XVI. *Farther Experiments and Observations on the Affections and Properties of Light. By* Henry Brougham, *Jun. Esq. Communicated by Sir* Charles Blagden, *Knt. Sec. R. S.*

Read June 15, 1797.

Having laid before the Royal Society an account of a course of experiments on light, in which I had been engaged, and also of the conclusions which these experiments had taught me to draw, I proceed in the following paper to relate the continuation of my observations; which I hope may not prove wholly uninteresting to such as honoured the former part with their attention. I am first to unfold a new and, I think, curious property of light, that may be indeed reckoned fourfold, as it holds, like the rest, equally with respect to refraction, reflexion, inflexion, and deflexion; thus preserving entire the same beautiful analogy in these four operations, which we have hitherto remarked. I shall then consider several phænomena connected either with this, or with the properties before described, and of which they afford some striking confirmations.

I.

Observation 1. The sun shining strongly into my darkened chamber, I placed, at a small hole in the window-shut, a prism with its refracting angle (of 65°) upwards, so that the spectrum was cast on a chart placed at right angles to the incident rays, and four feet from the prism.

In the rays, parallel to the chart, and two feet from it, I placed a pin, whose diameter was $\frac{1}{30}$ of an inch, and fixed it so, that the axis of its shadow on the spectrum might be parallel to the sides of the spectrum. A set of images by reflexion was formed (similar to those described above*), all inclining to the violet; but what I chiefly attended to at present was their shape. I had always observed that the part formed out of the red-making rays was broadest, and that the other parts diminished in breadth regularly towards the violet. I now delineated one or two, at about three inches from the shadow; and though (from the pin's irregularities) the sides were by no means smooth, yet the general shape was in every pin, and with every prism used, nearly as represented in fig. 1. (Tab. X.) divided in the direction R A, according to the colours of the spectrum in which they were formed; R O B A was red, and the broadest; that is, R A was broader than O B, the confines of the red and orange; and G D E V was the violet, narrowest of all.

Observation 2. Between the pin and the prism, $\frac{1}{10}$ of an inch from the pin, was placed a screen, through a small hole in which, of twice the pin's diameter, the rays of the spectrum passed, and were reflected into images by the pin; these were pretty distinct and well defined, when received on a chart $\frac{1}{2}$ a foot from the pin. They were oblong, having parallel sides and confused ends; they were wholly of the colour whose rays fell on the pin, unless when the white, mixed with those at the confines of the yellow and green, caused the images to be of all the

* Phil. Trans. for 1796, page 240.

colours. When the prism was turned round on its axis, so that different rays fell on the pin, the images changed their sizes as well as their positions; they were largest when red, and least when violet.

Observation 3. In case it may be thought that the sides of the hole, through which the rays passed in observation 2, by inflecting, might dispose them, before incidence, into beams of different sizes, I removed the screen, and placed the pin horizontally, the axis of the shadow being now at right angles to that of the prismatic spectrum; and moving the prism on its axis, again I observed the contraction and dilatation of the images by reflexion, though now they were rather less distinct, from the greater size of the incident beam; and to shew that there was both a change of size and of place, without any manner of deception, I placed one leg of a pair of compasses in a fixed point of the spectrum, and the other in the middle point of an image formed by the violet-making rays. The prism being then moved till the image became red, I again bisected it, and found its centre considerably beyond the point of the compasses, which was indeed evidently much nearer one end of the image than the other; besides, that the red image, when measured, was longer than the rest; and this satisfied me that there were two changes, one of place, with respect to the fixed point, the other of size, with respect to the centre of the image. Lastly, as far as I could judge, the dilatation and contraction appeared even and uniform.

Observation 4. I remarked that the fringes or images, by flexion, were always increased in size when formed out of red-making rays, and were less in every other colour, and least in violet (besides being moved farther from the edge of the shadow in the former rays than in the latter); and this agrees with an observation of Sir Isaac Newton, as far as he tried it, which was with respect to deflexion. In making several experiments with prisms, I hit on a very remarkable confirmation of this. I observed on each side of the spectrum four or five distinct fringes, like the images by reflexion, coloured in the order of the spectrum, but quite well defined at the edge, and even pretty distinct at the end; they were also much narrower than those images, but like them they inclined much to the violet, and were broadest in the red, growing narrower by degrees, and narrowest of all in the violet. I moved the prism and they disappeared, but when the prism was brought back to its former position, they also returned. I then observed the prism in open light, and saw that it had veins, chiefly opaque and white, running through it, and that there were several of these in the place where the light passed when the prism was held as before. But in case the inclination and shape of these images might be owing to the irregular order in which the veins were laid, I held another prism, which happened to have parallel veins; in many positions of this the fringes or images returned, not indeed always so regular nor always of the same kind; for some were confused and broader, formed (as I concluded from this and their position) by reflexion; others made by transparent veins and air-bubbles were also irregular, but inclined to the red, the violet being farthest from the perpendicular, and these were obviously caused by refraction; yet all agreed in this, that they were broadest in the red, and narrowest in the violet parts.

Observation 5. I held, in the direct rays of the sun at $\frac{1}{2}$ an inch from the small hole in the window-shut, a glass tube, free from scratches and opaque veins, but like most glass that is not finely wrought, having its surface of a structure somewhat fibrous. When this tube was slowly introduced into the light, and so held that none of the rays might be refracted, a streak, chiefly white, was seen, similar in shape and position to those described before.* When narrowly inspected, it was found to contain many images by reflexion in it. But these were much diluted by the abundance of white light, reflected without decomposition in the manner above mentioned.† This streak lay wholly on one side of the tube; but I moved the tube onward a little, and another streak darted through the shadow, and extended all round on both sides: and now, when the tube was in the middle of the rays, there were two streaks on both sides, one a little separated from the other and continued through the shadow, the other on each side of the shadow; the former was evidently produced by refraction; it contained many images very like those by reflexion, only more vivid in the colours, which were all in the inverted order, the violet being outermost, and the rest nearest the point of incidence. Images similar to these are also producible on the retina, as mentioned before.‡

Observation 6. I now placed a prism at the hole, and made the same images by refraction, out of homogeneal light. These inclined to the red, not (like images by reflexion) to the violet; but they were broadest in the red, and grew narrower towards the violet parts. In short, when viewed beside the images by reflexion, except in point of brightness and inclination they differed from them in no respect.

The three first experiments shew, that when homogeneal

* Phil. Trans. 1796, page 236. † Ibid. p. 237. ‡ Ibid. p. 243.

light is reflected, some rays are constantly disposed into larger images than others are, that is, into images more distended in length, though of the same breadth. The fourth experiment shews, that the same takes place when light is inflected and deflected; and the two last shew that the same happens when the rays are refracted in a way similar or analogous to that in which the other images were produced by reflexion and flexion.

We are now to shew, that this difference of size is not owing to the different reflexibilities and flexibilities of the rays. In order to this we shall both demonstrate, and then prove by experience, "that inflexion and deflexion do not decompound "heterogeneous rays, whose direction is such, that they fall on "the bending body." In fig. 2. let AB be the body; GH, EF, CD, the limits of its spheres of deflexion, inflexion, and reflexion, respectively; and let IP be a white ray of direct light entering at P the sphere of deflexion: through P draw LK at right angles to GH; IP will be separated into PR red, and PV violet, and the five other colorific rays according to their deflexibilities; at R and V draw the perpendiculars ST and QO; then the alternate angles PRT, RPL; and PVQ, VPL are equal each to each. But TRP and QVP are the angles of incidence, at which the red and violet enter the sphere of inflexion; and RPL, VPL are the angles of deflexion of the red and the violet; therefore the difference of the two latter, that is RPV, is likewise the difference of the two former. Suppose this difference equal to nothing; or that PV and PR are parallel; then *r*RS the angle of the red's inflexion will be less than *v*VO the angle of the violet's inflexion, by the angle RPV: (when not evanescent) add RPV to *r*RS; then *r*RS will be equal to *v*VO: that is, the divergence will be destroyed, and the rays enter the sphere of reflexion, parallel and undecompounded. It is evident, therefore, that the effect arising from the different deflexibilities of the rays is destroyed by the equal and opposite effect produced by their different inflexibilities; and the same thing may in like manner be shewn to happen in the return of the rays from the body after reflexion. But let the rays be so reflected that they shall pass by the body without entering any more than one sphere of flexion; then they will be separated by their flexibilities, as we before described. It appears, then, that if the rays of light were not differently reflexible, flexion could never produce the coloured images, by separating the compound light. And, indeed, this may be easily proved by fact. At 144 feet from the bending body, the greatest fringes by flexion are only half an inch in length, whereas the fourth or fifth images by reflexion are above half an inch at one foot from the reflecting surface: the one sort is therefore more than 144 times more distended than the other, whereas the flexion could, at the very farthest, only double them. Also the distinctness, and brightness, and regularity of the colouring, are quite different in the two cases; the supposed cause would neither account for the order of the colours, nor for their absence in common specular reflexion, and refraction through two prisms joined together with their angles the contrary ways. Lastly, if we suppose the images to be produced by flexion, and then reflected from the body, it would follow that light incident on a prism should be decompounded, formed into several coloured images, and then refracted, the violet being least and the red most bent; all which is perfectly the reverse of what actually happens. I have multiplied the proof of this proposition perhaps beyond what is

Fig. 1.

