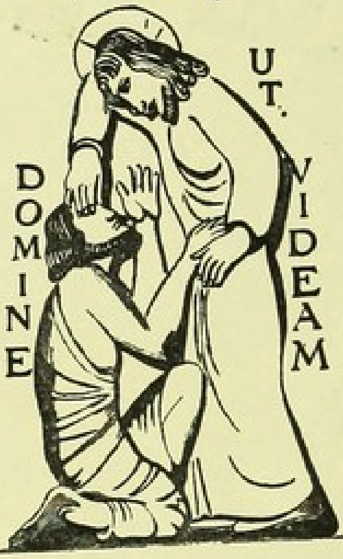
DOMINE
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PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCXCIII.

PART II.

LONDON,

SOLD BY PETER ELMSLY,
PRINTER TO THE ROYAL SOCIETY.
MDCCXCIII.

XVI. *Observations on Vision.* *By* Thomas Young. *Communicated by* Richard Brocklesby, *M. D. F. R. S.*

Read May 30, 1793.

It is well known that the eye, when not acted upon by any exertion of the mind, conveys a distinct impression of those objects only which are situated at a certain distance from itself; that this distance is different in different persons, and that the eye can, by the volition of the mind, be accommodated to view other objects at a much less distance: but how this accommodation is effected, has long been a matter of dispute, and has not yet been satisfactorily explained. It is equally true, though not commonly observed, that no exertion of the mind can accommodate the eye to view objects at a distance greater than that of indolent vision, as may easily be experienced by any person to whom this distance of indolent vision is less than infinite.

The principal parts of the eye, and of its appertenances, have been described by various authors. Winslow is generally very accurate; but Albinus, in Musschenbroek's *Introductio*, has represented several particulars more correctly. I shall suppose their account complete, except where I mention or delineate the contrary.

The first theory that I find of the accommodation of the eye is KEPLER's. He supposes the ciliary processes to contract the diameter of the eye, and lengthen its axis, by a muscular power. But the ciliary processes neither appear to contain any muscular fibres, nor have they any attachment by which they can be capable of performing this action.

DESCARTES imagined the same contraction and elongation to be effected by a muscularity of the crystalline, of which he supposed the ciliary processes to be the tendons. He did not attempt to demonstrate this muscularity, nor did he enough consider the connection with the ciliary processes. He says, that the lens in the mean time becomes more convex, but attributes very little to this circumstance.

DE LA HIRE maintains that the eye undergoes no change, except the contraction and dilatation of the pupil. He does not attempt to confirm this opinion by mathematical demonstration; he solely rests it on an experiment which has been shewn by Dr. SMITH to be fallacious. HALLER too has adopted this opinion, however inconsistent it seems with the known principles of optics, and with the slightest regard to hourly experience.

Dr. PEMBERTON supposes the crystalline to contain muscular fibres, by which one of its surfaces is flattened while the other is made more convex. But, besides that he has demonstrated no such fibres, Dr. JURIN has proved that a change like this is inadequate to the effect.

Dr. PORTERFIELD conceives that the ciliary processes draw forward the crystalline, and make the cornea more convex. The ciliary processes are, from their structure, attachment, and direction, utterly incapable of this action; and, by Dr.

JURIN's calculations, there is not room for a sufficient motion of this kind, without a very visible increase in the length of the eye's axis: such an increase we cannot observe.

Dr. JURIN's hypothesis is, that the uvea, at its attachment to the cornea, is muscular, and that the contraction of this ring makes the cornea more convex. He says, that the fibres of this muscle may as well escape our observation, as those of the muscle of the interior ring. But if such a muscle existed, it must, to overcome the resistance of the coats, be far stronger than that which is only destined to the uvea itself; and the uvea, at this part, exhibits nothing but radiated fibres, losing themselves, before the circle of adherence to the sclerotica, in a brownish granulated substance, not unlike in appearance to capsular ligament, common to the uvea and ciliary processes, but which may be traced separately from them both. Now at the interior ring of the uvea, the appearance is not absolutely inconsistent with an annular muscle. His theory of accommodation to distant objects is ingenious, but no such accommodation takes place.

MUSSCHENBROEK conjectures that the relaxation of his ciliary zone, which appears to be nothing but the capsule of the vitreous humour where it receives the impression of the ciliary processes, permits the coats of the eye to push forwards the crystalline and cornea. Such a voluntary relaxation is wholly without example in the animal economy, and were it to take place, the coats of the eye would not act as he imagines, nor could they so act unobserved. The contraction of the ciliary zone is equally inadequate and unnecessary.

Some have supposed the pressure of the external muscles, especially the two oblique muscles, to elongate the axis of the eye. But their action would not be sufficiently regular, nor sufficiently strong; for a much greater pressure being made on the eye, than they can be supposed capable of effecting, no sensible difference is produced in the distinctness of vision.

Others say that the muscles shorten the axis: these have still less reason on their side.

Those who maintain that the ciliary processes flatten the crystalline, are ignorant of their structure, and of the effect required: these processes are yet more incapable of drawing back the crystalline, and such an action is equally inconsistent with observation.

Probably other suppositions may have been formed, liable to as strong objections as those opinions which I have enumerated.

From these considerations, and from the observation of Dr. PORTERFIELD, that those who have been couched have no longer the power of accommodating the eye to different distances, I had concluded that the rays of light, emitted by objects at a small distance, could only be brought to foci on the retina by a nearer approach of the crystalline to a spherical form; and I could imagine no other power capable of producing this change than a muscularity of a part, or the whole, of its capsule.

But in closely examining, with the naked eye in a strong light, the crystalline from an ox, turned out of its capsule, I discovered a structure which appears to remove all the difficulties with which this branch of optics has long been obscured. On viewing it with a magnifier, this structure became more evident.

The crystalline lens of the ox is an orbicular, convex, transparent body, composed of a considerable number of similar coats, of which the exterior closely adhere to the interior. Each of these coats consists of six muscles, intermixed with a gelatinous substance, and attached to six membranous tendons. Three of the tendons are anterior, three posterior; their length is about two thirds of the semi-diameter of the coat; their arrangement is that of three equal and equidistant rays, meeting in the axis of the crystalline; one of the anterior is directed towards the outer angle of the eye, and one of the posterior towards the inner angle, so that the posterior are placed opposite to the middle of the interstices of the anterior; and planes passing through each of the six, and through the axis, would mark on either surface six regular equidistant rays. The muscular fibres arise from both sides of each tendon; they diverge till they reach the greatest circumference of the coat, and, having passed it, they again converge, till they are attached respectively to the sides of the nearest tendons of the opposite surface. The anterior or posterior portion of the six viewed together, exhibits the appearance of three penniformi-radiated muscles. The anterior tendons of all the coats are situated in the same planes, and the posterior ones in the continuations of these planes beyond the axis. Such an arrangement of fibres can be accounted for on no other supposition than that of muscularity. This mass is inclosed in a strong membranous capsule, to which it is loosely connected by minute vessels and nerves; and the connection is more observable near its greatest circumference. Between the mass and its capsule is found a considerable quantity of an aqueous fluid, the liquid of the crystalline.

I conceive, therefore, that when the will is exerted to view an object at a small distance, the influence of the mind is conveyed through the lenticular ganglion, formed from branches of the third and fifth pairs of nerves, by the filaments perforating the sclerotica, to the orbiculus ciliaris, which may be considered as an annular plexus of nerves and vessels; and thence by the ciliary processes to the muscle of the crystalline, which, by the contraction of its fibres, becomes more convex, and collects the diverging rays to a focus on the retina. The disposition of fibres in each coat is admirably adapted to produce this change; for, since the least surface that can contain a given bulk is that of a sphere, (Simpson's Fluxions, p. 486) the contraction of any surface must bring its contents nearer to a spherical form. The liquid of the crystalline seems to serve as a synovia in facilitating the motion, and to admit a sufficient change of the muscular part, with a smaller motion of the capsule.

It remains to be inquired, whether these fibres can produce an alteration in the form of the lens sufficiently great to account for the known effects.

In the ox's eye, the diameter of the crystalline is 700 thousandths of an inch, the axis of its anterior segment 225, of its posterior 350. In the atmosphere it collects parallel rays at the distance of 235 thousandths. From these data we find, by means of Smith's Optics, Art. 366, and a quadratic, that its ratio of refraction is as 10000 to 6574. Hauksbee makes it only as 10000 to 6832,7, but we cannot depend on his experiment, since he says that the image of the candle which he viewed was enlarged and distorted; a circumstance that he does not explain, but which was evidently occasioned by the greater density of the central parts. Supposing, with HAUKSBEE and others, the refraction of the aqueous and vitreous humours equal to that of water, viz. as 10000 to 7465, the ratio of refraction of the crystalline in the eye will be as 10000 to 8806, and it would collect parallel rays at the distance of 1226 thousandths of an inch: but the distance of the retina from the crystalline is 550 thousandths, and that of the anterior surface of the cornea 250; hence (by SMITH, Art. 367,) the focal distance of the cornea and aqueous humour alone must be 2329. Now, supposing the crystalline to assume a spherical form, its diameter will be 642 thousandths, and its focal distance in the eye 926. Then, disregarding the thickness of the cornea, we find (by SMITH, Art. 370,) that such an eye will collect those rays on the retina, which diverge from a point at the distance of 12 inches and 8 tenths. This is a greater change than is necessary for an ox's eye, for if it be supposed capable of distinct vision at a distance somewhat less than 12 inches, yet it probably is far short of being able to collect parallel rays. The human crystalline is susceptible of a much greater change of form.

The ciliary zone may admit of as much extension as this diminution of the diameter of the crystalline will require; and its elasticity will assist the cellular texture of the vitreous humour, and perhaps the gelatinous part of the crystalline, in restoring the indolent form.

It may be questioned whether the retina takes any part in supplying the lens with nerves; but, from the analogy of the olfactory and auditory nerves, it seems more reasonable to suppose that the optic nerve serves no other purpose than that of conveying sensation to the brain.

Although a strong light and close examination are required, in order to see the fibres of the crystalline in its intire state, yet their direction may be demonstrated, and their attachment shewn, without much difficulty. In a dead eye the tendons are discernible through the capsule, and sometimes the anterior ones even through the cornea and aqueous humour. When the crystalline falls, it very frequently separates as far as the centre into three portions, each having a tendon in its middle. If it be carefully stripped of its capsule, and the smart blast of a fine blow-pipe be applied close to its surface in different parts, it will be found to crack exactly in the direction of the fibres above described, and all these cracks will be stopped as soon as they reach either of the tendons. The application of a little ink to the crystalline is of great use in shewing the course of the fibres.

When first I observed the structure of the crystalline, I was not aware that its muscularity had ever been suspected. We have, however, seen that DESCARTES supposed it to be of this nature; but he seems to think that the accommodation of the eye to a small distance is principally performed by the elongation of the eye's axis. Indeed as a bell shakes a steeple, so must the coats of the eye be affected by any change in the crystalline; but the effect of this will be very inconsiderable; yet, as far as it does take place, it will co-operate with the other change.

But the laborious and accurate LEEUWENHOEK, by the help of his powerful microscopes, has described the course of the fibres of the crystalline, in a variety of animals; and he has even gone so far as to call it a muscle *; but no one has pur-

* Now if the cristaline humour (which I have sometimes called the crist. muscle)

sued the hint, and probably for this reason, that from examining only dried preparations, he has imagined that each coat consists of circumvolutions of a single fibre, and has intirely overlooked the attachment of the fibres to tendons; and if the fibres were continued into each other in the manner that he describes, the strict analogy to muscle would be lost, and their contraction could not have that effect on the figure of the lens, which is produced by help of the tendons. Yet notwithstanding neither he, nor any other physiologist, has attempted to explain the accommodation of the eye to different distances by means of these fibres, still much anatomical merit must be allowed to the faithful description, and elegant delineation, of the crystallines of various animals, which he has given in the Philosophical Transactions, Vol. XIV. p. 780, and Vol. XXIV. p. 1723. It appears, from his descriptions and figures, that the crystalline of hogs, dogs, and cats, resembles what I have observed in oxen, sheep, and horses; that in hares and rabbits, the tendons on each side are only two, meeting in a straight line in the axis; and that in whales they are five, radiated in the same manner as where there are three. It is evident that this variety will make no material difference in the action of the muscle. I have not yet had an opportunity of examining the human crystalline, but from its readily dividing into three parts, we may infer that it is similar to that of the ox. The crystalline in fishes being spherical, such a change as I attribute to the lens in quadrupeds cannot take place in that class of animals.

It has been observed that the central part of the crystalline

in our eyes, &c. Phil. Trans. Vol. XXIV. p. 1729.— *Crystallinum musculum, alias humorem crystallinum dictum, &c.* LEEUWENH. *op. omn.* I. p. 102.

becomes rigid by age, and this is sufficient to account for presbyopia, without any diminution of the humours; although I do not deny the existence of this diminution, as a concomitant circumstance.

I shall here beg leave to attempt the solution of some optical queries, which have not been much considered by authors.

1. MUSSCHENBROEK asks, What is the cause of the lateral radiations which seem to adhere to a candle viewed with winking eyes? I answer, the most conspicuous radiations are those which, diverging from below, form, each with a vertical line, an angle of about seven degrees; this angle is equal to that which the edges of the eyelids when closed make with a horizontal line; and the radiations are evidently caused by the reflection of light from those flattened edges. The lateral radiations are produced by the light reflected from the edges of the lateral parts of the pupillary margin of the uvea, while its superior and inferior portions are covered by the eyelids. The whole uvea being hidden before the total close of the eyelids, these horizontal radiations vanish before the perpendicular ones.

