

XCVIII. *An Account of some Experiments concerning the different Refrangibility of Light. By Mr. John Dollond. With a Letter from James Short, M. A. F. R. S. Acad. Reg. Suec. Soc.*

*To the Rev. Dr. Birch, Secret. R. S.*

Dear Sir,

Read June 8, 1758. **I** Have received the inclosed paper from Mr. Dollond, which he desires may be laid before the Royal Society. It contains the theory of correcting the errors arising from the different refrangibility of the rays of light in the object-glasses of refracting telescopes; and I have found, upon examination, that telescopes made according to this theory are intirely free from colours, and are as distinct as reflecting telescopes. I am,

Dear Sir,

Surrey-street,  
8th June, 1758.

Your most obedient humble Servant,

Ja. Short.

**I**T is well known, that a ray of light, refracted by passing thro' mediums of different densities, is at the same time proportionally divided or spread into a number of parts, commonly called homogeneal rays, each of a different colour; and that these, after refraction, proceed diverging: a proof, that they are differently

differently refracted, and that light consists of parts that differ in degrees of refrangibility.

Every ray of light passing from a rarer into a denser medium, is refracted towards the perpendicular; but from a denser into a rarer one, from the perpendicular; and the sines of the angles of incidence and refraction are in a given ratio. But light consisting of parts, which are differently refrangible, each part of an original or compound ray has a ratio peculiar to itself; and therefore the more a heterogeneous ray is refracted, the more will the colours diverge, since the ratios of the sines of the homogeneous rays are constant; and equal refractions produce equal divergencies.

That this is the case when light is refracted by one given medium only, as suppose any particular sort of glass, is out of all dispute, being indeed self-evident; but that the divergency of the colours will be the same under equal refractions, whatsoever mediums the light may be refracted by, tho' generally supposed, does not appear quite so clearly.

However, as no medium is known, which will refract light without diverging the colours, and as difference of refrangibility seems thence to be a property inherent in light itself, Opticians have, upon that consideration, concluded, that equal refractions must produce equal divergencies in every sort of medium: whence it should also follow, that equal and contrary refractions must not only destroy each other, but that the divergency of the colours from one refraction would likewise be corrected by the other; and there could be no possibility of producing any such thing as refraction, which would not be affected  
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by the different refrangibility of light ; or, in other words, that however a ray of light might be refracted backwards and forwards by different mediums, as water, glass, &c. provided it was so done, that the emergent ray should be parallel to the incident one, it would ever after be white ; and, conversely, if it should come out inclined to the incident, it would diverge, and ever after be coloured. From which it was natural to infer, that all spherical object-glasses of telescopes must be equally affected by the different refrangibility of light, in proportion to their apertures, whatever material they may be formed of.

But it seems worthy of consideration, that notwithstanding this notion has been generally adopted as an incontestable truth, yet it does not seem to have been hitherto so confirmed by evident experiment, as the nature of so important a matter justly demands ; and this it was that determined me to attempt putting the thing to issue by the following experiment.

I cemented together two plates of parallel glass at their edges, so as to form a prismatic or wedge-like vessel, when stopped at the ends or bases ; and its edge being turned downwards, I placed therein a glass prism with one of its edges upwards, and filled up the vacancy with clear water : thus the refraction of the prism was contrived to be contrary to that of the water, so that a ray of light transmitted thro' both these refracting mediums would be refracted by the difference only between the two refractions. Wherefore, as I found the water to refract more or less than the glass prism, I diminished

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or increased the angle between the glass plates, till I found the two contrary refractions to be equal; which I discovered by viewing an object thro' this double prism; which, when it appeared neither raised nor depressed, I was satisfied, that the refractions were equal, and that the emergent rays were parallel to the incident.

Now, according to the prevailing opinion, the object should have appeared thro' this double prism quite of its natural colour; for if the difference of refrangibility had been equal in the two equal refractions, they would have rectified each other: but the experiment fully proved the fallacy of this received opinion, by shewing the divergency of the light by the prism to be almost double of that by the water; for the object, tho' not at all refracted, was yet as much infected with prismatic colours, as if it had been seen thro' a glass wedge only, whose refracting angle was near 30 degrees.

*N. B.* This experiment will be readily perceived to be the same as that which Sir Isaac Newton mentions\*; but how it comes to differ so very remarkably in the result, I shall not take upon me to account for; but will only add, that I used all possible precaution and care in the process, and that I keep the apparatus by me to evince the truth of what I write, whenever I may be properly required so to do.