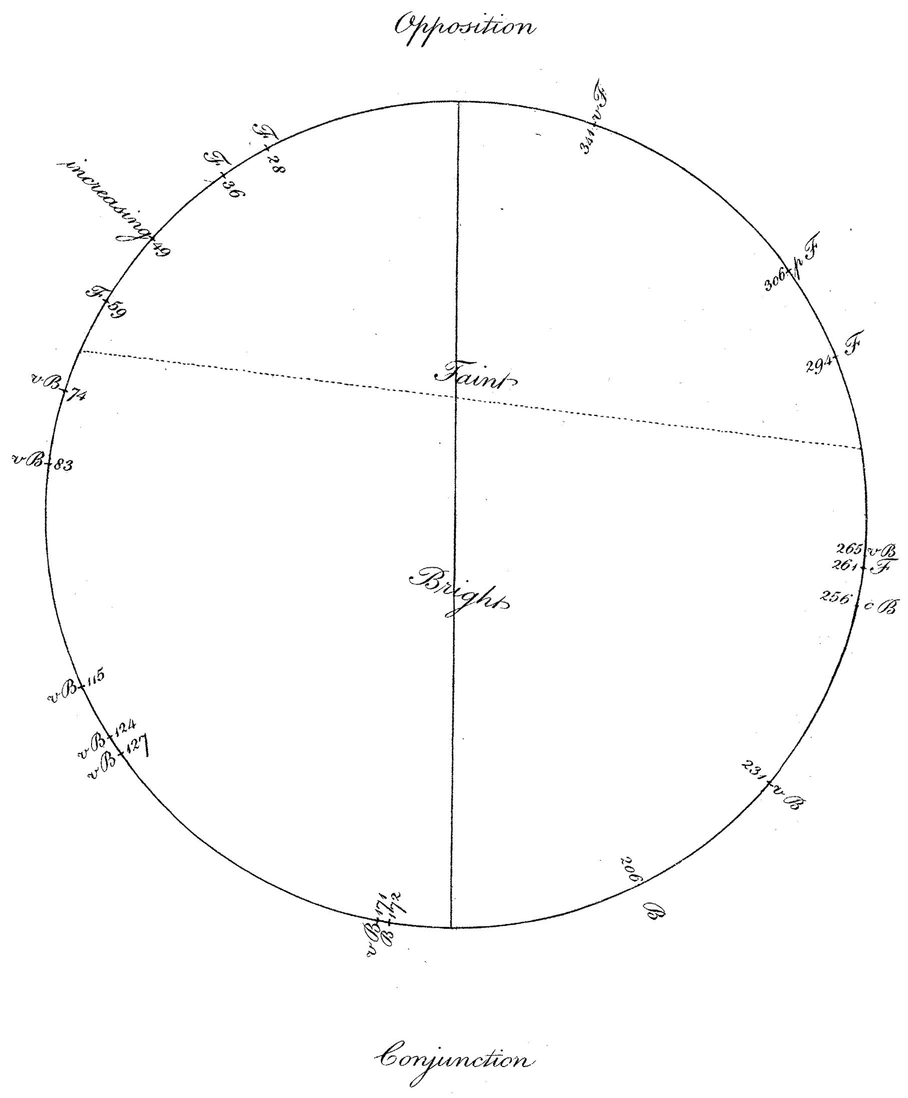
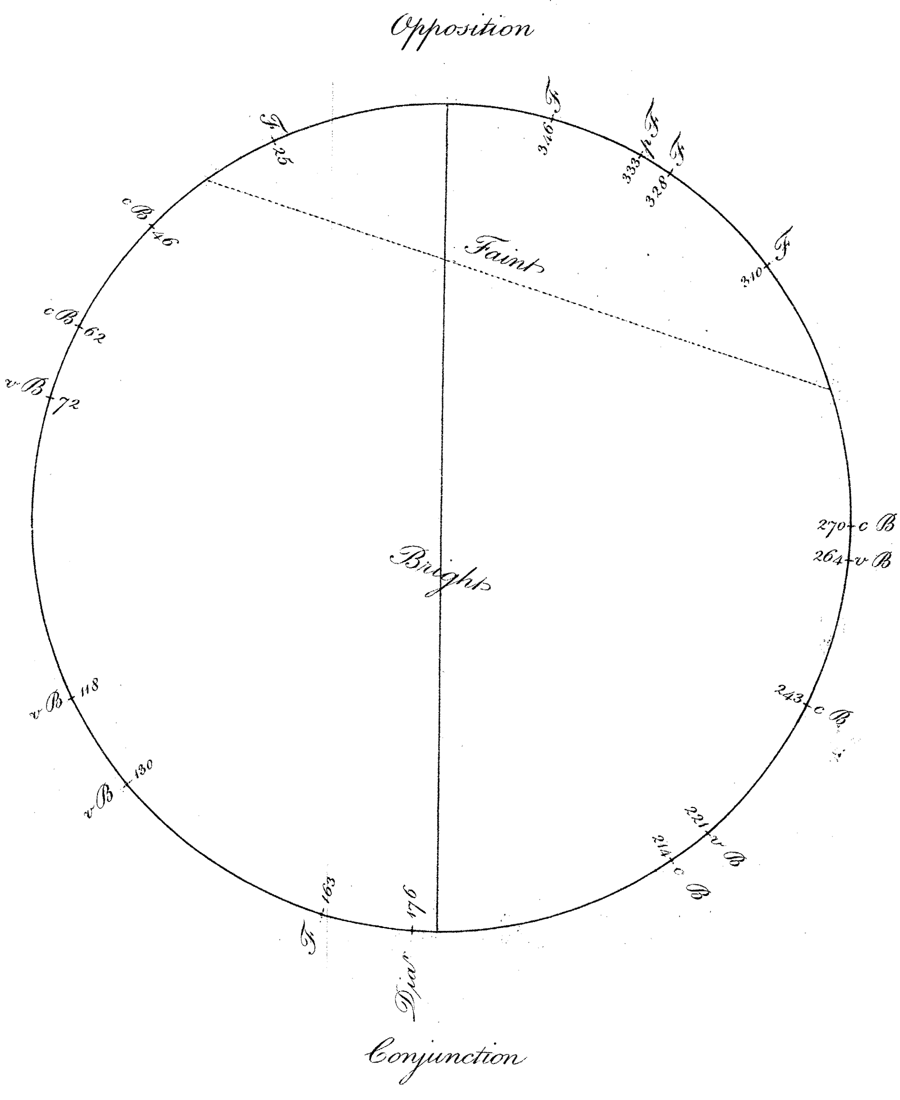

Opposition
F 28
F 36
increasing 49
F 59
vB 74
vB 83
vB 115
vB 124
vB 127
vB 171
B 172
341 vF
306 pF
294 F
265 vB
261 F
256 cB
231 vB
206 B
Faint
Bright
Conjunction

Fig. 2.



Basire Sc.

Fig. 3.

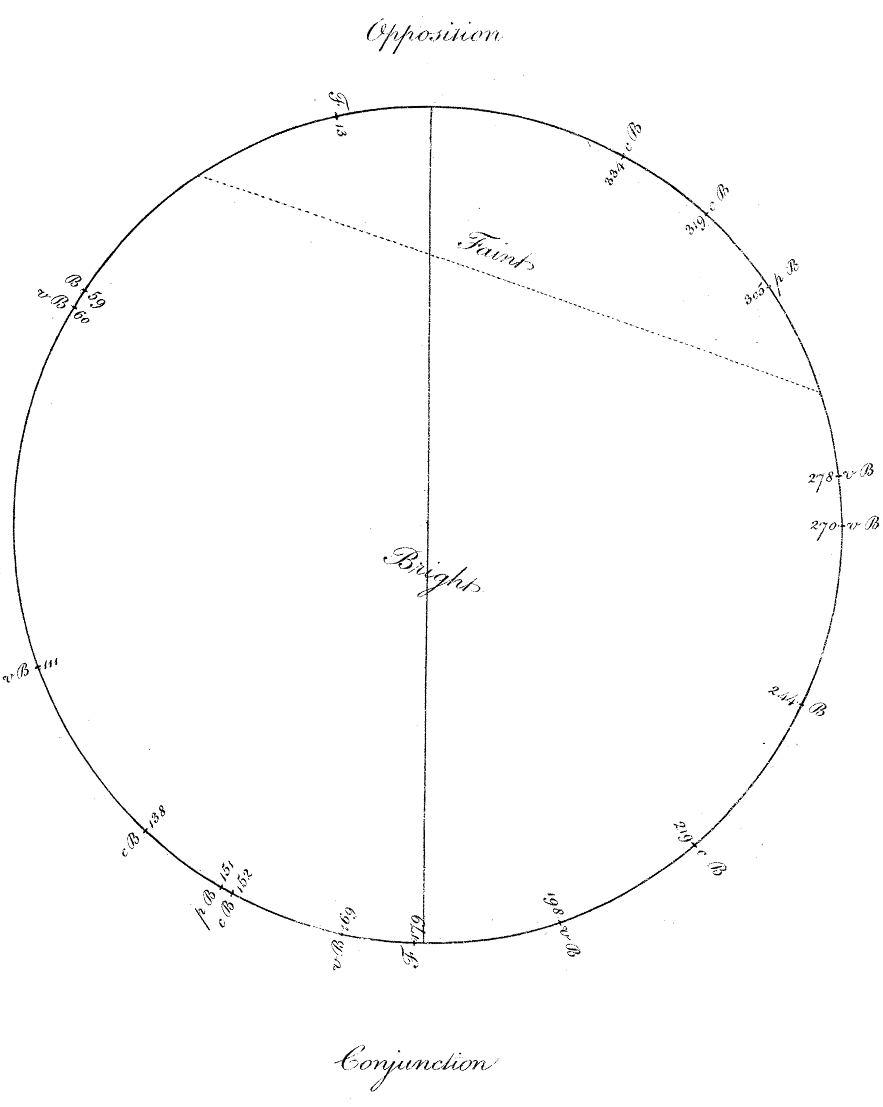
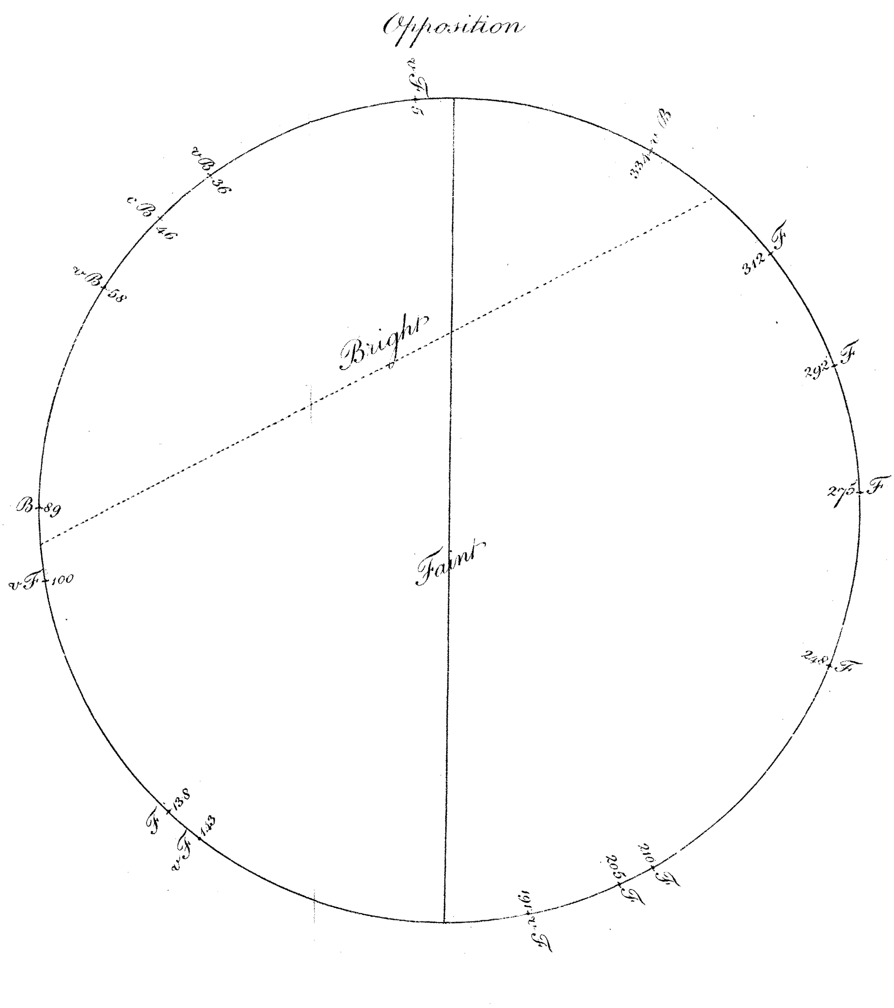