2. Some have inquired, Whence arises that luminous cross, which seems to proceed from the image of a candle in a looking-glass? This is produced by the direction of the friction by which the glass is polished: the scratches placed in a horizontal direction, exhibiting the perpendicular part of the cross, and the vertical scratches the horizontal part, in a manner that may easily be conceived.

3. Why do sparks appear to be emitted when the eye is rubbed or compressed in the dark? This is MUSSCHENBROEK's

fourth query. When a broadish pressure, as that of the finger, is made on the opaque part of the eye in the dark, an orbicular spectrum appears on the part opposite to that which is pressed: the light of the disc is faint, that of the circumference much stronger; but when a narrow surface is applied, as that of a pin's head, or of the nail, the image is narrow and bright. This is evidently occasioned by the irritation of the retina at the part touched, referred by the mind to the place from whence light coming through the pupil would fall on this spot; the irritation is greatest where the flexure is greatest, viz. at the circumference, and sometimes at the centre, of the depressed part. But in the presence of light, whether the eye be open or closed, the circumference only will be luminous, and the disc dark; and if the eye be viewing any object at the part where the image appears, that object will be totally invisible. Hence it follows, that the tension and compression of the retina destroys all the irritation, except that which is produced by its flexure; and this is so slight on the disc, that the apparent light there is fainter than that of the rays arriving at all other parts through the eyelids. This experiment demonstrates a truth, which may be inferred from many other arguments, and is indeed almost an axiom, viz. that the supposed rectification of the inverted image on the retina does not depend on the direction of the incident rays. NEWTON, in his sixteenth query, has described this phantom as of pavonian colours, but I can distinguish no other than white; and it seems most natural that this, being the compound or average of all existing sensations of light, should be produced when nothing determines to any particular colour. This average seems to resemble the middle form, which Sir JOSHUA REY-

NOLDS has elegantly insisted on in his discourses; so that perhaps some principles of beautiful contrast of colours may be drawn from hence, it being probable that those colours which together approach near to white light will have the most pleasing effect in apposition. It must be observed, that the sensation of light from pressure of the eye subsides almost instantly after the motion of pressure has ceased, so that the cause of the irritation of the retina is a change, and not a difference, of form; and therefore the sensation of light appears to depend immediately on a minute motion of some part of the optic nerve.

If the anterior part of the eye be repeatedly pressed, so as to occasion some degree of pain, and a continued pressure be then made on the sclerotica, while an interrupted pressure is made on the cornea; we shall frequently be able to observe an appearance of luminous lines, branched, and somewhat connected with each other, darting from every part of the field of view, towards a centre a little exterior and superior to the axis of the eye. This centre corresponds to the insertion of the optic nerve, and the appearance of lines is probably occasioned by that motion of the retina which is produced by the sudden return of the circulating fluid, into the veins accompanying the ramifications of the arteria centralis, after having been detained by the pressure which is now intermitted. As such an obstruction and such a re-admission must require particular circumstances, in order to be effected in a sensible degree, it may naturally be supposed that this experiment will not always easily succeed.

Fig. 1.

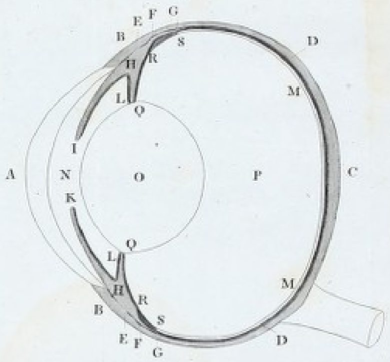

Fig. 2.

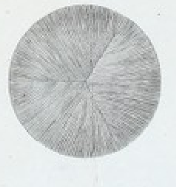

Fig. 3.

Explanation of the Figures.

Tab.XX. fig. 1. A vertical section of the ox's eye, of twice the natural size.

A. The cornea, covered by the tunica conjunctiva.

BCB. The sclerotica, covered at BB by the tunica albuginea, and tunica conjunctiva.

DD. The choroid, consisting of two laminas.

EE. The circle of adherence of the choroid and sclerotica.

FG, FG. The orbiculus ciliaris.

HI, HK. The uvea: its anterior surface the iris; its posterior surface lined with pigmentum nigrum.

IK. The pupil.

HL, HL. The ciliary processes, covered with pigmentum nigrum.

MM. The retina.

N. The aqueous humour.

O. The crystalline lens.

P. The vitreous humour.

QR, QR. The zona ciliaris.

RS, RS. The annulus mucosus.

Fig. 2. The structure of the crystalline lens, as viewed in front.

Fig. 3. A side view of the crystalline.

XVII. *Observations on a Current that often prevails to the Westward of* Scilly; *endangering the Safety of Ships that approach the* British *Channel. By* James Rennell, *Esq. F.R.S.*

Read June 6, 1793.

It is a circumstance well known to seamen, that ships, in coming from the Atlantic, and steering a course for the British channel, in a parallel somewhat to the *south* of the Scilly Islands; do, notwithstanding, often find themselves to the *north* of those islands: or, in other words, in the mouth of the St. George's, or of the Bristol channel. This extraordinary error has passed for the effects, either of bad steerage, bad observations of latitude, or the indraught of the Bristol channel: but none of these account for it satisfactorily; because, admitting that at times there may be an indraught, it cannot be supposed to extend to Scilly; and the case has happened in weather the most favourable for navigating, and for taking observations. The consequences of this deviation from the intended track, have very often been fatal: particularly in the loss of the Nancy packet, in our own times; and that of Sir Cloudesley Shovel, and others of his fleet, at the beginning of the present century. Numbers of cases, equally melancholy, but of less celebrity, have occurred; and many others, in which the danger has been imminent, but not fatal, have scarcely reached the public ear. All of these have been

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LONDON,

PRINTED BY W. BULMER AND CO. CLEVELAND-ROW, ST. JAMES'S;
AND SOLD BY PETER ELMSLY,
PRINTER TO THE ROYAL SOCIETY.
MDCCC.

of this Sloth, whereas in the same part of the *tridactylus* there were at least forty.

c, part of the rim of the pelvis.

Fig. 5, the upper limb of the *Lemur Loris*.

a, the head of the os brachii.

b, the axillary artery, proceeding along the arm, and dividing into seven or eight cylinders.

Fig. 6, the inguinal arteries of the *Lemur Loris.*

a, the iliac artery, dividing as it passes the groin into five or six cylinders.

b, the bony margin of the pelvis.

The figures are of the size of the different natural objects.

VII. *Outlines of Experiments and Inquiries respecting Sound and Light. By* Thomas Young, *M. D. F. R. S. In a Letter to* Edward Whitaker Gray, *M. D. Sec. R. S.*

Read January 16, 1800.

DEAR SIR,

It has long been my intention to lay before the Royal Society a few observations on the subject of sound; and I have endeavoured to collect as much information, and to make as many experiments, connected with this inquiry, as circumstances enabled me to do; but, the further I have proceeded, the more widely the prospect of what lay before me has been extended; and, as I find that the investigation, in all its magnitude, will occupy the leisure hours of some years, or perhaps of a life, I am determined, in the mean time, lest any unforeseen circumstances should prevent my continuing the pursuit, to submit to the Society some conclusions which I have already formed from the results of various experiments. Their subjects are, I. The measurement of the quantity of air discharged through an aperture. II. The determination of the direction and velocity of a stream of air proceeding from an orifice. III. Ocular evidence of the nature of sound. IV. The velocity of sound. V. Sonorous cavities. VI. The degree of divergence of sound. VII. The decay of sound. VIII. The harmonic sounds of pipes. IX. The vibrations of different elastic fluids. X. The analogy between light and sound. XI. The coalescence of musical sounds. XII.

The frequency of vibrations constituting a given note. XIII. The vibrations of chords. XIV. The vibrations of rods and plates. XV. The human voice. XVI. The temperament of musical intervals.

I. *Of the Quantity of Air discharged through an Aperture.*

A piece of bladder was tied over the end of the tube of a large glass funnel, and punctured with a hot needle. The funnel was inverted in a vessel of water; and a gage, with a graduated glass tube, was so placed as to measure the pressure occasioned by the different levels of the surfaces of the water. As the air escaped through the puncture, it was supplied by a phial of known dimensions, at equal intervals of time; and, according to the frequency of this supply, the average height of the gage was such as is expressed in the first Table. It appears, that the quantity of air discharged by a given aperture, was nearly in the subduplicate ratio of the pressure; and that the ratio of the expenditures by different apertures, with the same pressure, lay between the ratio of their diameters and that of their areas. The second, third, and fourth Tables show the result of similar experiments, made with some variations in the apparatus. It may be inferred, from comparing the experiments on a tube with those on a simple perforation, that the expenditure is increased, as in water, by the application of a short pipe.

Table I.

A	B	C
.00018	.25	3.9
.00018	.58	11.7
.00018	1.	15.6
.001	.045	7.8
.001	.2	15.6
.001	.7	31.2
.004	.35	46.8

A is the area, in square inches, of an aperture nearly circular. B, the pressure in inches. C, the number of cubic inches discharged in one minute.

All numbers throughout this paper, where the contrary is not expressed, are to be understood of inches, linear, square, or cubic.

Table II.

A	B	C
.07	1.	2000.
.07	2.	2900.

A is the area of the section of a tube about two inches long. B, the pressure. C, the quantity of air discharged in a minute, by estimation.

Table III.

A	B	C	D
.0064	1.15	.2	46.8
.0064	10.	.45	46.8
.0064	13.5	.35	31.2
.0064	13.5	.7	46.8

A is the area of the section of a tube. B, its length. C, the pressure. D, the discharge in a minute.

Table IV.

A	B	C
.003	.28.	46.8

A is the area of an oval aperture, formed by flattening a glass tube at the end: its diameters were .025 and .152. B, the pressure. C, the discharge.

II. *Of the Direction and Velocity of a Stream of Air.*

An apparatus was contrived for measuring, by means of a water-gage communicating with a reservoir of air, the pressure by which a current was forced from the reservoir through a cylindrical tube; and the gage was so sensible, that, a regular blast being supplied from the lungs, it showed the slight variation produced by every pulsation of the heart. The current of air issuing from the tube was directed downwards, upon a white plate, on which a scale of equal parts was engraved, and which was thinly covered with a coloured liquid; the breadth of the surface of the plate laid bare was observed at different distances from the tube, and with different degrees of pressure, care being taken that the liquid should be so shallow as to yield to the slightest impression of air. The results are collected in Tables V. and VI. and are exhibited to the eye in Plate III. Figs. 1—12. In order to measure with greater certainty and precision, the velocity of every part of the current, a second cavity, furnished with a gage, was provided, and pieces perforated with apertures of different sizes were adapted to its orifice: the axis of the current was directed as accurately as possible to the centres of these apertures, and the result of the experiments, with various pressures and distances, are inserted in Tables VII. VIII. and IX. The velocity of a stream being, both according to the commonly received opinion and to the experiments already related, nearly in the subduplicate ratio of the pressure occasioning it, it was inferred, that an equal pressure would be required to stop its progress, and that the velocity of the current, where it struck against the aperture, must be in the subduplicate ratio of the pressure marked by the gage. The ordinates of the curves in Figs. 13—23, were therefore taken reciprocally in the subduplicate ratio of the pressure marked by the second gage to that indicated by the first, at the various distances represented by the abscisses. Each figure represents a different degree of pressure in the first cavity. The curve nearest the axis, is deduced from observations in which the aperture opposed to the tube was not greater than that of the tube itself; and shows what would be the diameter of the current, if the velocities of every one of its particles in the same circular section, including those of the contiguous air, which must have acquired as much motion as the current has lost, were equal among themselves. As the central particles must be supposed to be less impeded in their motion than the superficial ones, of course, the smaller the aperture opposed to the centre of the current, the greater the velocity ought to come out, and the ordinate of the curve the smaller; but, where the aperture was not greater than that of the tube, the difference of the velocities at the same distance was scarcely perceptible. When the aperture was larger than that of the tube, if the distance was very small, of course, the average velocity came out much smaller than that which was inferred from a smaller aperture; but, where the ordinate of the internal curve became nearly equal to this aperture, there was but little difference between the velocities indicated with different apertures. Indeed, in some cases, a larger aperture seemed to indicate a greater velocity: this might have arisen in some degree from the smaller aperture not having been exactly in the centre of the current; but there is greater reason to suppose, that it was occasioned by some resistance derived from the air returning between the sides of the aperture and the current entering it. Where this took place, the external curves, which are so constructed as that their ordinates are reciprocally in the subduplicate ratio of the pressure observed in the second cavity, with apertures equal in semidiameter to their initial ordinate, approach, for a short distance, nearer to the axis than the internal curve: after this, they continue their course very near to this curve. Hence it appears, that no observable part of the motion diverged beyond the limits of the solid which would be formed by the revolution of the internal curve, which is seldom inclined to the axis in an angle so great as ten degrees. A similar conclusion may be made, from observing the flame of a candle subjected to the action of a blowpipe: there is no divergency beyond the narrow limits of the current; the flame, on the contrary, is every where forced by the ambient air towards the current, to supply the place of that which it has carried away by its friction. The lateral communication of motion, very ingeniously and accurately observed in water by Professor VENTURI, is exactly similar to the motion here shown to take place in air; and these experiments fully justify him in rejecting the tenacity of water as its cause: no doubt it arises from the relative situation of the particles of the fluid, in the line of the current, to that of the particles in the contiguous strata, which is such as naturally to lead to a communication of motion nearly in a parallel direction; and this may properly be termed friction. The lateral pressure which urges the flame of a candle towards the stream of air from a blowpipe, is probably exactly similar to that pressure which causes the inflection of a current of air near an obstacle. Mark the dimple which a slender stream of air makes on the surface of water; bring a convex body into contact with the side of the stream, and the place of the dimple will immediately show that the current is inflected towards the body; and, if the body be at liberty to move in every direction, it will be urged towards the current, in the same manner as, in VENTURI's experiments, a fluid was forced up a tube inserted into the side of a pipe through which water was flowing. A similar interposition of an obstacle in the course of the wind, is probably often the cause of smoky chimneys. One circumstance was observed in these experiments, which it is extremely difficult to explain, and which yet leads to very important consequences: it may be made distinctly perceptible to the eye, by forcing a current of smoke very gently through a fine tube. When the velocity is as small as possible, the stream proceeds for many inches without any observable dilatation; it then immediately diverges at a considerable angle into a cone, Plate IV. Fig. 24; and, at the point of divergency, there is an audible and even visible vibration. The blowpipe also affords a method of observing this phænomenon: as far as can be judged from the motion of the flame, the current seems to make something like a revolution in the surface of the cone, but this motion is too rapid to be distinctly discerned. When the pressure is increased, the apex of the cone approaches nearer to the orifice of the tube, Figs. 25, 26; but no degree of pressure seems materially to alter its divergency. The distance of the apex from the orifice, is not proportional to the diameter of the current; it rather appears to be the greater the smaller the current, and is much better defined in a small current than in a large one. Its distance in one experiment is expressed in Table x, from observations on the surface of a liquid; in other experiments, its respective distances were sometimes considerably less with the same degrees of pressure. It may be inferred, from the numbers of Tables VII and VIII, that in several instances a greater height of the first gage produced a less height of the second: this arose from the nearer approach of the apex of the cone to the orifice of the tube, the stream losing a greater portion of its velocity by this divergence than it gained by the increase of pressure. At first sight, the form of the current bears some resemblance to the *vena contracta* of a jet of water: but VENTURI has observed, that in water an increase of pressure increases, instead of diminishing, the distance of the contracted section from the orifice. Is it not possible, that the facility with which some spiders are said to project their fine threads to a great distance, may depend upon the small degree of velocity with which they are thrown out, so that, like a minute current, meeting with little interruption from the neighbouring air, they easily continue their course for a considerable time?

