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I have already told you that at the perusall of M^r^ Hooks Considerations on my Letter concerning Refractions & Colours[1], I found nothing that as I conceived might not without difficulty be answered. But I must confesse at the first receipt of those Considerations I was a little troubled to find a person so much concerned for an Hypothesis, from whome in particular I most expected an unconcerned & indifferent examination of what I propounded. But yet I doubt not but we have one common designe, a sincere endeavour after knowledg, without valuing uncertain speculations for their subtleties, or despising certainties for their plainesse: And on confidence of this it is that I make this returne to his Discourse

[2]The first thing that offers it selfe is lesse agreable to me, & I begin with it because it is so. M^r^ Hook thinks himselfe concerned to reprehend me for laying aside the thoughts of improving Optiques by Refractions. But he knows well that it is not for one man to prescribe Rules to the studies of another, especially not without understanding the grounds on which he proceeds. Had he obliged me by a private letter on this occasion, I would have acquainted him with my successes in the tryalls that I have made of that kind, which I shall now say have been lesse then I sometimes expected, & perhaps lesse then he at present hopes for. But since he is pleased to take it for granted that I have let this Subject passe without due examination, I must referre him to my former letter, by which that Conjecture will appeare to be ungrounded. For what I said there, was in respect of Telescopes of the ordinary construction, signifying that their improvement is not to be expected from the well figuring of Glasses as Opticians have imagined. But I despaired not of their improvement by other constructions, which made me cautious to insert nothing that might intimate the contrary. For although successive refractions which are all made the same way, doe necessarily more & more augment the errors of the first refraction; yet it seemed not impossible for contrary refractions so to correct each others inequalities, as to make their difference regular, & if <18v> that could be conveniently effected, there would be no further difficulty. Now to this end I examined what may be done not onely by Glasses alone, but more especially by a complication of divers successive Mediums, as by two or more Glasses or Chrystalls with water or some other fluid between them, all which together may performe the office of one Glasse, especially of the Object-Glasse on whose construction the perfection of the Instrument chiefly depends. But what the results in Theory or by Tryalls have been I may possibly find a more proper occasion to declare.

To the assertion that rays are lesse true reflected to a point by a Concave then refracted by a Convex, I cannot assent; nor do I understand that the focus of the latter is lesse a line then that of the former. The truth of the contrary you will rather perceive by this following Table computed for such a reflecting Concave & refracting Convex, on supposition that they have equall Apertures & collect parallel rays at an equall distance from their vertex. Which distance being divided into 15000 parts; the diameter of the concave Sphere will be 60000 of those parts, & of the convex 10000; supposing the sines of incidence & refraction to be in round numbers, as 2 to 3. And this Table shews how much the exterior rays at severall Apertures fall short of their principall focus.

The diameter of the aperture	The parts of the Axis intercepted between the Vertex & the rays		The error by	
	reflected.	refracted.	Reflexion.	Refraction.
2000	$14991\frac{2}{3}$	14865.	$8\frac{1}{3}$	135.
4000	14966.	14449.	33.	551.

6000	14924.	13699.	76.	1301.
8000	14865.	12475.	135.	2525.
10000	14787.	9472.	213.	5528.

By this you may perceive that the errors of the refracting Convex are So far from being the lesse, that they are more then sixteen times greater then the like errors of the reflecting Concave; especially in great aperture & that without respect to the heterogeneous constitution of light. So that however the contrary supposition might make M^r^ Hook reject reflexions as uselesse for the promoting of Optiques, yet I must for this as well as other considerations prefer them in the Theory before refractions.

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Whether the Parabola be more difficult to describe then the Hyperbola or Ellipsis, may be a query: But I see no absolute necessity of endeavouring after any of their descriptions. For if Metalls can be ground truly spherical, they will beare as great apertures as I beleive men will be well able to communicate an exact polish to. And for Dioptrique Telescopes I told you that the difficulty consisted not in the figure of the glasse but in the difformity of refractions. Which if it did not, I could tell you a better & more easy remedy then the use of the Conic Sections.

[4]Thus much conerning the Practique part of Optiques. I shall now take a view of M^r^ Hooks Considerations on my Theories. And those consist in ascribing an Hypothesis to me which is not mine; in asserting an Hypothesis which as to the principall parts of it is not against me; in granting the greatest part of my discourse if explicated by that Hypothesis; & in denying some things the truth of which would have appeared by an experimentall examination.