Opposition
F. 13
334 c B
319 c B
305 p B
Faint
B 59
v B 60
278 v B
270 v B
Bright
v B 111
244 B
c B 138
219 c B
p B 151
c B 152
198 v B
v B 169
F 179
Conjunction

Fig. 4.


Opposition
v.F. 5
334 v.B
vB 36
vB 46
vB 58
312 F
292 F
Bright
275 F
B 89
vF 100
Faint
248 F
F 138
vF 143
210 F
205 F
191 v.F
Conjunction

necessary; but its great importance to the whole theory will, I hope, plead my excuse.

Let us now suppose that a homogeneal beam passes through the spheres of flexion, it will follow that no divergence can take place from the bending power of the body; so that we have only to estimate the effect produced by the reflexion, and to inquire whether the different reflexibilities of the rays can cause the images to vary their sizes according as they are formed by different rays. In fig. 3. let A B be the body, C D the limit of its sphere of reflexion, and I P a beam of homogeneal rays, as red, incident at P and reflected to R, forming there the image R *r*. It is evident that the greater reflexibility of the rays I P can only alter the position of the centre of R*r*, making it nearer the perpendicular than the centre of an image formed by any other rays would be. But the greater length of R *r* shews that a greater quantity of rays is reflected, or that the same quantity is spread over a greater space, and that in the following way. Let I F *f i* be a beam of violet-making rays entering A B C D, and reflected so as to form the image R *v*. The force exerted by A B decreasing according to some law (of which we are as yet ignorant) as the distance increases, is not sufficient to turn the rays back till they have come a certain length within A B C D. But for the same reason it turns back all that it does reflect before they come nearer than a certain distance; between these two limits, therefore, the rays are turned back. But the limits are not the same to all the rays; some begin to be turned at a greater distance from the body than others, and consequently are reflected to a greater distance from the middle ray of the incident beam. Thus if I F *f i* be changed to a red-making beam, it begins to be turned back at *f*, and the rays farthest from AB are reflected to *r* instead of to *v*, where they fell when I F*fi* was violet-making; not but that the same quantity of rays is reflected, the only difference is, that the most reflexible are reflected farthest from the body by their greater reflexibility, and farthest from each other by this other property. Exactly the same happens in the case of refraction, *mutatis mutandis;* but there seems to be a slight variation in the *manner* in which the different rays are disposed into images of different sizes by flexion. In this case also the bending body's action reaches farther when exerted on some rays than when exerted on others: but then, the direction of the rays not passing through the body, those which are farthest off and at too great a distance to be bent, never coming nearer, are not bent at all; and consequently as the least flexible rays are in this predicament at the smallest distance, and the most flexible not till the distance is greater, the images formed out of the former must be less than those formed out of the latter. This difference in the way in which the phænomenon appears, does not argue the smallest difference in the cause: it only follows from the different position of the rays, with respect to the acting body, in the two cases. I infer then from the whole, that different sorts of rays come within the spheres of flexion, reflexion, and refraction, at different distances, and that the actions of bodies extend farthest when exerted on the most flexible. It may perhaps be consistent with accuracy and convenience to give a name to this property of light; we may, therefore, say that the rays of light differ in degree of refrangity, reflexity, and flexity, comprehending inflexity and deflexity. From these terms (uncouth as, like all new words, they at first appear) no confusion can arise, if we always remember that they allude to the degree of distance to which the rays are subject to the action of bodies. I shall only add an illustration of this property, which may tend to convey a clearer idea of its nature. Suppose a magnet to be placed so that it may attract from their course a stream of iron particles, and let this stream pass at such a distance that part of it may not be affected at all; those particles which are attracted may be conceived to strike on a white body placed beyond the magnet, and to make a mark there of a size proportional to their number. Let now another equal stream considerably adulterated by carbonaceous matter, oxygene, &c. pass by at the same distance, and in the same direction. Part of this will also be attracted, but not so far from its course, nor will an equal number be affected at all; so that the mark made on the white body will be nearer the direction of the stream, and of less size than that made by the pure iron. It matters not whether all this would actually happen, even allowing we could place the subjects in the situation described; the thing may easily be conceived, and affords a good enough illustration of what happens in the case of light.

Pursuant to the plan I before followed, I now tried to measure the different degrees of reflexity, &c. of the different rays; but though the measurements which I took agreed in this, that the red images were much larger than the rest, and the green appeared by them of a middle size, yet they did not agree well enough (from the roughness of the images, and several other causes of error), to authorize us to conclude with any certainty "that the action of bodies on the rays is in proportion to the "relative sizes of these rays." This, however, will most probably be afterwards found to be the case; in the mean time there is little doubt that the sizes are the cause of the fact.

II.

Several phænomena are easily explicable on the principles just now laid down.

1. If a pin, hair, thread, &c. be held in the rays of the sun refracted through a prism, extending through all the seven colours, a very singular deception takes place: the body appears of different sizes, being largest in the red and decreasing gradually towards the violet. This appearance seemed so extraordinary, that some friends who happened to see it as well as myself, suspected the body must be irregular in its shape. On inverting it, however, the same thing took place; and on turning the prism on its axis, so that the different rays successively fell on the same parts, the visible magnitude of the body varied with the rays that illuminated it. This appearance is readily accounted for by the different reflexity of the rays, and follows immediately from Obs. 2. and 3.

2. Sir ISAAC NEWTON found that the rings of colours made by thin plates and by thick plates of glass (as he calls them), when formed of homogeneal light, varied in size with the rays that made them, being largest in the most flexible rays. I have had the pleasure of observing several other sorts of rings, so extremely similar, and formed by flexion, that I can no longer doubt of this being also the cause of the phænomena observed by NEWTON. I shall first describe a species, to prove "that the colours by thick and thin plates are one and the "same phænomenon, only differing in the thickness of the

"plates." Happening to look by candle-light upon a round concave plate of brass, pretty well polished, so as to reflect light enough for shewing an image of the candle, I was surprised to see that image surrounded by several waves of colours, red, green, and blue, disposed in pretty regular order. This was so uncommon in a metallic speculum, that I examined the thing very minutely by a variety of experiments; these I shall not particularly now describe, but give a general idea of their results.

It must be observed, for the sake of clearness, that in the following inquiries concerning the formation of rings or fringes, the diameter of a ring or fringe means the line passing through the centre of that ring, and terminated at both ends by the circumference; whereas the breadth means that part of the diameter intercepted between the limits of the ring, or the distance between its extreme colours, red and violet.

In the first place, they were formed by the sun's light in the figure of rings surrounding the centre of the sphere to which the plate was ground, at greater distances increasing their breadths, the colours pretty bright, though inferior in brilliancy to those of concave specula.

Secondly, the order of the colours was in all red outermost, and violet or blue innermost, with a greyish-blue spot in the common centre of the whole; and on moving the plate from the perpendicular position, the rings moved and broke exactly like those of specula.

In the third place, homogeneal light made them of simple colours; they were broadest when red, narrowest when blue and violet.

Fourthly, they decreased in breadth from the centre; and I found, by a simple contrivance, that they were to one another in the very same ratio that the rays by specula follow.

In the fifth place, I compared the general appearance of the two sorts by viewing them at the same time, and was struck with their general appearance, unless that these of specula were most vivid and distinct.