Table v.

A	1.	2.	3.	3.8
B	C	C	C	C
1.	.1	.1	.1	
2.	.12	.12	.2	
3.	.17	.25	.3	
4.	.2	.4	.4	
5.	.25	.5		
6.	.30	.52		
7.	.35	.54	.5	
8.	.37	.56		
9.	.39	.58		
10.	.40	.6	.6	.5
15.		.7		
18.	.50			
20.				

The diameter of the tube .07. A is the distance of the liquid from the orifice. B, the pressure. C, the diameter of the surface of the liquid displaced.

Table VI.

A	1.	2.
B	C	C
1.	.1	.1
2.	.13	
3.	.2	.2
4.	.25	.3
6.	.3	.4
7.	.35	.5
10.	.35	.6
15.	.35	.7
20.	.35	.7

Diameter of the tube, .1. A, B, and C, as in Table V.

Table VII.

A	.5	
B	.06	.15
C	D	D
.1	.083	
.2	.16	
.3	.25	.1
.4	.35	
.5	.45	
.6	.53	.2
.7	.6	
.8		.3
1.	.5	
1.2	.4	.4
1.5	.6	
2.	.67	.55
4.	1.3	1.
8.		2.
9.	.3	
14.	.5	

Diameter of the tube .06. A is the distance of the opposite aperture, from the orifice of the tube. B, the diameter of the aperture. C, the pressure, indicated by the first gage. D, the height of the second gage.

Table VIII.

A	.5				1.				2.				4.			
B	.06	.15	.3	.5	.06	.15	.3	.5	.06	.15	.3	5.	.06	.15	.3	.5
C	D	D	D	D	D	D	D	D	D	D	D	D	D	D	D	D
.1	.05	.05			.03				.017							
.2	.1	.1			.12	.08	.02		034							
.5	.2	.22			.1	.00			00							
1.	.32	.36	.1		.17	.1	.1	.05	.04							
2.	.52	.6	.2		.28	.22	.21	.08	.07							
3.	.8	.9	.3		.4	.36	.32	.12	.12		.1	.1				
4.	1.1	1.2	.4		.58	.52	.42	.16	.18		.15	.14				
5.		1.5	.5		.8	.68	.52	.2	.23		.2	.18	.04		.04	.05
6.		1.7	.6		1.	.83	.63	.25	.3		.25	.22	.05		.05	.06
7.		1.9	.7		1.2	1.	.75	.3	.35		.3	.26	.06		.06	.07
8.		2.1	.8		1.5	1.2	.88	.34	.4		.34	.3	.07		.07	.07
9.		2.3	.9		1.7	1.4	1.	.37	.45		.37	.34	.08		.08	.08
10.		2.6	1.		1.9	1.6	1.1	.4	.5		.4	.37	.09		.09	.09

Diameter of the tube .1. A, B, C, and D, as in Table VII.

Table IX.

A	1.15				3.3			4.
B	.15	.3	.5	1.	.06	.15	1.	.06
C	D	D	D	D	D	D	D	D
.5	.1	.1	.1					
1.	.2	.2	.2					
2.	.4	.35	.34	.13	.1	.1	.125	
3.	.6	.5	.5	.2	.15	.15	.18	.1

Diameter of the tube .3.

A, B, C, and D, as in Table VII.

Table X.

A	B
.4	6.
.8	3.
1.2	1.5
1.8	1.
2.	.5
4.	.0

A is the pressure, B, the distance of the apex of the cone from the orifice of a tube .1 in diameter.

III. *Ocular Evidence of the Nature of Sound.*

A tube about the tenth of an inch in diameter, with a lateral orifice half an inch from its end, filed rather deeper than the axis of the tube, Fig. 27, was inserted at the apex of a conical cavity containing about twenty cubic inches of air, and luted perfectly tight: by blowing through the tube, a sound nearly in unison with the tenor C was produced. By gradually increasing the capacity of the cavity as far as several gallons, with the same mouth-piece, the sound, although faint, became more and more grave, till it was no longer a musical note. Even before this period a kind of trembling was distinguishable; and this, as the cavity was still further increased, was changed into a succession of distinct puffs, like the sound produced by an explosion of air from the lips; as slow, in some instances, as 4 or 3 in a second. These were undoubtedly the single vibrations, which, when repeated with sufficient frequency, impress on the auditory nerve the sensation of a continued sound. On forcing a current of smoke through the tube, the vibratory motion of the stream, as it passed out at the lateral orifice, was evident to the eye; although, from various circumstances, the quantity and direction of its motion could not be subjected to exact mensuration. This species of sonorous cavity seems susceptible of but few harmonic sounds. It was observed, that a faint blast produced a much greater frequency of vibrations than that which was appropriate to the cavity: a circumstance similar to this obtains also in large organ pipes; but, several minute observations of this kind, although they might assist in forming a theory of the origin of vibrations, or in confirming such a theory drawn from other sources, yet, as they are not alone sufficient to afford any general conclusions, are omitted at present, for the sake of brevity.

IV. *Of the Velocity of Sound.*

It has been demonstrated, by M. De la Grange and others, that any impression whatever communicated to one particle of an elastic fluid, will be transmitted through that fluid with an uniform velocity, depending on the constitution of the fluid, without reference to any supposed laws of the continuation of that impression. Their theorem for ascertaining this velocity is the same as Newton has deduced from the hypothesis of a particular law of continuation: but it must be confessed, that the result differs somewhat too widely from experiment, to give us full confidence in the perfection of the theory. Corrected by the experiments of various observers, the velocity of any impression transmitted by the common air, may, at an average, be reckoned 1130 feet in a second.

V. *Of sonorous Cavities.*

M. De la Grange has also demonstrated, that all impressions are reflected by an obstacle terminating an elastic fluid, with the same velocity with which they arrived at that obstacle.

When the walls of a passage, or of an unfurnished room, are smooth and perfectly parallel, any explosion, or a stamping with the foot, communicates an impression to the air, which is reflected from one wall to the other, and from the second again towards the ear, nearly in the same direction with the primitive impulse: this takes place as frequently in a second, as double the breadth of the passage is contained in 1130 feet; and the ear receives a perception of a musical sound, thus determined in its pitch by the breadth of the passage. On making the experiment, the result will be found accurately to agree with this explanation. If the sound is predetermined, and the frequency of vibrations such as that each pulse, when doubly reflected, may coincide with the subsequent pulse proceeding directly from the sounding body, the intensity of the sound will be much increased by the reflection; and also, in a less degree, if the reflected pulse coincides with the next but one, the next but two, or more, of the direct pulses. The appropriate notes of a room may readily be discovered by singing the scale in it; and they will be found to depend on the proportion of its length or breadth to 1130 feet. The sound of the stopped diapason pipes of an organ is produced in a manner somewhat similar to the note from an explosion in a passage; and that of its reed pipes to the resonance of the voice in a room: the length of the pipe in one case determining the sound, in the other, increasing its strength. The frequency of the vibrations does not at all immediately depend on the diameter of the pipe. It must be confessed, that much remains to be done in explaining the precise manner in which the vibration of the air in an organ pipe is generated. M. DANIEL BERNOULLI has solved several difficult problems relating to the subject; yet some of his assumptions are not only gratuitous, but contrary to matter of fact.

VI. *Of the Divergence of Sound.*

It has been generally asserted, chiefly on the authority of NEWTON, that if any sound be admitted through an aperture into a chamber, it will diverge from that aperture equally in all directions. The chief arguments in favour of this opinion are deduced from considering the phænomena of the pressure of fluids, and the motion of waves excited in a pool of water. But the inference seems to be too hastily drawn: there is a very material difference between impulse and pressure; and, in the case of waves of water, the moving force at each point is the power of gravity, which, acting primarily in a perpendicular direction, is only secondarily converted into a horizontal force, in the direction of the progress of the waves, being at each step disposed to spread equally in every direction: but the impulse transmitted by an elastic fluid, acts primarily in the direction of its progress. It is well known, that if a person calls to another with a speaking trumpet, he points it towards the place where his hearer stands: and I am assured by a very respectable Member of the Royal Society, that the report of a cannon appears many times louder to a person towards whom it is fired, than to one placed in a contrary direction. It must have occurred to every one's observation, that a sound such as that of a mill, or a fall of water, has appeared much louder after turning a corner, when the house or other obstacle no longer intervened; and it has been already remarked by EULER, on this head, that we are not acquainted with any substance perfectly impervious to sound. Indeed, as M. LAMBERT has very truly asserted, the whole theory of the speaking trumpet, supported as it is by practical experience, would fall to the ground, if it were demonstrable that sound spreads equally in every direction. In windy weather it may often be observed, that the sound of a distant bell varies almost instantaneously in its strength, so as to appear at least twice as remote at one time as at another; an observation which has also occurred to another gentleman, who is uncommonly accurate in examining the phænomena of nature. Now, if sound diverged equally in all directions, the variation produced by the wind could never exceed one-tenth of the apparent distance: but, on the supposition of a motion nearly rectilinear, it may easily happen that a slight change in the direction of the wind, may convey the sound, either directly or after reflection, in very different degrees of strength, to the same spot. From the experiments on the motion of a current of air, already related, it would be expected that a sound, admitted at a considerable distance from its origin through an aperture, would proceed, with an almost imperceptible increase of divergence, in the same direction; for, the actual velocity of the particles of air, in the strongest sound, is incomparably less than that of the slowest of the currents in the experiments related, where the beginning of the conical divergence took place at the greatest distance. Dr. MATTHEW YOUNG has objected, not without reason, to M. HUBE, that the existence of a condensation will cause a divergence in sound: but a much greater degree of condensation must have existed in the currents described than in any sound. There is indeed one difference between a stream of air and a sound; that, in sound, the motions of different particles of air are not synchronous: but it is not demonstrable that this circumstance would affect the divergency of the motion, except at the instant of its commencement, and perhaps not even then in a material degree; for, in general, the motion is communicated with a very gradual increase of intensity. The subject, however, deserves a more particular investigation; and, in order to obtain a more solid foundation for the argument, it is proposed, as soon as circumstances permit, to institute a course of experiments for ascertaining, as accurately as possible, the different strength of a sound once projected in a given direction, at different distances from the axis of its motion.

VII. *Of the Decay of Sound.*

Various opinions have been entertained respecting the decay of sound. M. DE LA GRANGE has published a calculation, by which its force is shown to decay nearly in the simple ratio of the distances; and M. DANIEL BERNOULLI's equations for the sounds of conical pipes lead to a similar conclusion. The same inference would follow from a completion of the reasoning of Dr. HELSHAM, Dr. MATTHEW YOUNG, and Professor VENTURI. It has been very elegantly demonstrated by MACLAURIN, and may also be proved in a much more simple manner, that when motion is communicated through a series of elastic bodies increasing in magnitude, if the number of bodies be supposed infinitely great, and their difference infinitely small, the motion of the last will be to that of the first in the subduplicate ratio of their respective magnitudes; and since, in the case of concentric spherical laminæ of air, the bulk increases in the duplicate ratio of the distance, the motion will in this case be directly, and the velocity inversely, as the distance. But, however true this may be of the first impulse, it will appear, by pursuing the calculation a little further, that every one of the elastic bodies, except the last, receives an impulse in a retrograde direction, which ultimately impedes the effect of the succeeding impulse, as much as a similar cause promoted that of the preceding one: and thus, as sound must be conceived to consist of an infinite number of impulses, the motion of the last lamina will be precisely equal to that of the first; and, as far as this mode of reasoning goes, sound must decay in the duplicate ratio of the distance. Hence it appears, that the proposal for adopting the logarithmic curve for the form of the speaking trumpet, was founded on fallacious reasoning. The calculation of M. De la Grange is left for future examination; and it is intended, in the mean time, to attempt to ascertain the decay of sound as nearly as possible by experiment: should the result favour the conclusions from that calculation, it would establish a marked difference between the propagation of sound and of light.