Of these particulars I shall discourse in order. And first of the Hypothesis which M^r^ Hook hath assigned me in these words: But grant his first supposition that light is a body, & that as many colours or degrees thereof as there may be so many bodies there may be, all which compounded together would make white &c. This it seemes M^r^ Hook takes for my Hypothesis. 'Tis true that from my Theory I argue the corporeity of light, but I doe it without any absolute positivenesse, as the word perhaps intimates, & make it at most but a very plausible consequence of the Doctrine, & not a fundamentall supposition, nor so much as any part of it, which was wholly comprehended in the precedent Propositions. And I wonder how M^r^ Hook could imagin that when I had asserted the Theory with the greatest rigor, I should be so forgetfull as afterwards to assert the fundamentall, supposition it selfe with no more then a perhaps. Had I intended any such Hypothesis I should somewhere have explained it. But I knew that the Properties which I declared of light were in some measure capable of being explicated, not onely by that, but by many other Mechanical Hypotheses. And therefore I chose to decline them all, & speake of light in generall termes, considering it abstractedly as something or other propagated every way in streight lines from luminous bodies, without determining what that thing is, whether a confused <19v> mixture of difform qualities, or modes of bodies, or of bodies themselves, or of any virtues powers or beings whatsoever. And for the same reason I chose to speake of Colours according to the information of our senses, as if they were qualities of light without us. Whereas by that Hypothesis I must have considered them rather as modes of sensation excited in the mind by various motions figures or sizes of the corpuscles of light making varioius Mechanicall impressions on the Organ of Sense, as I expressed it in that place where I spake of the corporeity of Light.

But supposing I had propounded this Hypothesis, I understand not why M^r^ Hook should so much endeavour to oppose it. For certainly it hath a much greater affinity with his own Hypothesis then he seemes to be aware of: the vibrations of Æther being as usefull & necessary in this, as in his own. For assuming the rays of Light to be small bodies emitted every way from shining substances, those when they impinge on any refracting or reflecting superficies, must as necessarily excite vibrations in the Æther, as stones do in water when thrown into it. And supposing these vibrations to be of severall depths or thicknesses, accordingly as they are excited by the said corpuscular rays of various sizes & velocities; of what use they will be for explicating the manner of reflexion & refraction, the production of heate by the sun beames, the emission of light from burning putrifying or other substances whose pars are vehemently agitated, the Phænomena of thin transparent plates

& bubbles, & of all naturall bodies, the manner of vision, & the difference of colours, as also their Harmony & Discord, I shall leave to their consideration who may think it worth their endeavour to apply this Hypothesis to the solution of Phænomena.

[5]In the second place I told you that M^{r} Hooks Hypothesis as to the fundamentall part of it. is not against me. The fundamentall supposition is, that the parts of bodies when bisquely agitated, do excite vibrations in the Æther, which are propagated every way from those bodies in streight lines, & cause a sensation of light by beating & dashing against the bottom of the eye, something after the manner that vibrations in the Air cause a sensation of Sound by beating against the Organs of hearing. Now the most free & naturall application of this Hypothesis to the solution of Phænomena I take to be this: That the agitated parts of bodies according to their severall sizes, figures, & <20r> motions, excite vibrations in the Æther of various depths or bignesses, which being promiscuously propagated through that Medium to our eyes, effect in us a sensation of light of a white colour; but if by any meanes those of unequall bignesses be separated from one another, the largest beget a sensation of a Red colour, the least or shortest of a deep Violet, & the intermediate ones of intermediate colours: Much after the manner that bodies according to their severall sizes shapes & motions, excite vibrations in the air of various bignesses, which according to those bignesses make severall tones in sound. That the largest Vibrations are best able to overcome the resistance of a refracting superficies, & so break through it with least refraction: Whence the vibrations of severall bignesses, that is, the rays of severall colours, which are blended together in light, must be parted from one another by refraction, & so cause the Phænomena of Prisms & other refracting substances. And that it depends on the thicknesse of a thin transparent Plate or Bubble, whether a vibration shall be reflected at its further superficies or transmitted; so that according to the number of vibrations interceding the two superficies they may be reflected or transmitted for many successive thicknesses, And since the vibrations which make Blew & Violet are supposed shorter then those which make Red & Yellow, they must be reflected at a lesse thicknesse of the Plate : which is sufficient to explicate all the ordinary Phænomena of those Plates or Bubbles, & also of all naturall Bodies whose parts are like so many fragments of such Plates.

These seem to be the most plaine genuine & necessary conditions of this Hypothesis. And they agree so justly with my Theories, that if M^{r} Hook think fit to apply them, he need not on that account feare a divorce from it. But yet how he will defend it from other difficulties I know not: For to me the fundamentall supposition it selfe seemes impossible; namely that the waves or vibrations of any fluid can like the rays of Light be propagated in streight lines, without a continuall & very extravagant spreading & bending every way into the quiescent Medium where they are terminated by it. I am mistaken if there be not both Experiment & Demonstration to the contrary. And as to the other two or three Hypotheses which he mentions, I had rather beleive them Subject to the like difficulties, then suspect that M^{r} Hook should select the worst for his own.