These things made me suspect that they were actually caused by the thin coat of gums with which the surface of the plate was varnished, called lacker. Accordingly I took it off with spirit of wine, and found the rings disappear; on lackering it again they returned; and in like manner I caused a well finished concave metal speculum to form the rings of which we are speaking, by giving it a thin coat of lacker. This is a clear proof that these rings were exactly the same with those of thick plates (to use NEWTON'S expression), for the coat of gums is, when thin, pretty transparent, as may be seen by laying one on glass plates.

But this coat is extremely thin, and cannot exceed the 200th part of an inch; so that the colours of thick plates are in fact the very same with those of thin plates, except that the two kinds are made by different sized plates. We cannot, therefore, distinguish them, any more than we do the spectrum made by a prism whose angle is 90° from that made by one whose angle is 20°. This kind of colours is not the only one I have observed of nearly the same kind with those of plates; we shall presently see another much more curious and remarkable.

III.

In reflecting on the observations and conclusions contained in my former paper, several consequences seemed to follow, which appeared so new and uncommon, that I began to doubt a little the truth of the premises; but at any rate was resolved to examine more minutely how far these inferences might be consistent with fact: and I am happy in being able to announce the completeness of that consistency, even beyond my expectations. The chief consequences were the following.

1. That a speculum should produce, by flexion and reflexion, colours in its reflected light wherever it has the least scratch or imperfection on its surface.

2. That on great inclinations to the incident rays all specula, however pure and highly polished, should produce colours by flexion.

3. That they should also in the same case produce colours by reflexion.

4. That lenses, having the smallest imperfections, should produce by flexion colours in their refracted light.

5. That there should be many more than three, or even four fringes by flexion, invisible to the naked eye. And,

6. That Iceland crystal should have some peculiarities with respect to flexion and reflexion; or if not, that some information should be acquired concerning its singular properties respecting refraction.

The manner in which the first of these propositions is demonstrated *a priori*, is evident from the 4th figure, where C D is the reflecting surface, *v o* a concavity bearing a small ratio to C D, A *o* and A B rays proceeding to C D. The one, A B, will be separated into B *r* red, and B *v* violet, by deflexion from *o*, and will be reflected to *r′ v′*, forming there the fringes. The other, A *o*, being reflected, will be separated into B *x* and B *y*, by deflexion from *v*, forming other fringes, *x y*, on the side of *v o*'s shadow opposite to *r' v'*. Also when *v o* is convex instead of concave, the like fringes will be produced by the rays being deflected in passing by its sides. Lastly, when *v o* is a polished streak, images by reflexion will be produced, as described Phil. Trans. for 1796, p. 269. The same passage will also shew the reason why, on great inclinations, colours by reflexion should be produced. And the second proposition, with respect to flexion, follows from what was demonstrated in this paper (p. 357 and 358); it being that case where the rays either leave or fall on the speculum at such an inclination, as to come only within the sphere of inflexion, without being deflected. The fourth proposition is merely a simple case of flexion. And the two last require no illustration. I shall now relate how I inquired into the truth of these things *a posteriori*.

Observation 1. Looking at a plane glass mirror exposed to the sun's light, I observed that up and down its surface there were minute scratches (called hairs by workmen), and that each of these reflected a bright colour, some red, others green, and others blue. On moving the mirror to a different inclination, or my eye to a different position with respect to the mirror, I saw the species of the colours change; the red, for instance, became green, and the green blue. I applied my eye close to the mirror, and received on it the light reflected from one hair. I observed several distinct images of the sun much distended and regularly coloured, just like those described above; the same appearances were observable in all specula, metal and glass, which had these hairs, and I never saw any metal one without some: their size is exceedingly small, not above $\frac{1}{1000}$ of an inch. Rubbing a minute particle of grease on the surface of the speculum, images were seen on the fibrous surface;

and they always lay at right angles to that direction in which the grease was disposed by drawing the hand along it.

Observation 2. Besides these polished hairs, many specula have fewer or more small specks and threads, rough and black. Perhaps every polished surface is studded with a number of small ones, invisible to the naked eye from the quantity of regular light which it reflects. I took, from a reflecting telescope, a small concave speculum not very well finished; its surface shewed several specks to the naked eye, and many with a microscope. Its diameter was $\frac{37}{50}$ of an inch, its focal distance two inches, and the sphere to which it was ground eight inches diameter. I placed it at right angles to the rays of the sun, coming through a small hole $\frac{1}{42}$ of an inch diameter, into a very well darkened room; I then moved it vertically, so that the rays might be reflected to a chart 12 inches from the speculum, and consequently 10 from the focus: and though the focus appeared white and bright, yet on the chart the broad image was very different. It was mottled with a vast number of dark spots; these were of two sorts chiefly, circular and oblong. Of the former a considerable number were distinct and large, the rest smaller and more confused, but so numerous that they seemed to fill the whole image. None were quite black, but rather of a bluish grey, and the oblong ones had a line of faint light in the middle, just as is the case in shadows of small bodies. But the chief thing which I remarked was the colours. Each oblong and round spot was bordered by a gleam of white, and several coloured fringes separated by small dark spaces. The fringes were exactly like those surrounding the shadows of bodies, of the same shape with the dark space, having the colours in the order, red on the outside, blue or violet in the inside; the innermost fringe was broadest, the others decreasing in order from the first. I could sometimes see four of them, and when made at the edge of the large image, I could indistinctly discern the lineaments of a fifth: when two of the spots were very near one another, their rings or fringes ran into one another, crossing.

Observation 3. When the chart was removed to a greater distance, as six feet, the fringes were very distinct and large in proportion; also the smaller spots became more plain, and their rings were seen, though confusedly, from mixing with one another. When the speculum was turned round horizontally, so that its inclination to the incident rays might be greater, the distance of the chart remaining the same (by being drawn round in a circle), the spots and fringes evidently were distended in breadth. I have endeavoured to exhibit the sun's image, as mottled with fringes or rings and spots, in fig. 5.

Observation 4. I placed the speculum behind a screen with a hole in it, through which were let pass the homogeneal rays of the sun, separated by refraction through a prism; this being turned on its axis, the rays which fell on the speculum were changed; the fringes were now of that colour whose rays fell, and when the rays shifted, the fringes contracted or dilated, being broadest in the most flexible rays, and consequently in those whose flexity is greatest.

Observation 5. The direct light falling on the speculum, and part of the reflected light on the horizontal white stage of a very accurate micrometer, I measured the breadth of the fringes, spots, &c. These, with the distance of the speculum from the window and micrometer, and the size of the sun's image, are set down in the following table, all reduced to inches and decimals.

	Inches.	Parts.
Distance of the speculum from the hole in the window-shut - - - -	24.	
Distance of the speculum from the stage of the micrometer - - - -	18.	
Transverse axis of the sun's image - -	2.	6
Conjugate axis of the sun's image - -	1.	4
Length of the oblong dark spot - -		.4
Breadth of the oblong dark spot - -		.0074
Breadth of its first fringe - - -		.0022
Elliptic spot's transverse axis - -		.0116
———— conjugate axis - - -		.0068
Breadth of its first fringe - - -		.0034
Transverse axis of a larger elliptic spot -		.013
Conjugate axis of the same spot - -		.0076

In the image where these measures were taken, there were seven other elliptic spots, a little less and nearly equal; all the others were much smaller and more confused.

Observation 6. On viewing the surface of the speculum attentively in that place whence the rays formed the oblong and first mentioned elliptic spots, I saw a dark but very thin long scratch, and a dark dent, similar in shape to the dark spaces on the image; the dark spot measured less than $\frac{1}{250}$ of an inch; which makes its whole surface to the whole polished surface as 1 to 34225, supposing the former circular or nearly so. All these measures will be found to agree very well, for their smallness and delicacy: thus, the ratio last mentioned is nearly the same which we obtain by comparing the image and the spot; the like may be said of the two spots mentioned in the table, *i. e.* their axes are proportional. I now could produce what spots I pleased, by gently scratching the speculum, or by making lines, dots, &c. with ink, and allowing it to dry; for these last formed convex fibres, which produced coloured fringes as well as the concavities, agreeably to what was deduced *a priori.*

Observation 7. The whole appearance which I have been describing bore such a close and complete resemblance to the fringes made round the shadows of bodies, that the identity of the cause in both cases could not be doubted. In order however to shew it still further, I measured the breadths of two contiguous fringes in several different sets; the measurements agreed very well, and gave the breadth of the first fringe .0056, and of the second .0034; or of the first .0066, and of the second .0034. The ratio of the breadths by the first is 28 to 17; by the second 30 to 17; of which the medium is 29 to 17, and this is precisely the ratio of the two innermost fringes made by a hair, according to Sir ISAAC NEWTON'S measurement: the first being, according to him, $\frac{1}{170}$ of an inch; the second $\frac{1}{290}$ of an inch.* Farther, the two innermost rings made by plates have their diameters (not breadths) in the ratio of $1\frac{11}{16}$ to $2\frac{3}{8}$†, and the distance between the middle of the innermost fringes (made by a hair), on either side the shadow, is to the same distance in the second fringes as $\frac{1}{38}$ to $\frac{2}{47}$; therefore the diameters of the two first rings made by the specks in the speculum, are as $\frac{209}{663}$ to $\frac{627}{1363}$; which ratio differs exceedingly little from that of $1\frac{11}{16}$ to $2\frac{3}{8}$, the ratio of the diameters of rings made by plates, either those called by NEWTON thick, or those which

* Optics, Book 3. Obs. 3.

† Book 2. Parts 1 and 4.

he names thin: for suppose this difference nothing, $2\frac{3}{8} \times \frac{209}{663} = 1\frac{11}{16} \times \frac{627}{1363}$; and the difference between these two products (now stated equal) is not much above $\frac{8}{9}$ in reality.