I plainly saw then, that if the refracting angle of the water-vessel could have admitted of a sufficient

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\* Book I. Part ii. Prop. 3. Experiment 8. of his Optics.

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increase, the divergency of the coloured rays would have been greatly diminished, or intirely rectified; and there would have been a very great refraction without colour, as now I had a great discolouring without refraction: but the inconveniency of so large an angle, as that of the vessel must have been, to bring the light to an equal divergency with that of the glass prism, whose angle was about 60 degrees, made it necessary to try some experiments of the same kind, by smaller angles.

I ground a wedge of common plate glass to an angle of somewhat less than 9 degrees, which refracted the mean rays about 5 degrees. I then made a wedge-like vessel, as in the former experiment, and filling it with water, managed it so, that it refracted equally with the glass wedge; or, in other words, the difference of their refractions was nothing, and objects viewed thro' them appeared neither raised nor depressed. This was done with an intent to observe the same thing over again in these small angles, which I had seen in the prism: and it appeared indeed the same in proportion, or as near as I could judge; for notwithstanding the refractions were here also equal, yet the divergency of the colours by the glass was vastly greater than that by the water; for objects seen by these two refractions were very much discoloured. Now this was a demonstration, that the divergency of the light, by the different refrangibility, was far from being equal in these two refractions. I also saw, from the position of the colours, that the excess of divergency was in the glass; so that I increased the angle of the water-wedge, by different trials, till the divergency of the

light by the water was equal to that by the glass; that is, till the object, tho' considerably refracted, by the excess of the refraction of the water, appeared nevertheless quite free from any colours proceeding from the different refrangibility of light; and, as near as I could then measure, the refraction by the water was about  $\frac{1}{4}$  of that by the glass. Indeed I was not very exact in taking the measures, because my business was not at that time about the proportions, so much as to shew, that the divergency of the colours, by different substances, was by no means in proportion to the refractions; and that there was a possibility of refraction without any divergency of the light at all.

Having, about the beginning of the year 1757, tried these experiments, I soon after set about grinding telescopic object-glasses upon the new principles of refractions, which I had gathered from them; which object-glasses were compounded of two spherical glasses with water between them. These glasses I had the satisfaction to find, as I had expected, free from the errors arising from the different refrangibility of light: for the refractions, by which the rays were brought to a focus, were every-where the differences between two contrary refractions, in the same manner, and in the same proportions, as in the experiment with the wedges.

However, the images formed at the foci of these object-glasses were still very far from being so distinct as might have been expected from the removal of so great a disturbance; and yet it was not very difficult to guess at the reason, when I considered, that the radii of the spherical surfaces of those glasses  
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were required to be so short, in order to make the refractions in the required proportions, that they must produce aberrations, or errors, in the image, as great, or greater, than those from the different refrangibility of light. And therefore, seeing no method of getting over that difficulty, I gave up all hopes of succeeding in that way.

And yet, as these experiments clearly proved, that different substances diverged the light very differently, in proportion to the refraction ; I began to suspect, that such variety might possibly be found in different sorts of glass, especially as experience had already shewn, that some made much better object-glasses, in the usual way, than others : and as no satisfactory cause had as yet been assigned for such difference, there was great reason to presume, that it might be owing to the different divergency of the light by their refractions.

Wherefore, the next business to be undertaken, was to grind wedges of different kinds of glass, and apply them together, so that the refractions might be made in contrary directions, in order to discover, as in the foregoing experiments, whether the refraction and divergency of the colours would vanish together. But a considerable time elapsed before I could set about that work ; for tho' I was determined to try it at my leisure, for satisfying my own curiosity, yet I did not expect to meet with a difference sufficient to give room for any great improvement of telescopes ; so that it was not till the latter end of the year that I undertook it, when my first trials convinced me, that this business really deserved my utmost attention and application.

I discovered a difference, far beyond my hopes, in the refractive qualities of different kinds of glass, with respect to their divergency of colours. The yellow or straw-coloured foreign sort, commonly called Venice glass, and the English crown glass, are very near alike in that respect, tho' in general the crown glass seems to diverge the light rather the least of the two. The common plate glass made in England diverges more; and the white crystal or flint English glass, as it is called, most of all.

