

Letter from Newton to Henry Oldenburg, 6 July 1672

Author: Isaac Newton

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< insertion from the bottom of f 20v >

An Extract of M^r Newtons letter to M. Oldenburg concerning the genuine method of determining the Truth of his Doctrine of Light and Colors.

< text from f 19r resumes >

Stoake July 6^t. 1672.

Sir

In {your}{the} inquiry which in yours of June the 25^t you propound in these words: Whether a physicall point in a Glasse may not by the diversity of the pores & angles in it cause in the rays falling thereon such really different though seemingly equall refractions, that thence may proceed those severall distinct colours which in my Doctrine are esteemed to proceed from the aggregate if the rays if light? I know not what to understand by really different though seemingly equall refractions. For if you meane those different refractions from whence I denominate Light unequally refrangible, their differences are so great that they are far from being seemingly equall. And I apprehend not what other differences you should meane (if there be any other) since there is so constant & strict an Analogy between these & the severall species of colours. However since you suppose those unequall refractions to proceed from the diversity of pores & angles in the Glass, they must be comprehended under the contingent irregularities which I have already disproved in my Answer to M^r Hook & P. Pardies. And further if colours were originated from refractions as is supposed in your inquiry, then all colours would be changeable by refractions, contrary to what I find by experience. From either of these two heads, your inquiry is determined negatively, which if you think requisite I shall further explain hereafter.

philosophical Transactions N. 85. p. 5004 — In the meane while give me leave to insinuate that I cannot think it effectuall for determining truth to examin the severall ways by which Phænomena may be explained, unlesse where there can be a perfect enumeration of all those ways. You know the proper Method for inquiring after the properties of things is to deduce them from Experiments. And I told you that the Theory which I propounded was evinced to me, not by inferring tis thus because not otherwise, that is not by deducing it onely from a confutation of contrary suppositions, but by deriving it from Experiments concluding positively & directly. The way therefore to examin it is by considering whether the experiments which I propound do prove those parts of the Theory to which they are applyed, or by prosecuting other experiments which the Theory may suggest for its examination. And this I would have done in a due Method; the Laws of Refraction being thoroughly inquired into & determined before the nature of colours be taken into consideration. It may not be amiss to proceed according to the series of these Queries: The decision of which I could wish to be stated, & the events declared by those that <19v> may have the curiosity to examin them.

1. Whether rays that are alike incident on the same Medium have unequall refractions, & how great are the inequalities of their refractions at any incidence?
2. What is the law according to which each ray is more or lesse refracted, whether it be that the same ray is ever refracted according to the same ratio of the sines of incidence & refraction; & divers rays, according to divers ratios; Or that the refraction of each ray is greater or lesse without any certain rule? That is, whether each ray have a certain degree of refrangibility according to which its refraction is performed, or is refracted without that regularity?
3. Whether rays which are indued with particular degrees of refrangibility, when they are by any meanes separated, have particular colours constantly belonging to them: viz, the least refrangible, scarlet; the most refrangible, deep violet; the middle, Sea-green; & others, other colours? And on the contrary?
4. Whether the colour of any sort of rays apart may be changed by refraction?
5. Whether colours by coalescing do really change one another to produce a new colour, or produce it by mixing onely?
6. Whether a due mixture of rays, indued with all variety of colours, produces light perfectly like that of the Sun, & which hath all the same properties & exhibits the same Phænomena?

✦ < insertion from lower down f 20v > Between the 6^t & 8th Query you may insert this in the 7th place

✦ [7. Whether the component colours of any mixture be really changed or onely separated, when out of that mixture various colours are again produced by refraction?]

< text from f 19v resumes >

8. Whether there be any other colours produced by refractions then such, as ought to result from the colours belonging to the diversly refrangible rays by their being separated or mixed by that refraction?

To determin by experiments these & such like Queries which involve the propounded Theory seemes the most proper & direct way to a conclusion. And therefore I could wish all objections were suspended, taken from Hypotheses or any other Heads then these two; Of showing the insufficiency of experiments to determin these Queries or prove any other parts or my Theory, by assigning the flaws & defects in my Conclusions drawn from them; Or of producing other Experiments which directly contradict me, if any such may seem to occur. For if the Experiments, which I urge be defective it cannot be difficult to show the defects, but if valid, then by proving the Theory they must render all other Objections invalid.]

In the margin of my Answer to M^r Hook I noted the contents of it in 12 Particulars, which when I came to number them in the Copy I found 13, so that there is either a marginall note omitted, or else slipt over without its number prefixt. If the last hath happened, you may prefix its number & alter the numbers of those that follow. But if the first, I will supply the note when I returne to Cambridge where my papers are, because ther may possibly be occasion of referring to that discourse hereafter. Sir I am

Your humble Servant

I. Newton

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Yours dated June the 20th I doubt I shall not receive till my returne to Cambridge. I desire you would suspend the impression of P. Pardies second Letter. If you write to me before July 14 pray direct your Letter to me at M^{ris} Arundells House in Stoake Park in Northampton-shire. And assigne it to be left with the Post-Master of Towcester to be sent thither.

Newton.

If you see M^r Collins pray acquaint him that there are three more books of M^r Kersies Algebra desired in Cambridg for which he may at present subscribe my name.

Since the writing of this I received your two letters dated June 20th & July the 2^d. I understand that John Stiles is ordered to call upon you for what you are pleased to promise me, otherwise I should have ordered another Carrier have brought it hither; For the transmission of it from Cambridg hither will not be so suddein. I am much obliged to Monsieur Hugens for what he hath wrote to you which I should have answered now but for want of time & room

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These

To Henry Oldenburg Esquire
at his house about the middle
of the old Pall-mail in

Westminster

London 2

July 8. 72.

Answ. july 9. acquiesce in his answer to Hooke. Intend to print his set of Inquiries, and to recommend them at the Royal Society Desired to take off the suspension of printing the 2^d Letter of Pardies and to send me his answer to Hugens.