Observation 8. The last thing worth mentioning in these phænomena was this: I viewed the fringes through a prism, holding the refracting angle upwards, and the axis parallel to that of the dark space; then moving it till the objects ceased descending, I saw in that posture the fringes much more distinct and numerous; for I could now see five with ease, and several more less distinctly. This led me to try more minutely the truth of the 5th proposition, with respect to the number of the fringes surrounding the shadows of bodies in direct light. Having produced a bright set of these by a blackened pin $\frac{1}{25}$ of an inch in diameter, I viewed them through a well made prism, whose refracting angle was only 30°, and held this angle upwards, when the fringes were on the side of the shadow opposite to me; I then moved the prism round on its axis, and when it was in the posture between the ascent and descent of the objects, I was much pleased to see five fringes plainly, and a great number beyond, decreasing in size and brightness till they became too small and confused for sight. In like manner those formed by a double flexion of two bodies, and those made out of homogeneal light, were seen to a much greater number when carefully viewed through the prism. And this experiment I also tried with all the species of fringes by flexion which I could think of.

Observation 9. The same appearances which were occasioned by the metal speculum, might be naturally expected to appear when a glass one was used. But I also found the like rings or fringes of colours and spots in the image beyond the focus of a lens; nor was a very excellent one belonging to a DOLLOND'S telescope free from them. The rings with their dark intervals resembled those floating specks so often observed on the surface of the eye, and called "*muscæ volitantes,*" only that the muscæ are transparent in the middle, because formed by drops of humor: they will, however, be found to be compassed by rings of faint colours, which will become exceedingly vivid if the eyes be shut and slowly opened in the sun's light, so that the humor may be collected; they also appear by reflexion, mixed with the colours described in Phil. Trans. for 1796, p. 268.

Observation 10. The sun shining strongly on the concave metal speculum, placed at such a distance from the hole in the window that it was wholly covered with the light; upon inclining it a little, the image on the chart was bordered on the inside with three fringes similar to those already described; on increasing the inclination these were distended, becoming very bright and beautiful; when the inclination was great, and when it was still increased, another set of colours emerged from the side next the speculum, and was concave to that side. Here I stopped the motion, and the image on both sides of the focus had three sets of fringes, and four fringes in each set; but when viewed through a prism (as before described), the numbers greatly increased, both the fringes and the dark intervals decreasing regularly. The appearance to the naked eye is represented in fig. 6. where A D C being the image, A and C are the sets of fringes at the edges, and B the third set, there being none at E and D the sides, since the light which illuminates these quarters comes not from the edges of the speculum in so great inclinations. I now viewed the surface of the speculum, and saw it, in the place answering to B in the image, covered with fringes exactly corresponding with those at B; and on changing the figure of that part of the speculum's edge between them and the sun, the fringes likewise had their figure altered in the very same way. On moving the speculum farther round, B came nearer to A in the image, according as the fringes on the speculum receded from that side which formed them; and before they vanished alike from the speculum and image, they mixed with the colours at A in the image, and formed in their motion a variety of new and beautiful compound colours; among these I particularly remarked a brown chocolate colour, and various other shades and tinges of brown and purple. Just before the fringes at B appeared, the space between A and C was filled with colours by reflexion, totally different in appearance from the fringes; but I could not examine them so minutely as I wished in this broad image, I therefore made the following experiment.

Observation 11. At the hole in the window-shut I held the speculum, and moved it to such an inclination that the colours by reflexion might be formed in the image; they were much brighter and far more distended than the fringes, and were in every respect like the images by reflexion in the common way, only that the colours were a little better and more regular. They were also seen on the speculum as the third set of fringes had before been in *Obs.* 10.; but by letting the rays fall on the half next the chart, and inclining that half very much, I could produce them, though less distinctly, by a single reflexion. I now held a plain metal speculum so that the rays might be reflected to form a white image on a chart. On inclining the speculum much, I saw the image turn red at the edge; it then became a little distended; and lastly, fringes emerged from it well coloured, and in regular order, with their dark intervals. This may easily be tried by candle-light with a piece of looking-glass, and those who without much trouble would satisfy themselves of the truth of the whole experiment contained in this and the last observation, may easily do it in this way with a concave speculum; but the beauty of the appearance is hereby quite impaired. After this detail it is almost superfluous to add, that the fringes at B, fig. 6. are formed by deflexion from the edge of the speculum next the sun, and then falling on it are reflected to the chart; that the images by reflexion are either formed by the light being decompounded at its first reflexion, and then undergoing a second, or, in other instances, without this second reflexion; and that the other fringes are produced exactly as described above, from the necessary consequences of the theory. I shall only add, that nothing could have been more pleasing to me than the success of this experiment; not only because in itself it was really beautiful from its variety, but also because it was the most peremptory confirmation of what followed from the theory *a priori*, and in that point where the singularity of its consequences most inclined me to doubt its truth.

Let us now attend to several conclusions to which the foregoing observations lead, independently of the propositions (*viz.* the five first) which they were made to examine.

I. We must be immediately struck with the extreme resemblance between the rings surrounding the black spots on the image made by an ill polished speculum, and those produced by thin plates observed by NEWTON; but perhaps the resemblance is still more conspicuous in the colours surrounding the image made by any speculum whatever, and fully described in

Obs. 10. and 11. The only difference in the circumstances is now to be reconciled. The rings surrounding the black spot on the top of a bubble of water, and those also surrounding the spot between two object glasses,* have dark intervals (exactly like those rings I have just now described, and the fringes surrounding the shadows of bodies); but these intervals transmit other fringes of the same nature, though with colours in the reverse order; from which Sir ISAAC NEWTON justly inferred, that at one thickness of a plate the rays were transmitted in rings, and at another reflected in like rings. Now it is evident, that neither reflexibility nor refrangibility will account for either sort of rings, because the plate is far too thin for separating the rays by the latter, and because the colours are in the wrong order for the former; and also because the whole appearance is totally unlike any that refrangibility and reflexibility ever produce. To say that they are formed by the thickness of the plates, is not explaining the thing at all. It is demanded in what way? and indeed we see the like dark intervals and the same fringes formed at a distance from bodies by flexion, where there is no plate through which the rays pass. The state of the case then seems to be this: "when a "phænomenon is produced in a particular combination of cir-"cumstances, and the same phænomenon is also produced in "another combination, where some of the circumstances, be-"fore present, are wanting; we are intitled to conclude that "the latter is the most general case, and must try to resolve "the other into it." In the first place, the order of the colours in the NEWTONIAN rings is just such as flexion would produce; that is, those which are transmitted have the red inner-

* Optics, Book II. P. I.

most, those which are reflected have the red outermost; the former are the colours arranged as they would be by inflexion, the latter as they would be by deflexion; and here by outermost and innermost must be understood relative position only, or position with respect to the thickness of the plate, not of the central spot. Secondly, the thinnest plate makes the broadest ring (the diameter of the rings being in the inverse subduplicate ratio of the plate's thickness); just so is it with fringes by flexion; nearer the body the fringes are broadest, and their diameters increase in the same ratio with the diameters of the rings by plates whose thickness is uniform; each distance from the bending body therefore corresponds with a ring or fringe of a particular breadth, and the alternate distances correspond with the dark intervals: the question then is, what becomes of the light which falls on or passes at these alternate distances? In the case of thin plates, this light is transmitted in other rings; we should therefore be led to think that in the case of the light passing by bodies, it should be at one distance inflected, and at another deflected; and in fact the phænomena agree with this, for fringes are formed by inflexion within the shadows of bodies; they are separated by dark intervals; the fringes and the intervals without the shadow decrease in breadth according to the same law; so that the fringes and intervals within the shadow correspond with the intervals and fringes without, respectively. Nor will this explanation at all affect the theory formerly laid down; it will only (if found consistent with farther induction) change the definite spheres of inflexion and deflexion into alternate spheres. At any rate, the facts here being the same with those described by NEWTON, but in different circumstances, teach us to reconcile the difference, which we have attempted to do, as far as is consistent with strictness; and what we have seen not only entitles us to conclude that the cause is the same, but also inclines us to look for farther light concerning that cause's general operation: and I trust some experiments which I have planned, with an instrument contrived for the purpose of investigating the ratio of the bending power to the distances at which it acts, will finally settle this point.

II. Another conclusion follows from the experiments now related, *viz.* that we see the great importance of having specula for reflectors delicately polished; not only because the more dark imperfections there are on the surface, the more light is lost, and the more colours are produced by flexion (these colours would be mostly mixed and form white in the focus), but also because the smallest scratches or hairs, being polished, produce colours by reflexion, and these diverging irregularly from the point of incidence are never collected into a focus, but tend to confuse the image. Indeed it is wonderful that reflectors do not suffer more from this cause, considering the almost impossibility of avoiding the hairs we speak of: however, that they do actually suffer is proved by experience. I have tried several specula from reflecting telescopes, and found that though they performed very well, from having a good figure, yet from the focus (when they were held in the sun's light) several streaks diverged, and were never corrected; others had the hairs so small, that it was very difficult to perceive the colours produced by them, unless they fell on the eye. Glass concaves were freer from these hairs, but they were much more hurt by dark spots, &c. In general the hairs are so small in well wrought metals, that they do little hurt; but when enlarged by any length of exposure to the light and heat in solar observations, they produce irregularities round the image. Such at least I take to be the explanation of the phænomenon, observed at Paris by M. DE BARROS during the transit of Mercury in 1743, and recorded in Phil. Trans. for 1753. But there is another more serious impediment to the performance of reflectors, and which it is to be feared we have no means of removing. In making the experiments of which the history has been given, on viewing attentively the surface of the speculum, every part of it was seen covered with points of colours, formed by reflexion from the small specular particles of the body. I never saw a speculum free in the least from these, so that the image formed in the focus must be rendered much more dim and confused by them, than it otherwise would be.

