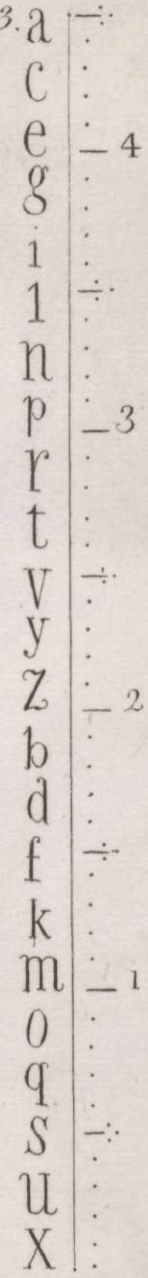


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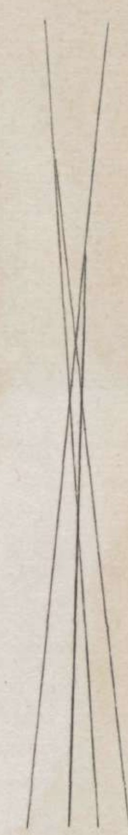

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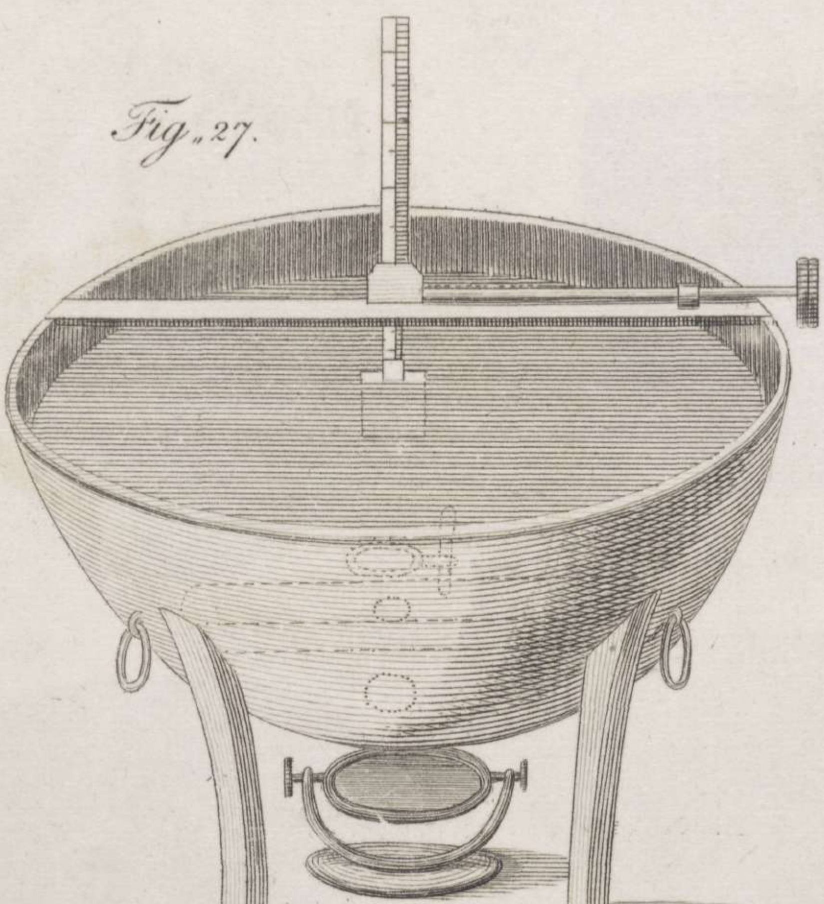

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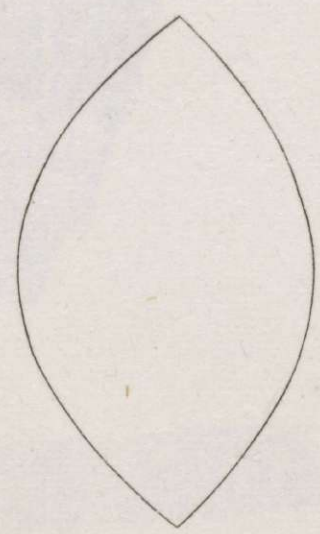