VIII. *Of the harmonic Sounds of Pipes.*

In order to ascertain the velocity with which organ pipes of different lengths require to be supplied with air, according to the various appropriate sounds which they produce, a set of experiments was made, with the same mouth-piece, on pipes of the same bore, and of different lengths, both stopped and open. The general result was, that a similar blast produced as nearly the same sound as the length of the pipes would permit; or at least that the exceptions, though very numerous, lay equally on each side of this conclusion. The particular results are expressed in Table XI. and in Plate IV. Fig. 28. They explain how a note may be made much louder on a wind instrument by a swell, than it can possibly be by a sudden impression of the blast. It is proposed, at a future time, to ascertain by experiment, the actual compression of the air within the pipe under different circumstances: from some very slight trials, it seemed to be nearly in the ratio of the frequency of vibrations of each harmonic.

Table XI.

OPEN.						STOPPED.					
A	B	C	D	E	F	A	B	C	D	E	F
4.5	4.1	0.7 8.8	8.8	$\overset{\equiv}{d}$*	1 2	4.5	1.2 5.0	0.3 1.7 9.0	1.8 10.0	$\overset{=}{d}$	1 3 5
9.4	0.8 2.0 5.0 16.5 19.0	0.3 8.0 18.0 20.0	0.9 8.0 18.0 20.0	$\overset{=}{f}$	1 2 3 4 5 6	9.4	1.1 7.0	0.2 0.45 1.6 8.0	0.4 1.6 8.5	$\overline{f}$	1 3 5 7
16.1	0.8 1.2 2.2 3.4 4.0 6.5	0.4 1.0 2.2 4.7 10.0	1.0 2.2 4.7 11.5 13.5 15.0	$\overset{=}{g}$*	2 3 4 5 6 7 8	16.1	0.6 0.9 1.6 2.5 6.0	0.4 0.65 1.1 2.4 4.8 7.0	0.6 1.1 2.4 4.9 9.0	$\overset{=}{d}$*	3 5 7 9 11 13
20.5	1.1 4.5	0.6 0.8 1.9 5.7	0.8 1.9 5.7	$\overline{b}$	3 4 5 8	20.5	1.0 1.8 3.2	0.8 1.1 3.8 12.	1.1 3.8 3.8 12.	$\overset{\equiv}{c}$* o	7 9 11 17 00

A, is the length of the pipe from the lateral orifice to the end. C, the pressure at which the sound began. B, its termination, by lessening the pressure; D, by increasing it. E, the note answering to the first sound of each pipe, according to the German method of notation. F, the number showing the place of each note in the regular series of harmonics. The diameter of the pipe was .35; the air duct of the mouth-piece measured, where smallest, .25 by .035; the lateral orifice .25 by .125. The apparatus was not calculated to apply a pressure of above 22 inches. Where no number stands under C, a sudden blast was required to produce the note.

IX. *Of the Vibrations of different elastic Fluids.*

All the methods of finding the velocity of sound, agree in determining it to be, in fluids of a given elasticity, reciprocally in the subduplicate ratio of the density: hence, in pure hydrogen gas it should be $\sqrt{13} = 3.6$ times as great as in common air; and the pitch of a pipe should be a minor fourteenth higher in this fluid than in the common air. It is therefore probable that the hydrogen gas used in Professor CHLADNI's late experiments, was not quite pure. It must be observed, that in an accurate experiment of this nature, the pressure causing the blast ought to be carefully ascertained. There can be no doubt but that, in the observations of the French Academicians on the velocity of sound, which appear to have been conducted with all possible attention, the dampness and coldness of the night air must have considerably increased its density: hence, the velocity was found to be only 1109 feet in a second; while DERHAM's experiments, which have an equal appearance of accuracy, make it amount to 1142. Perhaps the average may, as has been already mentioned, be safely estimated at 1130. It may here be remarked, that the well known elevation of the pitch of wind instruments, in the course of playing, sometimes amounting to half a note, is not, as is commonly supposed, owing to any expansion of the instrument, for this should produce a contrary effect, but to the increased warmth of the air in the tube. Dr. SMITH has made a similar observation, on the pitch of an organ in summer and winter, which he found to differ more than twice as much as the English and French experiments on the velocity of sound. BIANCONI found the velocity of sound, at Bologna, to differ at different times, in the ratio of 152 to 157.

X. *Of the Analogy between Light and Sound.*

Ever since the publication of Sir ISAAC NEWTON's incomparable writings, his doctrines of the emanation of particles of light from lucid substances, and of the formal pre-existence of coloured rays in white light, have been almost universally admitted in this country, and but little opposed in others. LEONARD EULER indeed, in several of his works, has advanced some powerful objections against them, but not sufficiently powerful to justify the dogmatical reprobation with which he treats them; and he has left that system of an ethereal vibration, which after HUYGENS and some others he adopted, equally liable to be attacked on many weak sides. Without pretending to decide positively on the controversy, it is conceived that some considerations may be brought forwards, which may tend to diminish the weight of objections to a theory similar to the HUYGENIAN. There are also one or two difficulties in the NEWTONIAN system, which have been little observed. The first is, the uniform velocity with which light is supposed to be projected from all luminous bodies, in consequence of heat, or otherwise. How happens it that, whether the projecting force is the slightest transmission of electricity, the friction of two pebbles, the lowest degree of visible ignition, the white heat of a wind furnace, or the intense heat of the sun itself, these wonderful corpuscles are always propelled with one uniform velocity? For, if they differed in velocity, that difference ought to produce a different refraction. But a still more insuperable difficulty seems to occur, in the partial reflection from every refracting surface. Why, of the same kind of rays, in every circumstance precisely similar, some should always be reflected, and others transmitted, appears in this system to be wholly inexplicable. That a medium resembling, in many properties, that which has been denominated ether, does really exist, is undeniably proved by the phænomena of electricity; and the arguments against the existence of such an ether throughout the universe, have been pretty sufficiently answered by Euler. The rapid transmission of the electrical shock, shows that the electric medium is possessed of an elasticity as great as is necessary to be supposed for the propagation of light. Whether the electric ether is to be considered as the same with the luminous ether, if such a fluid exists, may perhaps at some future time be discovered by experiment; hitherto I have not been able to observe that the refractive power of a fluid undergoes any change by electricity. The uniformity of the motion of light in the same medium, which is a difficulty in the Newtonian theory, favours the admission of the Huygenian; as all impressions are known to be transmitted through an elastic fluid with the same velocity. It has been already shown, that sound, in all probability, has very little tendency to diverge: in a medium so highly elastic as the luminous ether must be supposed to be, the tendency to diverge may be considered as infinitely small, and the grand objection to the system of vibration will be removed. It is not absolutely certain, that the white line visible in all directions on the edge of a knife, in the experiments of Newton and of Mr. Jordan, was not partly occasioned by the tendency of light to diverge. Euler's hypothesis, of the transmission of light by an agitation of the particles of the refracting media themselves, is liable to strong objections; according to this supposition, the refraction of the rays of light, on entering the atmosphere from the pure ether which he describes, ought to be a million times greater than it is. For explaining the phænomena of partial and total reflection, refraction, and inflection, nothing more is necessary than to suppose all refracting media to retain, by their attraction, a greater or less quantity of the luminous ether, so as to make its density greater than that which it possesses in a vacuum, without increasing its elasticity; and that light is a propagation of an impulse communicated to this ether by luminous bodies: whether this impulse is produced by a partial emanation of the ether, or by vibrations of the particles of the body, and whether these vibrations are, as EULER supposed, of various and irregular magnitudes, or whether they are uniform, and comparatively large, remains to be hereafter determined. Now, as the direction of an impulse transmitted through a fluid, depends on that of the particles in synchronous motion, to which it is always perpendicular, whatever alters the direction of the pulse, will inflect the ray of light. If a smaller elastic body strike against a larger one, it is well known that the smaller is reflected more or less powerfully, according to the difference of their magnitudes: thus, there is always a reflection when the rays of light pass from a rarer to a denser stratum of ether; and frequently an echo when a sound strikes against a cloud. A greater body striking a smaller one, propels it, without losing all its motion: thus, the particles of a denser stratum of ether, do not impart the whole of their motion to a rarer, but, in their effort to proceed, they are recalled by the attraction of the refracting substance with equal force; and thus a reflection is always secondarily produced, when the rays of light pass from a denser to a rarer stratum. Let AB, Plate V. Fig. 29, be a ray of light falling on the reflecting surface FG; *cd* the direction of the vibration, pulse, impression, or condensation. When *d* comes to H, the impression will be, either wholly or partly, reflected with the same velocity as it arrived, and EH will be equal to DH; the angle EIH to DIH or CIF; and the angle of reflection to that of incidence. Let FG, Fig. 30, be a refracting surface. The portion of the pulse IE, which is travelling through the refracting medium, will move with a greater or less velocity in the subduplicate ratio of the densities, and HE will be to KI in that ratio. But HE is, to the radius IH, the sine of the angle of refraction; and KI that of the angle of incidence. This explanation of refraction is nearly the same as that of EULER. The total reflection of a ray of light by a refracting surface, is explicable in the same manner as its simple refraction; HE, Fig. 31, being so much longer than KI, that the ray first becomes parallel to FG, and then, having to return through an equal diversity of media, is reflected in an equal angle. When a ray of light passes near an inflecting body, surrounded, as all bodies are supposed to be, with an atmosphere of ether denser than the ether of the ambient air, the part of the ray nearest the body is retarded, and of course the whole ray inflected towards the body, Fig. 32. The repulsion of inflected rays has been very ably controverted by Mr. JORDAN, the ingenious author of a late publication on the Inflection of Light. It has already been conjectured by EULER, that the colours of light consist in the different frequency of the vibrations of the luminous ether: it does not appear that he has supported this opinion by any argument; but it is strongly confirmed, by the analogy between the colours of a thin plate and the sounds of a series of organ pipes. The phænomena of the colours of thin plates require, in the NEWTONIAN system, a very complicated supposition, of an ether, anticipating by its motion the velocity of the corpuscles of light, and thus producing the fits of transmission and reflection; and even this supposition does not much assist the explanation. It appears, from the accurate analysis of the phænomena which NEWTON has given, and which has by no means been superseded by any later observations, that the same colour recurs whenever the thickness answers to the terms of an arithmetical progression. Now this is precisely similar to the production of the same sound, by means of an uniform blast, from organ-pipes which are different multiples of the same length. Supposing white light to be a continued impulse or stream of luminous ether, it may be conceived to act on the plates as a blast of air does on the organ-pipes, and to produce vibrations regulated in frequency by the length of the lines which are terminated by the two refracting surfaces. It may be objected that, to complete the analogy, there should be tubes, to answer to the organ-pipes: but the tube of an organ-pipe is only necessary to prevent the divergence of the impression, and in light there is little or no tendency to diverge; and indeed, in the case of a resonant passage, the air is not prevented from becoming sonorous by the liberty of lateral motion. It would seem, that the determination of a portion of the track of a ray of light through any homogeneous stratum of ether, is sufficient to establish a length as a basis for colorific vibrations. In inflections, the length of the track of a ray of light through the inflecting atmosphere may determine its vibrations: but, in this case, as it is probable that there is a reflection from every part of the surface of the surrounding atmosphere, contributing to the appearance of the white line in every direction, in the experiments already mentioned, so it is possible that there may be some second reflection at the immediate surface of the body itself, and that, by mutual reflections between these two surfaces, something like the anguiform motion suspected by Newton may really take place; and then the analogy to the colours of thin plates will be still stronger. A mixture of vibrations, of all possible frequencies, may easily destroy the peculiar nature of each, and concur in a general effect of white light. The greatest difficulty in this system is, to explain the different degree of refraction of differently coloured light, and the separation of white light in refraction: yet, considering how imperfect the theory of elastic fluids still remains, it cannot be expected that every circumstance should at once be clearly elucidated. It may hereafter be considered how far the excellent experiments of Count Rumford, which tend very greatly to weaken the evidence of the modern doctrine of heat, may be more or less favourable to one or the other system of light and colours. It does not appear that any comparative experiments have been made on the inflection of light by substances possessed of different refractive powers; undoubtedly some very interesting conclusions might be expected from the inquiry.

