

Letter from Newton to Henry Oldenburg, dated 15 February 1675/6

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Feb 15. 167 $\frac{5}{6}$. Cambridge

Sir

I thank you for giving me notice of the objection which some have made. If I understand it right they mean that colour may proceed from the different velocities which æthereall pulses or rays of light may have as they come immediately from the Sun. But if this be their meaning, they propound not an objection but an Hypothesis to explain my Theory. For the better understanding of this I shall desire you to consider that I put not the different refrangibility of rays to be the internal or essential cause of colours, but only the means whereby rays of different colours are separated. Neither do I say what is that cause, either of colour or of different refrangibility, but leave these to be explained by Hypotheses, & only say that rays which differ in colour, differ also in refrangibility, & that different refrangibility conduces to the production of colour no other way then by causing a different refraction & thereby a separation of those rays which had different colours before, but could not appear in their own colours till they were separated. Suppose red & blew pouders (as Minium & Bise) were equally mixed, the compound would be neither a good red nor a good blew but a midling dirty colour. Suppose further this mixture was put into water, & after the water had been well stirred, the pouders left to subside; if the red was much more ponderous then the blew, it would subside fastest & leave most of the blew to subside after it; & by consequence the heape would appear red at bottom & blew at top, & of intermediate colours between. Here then are various colours produced out of a dirty colour by means of different gravity, & yet that different gravity was not the internal cause of those colours, but only the cause of the separation of the particles of several colours. And so it is in the production of colours by the Prism: the different refrangibility of rays is no otherwise the cause of colour in this case then the different gravity of the pouders was in the other; it only causes a divers refraction of the rays originally qualified to exhibit divers colours, & by that divers refraction they are separated & when separated they must needs exhibit each their own colours which they could not do while mixed. Had I supposed different refrangibility the internal cause of colours, it would have been strangely precarious, & scarcely intelligible; but to make it only the cause of separation of rays indowed with different colours is nothing but experiment, & all that I have asserted in my writings. In like manner where I make different reflexibility the cause of colours (as in the case of thin transparent plates) I say not that it is their internal cause but only the means of their separation. For I apprehend that all the Phænomena of colours in the world result from nothing but separations or mixtures of difform rays & that different refrangibility & reflexibility are only the means by which those separations or mixtures are made.

This being apprehended, I presume you will easily see that you have <51v> not sent me an objection but only an Hypothesis to explain my Theory by. For to suppose different velocities of the rays the principle of colour

is only to assign a cause of the the different colours which rays are originally disposed to exhibit & do exhibit when separated by different refractions. And though this should be the true essential cause of those different colours yet it hinders not but that the different refrangibility of the rays may be their accidental cause by making a separation of pulses of different swiftness. Yea so far is this Hypothesis from contradicting me that if it be supposed it infers all my Theory. For if it be true, then is the sun's light an aggregate of heterogeneal rays such are originally disposed to exhibit various colours: then is the whiteness of that light a mixture of those colours, being the result of the mixture of those unequal colorific motions: then is there nothing requisite for the production of colours but a separation of these rays so that the swiftest may go to one place by themselves & the slowest to another by themselves, or one sort be stifled & another remain: then must all the phenomena of colours proceed from the separations of these rays of unequal swiftness because while they continue blended together, as in the sun's original light, they can exhibit no other colour but white: & lastly; then must various refrangibility & reflexivity be the instrumental causes of the Phenomena of colour, those two being the proper means whereby difform rays are separated.

Were I to apply this Hypothesis to my notions I would say therefore that the slowest pulses being weakest are more easily turned out of the way by any refracting superficies then the swiftest, & so cæteris paribus are more refracted: and that the Prism by refracting them more separates them from the swiftest, & then they being freed from the alloy of o{n}e another strike the sence distinctly each with their own motions apart & so beget sensations of colour different both from one another & from that which they begat while mixed together; suppose the swiftest the strongest colour, red; and the slowest the weakest, blew.

To all this I might add concerning the different swiftness of rays that I my self have formerly applyed it to my notions in mentioning other Hypotheses, as you may see in my answer to M^r Hook sect. 4, & I think also in the Hypothesis I lately sent you. I say I applyed it in other Hypotheses; for in this of M^r Hook I think it is much more natural to suppose the pulses equally swift & to differ only in bigness, because it is so in the air, & the laws of undulation are without doubt the same in æther that they are in air.

Having thus answered, as I conceive, your objection in particular; I shall now for a conclusion remind you of what I have formerly said in general to the same purpose: so that I may at once cut off all objections that may be raised for the future either from this or any other Hypothesis whatever. If you consider what I said both in my second letter to P. Pardies & in my answer to M^r Hook sect 4. concerning the application of all Hypotheses to my Theory you may thence gather this general rule. That in any Hypothesis whence the rays may be supposed to have any originall diversities, whether as to size or figure or motion or force or quality or any thing els imaginable which may suffice to difference those rays in colour & refrangibility, there is no need to seek for other causes if these effects then those original diversities. This rule being laid down, I argue thus. In any <52r> Hypothesis whatever, light as it comes from the Sun must be supposed either homogeneal or heterogeneal. If the last, then is that Hypothesis comprehended in this general rule & so cannot be against me: if the first then must refractions have a power to modify light so as to change it's colorifick qualification & refrangibility; which is against experience.

Since the writing of this I receivd your other letter. I thank you for your account of M^r Berchhenshaw's scale of Musick though I have not so much skill in that science as to understand it well. If you should register the papers in your hand before you return them, I would desire you to leave out the last paragraph of the Hypothesis where I mention M^r Hook & Grimaldo together: but since you are to receive those papers again (that of the observations at least; for the Hypothesis I am more inclind to suppress,) I suppose it will not be necessary that you should put your self to the trouble of registering them.

I remain

Sir

Your humble Servant

Is. Newton

Answ. March. 1. 75/6.

For Henry Oldenburg Esquire
at his house about the middle
of the Old Pall-mall in

Westminster

London