III. The last conclusion which may be drawn from these experiments, is a very clear demonstration in confirmation of what was otherwise shewn, concerning the difference between coloured images produced by reflexion, and those made by flexion. This complete diversity is most evident in the experiments with specula, the colours produced by which, in the form of fringes and rings, ought, as well as the others described as images by reflexion in *Obs.* 11, to be the same in appearance with those formed by pins; whereas no two things can be more dissimilar.

It remains to examine the 6th proposition: for this purpose I made the following observations.

Observation 1. Having procured a good specimen of Iceland crystal, I split it into several pieces, and chose one whose surface was best polished. I exposed this to a small cone of the sun's light, and received the reflected rays on a chart; nothing was observable in the image, farther than what happens in reflexion from any other polished body. Some pieces, indeed, doubled and tripled the image, but only such as were rough on the surface, and consequently presented several surfaces to the rays. When smooth and well polished, a single image was all that they formed. The same happened if I viewed a candle, the letters of a book, &c. by reflexion from the Iceland crystal.

Observation 2. I ground a small piece of Iceland crystal round at the edge, and gave it a tolerable polish here and there by rubbing it on looking-glass, and sometimes by a burnisher (it would have been next to impossible to polish it completely). I then placed the polished part in the rays near the hole in the window-shut, and saw the chart illuminated with a great variety of colours by reflexion, irregularly scattered, as described above;* I therefore held the edge in the smoke of a candle and blackened it all over, then rubbed off a very little of the soot, and exposed it again in the rays. I now got a pretty good streak of images by reflexion, in no respect differing from those made in the common way. Nor could I ever produce a double set, or a single set of double images, by any specimen properly prepared, either on a chart by the rays of the sun, or on my eye by those of a candle.

Observation 3. I ground to an even and pretty sharp edge two pieces of Iceland crystal, and placed one in the sun's rays. At some feet distance I viewed the fringes with which its shadow was surrounded, and saw the usual number in the usual order. I then applied the other edge so near that their spheres of flexion might interfere in the manner before described,† and

* Phil. Trans. for 1796, p. 270. † Ibid. p. 256.

thus the fringes might be distended; still no uncommon appearance took place; nor when other bodies were used with one edge of crystal, nor when polished pieces of different shapes and sizes were employed. The same things happened by candlelight, and also by refracted homogeneal light. In short, I repeated most of my experiments on flexion with Iceland crystal, and found that they were not changed at all in their results.

Observation 4. Having great reason to doubt the accuracy of an experiment tried by Mr. MARTIN, and in which, by a prism of Iceland crystal, he thought six spectra were produced, I was not much surprised to find, that a prism made by polishing the two contiguous sides of a parallelopiped of Iceland crystal produced only two equal and parallel images, in whatever position the prism was held. But though, from the imperfect account which MARTIN gives of this appearance, it was impossible to discover his error from his own words, yet chance led me to find out what most probably had misled him; for looking at a candle through the opposite sides of a specimen of Iceland crystal, I saw four coloured images (besides two white ones) of the candle. These were parallel to one another, and in the same line, as represented in fig. 7. where E represents the two regular images, G and F two others coloured very irregularly, and changing colours as the crystal was moved horizontally, sometimes appearing each two-fold, and its two parts of the same or different colours. A and B were regularly coloured, and evidently formed by refraction, and reflected back from the sides. On turning the crystal round, so that its position might be at right angles to its former position, the images moved round, and were in a line perpendicular to AB, as CD. All this happened in like manner in the sun's rays; and on viewing the specimen, I found it was split and broken in the inside, so as to be lamellated in directions parallel, or nearly so, to the sides; on these plates there were colours in the day time by the light of the clouds: and it is evident that it was these fractures which caused the irregular images G and F, for other specimens shewed no such appearance. I would therefore conclude, that Iceland crystal separates the rays of light into two equal and similar beams by refraction, and no more.*

As to the cause of the separation, I would hope that some information may be obtained from the experiments I have related: for from them it appears, that this singular property extends no farther than to the action of the particles of Iceland crystal on the particles of light in their passage through the body; and from *Obs.* 4. it is farther evident, that it is not owing to the different properties which Sir ISAAC NEWTON conjectures the different sides of rays to have; for if this were the cause, when the rays pass between two pieces of crystal, an uncommon flexion would take place. Lastly, another fact (mis-stated by BARTOLIN † and ROME' DE LISLE) ‡ shews, that the unusual refraction takes place within the body, while the

* Mentioning this account of MARTIN's mistake to Professor ROBISON, of this university, I was pleased to find a full confirmation of it. It was that excellent philosopher who shewed the appearance to MARTIN; but he not understanding it, took the liberty of publishing the observation as his own, after first mangling it in such a way as to give him, indeed, some pretext for the appropriation. The Professor merely mentioned his having communicated it to Mr. MARTIN; how the latter used it we have shewn in the text: the theory of the appearance is somewhat more complex than appears by my observations. I was therefore pleased to find that the Professor was in possession of the true account of it; which is, however, foreign to the present purpose.

† *Experimenta Crystalli,* abridged in Phil. Trans. Vol. V.

‡ *Cristallographie,* Vol. I.

other, like all refractions, begins at some small distance before the rays enter.

The writers just now quoted assert, that if the crystal be turned round so as to assume different positions, there is one in which the line appears single. The fact is very different, as follows. When the crystal is turned round, the unusual image moves round also, and appears above the other; the greatest distance between the two images is when they are parallel to the line bisecting one of the acute angles of the parallelogram through which the rays pass; when the images are parallel to a line bisecting one of the obtuse angles they seem to coincide; but they will be found, if observed more nearly, to coincide only in part. Thus (in fig. 9.) A B and C D are the two black lines at their greater distance, and their extremities A and C, B and D are even with one another; that is, the figure formed by joining A and C, B and D is a rectangle. But in the other case (fig. 8.) A B and C D being the lines, the space C B (equal in depth of colour to the real line on the paper), is the only place in which the lines (or images) coincide. The space A C of A B, and B D of C D are still of a light colour, and the two lines A B and C D do not coincide, by the difference A C or B D; that is, by the difference O P, the greatest distance (fig. 9.). In short, the unusual line's extremities describe circles (in the motion of the crystal) whose centres are the extremities of the usual line, and whose radii are the greatest distance. From this it appears evident, that the unusual image is formed within the crystal, and turns round with the side of the particle, or rhomboidal mass of particles, which forms it. Farther, it is evident that the power which produces the division of the incident light, is very different from common refraction, from the motion, and the effect taking place when the rays are perpendicular. Suspecting, therefore, that it might be owing to flexion, I made the following experiment, which undeceived me.

Observation 5. I covered one side of a specimen of Iceland crystal, three inches deep, with black paper, all but a small space $\frac{1}{30}$ of an inch in diameter, and placed a screen with a hole of the same size, six feet from the hole in the window-shut of my darkened chamber, so that the rays might pass through the screen, and fall on a prism placed behind, to refract them into a small and well defined spectrum, which was received on a chart two feet from the prism. This spectrum I viewed through the crystal, and of course saw it doubled; but the two images were by no means parallel; the unusual one inclined to the red, and its violet was considerably farther removed from the violet of the other, than the two reds were from one another; which shews, that the most refrangible or least flexible rays were farthest moved from their course by the unusual action, and proves this to be very different from flexion.*

From all these observations this conclusion follows; that the remarkable phænomenon in question arises from an action very different from either refraction or flexion; and whose nature well deserves to be farther considered. It may possibly belong to the particles of Iceland crystal, and in a degree to those of rock crystal, from the form and angles of the rhomboidal masses, whereof these bodies are composed. Nor is this conjecture at all disproved by the fact that glass shaped like these bodies wants the property; for we cannot mould the *particles* of glass, we can only shape large *masses* of these; whereas we

* When a candle or line is viewed through a deep specimen, the unusual image is tinged with colours.

cannot doubt that in crystallization the smallest masses assume the same form with the largest: but then other hypotheses may perhaps also account for the fact, such as atmospheres, electric fluid, &c. &c.; so that till farther observations are made we ought to rest contented with barely suggesting the query. In the mean time, reserving to a future opportunity some inquiries concerning the chemical properties of light, and the nature of the forces which bodies exert on it internally, I conclude at present with a short summary of propositions. But first, may I be permitted to express a hope, that what has been already attempted (and for which no praise can be claimed farther than what is due to attentive observation, according to the rules of the immortal BACON), may prove acceptable to such as love to admire the beautiful regularity of nature, or more particularly to trace her operations, as exhibited in one of the most pleasing, most important, and most unerring walks of physical science.

Proposition I. The sun's light consists of parts which differ in degree of refrangity, reflexity, inflexity, and deflexity; and the rays which are most flexible have also the greatest refrangity, reflexity, and flexity; or are most *refrangile*, *reflexile*, and *flexile*.

Proposition II. Rays of compound light passing through the spheres of flexion and falling on the bending body, are not separated by their flexibility, either in their approach to, or return from the body.

Proposition III. The colours of thin and those of thick plates are precisely of the same nature; differing only in the thickness of the plate which forms them.

Proposition IV. The colours of plates are caused by flexion, and may be produced without any transmission whatever.

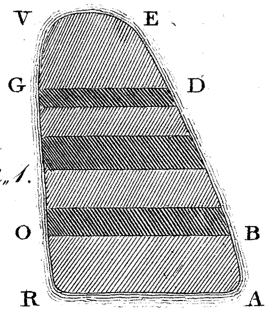
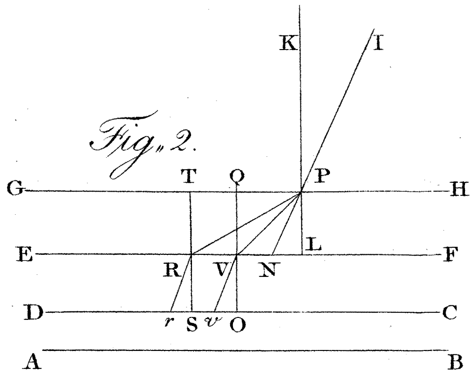


Fig. 1.