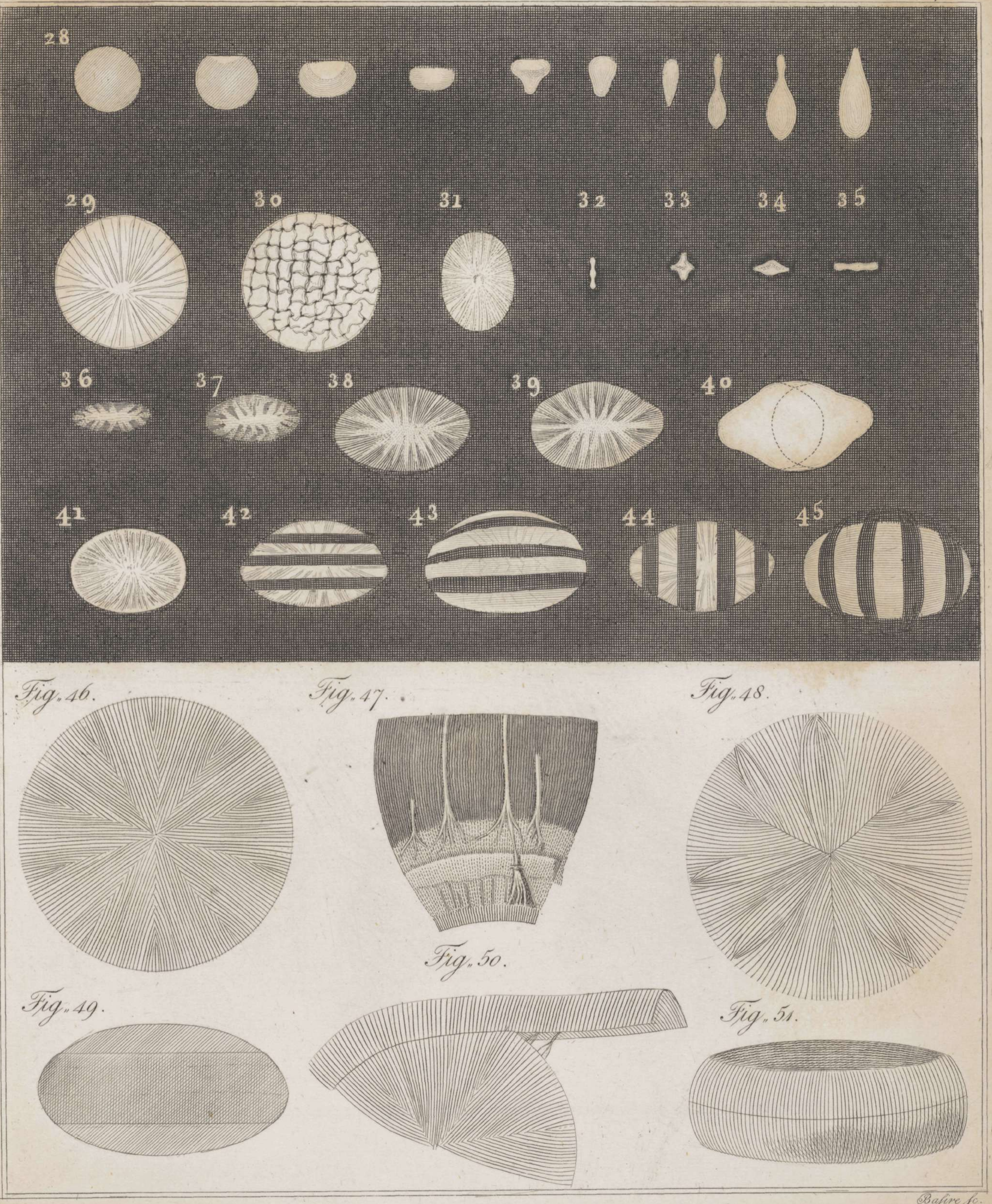
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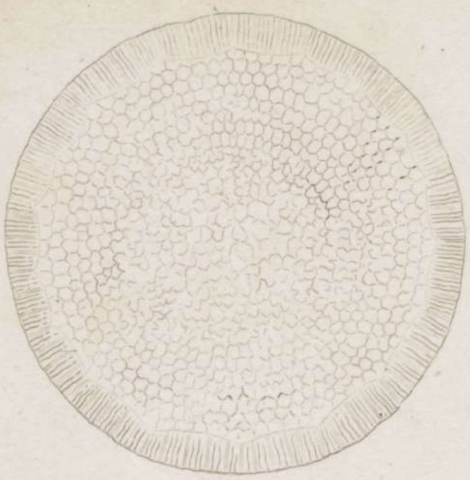
Fig 52.



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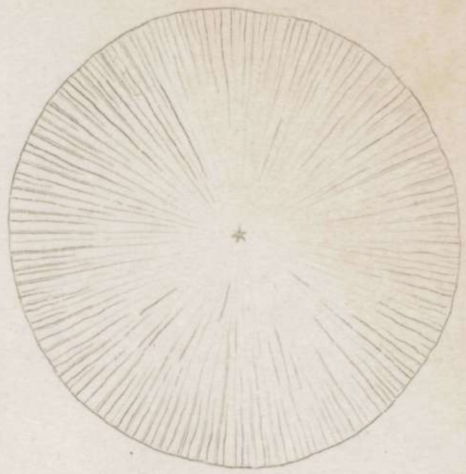


Fig 54.