It was not now my business to examine into the particular qualities of every kind of glass that I could come at, much less to amuse myself with conjectures about the cause, but to fix upon such two sorts as their difference was the greatest; which I soon found to be the crown, and the white flint or crystal. I therefore ground a wedge of white flint of about 25 degrees, and another of crown of about 29 degrees, which refracted nearly alike; but their divergency of the colours was very different. I then ground several others of crown to different angles, till I got one, which was equal, with respect to the divergency of the light, to that in the white flint: for when they were put together, so as to refract in contrary directions, the refracted light was intirely free from colour. Then measuring the refractions of each wedge, I found that of the white glass to be to that of the crown nearly as 2 to 3; and this proportion would hold very nearly in all small angles. Wherefore any two wedges made in this proportion, and applied together, so as to refract in a contrary direction, would refract the light without any difference of refrangibility.

To make therefore two spherical glasses, that shall refract the light in contrary directions, it is easy to understand, that one must be concave, and the other convex; and as the rays are to converge to a real focus, the excess of refraction must evidently be in the convex; and as the convex is to refract most, it appears from the experiment, that it must be made with crown glass, and the concave with white flint glass.

And further, as the refractions of spherical glasses are in an inverse ratio of their focal distances; it follows, that the focal distances of the two glasses should be inversely as the ratio's of the fractions of the wedges: for being thus proportioned, every ray of light, that passes thro' this combined glass, at whatever distance it may pass from its axe, will constantly be refracted, by the difference between two contrary refractions, in the proportion required; and therefore the different refrangibility of the light will be intirely removed.

Having thus got rid of the principal cause of the imperfection of refracting telescopes, there seemed to be nothing more to do, but to go to work upon this principle: but I had not made many attempts, before I found, that the removal of one impediment had introduced another equally detrimental (the same as I had before found in two glasses with water between them): for the two glasses, that were to be combined together, were the segments of very deep spheres; and therefore the aberrations from the spherical surfaces became very considerable, and greatly disturbed the distinctness of the image. Tho' this appeared at first a very great difficulty, yet I was

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not long without hopes of a remedy: for considering, the surfaces of spherical glasses admit of great variations, tho' the focal distance be limited, and that by these variations their aberrations may be made more or less, almost at pleasure; I plainly saw the possibility of making the aberrations of any two glasses equal; and as in this case the refractions of the two glasses were contrary to each other, their aberrations, being equal, would intirely vanish.

And thus, at last, I obtained a perfect theory for making object-glasses, to the apertures of which I could scarce conceive any limits: for if the practice could come up to the theory, they must certainly admit of very extensive ones, and of course bear very great magnifying powers.

But the difficulties attending the practice are very considerable. In the first place, the focal distances, as well as the particular surfaces, must be very nicely proportioned to the densities or refracting powers of the glasses; which are very apt to vary in the same sort of glass made at different times. Secondly, the centres of the two glasses must be placed truly on the common axis of the telescope, otherwise the desired effect will be in a great measure destroyed. Add to these, that there are four surfaces to be wrought perfectly spherical; and any person, but moderately practised in optical operations, will allow, that there must be the greatest accuracy throughout the whole work.

Notwithstanding so many difficulties, as I have enumerated, I have, after numerous trials, and a resolute perseverance, brought the matter at last to such an issue, that I can construct refracting telescopes,

scopes, with such apertures and magnifying powers, under limited lengths, as, in the opinion of the best and undeniable judges, who have experienced them, far exceed any thing that has been hitherto produced, as representing objects with great distinctness, and in their true colours.

John Dollond.

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*XCIX. An Account of some extraordinary Effects arising from Convulsions; being Part of a Letter to John Huxham, M.D. and F.R.S. from William Watson, M.D. F. R. S.*

6 June, 1758.

Read June 15.  
1758.

**I**N the month of January 1757, I was concerned for a young gentleman, who, if the number, continuance, and frequency of their returns, be considered, suffered the most violent and severe convulsions I ever knew. At some times the muscular spasms were general; at other times single muscles only, or a number of them, subservient to some particular purpose in the animal oeconomy, were affected. And such was the peculiarity of this case, that after and in proportion as any single muscle, or any determined number of muscles, had been in a state of spasm, a paralytic inability succeeded to those muscles, which very much disordered and impaired, and several times even for no small continuance prevented the patient