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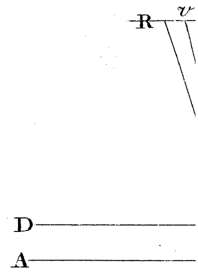

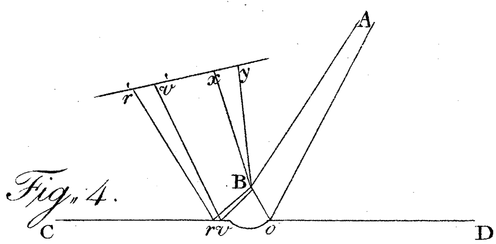

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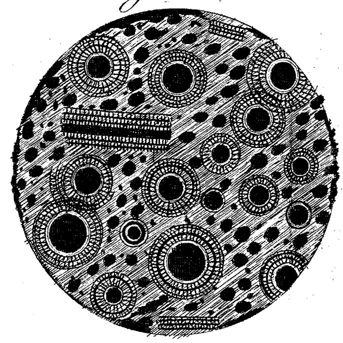


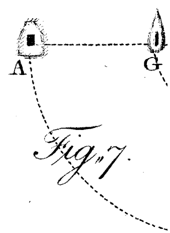
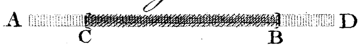


Fig. 7.

Fig. 8.



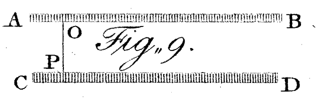
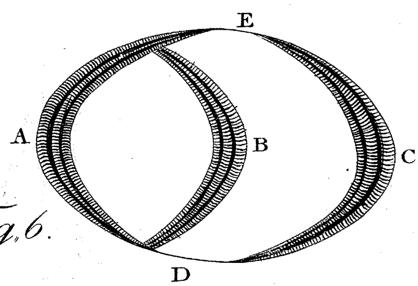


Fig. 9.



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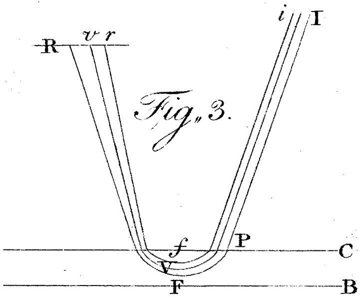
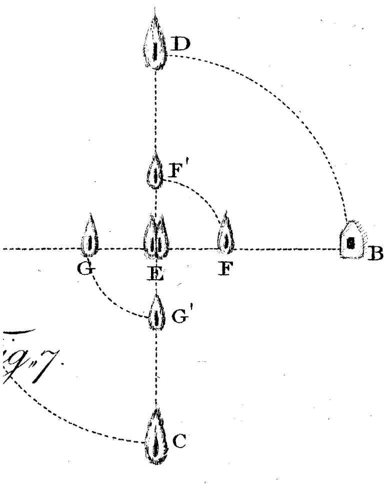


Fig. 3.



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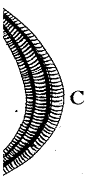


Basire Sc.

Proposition V. All the consequences deducible from the theory *a priori* are found to follow in fact.

Proposition VI. The common fringes by flexion (called hitherto the "*three fringes*"), are found to be as numerous as the others.

Proposition VII. The unusual image by Iceland crystal is caused by some power inherent in its particles, different from refraction, reflexion, and flexion.

Proposition VIII. This power resembles refraction in its degree of action on different rays; but it resembles flexion within the body, in not taking place at a distance from it, in acting as well on perpendicular as on oblique rays, and in its sphere or space of exertion moving with the particles which it attends.

Fig. 1.

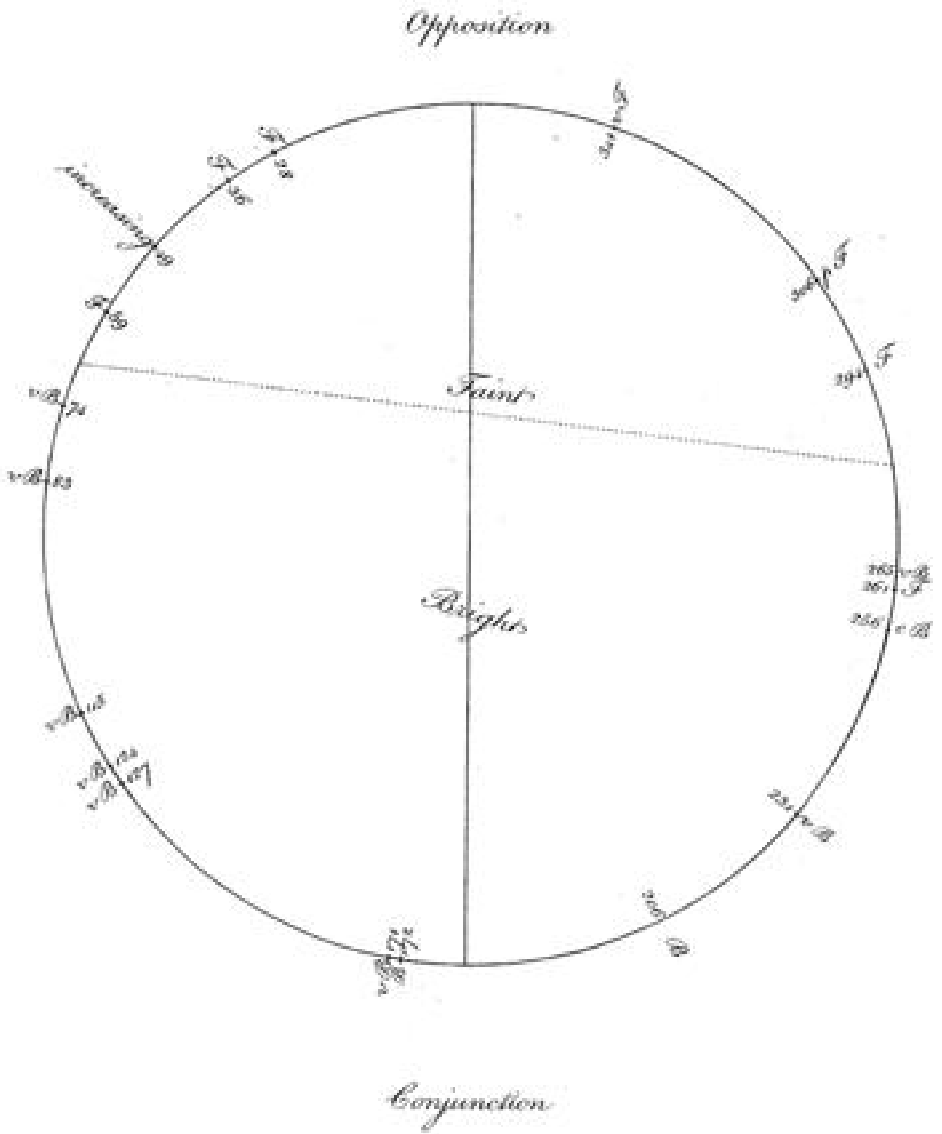
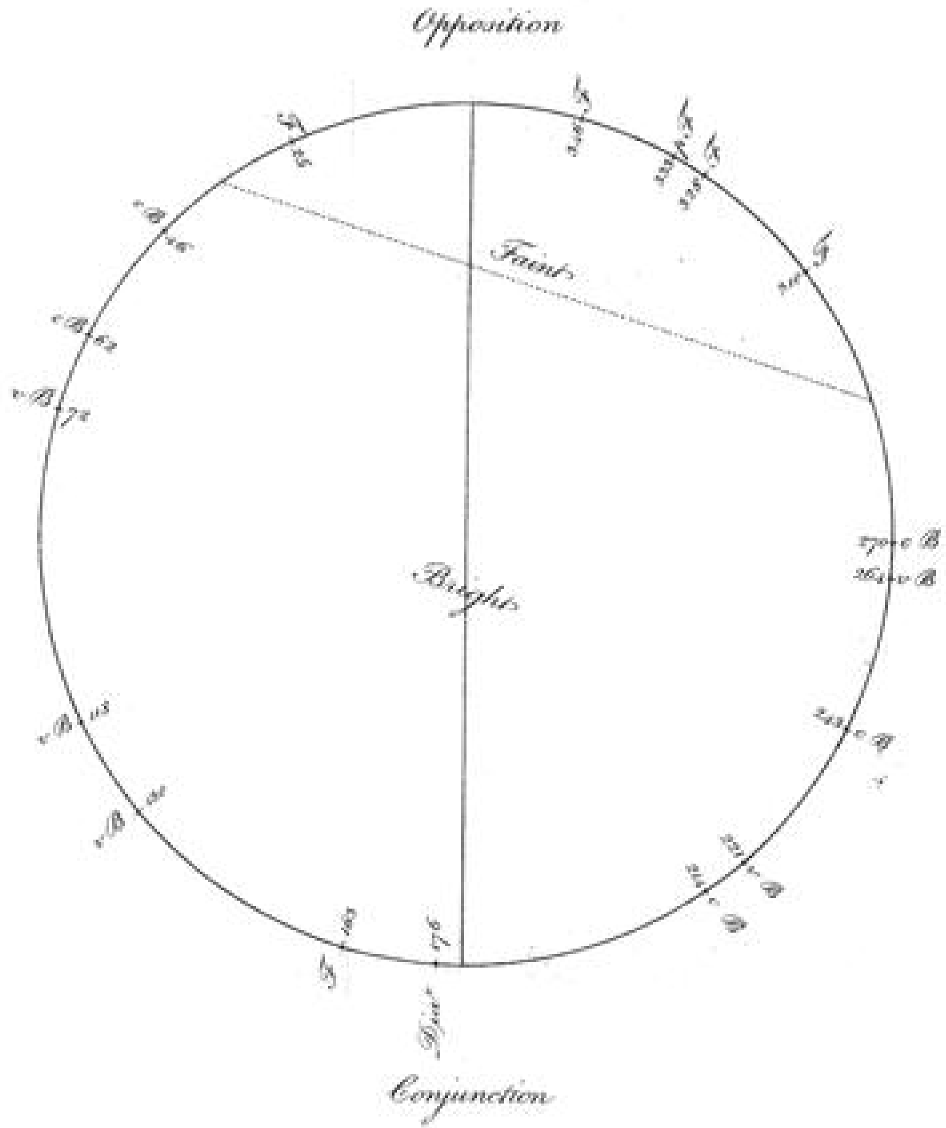


