

Letter from Newton (probably to Henry Oldenburg), dated 3 April 1673

Author: Isaac Newton

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M^r Newtons Answer to Monsieur Hugenius's Letter of Jan. 14. 16 ^{$\frac{72}{73}$} .

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[1] It seems to me that M. Hugen takes an improper way of examining the nature of colours whilst he proceeds upon compounding those that are already compounded, as he doth in the former part of his letter. Perhaps he would sooner satisfy himself by resolving light into colours as far as may be done by Art, and then by examining the properties of those colours apart, and afterwards by trying the effects of reconjoyning two or more or all of those, & lastly by separating them again to examin what changes that reconjunction had wrought in them. This will prove a tedious & difficult task to do it as it ought to be done but I could not be satisfied till I had gone through it. However lonely propound it, and leave every man to his own method.

As to the contents of his letter I conceive my former answer to the Quære about the number of colours is sufficient, which was to this effect, that all colours cannot practically be derived out of Yellow & Blew, & consequently that those Hypotheses are fals which imply they may. If you ask what colours cannot be derived out of Yellow & Blew, I answer none of those which I defined to be originall and if he can show by experiment how they may I will acknowledg my self in an error. [2] Nor is it easier to frame an Hypothesis by assuming onely two originall colours rather than an indefinite variety, unless it be easier to suppose that there are but two figures sizes & degrees of velocity or force of the æthereal corpuscles or pulses rather than an indefinite variety, which certainly would be a very harsh supposition. No man wonders at the indefinite variety of waves of the sea or of sands on the Shore, but were they all of but two sizes it would be a very puzzeling phænomenon. And I should think it as unaccountable if the severall parts or corpuscles of which a shining body consists, which must be supposed of various figures sizes and motions, should impress but two sorts of motion on the adjacent æthereall Medium, or any other way beget but two sorts of rays. But to examin how colours may be thus explained Hypothetically is besides my purpose. I never intended to show wherein consists the nature and difference of colours, but onely to show that de facto they are originall & immutable qualities of the rays which exhibit them, & to leave it to others to explicate by Mechanicall Hypotheses the nature & difference of those qualities; which I take to be no very difficult matter. But I would not be understood as if their difference consisted in the different refrangibility of those rays. For that different refrangibility conduces to their production no otherwise then by separating the rays whose qualities they are. Whence it is that the same rays exhibit the same colours when separated by any other meanes; as by their different reflexivity; a quality not yet discoursed of.

In the next particular where M. Hugens would show that it is not necessary to mix all colours for the production of white, the mixture of yellow green & blew, without red & violet, which he propounds for that end, will not produce white but green, & the brightest part of the yellow will afford us no other colour but yellow, if the experiment be made in a room well darkened, as it ought, because the coloured light is much weakened by the reflexion, & so apt to be diluted by the mixing of any other scattering light. But yet there is an experiment or two mentioned in my letter in the Transactions Number 88 by which I have produced white out of two colours alone and that variously, as out of Orang and a full Blew, and out of Red & <31v> pale blew, & out of yellow & violet, as also out of other paires of intermediate colours. The most convenient experiment for performing this was that of casting the colours of one Prism upon those of another after a due manner. But what M. Hugens can deduce from hence I see not. For the two colours were compounded of all others, & so the resulting white to speake properly was compounded of them all & onely decomposed of those two. For instance, the Orange was compounded of Red, Orang, Yellow & some Green, & the Blew of Violet, full Blew, light Blew & some Green, with all their intermediate degrees; & consequently the Orang & Blew together made an aggregate of all colours to constitute the white. Thus if one mix red, orang, & yellow pouders to make an Orang; & green, blew & violet pouders to make a Blew, & lastly the two mixtures to make a Grey, that Grey though decomposed of no more then two mixtures is yet compounded of all the six pouders as truly as if the pouders had been all mixed at once.

This is so plain that I conceive there can be no further scruple especially to them who know how to examin whether a colour be simple or compounded & of what colours it is compounded: which having explained in another place, I need not now repeat. If therefore M. Hugens, would conclude any thing, he must show how white may be produced out of two uncompounded colours; which when he hath done, I will further tell him why he can conclude nothing from that. But I believe there cannot be found an experiment of that kind, because as I remember I once tryed by graduall succession the mixture of all paires of uncompounded colours, & though some of them were paler & nearer to white then others yet none could be truly called white. But it being some yeares since this tryall was made, I remember not well the circumstances, & therefore recommend it to others to be tryed again.

In the last place, had I thought the distinctness of the picture which (for instance) a twelve foot Object-glass casts into a darkened Room, to be so contrary to me as M. Hugens is pleased to affirm, I should have mended my Theory in that point before I propounded it. [3] For that I had thought on that difficulty you may easily guess by an expression somewhere in my first letter to this purpose that I wondered how Telescopes could be brought to so great perfection by refractions which were so irregular. But to take away the difficulty I must acquaint you, first, That though I put the greatest laterall error of the rays from one another to be about $\frac{1}{50}$ of the glasses diameter, yet their greatest error from the points, on which they ought to fall, will be but $\frac{1}{100}$ of that diameter: And then, that the rays, whose error is so great, are but very few in comparison to those which are refracted more justly. For the rays which fall upon the middle parts of the glass are refracted with sufficient exactness, as also are those that fall neare the perimeter & have a mean degree of refrangibility. So that there remain onely the rays which fall neare the perimeter & are most or least refrangible to cause any <32r> sensible confusion in the picture. And these are yet so much further weakened by the greater space through which they are scattered, that the light which falls on the due point is infinitely more dense then that which falls on any other point round about it. Which though it may seem a Paradox yet is easily demonstrable. Yea although the light which passeth through the middle parts of the glass were wholly intercepted yet would the remaining light convene infinitely more dense at the due points then at other places. And by this excess of density, the light which falls in or insensibly neare the just point, may I conceive, strike the sensorium so vigorously that the impress of the weak light which errs round about it, shall in comparison not be strong enough to be animadverted, or to cause any more sensible confusion in the picture then is found by experience. This I conceive is enough to show why the picture appeares so distinct notwithstanding the irregular refraction. But if this satisfy not, M. Hugens may try if he please how distinct the picture will appear when all the Lens is covered excepting a little hole next its edg on one side onely. And if in this case he please to measure the breadth of the colours thus made at the edg of the suns picture, he will perhaps find it approach nearer to my proportion then he expects. Cambridg. April 3. 1673.

[1] Philosophical Transactions N. 97. p. 6108.

[2] *The contents of this note are only visible in the diplomatic transcript because they were deleted on the original manuscript*

[3] n^o 80. p. 3079.