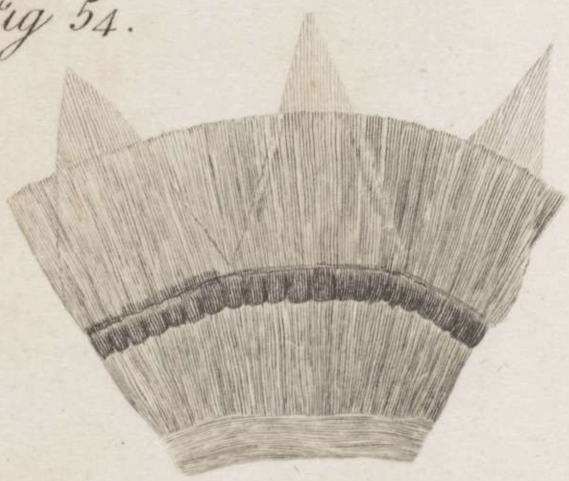


Fig 55.

Fig 56.

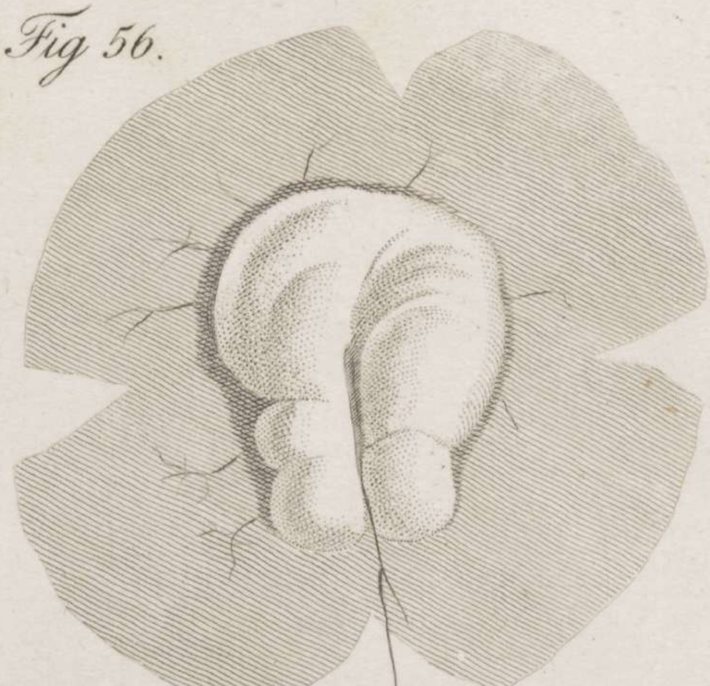
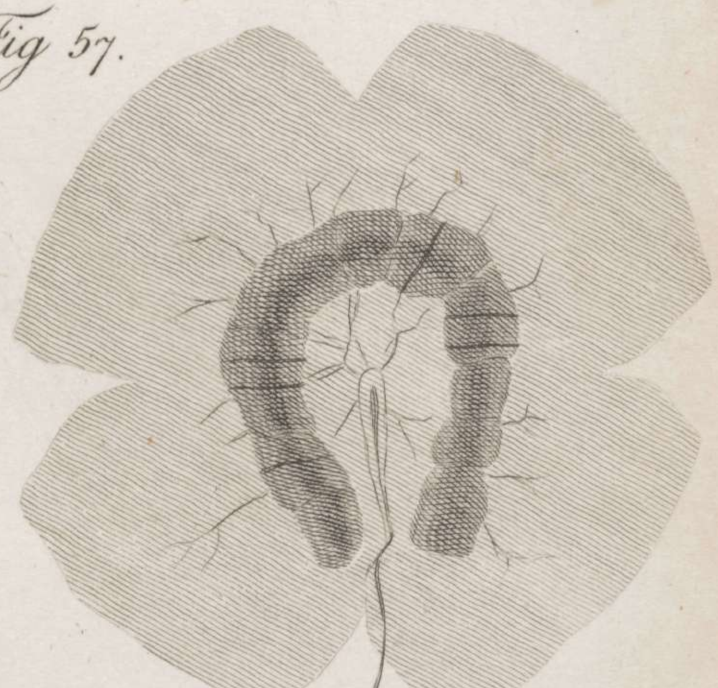


Fig 57.



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