XI. *Of the Coalescence of musical Sounds.*

It is surprising that so great a mathematician as Dr. Smith could have entertained for a moment, an idea that the vibrations constituting different sounds should be able to cross each other in all directions, without affecting the same individual particles of air by their joint forces: undoubtedly they cross, without disturbing each other's progress; but this can be no otherwise effected than by each particle's partaking of both motions. If this assertion stood in need of any proof, it might be amply furnished by the phænomena of beats, and of the grave harmonics observed by ROMIEU and TARTINI; which M. DE LA GRANGE has already considered in the same point of view. In the first place, to simplify the statement, let us suppose, what probably never precisely happens, that the particles of air, in transmitting the pulses, proceed and return with uniform motions; and, in order to represent their position to the eye, let the uniform progress of time be represented by the increase of the absciss, and the distance of the particle from its original position, by the ordinate, Fig. 33—38. Then, by supposing any two or more vibrations in the same direction to be combined, the joint motion will be represented by the sum or difference of the ordinates. When two sounds are of equal strength, and nearly of the same pitch, as in Fig. 36, the joint vibration is alternately very weak and very strong, producing the effect denominated a beat, Plate VI. Fig. 43, B and C; which is slower and more marked, as the sounds approach nearer to each other in frequency of vibrations; and, of these beats there may happen to be several orders, according to the periodical approximations of the numbers expressing the proportions of the vibrations. The strength of the joint sound is double that of the simple sound only at the middle of the beat, but not throughout its duration; and it may be inferred, that the strength of sound in a concert will not be in exact proportion to the number of instruments composing it. Could any method be devised for ascertaining this by experiment, it would assist in the comparison of sound with light. In Plate V. Fig. 33, let P and Q be the middle points of the progress or regress of a particle in two successive compound vibrations; then, CP being = PD, KR = RN, GQ = QH, and MS = SO, twice their distance, 2 RS = 2 RN +

$2NM + 2MS = KN + NM + NM + MO = KM + NO$, is equal to the sum of the distances of the corresponding parts of the simple vibrations. For instance, if the two sounds be as 80 : 81, the joint vibration will be as 80.5; the arithmetical mean between the periods of the single vibrations. The greater the difference in the pitch of two sounds, the more rapid the beats, till at last, like the distinct puffs of air in the experiments already related, they communicate the idea of a continued sound; and this is the fundamental harmonic described by Tartini. For instance, in Plate V. Fig. 34—37, the vibrations of sounds related as 1 : 2, 4 : 5, 9 : 10, and 5 : 8, are represented; where the beats, if the sounds be not taken too grave, constitute a distinct sound, which corresponds with the time elapsing between two successive coincidences, or near approaches to coincidence: for, that such a tempered interval still produces a harmonic, appears from Plate V. Fig. 38. But, besides this primary harmonic, a secondary note is sometimes heard, where the intermediate compound vibrations occur at a certain interval, though interruptedly; for instance, in the coalescence of two sounds related to each other as 7 : 8, 5 : 7, or 4 : 5, there is a recurrence of a similar state of the joint motion, nearly at the interval of $\frac{5}{15}$, $\frac{4}{12}$, or $\frac{3}{9}$ of the whole period: hence, in the concord of a major third, the fourth below the key note is heard as distinctly as the double octave, as is seen in some degree in Plate V. Fig. 35; AB being nearly two-thirds of CD. The same sound is sometimes produced by taking the minor sixth below the key note; probably because this sixth, like every other note, is almost always attended by an octave, as a harmonic. If the angles of all the figures resulting from the motion thus assumed be rounded off, they will approach more nearly to a representation of the actual circumstances; but, as the laws by which the motion of the particles of air is regulated, differ according to the different origin and nature of the sound, it is impossible to adapt a demonstration to them all: if, however, the particles be supposed to follow the law of the harmonic curve, derived from uniform circular motion, the compound vibration will be the harmonic instead of the arithmetical mean; and the secondary sound of the interrupted vibrations will be more accurately formed, and more strongly marked, Plate VI. Figs. 41, 42: the demonstration is deducible from the properties of the circle. It is remarkable, that the law by which the motion of the particles is governed, is capable of some singular alterations by a combination of vibrations. By adding to a given sound other similar sounds, related to it in frequency as the series of odd numbers, and in strength inversely in the same ratios, the right lines indicating an uniform motion may be converted very nearly into figures of sines, and the figures of sines into right lines, as in Plate V. Figs. 39, 40.

XII. *Of the Frequency of Vibrations constituting a given Note.*

The number of vibrations performed by a given sound in a second, has been variously ascertained; first, by SAUVEUR, by a very ingenious inference from the beats of two sounds; and since, by the same observer and several others, by calculation from the weight and tension of a chord. It was thought worth while, as a confirmation, to make an experiment suggested, but coarsely conducted, by MERSENNUS, on a chord 200 inches in length, stretched so loosely as to have its single vibrations visible; and, by holding a quill nearly in contact with the chord, they were made audible, and were found, in one experiment, to recur 8.3 times in a second. By lightly pressing the chord at one-eighth of its length from the end, and at other shorter aliquot distances, the fundamental note was found to be one-sixth of a tone higher than the respective octave of a tuning-fork marked C: hence, the fork was a comma and a half above the pitch assumed by SAUVEUR, of an imaginary C, consisting of one vibration in a second.

XIII. *Of the Vibrations of Chords.*

By a singular oversight in the demonstration of Dr. BROOK TAYLOR, adopted as it has been by a number of later authors, it is asserted, that if a chord be once inflected into any other form than that of the harmonic curve, it will, since those parts which are without this figure are impelled towards it by an excess of force, and those within it by a deficiency, in a very short time arrive at or very near the form of this precise curve. It would be easy to prove, if this reasoning were allowed, that the form of the curve can be no other than that of the axis, since the tending force is continually impelling the chord towards this line. The case is very similar to that of the NEWTONIAN proposition respecting sound. It may be proved, that every impulse is communicated along a tended chord with an uniform velocity; and this velocity is the same which is inferred from Dr. TAYLOR'S theorem; just as that of sound, determined by other methods, coincides with the NEWTONIAN result. But, although several late mathematicians have given admirable solutions of all possible cases of the problem, yet it has still been supposed, that the distinctions were too minute to be actually observed; especially, as it might have been added, since the inflexibility of a wire would dispose it, according to the doctrine of elastic rods, to assume the form of the harmonic curve. The theorem of EULER and DE LA GRANGE, in the case where the chord is supposed to be at first at rest, is in effect this: continue the figure each way, alternately on different sides of the axis, and in contrary positions; then, from any point of the curve, take an absciss each way, in the same proportion to the length of the chord as any given portion of time bears to the time of one semivibration, and the half sum of the ordinates will be the distance of that point of the chord from the axis, at the expiration of the time given. If the initial figure of the chord be composed of two right lines, as generally happens in musical instruments and experiments, its successive forms will be such as are represented in Plate VI. Figs. 47, 48: and this result is fully confirmed by experiment. Take one of the lowest strings of a square piano forte, round which a fine silvered wire is wound in a spiral form; contract the light of a window, so that, when the eye is placed in a proper position, the image of the light may appear small, bright, and well defined, on each of the convolutions of the wire. Let the chord be now made to vibrate, and the luminous point will delineate its path, like a burning coal whirled round, and will present to the eye a line of light, which, by the assistance of a microscope, may be very accurately observed. According to the different ways by which the wire is put in motion, the form of this path is no less diversified and amusing, than the multifarious forms of the quiescent lines of vibrating plates, discovered by Professor CHLADNI; and is indeed in one respect even more interesting, as it appears to be more within the reach of mathematical calculation to determine it; although hitherto, excepting some slight observations of BUSSE and CHLADNI, principally on the motion of rods, nothing has been attempted on the subject. For the present purpose, the motion of the chord may be simplified, by tying a long fine thread to any part of it, and fixing this thread in a direction perpendicular to that of the chord, without drawing it so tight as to increase the tension: by these means, the vibrations are confined nearly to one plane, which scarcely ever happens when the chord vibrates at liberty. If the chord be now inflected in the middle, it will be found, by comparison with an object which marked its quiescent position, to make equal excursions on each side of the axis; and the figure which it apparently occupies will be terminated by two lines, the more luminous as they are nearer the ends, Plate VI. Fig. 49. But, if the chord be inflected near one of its extremities, Fig. 50, it will proceed but a very small distance on the opposite side of the axis, and will there form a very bright line, indicating its longer continuance in that place; yet it will return on the former side nearly to the point from whence it was let go, but will be there very faintly visible, on account of its short delay. In the middle of the chord, the excursions on each side the axis are always equal; and, beyond the middle, the same circumstances take place as in the half where it was inflected, but on the opposite side of the axis; and this appearance continues unaltered in its proportions, as long as the chord vibrates at all: fully confirming the non-existence of the harmonic curve, and the accuracy of the construction of EULER and DE LA GRANGE. At the same time, as M. BERNOULLI has justly observed, since every figure may be infinitely approximated, by considering its ordinates as composed of the ordinates of an infinite number of trochoids of different magnitudes, it may be demonstrated, that all these constituent curves would revert to their initial state, in the same time that a similar chord bent into a trochoidal curve would perform a single vibration; and this is in some respects a convenient and compendious method of considering the problem. But, when a chord vibrates freely, it never remains long in motion, without a very evident departure from the plane of the vibration; and, whether from the original obliquity of the impulse, or from an interference with the reflected vibrations of the air, or from the inequability of its own weight or flexibility, or from the immediate resistance of the particles of air in contact with it, it is thrown into a very evident rotatory motion, more or less simple and uniform according to circumstances. Some specimens of the figures of the orbits of chords are exhibited in Plate VI. Fig. 44. At the middle of the chord, its orbit has always two equal halves, but seldom at any other point. The curves of Fig. 46, are described by combining together various circular motions, supposed to be performed in aliquot parts of the primitive orbit: and some of them approach nearly to the figures actually observed. When the chord is of unequal thickness, or when it is loosely tended and forcibly inflected, the apsides and double points of the orbits have a very evident rotatory motion. The compound rotations seem to demonstrate to the eye the existence of secondary vibrations, and to account for the acute harmonic sounds which generally attend the fundamental sound. There is one fact respecting these secondary notes, which seems intirely to have escaped observation. If a chord be inflected at one-half, one-third, or any other aliquot part of its length, and then suddenly left at liberty, the harmonic note which would be produced by dividing the chord at that point is intirely lost, and is not to be distinguished during any part of the continuance of the sound. This demonstrates, that the secondary notes do not depend upon any interference of the vibrations of the air with each other, nor upon any sympathetic agitation of auditory fibres, nor upon any effect of reflected sound upon the chord, but merely upon its initial figure and motion. If it were supposed that the chord, when inflected into right lines, resolved itself necessarily into a number of secondary vibrations, according to some curves which, when properly combined, would approximate to the figure given, the supposition would indeed in some respects correspond with the phænomenon related; as the coefficients of all the curves supposed to end at the angle of inflection would vanish. But, whether we trace the constituent curves of such a figure through the various stages of their vibrations, or whether we follow the more compendious method of EULER to the same purpose, the figures resulting from this series of vibrations are in fact so simple, that it seems inconceivable how the ear should deduce the complicated idea of a number of heterogeneous vibrations, from a motion of the particles of air which must be extremely regular, and almost uniform; an uniformity which, when proper precautions are taken, is not contradicted by examining the motion of the chord with the assistance of a powerful magnifier. This difficulty occurred very strongly to EULER; and DE LA GRANGE even suspects some fallacy in the experiment, and that a musical ear judges from previous association. But, besides that these sounds are discoverable to an ear destitute of such associations, and, when the sound is produced by two strings in imperfect unison, may be verified by counting the number of their beats, the experiment already related is an undeniable proof that no fallacy of this kind exists. It must be confessed, that nothing fully satisfactory has yet occurred to account for the phænomena; but it is highly probable that the slight increase of tension produced by flexure, which is omitted in the calculations, and the unavoidable inequality of thickness or flexibility of different parts of the same chord, may, by disturbing the isochronism of the subordinate vibrations, cause all that variety of sounds which is so inexplicable without them. For, when the slightest difference is introduced in the periods, there is no difficulty in conceiving how the sounds may be distinguished; and indeed, in some cases, a nice ear will discover a slight imperfection in the tune of harmonic notes: it is also often observed, in tuning an instrument, that some of the single chords produce beating sounds, which undoubtedly arise from their want of perfect uniformity. It may be perceived that any particular harmonic is loudest, when the chord is inflected at about one-third of the corresponding aliquot part from one of the extremities of that part. An observation of Dr. WALLIS seems to have passed unnoticed by later writers on harmonics. If the string of a violin be struck in the middle, or at any other aliquot part, it will give either no sound at all, or a very obscure one. This is true, not of inflection, but of the motion communicated by a bow; and may be explained from the circumstance of the successive impulses, reflected from the fixed points at each end, destroying each other: an explanation nearly analogous to some observations of Dr. MATTHEW YOUNG on the motion of chords. When the bow is applied not exactly at the aliquot point, but very near it, the corresponding harmonic is extremely loud: and the fundamental note, especially in the lowest harmonics, scarcely audible: the chord assumes the appearance, at the aliquot points, of as many lucid lines as correspond to the number of the harmonic, more nearly approaching to each other as the bow approaches more nearly to the point, Plate VI. Fig. 51. According to the various modes of applying the bow, an immense variety of figures of the orbits are produced, Fig. 45, more than enough to account for all the difference of tone in different performers. In observations of this kind, a series of harmonics is frequently heard in drawing the bow across the same part of the chord: these are produced by the bow; they are however not proportionate to the whole length of the bow, but depend on the capability of the portion of the bowstring, intercepted between its end and the chord, of performing its vibrations in times which are aliquot parts of the vibration of the chord: hence it would seem, that the bow takes effect on the chord but at one instant during each fundamental vibration. In these experiments, the bow was strung with the second string of a violin: and, in the preparatory application of resin, the longitudinal sound of CHLADNI was sometimes heard; but it was observed to differ at least a note in different parts of the string.

XIV. *Of the Vibrations of Rods and Plates.*

Some experiments were made, with the assistance of a most excellent practical musician, on the various notes produced by a glass tube, an iron rod, and a wooden ruler; and, in a case where the tube was as much at liberty as possible, all the harmonics corresponding to the numbers from 1 to 13, were distinctly observed; several of them at the same time, and others by means of different blows. This result seems to differ from the calculations of EULER and Count RICCATI, confirmed as they are by the repeated experiments of Professor CHLADNI; it is not therefore brought forward as sufficiently controverting those calculations, but as showing the necessity of a revision of the experiments. Scarcely any note could ever be heard when a rod was loosely held at its extremity; nor when it was held in the middle, and struck one-seventh of the length from one end. The very ingenious method of Professor CHLADNI, of observing the vibrations of plates by strewing fine sand over them, and discovering the quiescent lines by the figures into which it is thrown, has hitherto been little known in this country: his treatise on the phænomena is so complete, that no other experiments of the kind were thought necessary. Glass vessels of various descriptions, whether made to sound by percussion or friction, were found to be almost intirely free from harmonic notes; and this observation coincides with the experiments of CHLADNI.