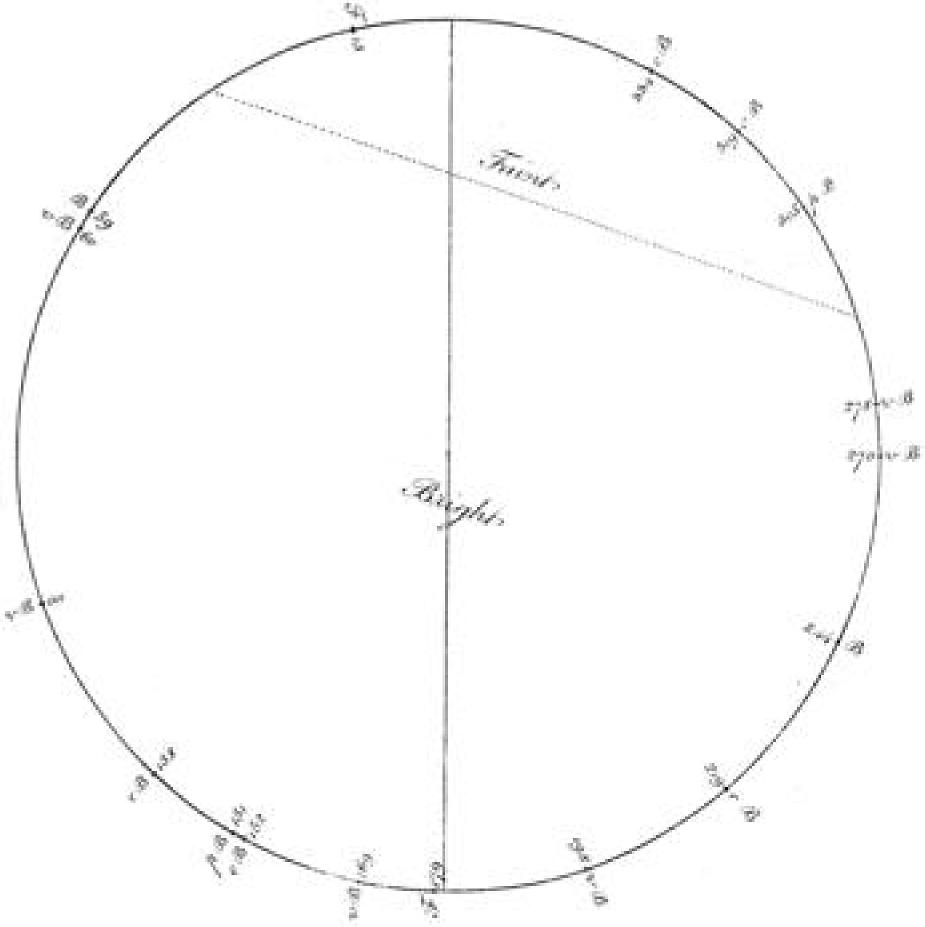
Fig. 2.



Basire sc.

Fig. 3.

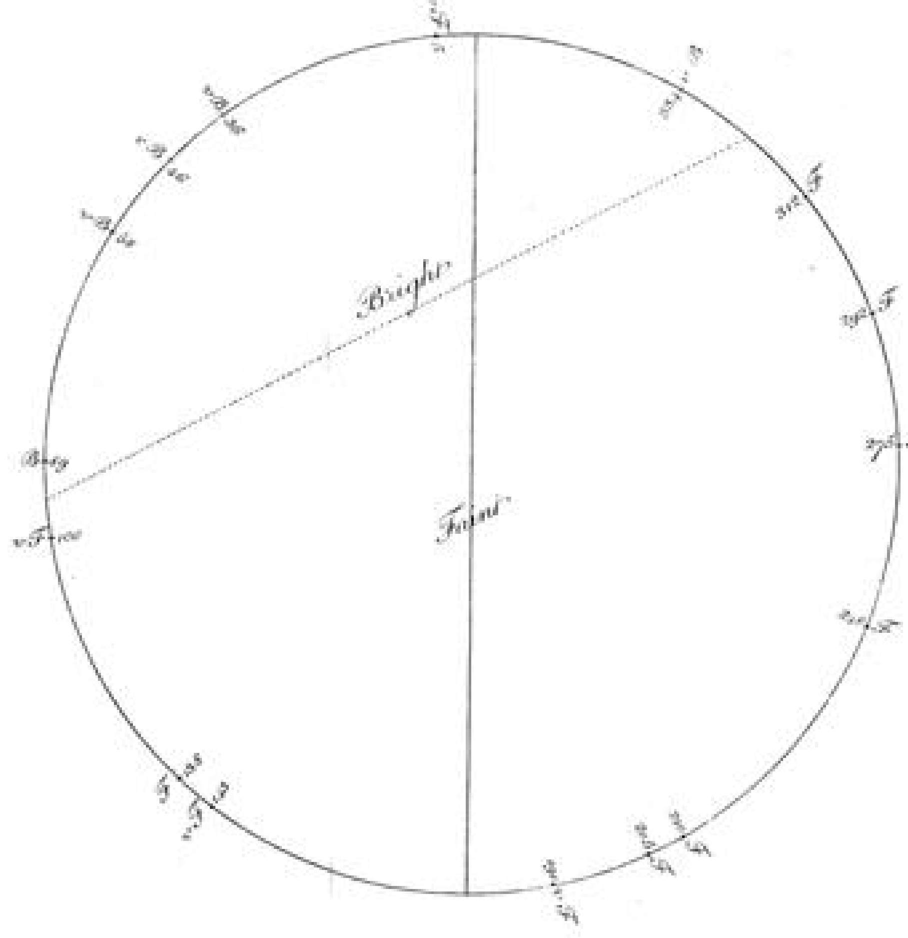
Opposition



Conjunction

Fig. 4.

Opposition



Conjunction

Fig. 1.

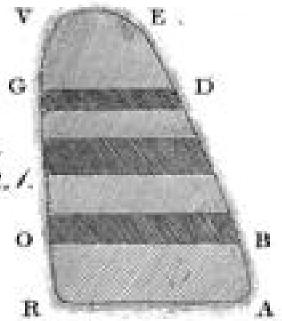


Fig. 2.

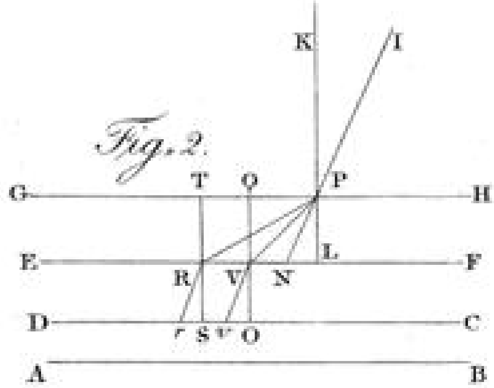


Fig. 3.

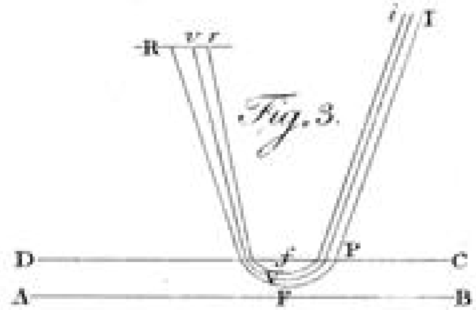


Fig. 4.

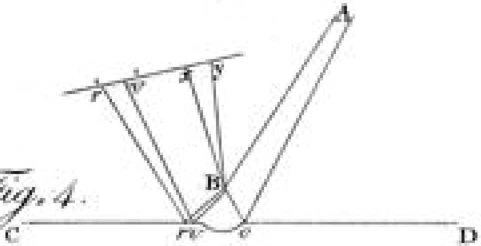


Fig. 5.

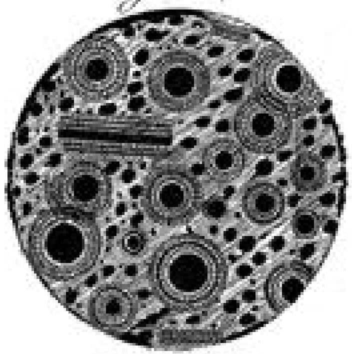


Fig. 7.

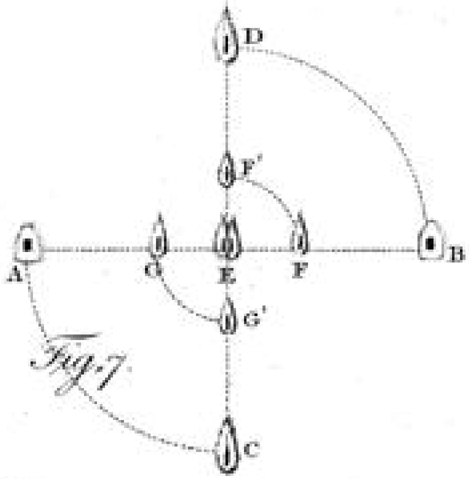


Fig. 8.

Fig. 9.

Fig. 6.