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.

IT 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 heterogene ray is refracted, the more will the colours diverge, ſince the ratios of the ſines of the homogene 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 by

by the different refrangibilty 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

or

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

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

increaſe

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 $\frac{5}{4}$ 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 were

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

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 teleleſcopes, 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

not

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.

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. IN the month of January 1757, I was concerned for a young gentlewoman, 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