XV. *Of the human Voice.*

The human voice, which was the object originally proposed to be illustrated by these researches, is of so complicated a nature, and so imperfectly understood, that it can be on this occasion but superficially considered. No person, unless we except M. FERREIN, has published any thing very important on the subject of the formation of the voice, before or since DODART; his reasoning has fully shown the analogy between the voice and the *voix humaine* and regal organ-pipes: but his comparison with the whistle is unfortunate; nor is he more happy in his account of the falsetto. A kind of experimental analysis of the voice may be thus exhibited. By drawing in the breath, and at the same time properly contracting the larynx, a slow vibration of the ligaments of the glottis may be produced, making a distinct clicking sound:

upon increasing the tension, and the velocity of the breath, this clicking is lost, and the sound becomes continuous, but of an extremely grave pitch: it may, by a good ear, be distinguished two octaves below the lowest A of a common bass voice, consisting in that case of about 26 vibrations in a second. The same sound may be raised nearly to the pitch of the common voice; but it is never smooth and clear, except perhaps in some of those persons called ventriloquists. When the pitch is raised still higher, the upper orifice of the larynx, formed by the summits of the arytænoid cartilages and the epiglottis, seems to succeed to the office of the ligaments of the glottis, and to produce a retrograde falsetto, which is capable of a very great degree of acuteness. The same difference probably takes place between the natural voice and the common falsetto: the rimula glottidis being too long to admit of a sufficient degree of tension for very acute sounds, the upper orifice of the larynx supplies its place; hence, taking a note within the compass of either voice, it may be held, with the same expanse of air, two or three times as long in a falsetto as in a natural voice; hence, too, the difficulty of passing smoothly from the one voice to the other. It has been remarked, that the larynx is always elevated when the sound is acute: but this elevation is only necessary in rapid transitions, as in a shake; and then probably because, by the contraction of the capacity of the trachea, an increase of the pressure of the breath can be more rapidly effected this way, than by the action of the abdominal muscles alone. The reflection of the sound thus produced from the various parts of the cavity of the mouth and nostrils, mixing at various intervals with the portions of the vibrations directly proceeding from the larynx, must, according to the temporary form of the parts, variously affect the laws of the motion of the air in each vibration, or, according to EULER's expression, the equation of the curve conceived to correspond with this motion, and thus produce the various characters of the vowels and semi-vowels. The principal sounding board seems to be the bony palate: the nose, except in nasal letters, affords but little resonance; for the nasal passage may be closed, by applying the finger to the soft palate, without much altering the sound of vowels not nasal. A good ear may distinctly observe, especially in a loud bass voice, besides the fundamental note, at least four harmonic sounds, in the order of the natural numbers; and, the more reedy the tone of the voice, the more easily they are heard. Faint as they are, their origin is by no means easy to be explained. This observation is precisely confirmed, in a late dissertation of M. KNECHT, published in the musical newspaper of Leipsic. Perhaps, by a close attention to the harmonics entering into the constitution of various sounds, more may be done in their analysis than could otherwise be expected.

XVI. *Of the Temperament of musical Intervals.*

It would have been extremely convenient for practical musicians, and would have saved many warm controversies among theoretical ones, if three times the ratio of 4 to 5, or four times that of 5 to 6, had been equal to the ratio of 1 to 2. As it happens to be otherwise, it has been much disputed in what intervals the imperfection should be placed. The ARISTOXENIANS and PYTHAGOREANS were in some sense the beginners of the controversy. SAUVEUR has given very comprehensive tables of a great number of systems of temperament; and his own now ranks among the many that are rejected. Dr. SMITH has written a large and obscure volume, which, for every purpose but for the use of an impracticable instrument, leaves the whole subject precisely where it found it. Kirnberger, Marpurg, and other German writers, have disputed with great bitterness, almost every one for a particular method of tuning. It is not with any confidence of success, that one more attempt is made, which rests its chief claim to preference, on the similarity of its theory to the actual practice of the best instrument-makers. However we estimate the degree of imperfection of two tempered concords of the same nature, it will appear, that the manner of dividing the temperament between them does not materially alter its aggregate sum; for instance, the imperfection of a comma in a major-third, occasions it to beat very nearly twice as fast as that of half a comma. If indeed the imperfection were great, it might affect an interval so materially as to destroy its character; as, in some methods of temperament, a minor third diminished by two commas approaches more nearly to the ratio 6 : 7, than to 5 : 6; but, with this limitation, the sum of harmony is nearly equal in all systems. Hence, if every one of the twelve major and minor thirds occurred equally often in the compositions which are to be performed on an instrument, it would be of no great consequence, to the sum of the imperfections, among which of the thirds they were divided: and, even in this case, the opinion of the best practical authors is, that the difference of character produced by a difference of proportions in various keys, would be of considerable advantage in the general effect of modulation. But, when it is considered, that upon an average of all the music ever composed, some particular keys occur at least twice as often as others, there seems to be a very strong additional reason for making the harmony the most perfect in those keys which are the most frequently used; since the aggregate sum of all the imperfections which occur in playing, must by this means be diminished in the greatest possible degree, and the diversity of character at the same time preserved. Indeed, in practice, this method, under different modifications, has been almost universal; for, although many have pretended to an equal temperament, yet the methods which they have employed to attain it have been evidently defective. It appears to me, that every purpose may be answered, by making C : E too sharp by a quarter of a comma, which will not offend the nicest ear; E : G*, and A^b : C, equal; F* : A* too sharp by a comma; and the major thirds of all the intermediate keys more or less perfect, as they approach more or less to C in the order of modulation. The fifths are perfect enough in every system. The results of this method are shown in Table XII. In practice, nearly the same effect may be very simply produced, by tuning from C to F, B^b, E^b, G*, C*, F* six perfect fourths; and C, G, D, A, E, B, F*, six equally imperfect fifths, Plate VI. Fig. 52. If the unavoidable imperfections of the fourths be such as to incline them to sharpness, the temperament will approach more nearly to equality, which is preferable to an inaccuracy on the other side. An easy method of comparing different systems of temperament is exhibited in Plate VII. Fig. 53, which may easily be extended to all the systems that have ever been invented.

Table XII.

A		B		C	
C	50000	1 C	+ .0013487	1 A, E	— .0023603
B	53224	2 G, F	.0019006	2 D, B	.0029122
B♭	56131	3 D, B♭	.0024525	3 G, F*	.0034641
A	59676	4 A, E♭	.0034641	4 C, C*	.0044756
G*	63148	5 E, A♭	.0044756	5 F, G*	.0049353
G	66822	6 B, C*	.0049353	6 B♭, E♭	.0053950
F*	71041	7 F*	.0053950		
F	74921				
E	79752	D			
E♭	83810				
D	89304	1 E♭, G*, C*, F*	— .0000000		
C*	94723	2 F, B♭, E, B	.0004597		
C	100000	3 C, G, D, A	.0011562		

A, shows the division of a monochord corresponding to each note, in the system proposed. B, the logarithm of the temperament of each of the major thirds. C, of the minor thirds. D, of the fifths; C and D being both negative.

Thus, Sir, I have endeavoured to advance a few steps only, in the investigation of some very obscure but interesting subjects. As far as I know, most of these observations are new; but, if they should be found to have been already made by any other person, their repetition in a connected chain of inference may still be excusable. I am persuaded also, that at least some of the positions maintained are incontrovertibly consistent with truth and nature; but, should further experiments tend to confute any opinions that I have suggested, I shall relinquish them with as much readiness as I have long since abandoned the hypothesis which I once took the liberty of submitting to the Royal Society, on the functions of the crystalline lens.

I am, &c.

Emanuel College, Cambridge, 8th July, 1799.

THOMAS YOUNG.

EXPLANATION OF THE FIGURES.

(See Plates III. IV. V. VI. and VII.)

Plate III.

Figs. 1—6. The section of a stream of air from a tube .07 inch in diameter, as ascertained by measuring the breadth of the impression on the surface of a liquid. The pressure impelling the current, was in Fig. 1, 1 inch. Fig. 2, 2. Fig. 3, 3. Fig. 4, 4. Fig. 5, 7. Fig. 6, 10.

Figs. 7—12. A similar section, where the tube was .1 in diameter, compared with the section as inferred from the experiments with two gages, which is represented by a dotted line. From this comparison it appears, that where the velocity of the current was small, its central parts only displaced the liquid; and that, where it was great, it displaced, on meeting with resistance, a surface somewhat greater than its own section. The pressure was in Fig. 7, 1. Fig. 8, 2. Fig. 9, 3. Fig. 10, 4. Fig. 11, 7. Fig. 12, 10.

Figs. 13—20. A, the half section of a stream of air from a tube .1 in diameter, as inferred from experiments with two water gages. The pressure was in Fig. 13, .1. Fig. 14, .2. Fig. 15, .5. Fig. 16, 1. Fig. 17, 3. Fig. 18, 5. Fig. 19, 7. Fig. 20, 10. The fine lines, marked B, show the result of the observations with an aperture .15 in diameter opposed to the stream; C with .3; and D with .5.

Figs. 21—23. A, the half section of a current from a tube .3 in diameter, with a pressure of .5, of 1, and of 3. B shows the course of a portion next the axis of the current, equal in diameter to those represented by the last figures.

Plate IV.

Fig. 24. The appearance of a stream of smoke forced very gently from a fine tube. Fig. 25 and 26, the same appearance when the pressure is gradually increased.

Fig. 27. See Section III.

Fig. 28. The perpendicular lines over each division of the horizontal line show, by their length and distance from that line, the extent of pressure capable of producing, from the respective pipes, the harmonic notes indicated by the figures placed opposite the beginning of each, according to the scale of 22 inches parallel to them. The larger numbers, opposite the middle of each of these lines, show the number of vibrations of the corresponding sound in a second.

Plate V.

Figs. 29—33. See Section X.

Fig. 34. The combination of two equal sounds constituting the interval of an octave, supposing the progress and regress of the particles of air equable. Figs. 35, 36, 37, a similar representation of a major third, major tone, and minor sixth.

Fig. 38. A fourth, tempered about two commas.

Fig. 39. A vibration of a similar nature, combined with subordinate vibrations of the same kind in the ratios of 3, 5, and 7.

Fig. 40. A vibration represented by a curve of which the ordinates are the sines of circular arcs increasing uniformly, corresponding with the motion of a cycloidal pendulum, combined with similar subordinate vibrations in the ratios of 3, 5, and 7.

Plate VI.

Figs. 41 and 42. Two different positions of a major third, composed of similar vibrations, as represented by figures of sines.

Fig. 43. A contracted representation of a series of vibrations. A, a simple uniform sound. B, the beating of two equal sounds nearly in unison, as derived from rectilinear figures. C, the beats of two equal sounds, derived from figures of sines. D, a musical consonance, making by its frequent beats a fundamental harmonic. E, the imperfect beats of two unequal sounds.

Fig. 44. Various forms of the orbit of a musical chord, when inflected, and when struck.

Fig. 45. Forms of the orbit, when the sound is produced by means of a bow.

Fig. 46. Epitrochoidal curves, formed by combining a simple rotation or vibration with other subordinate rotations or vibrations.

Figs. 47 and 48. The successive forms of a tended chord, when inflected and let go, according to the construction of De la Grange and Euler.

Fig. 49. The appearance of a vibrating chord which had been inflected in the middle, the strongest lines representing the most luminous parts.

Fig. 50. The appearance of a vibrating chord, when inflected at any other point than the middle.

Fig. 51. The appearance of a chord, when put in motion by a bow applied nearly at one third of the length from its end.

Fig. 52. The method of tuning recommended for common use.

Plate VII.

Fig. 53. A comparative view of different systems of temperament. The whole circumference represents an octave. The inner circle L is divided into 30103 parts, corresponding with the logarithmical parts of an octave. The next circle R shows the magnitude of the simplest musical and other ratios. Q is divided into twelve equal parts, representing the semitones of the equal temperament described by ZARLINO, differing but little from the system of ARISTOXENUS, and warmly recommended by MARPURG and other late writers. Y exhibits the system proposed in this paper as the most desirable; and P the practical method nearly approaching to it, which corresponds with the eleventh method in MARPURG's enumeration, except that, by beginning with C instead of B, the practical effect of the temperament is precisely inverted. K is the system of KIRNBERGER and SULZER; which is derived from one perfect third, ten perfect and two equally imperfect fifths. M is the system of mean tones, the *sistema participato* of the old Italian writers, still frequently used in tuning organs, approved also by Dr. SMITH for common use. S shows the result of all the calculations in Dr. SMITH's harmonics, the system proposed for his changeable harpsichord, but neither in that nor any other form capable of practical application.

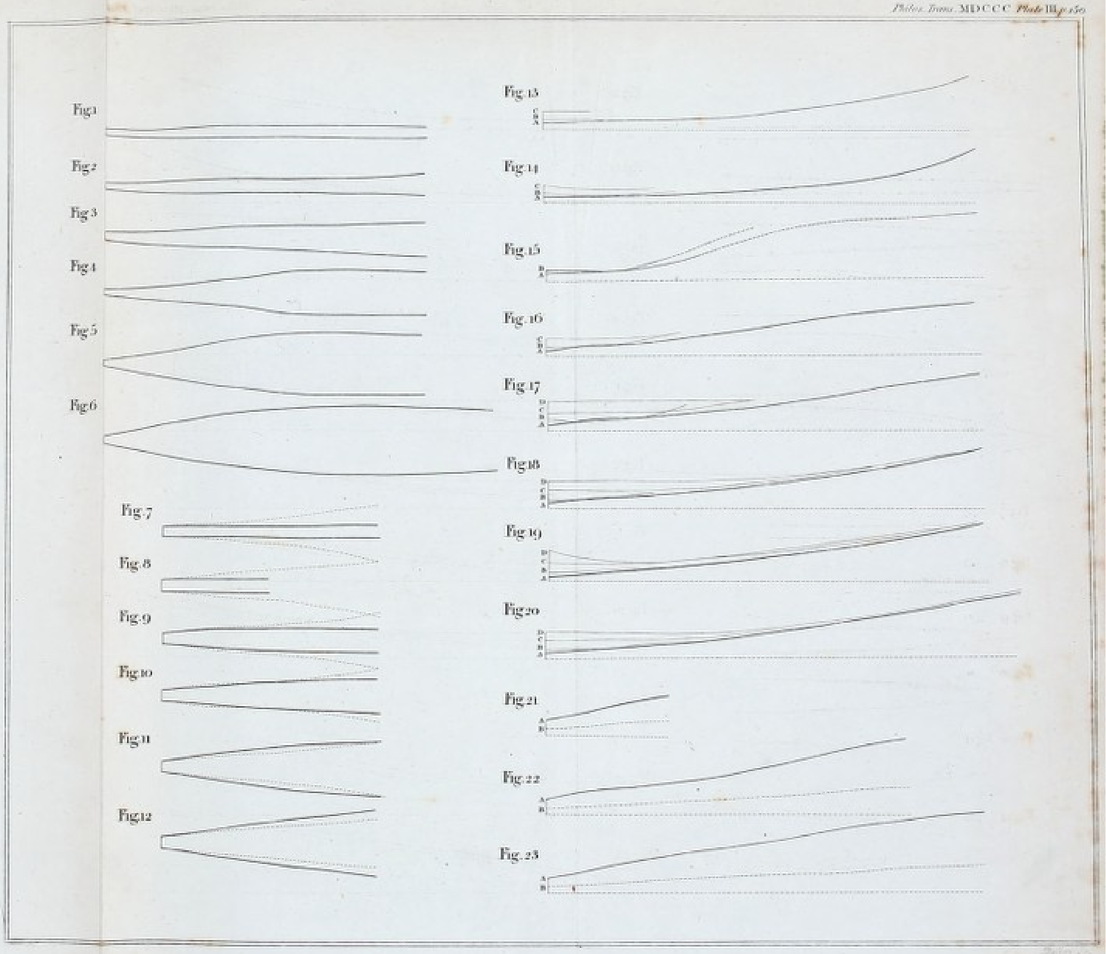

Fig. 1
Fig. 2
Fig. 3
Fig. 4
Fig. 5
Fig. 6
Fig. 7
Fig. 8
Fig. 9
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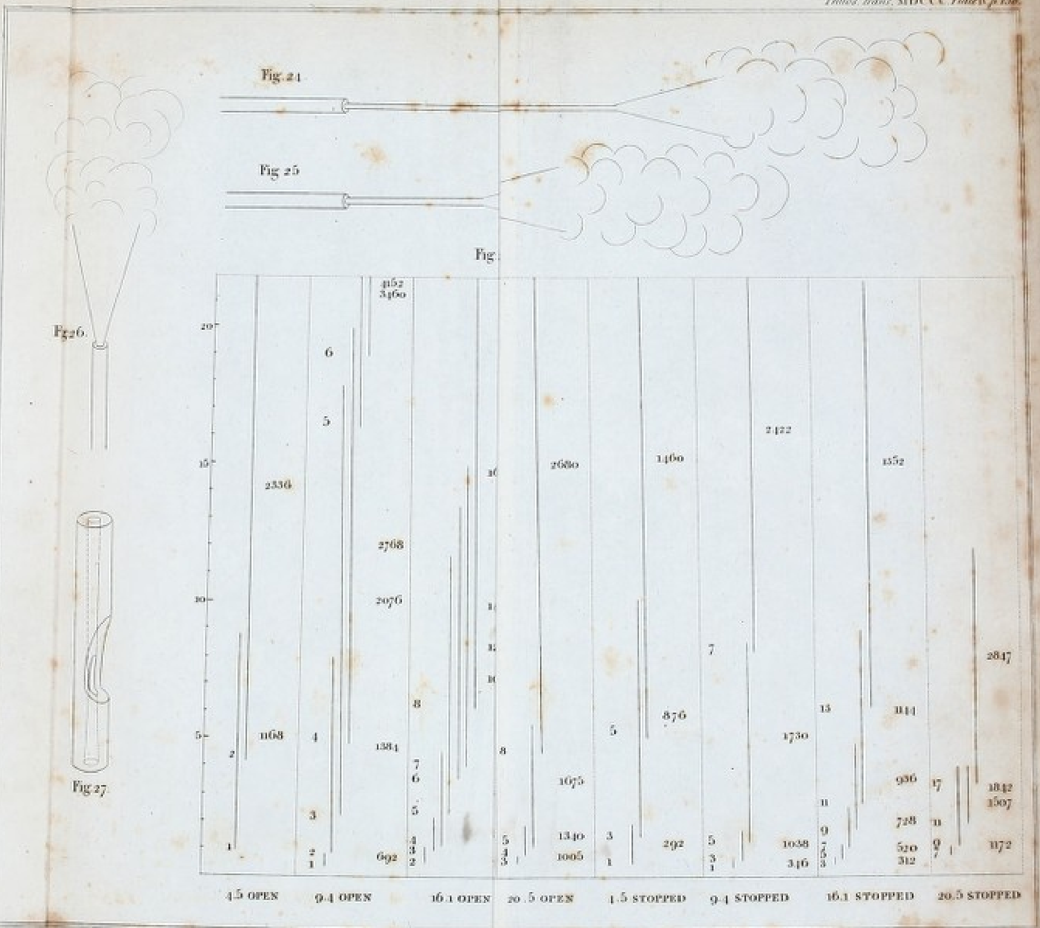
Fig. 24.
Fig. 25.
Fig. 26.
Fig. 27.
4.5 OPEN
9.4 OPEN
16.1 OPEN
20.5 OPEN
4.5 STOPPED
9.4 STOPPED
16.1 STOPPED
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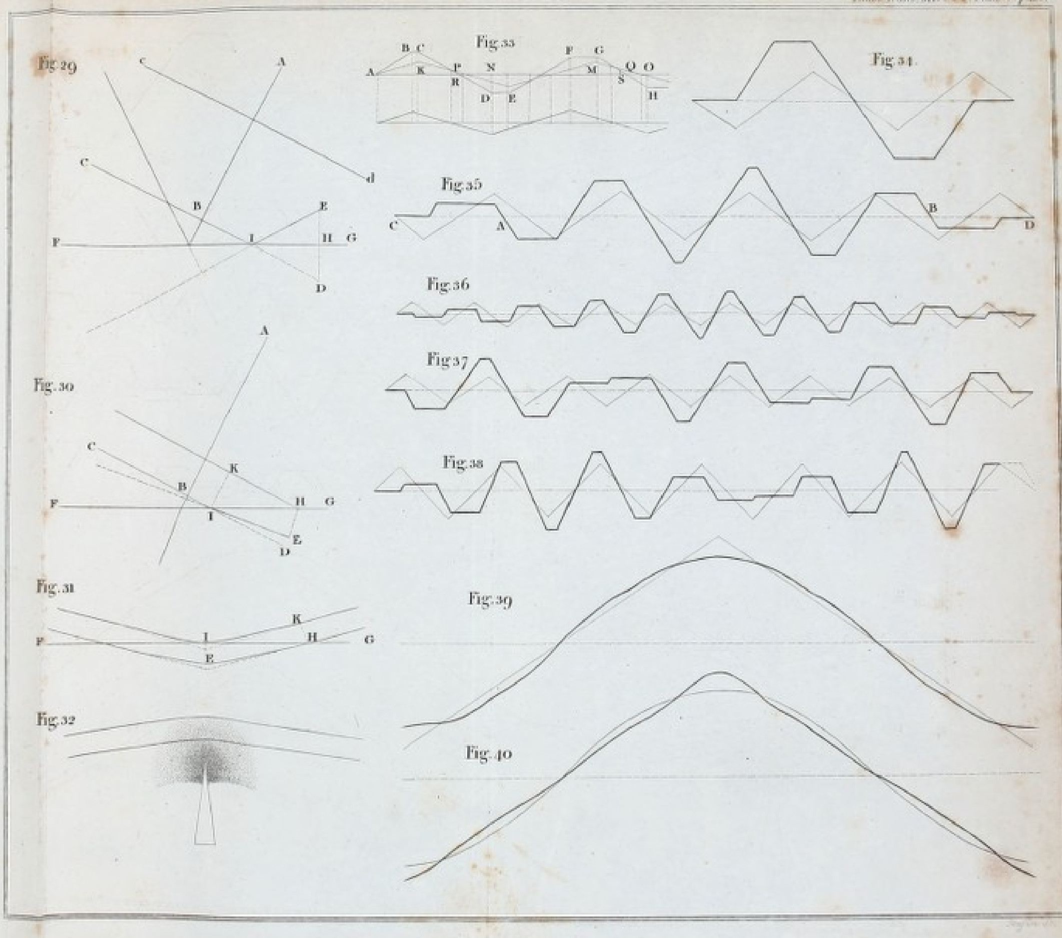
Fig. 29
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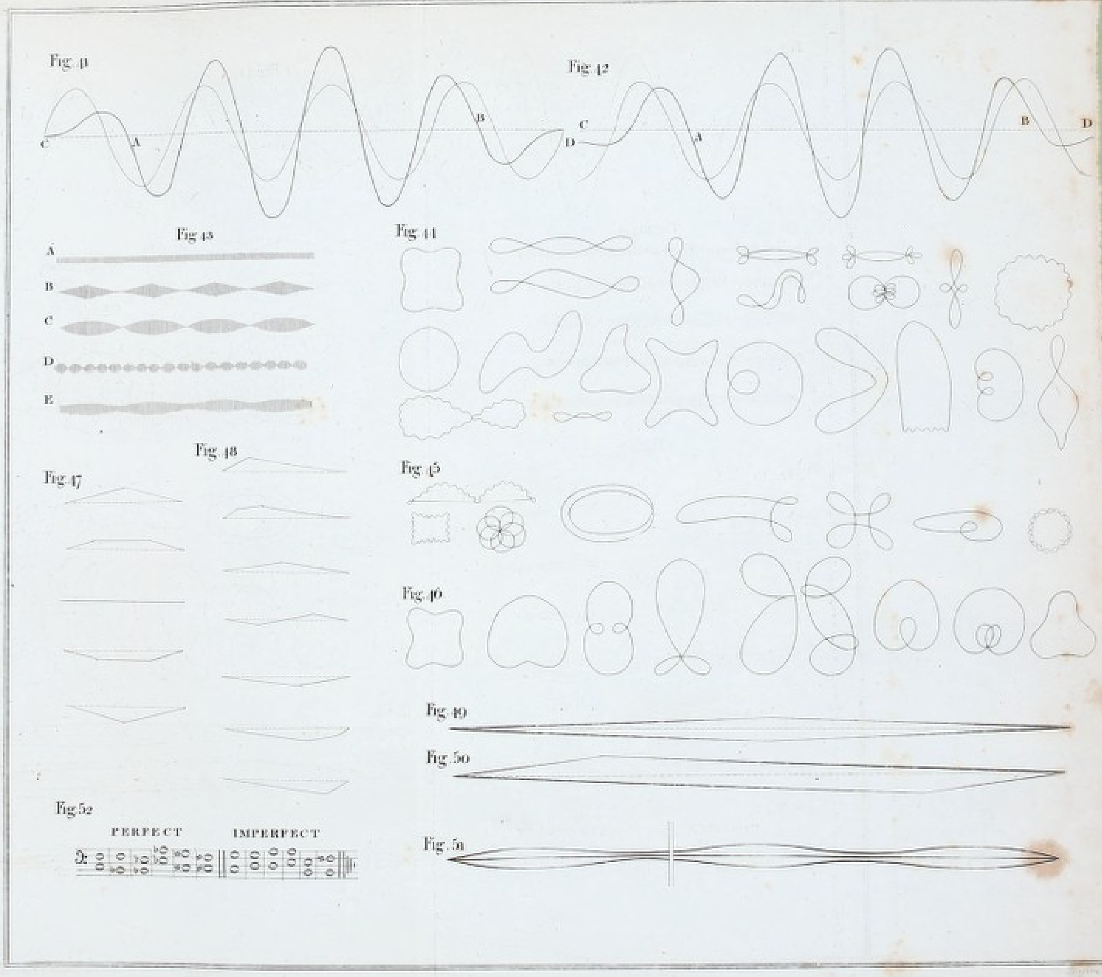
Fig. 41
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Fig. 42
C
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Fig. 43
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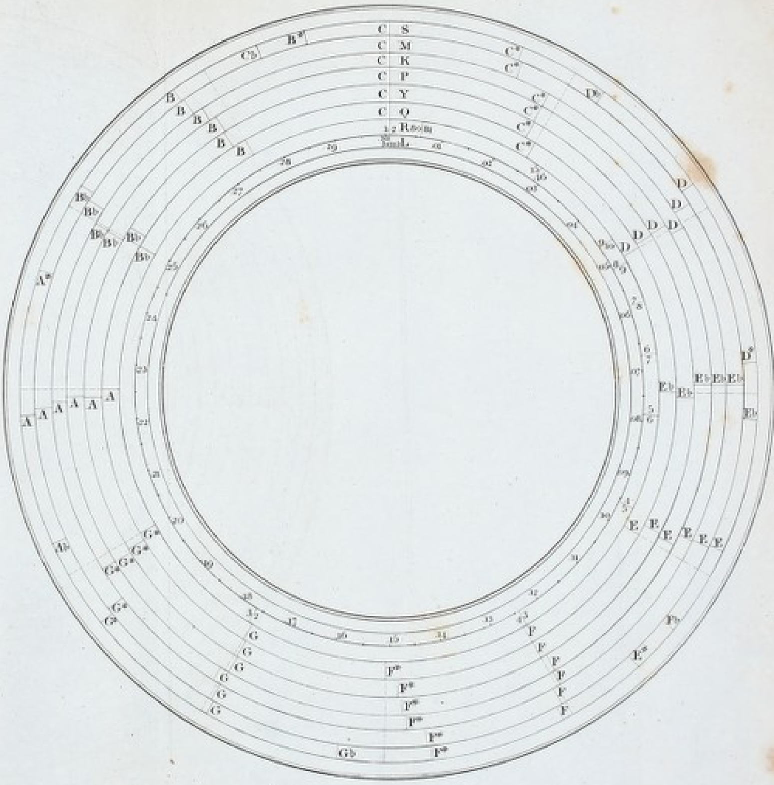
Fig. 44
Fig. 45
Fig. 46
Fig. 47
Fig. 48
Fig. 49
Fig. 50
Fig. 51
Fig. 52
PERFECT
IMPERFECT

Fig. 53.



PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCI.

PART I.

LONDON,

PRINTED BY W. BULMER AND CO. CLEVELAND-ROW, ST. JAMES'S;
AND SOLD BY PETER ELMSLY,
PRINTER TO THE ROYAL SOCIETY.
MDCCCI.

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

Read November 27, 1800.

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. fo[illegible], p. 146. ‡ Edinb. Me[illegible] 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 $mn':m'n$. For instance, let the three mediums be glass, water, and air; then $m=3$, $n=2$, $m'=4$, $n'=3$, $mn'=9$, and $m'n=8$. If the ratios be 4 : 3, and 13 : 14, we have $mn':m'n$: : 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 $\frac{nd}{nd+me}$, or (AD =) $\frac{md}{md+ne}$; 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 $\frac{nd}{m}$ or $\frac{md}{n}$ 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 $e=\frac{mduu}{mdu-ndt-ntt}$.

Corollary 2. For a plane surface, $e=\frac{mduu}{-ntt}$.

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, $e = \frac{b\,d}{b + d}$.

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, $e = \frac{n\,t\,u}{2\,(m\,u - n\,t)}$; which varies ultimately as the product of the cosines, or as $\frac{m + n}{n\,n}\,t + t^2$.

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 $\frac{1}{l}$, 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 $= \frac{2}{(r-1)s^{\frac{r+1}{r-1}}(aA + bB + cC + \ldots) + 2aA + 6bBs^2 + 10cCs^4 + \ldots}$, 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 eyelashes.

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 n t u, read n t t*; 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* $2\,m\,u\,u$.

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, $Z = s\,x^{-1}$, $Dc = \dfrac{s}{2\,x\,x\sqrt{x^{-\frac{2}{r}} - s^2 x^{-2}}}$ $= \frac{1}{2}\,s\,x^{\frac{1}{r}-2} \cdot \overline{1 - s^2 x^{\frac{2}{r}-2}}\,\big|^{-\frac{1}{2}}$, and the fluxion of the area described by the radius $= -\frac{1}{2}\,s\,x^{\frac{1}{r}-2}\,\dot{x} \cdot \overline{1 - s^2 x^{\frac{2}{r}-2}}\,\big|^{-\frac{1}{2}}$. 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.

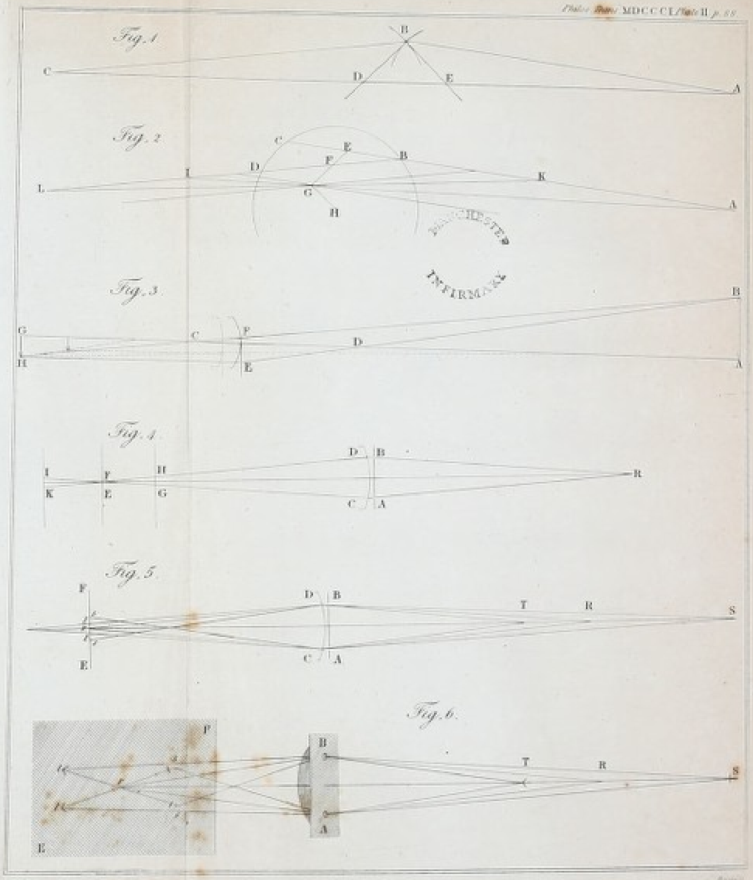
Fig. 53. The order of the fibres in the lens of birds and fishes.

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.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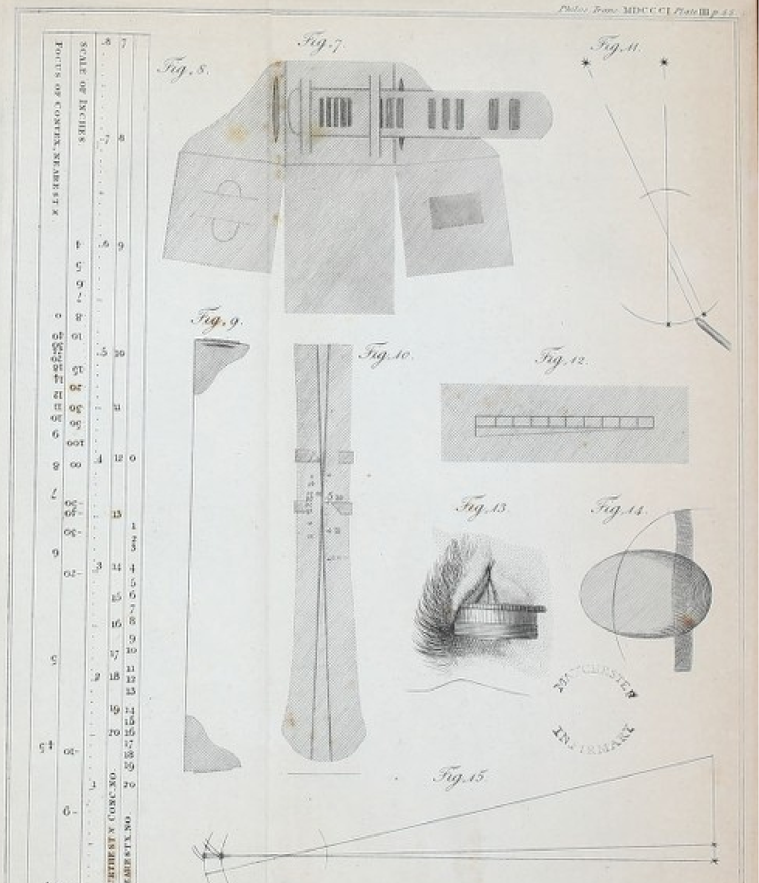
Philos. Trans. MDCCCI. Plate III. p. 45.



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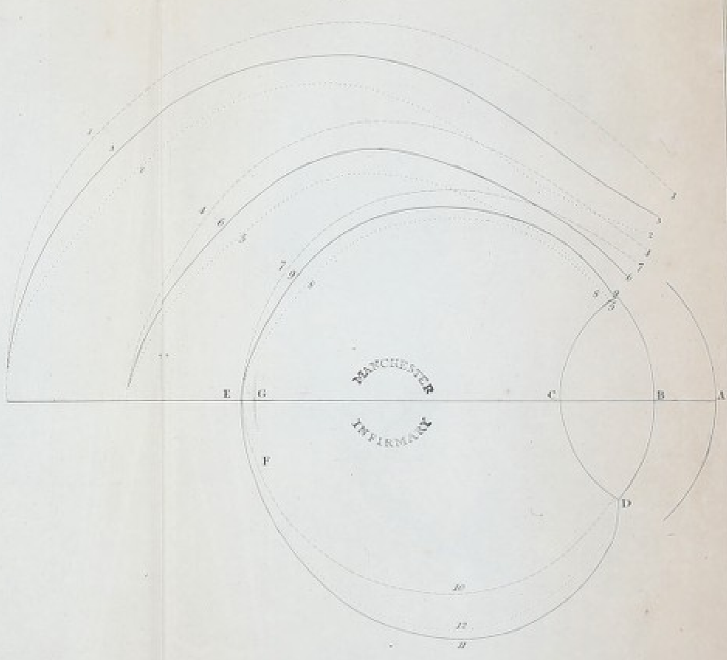

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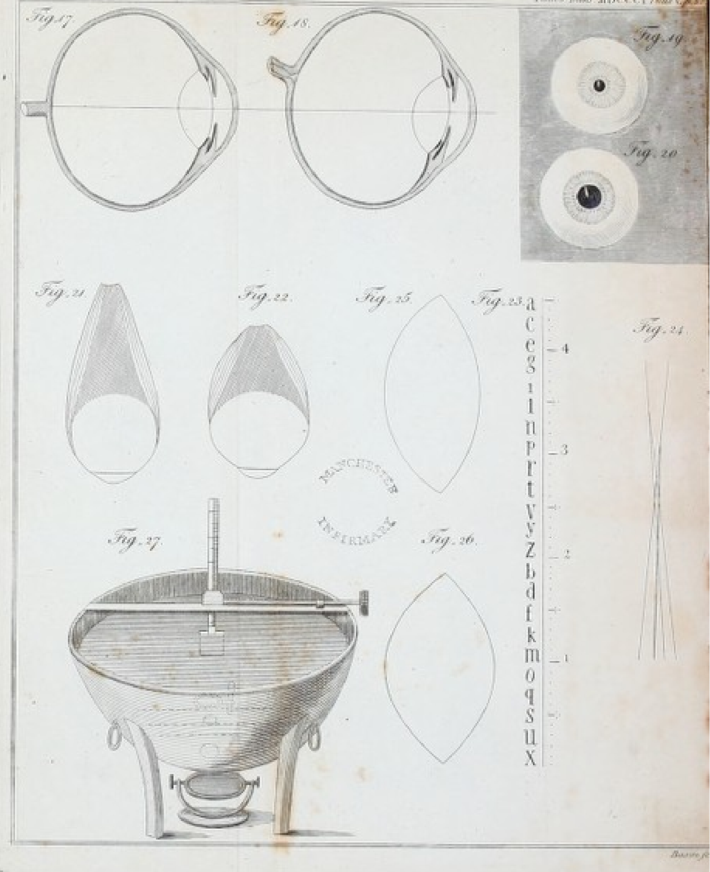
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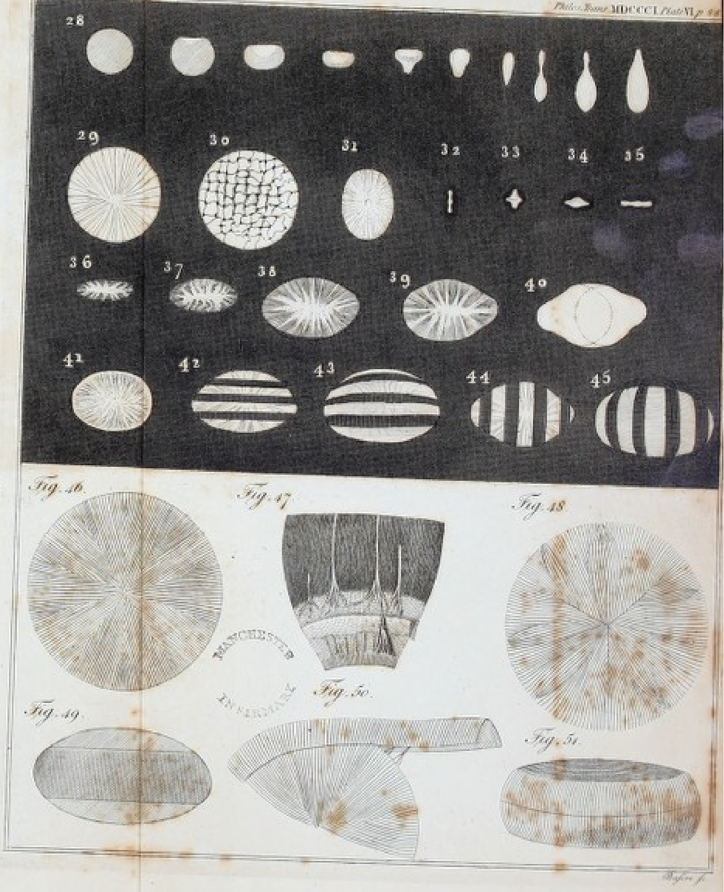
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Fig. 50.
Fig. 51.

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