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What I have said of this, may be easily applyed to all other Mechanicall Hypotheses in which light is supposed to be caused by any pression or motion whatsoever excited in the Æther by the agitated parts of luminous Bodies. For it seems impossible that any of those motions or pressions can be propagated in streight lines without the like spreading every way into the shaddowed Medium on which they border. But yet if any man can think it possible, he must at least allow that those motions or endeavours to motion caused in the Æther by the severall parts of any lucid body which differ in size figure & agitation, must necessarily be unequall. Which is enough to denominate light an aggregate of difform rays according to any of those Hypotheses. And if those originall inequalities may suffice to difference the rays in colour & refrangibility, I see no reason why they that adhere to any of those Hypotheses, should seek for other causes of these effects, unlesse (to use M^{r} Hooks argument) they will multiply entities without necessity.

[6]The third thing to be considered is the condition of M^{r} Hooks concessions, which is that I would explicate my Theories by his Hypothesis. And if I could but comply with him in that point there would be little or no difference between us. For he grants that without any respect to a different incidence of rays there are different refractions; but he would have it explicated not by the different refrangibility of severall rays, but by the splitting & rarefying of æthereall pulses. He grants my third, fourth, & sixt Propositions, the sense of which is, that uncompounded colours are unchangeable, & that compounded colours are changeable onely by resolving them into the colours are changeable onely by resolving them into the colours of which they are

compounded, & that all the changes which can be wrought in colours are effected onely by variously mixing or parting them: but he grants them on condition that I will explicate colours by the two sides of a split pulse, & so make but two species of them, accounting all other colours in the world to be but various degrees & dilutings of those two. And hee further grants that whitenesse is produced by the convention of all colours but then I must allow it to be not onely by mixture of those colours, but by a further uniting of the parts of the ray supposed to be formerly split.

If I would proceed to examin these his explications, I think it would be no difficult matter to shew that they are <21r> onely insufficient, but in some respects unintelligible. For though it be easy to conceive how motion may be dilated & spread, or how parallel motions may become diverging, yet I understand not by what Artifice any linear motion can by a refracting superficies be infinitely dilated & rarefied so as to become superficiall; or if that be supposed, yet I understand as little why it should be split at so small an angle onely, & not rather spread & dispersed through the whole angle of refraction. And further though I can easily imagin how unlike motions may cross one another, yet I cannot well conceive how they should coalesce into one uniforme motion, & then part again & recover their former unlikenesse; notwithstanding that I conjecture the ways by which M^{r} Hook may endeavour to explain it. So that the direct uniform & undisturbed pulses should be split & disturbed by refraction, & yet the oblique & disturbed pluses persist without splitting or further disturbance by following refractions, is as unintelligible. And there is as great a difficulty in the number of colours as you will see hereafter.

[7]But whatever be the advantages or disadvantages of this Hypothesis, I hope M^{r} Hook will excuse me from taking it up, since I do not think it needfull to explicate my Doctrine by any Hypothesis at all. For if Light be considered abstractedly without respect to any Hypothesis, I can as easily conceive that the severall parts of a shining body may emit rays of differing colours & other qualities, of all which light is constituted, as that the severall parts of a false or uneven string, or of unevenly agitated water in a Brook or Cataract, or the severall Pipes of an Orga**{**ne**}** inspired all at once, or all the variety of sounding bodies in the world together, should produce sounds of severall tones, & propagate them through the Air confusedly intermixed. And if there were any naturall bodies which could reflect sounds of one tone & stifle or transmit those of another; then as the Echo of a confused aggregate of all tones would be that particular tone which the echoing body is disposed to reflect; so since (even by M^{r} Hooks concessions) there are bodies apt to reflect rays of one colour & stifle or transmit those of another, I can as easily conceive that those bodies when illuminated by a mixture of all colours must appear of that colour onely which they reflect.

But when M^{r} Hook would insinuate a difficulty in these things by alluding to sounds in the string of a musicall Instrument before percussion, or in the Air of an Organ Bellows before its arrivall at the Pipes, I must confesse I understand it as little as if he had spoke of light <21v> in a piece of wood before it be set on fire, or in the oyle of a Lamp before it ascend up the Match to feed the flame.

[8]You see therefore how much it is besides the buisinesse in hand to dispute about Hypotheses: For which reason I shall now in the last place proceed to abstract the difficulties involved in M^{r} Hooks discourse, & without having regard to any Hypothesis consider them in generall termes. And they may be reduced to these three Queries. Whether the unequall refractions made without respect to any inequality of incidence, be caused by the different refrangibility of severall rays, or by the splitting breaking or dissipating the same ray into diverging parts; Whether there be more then two sorts of colours; & whether whitenesse be a mixture of all colours

[9]The first of these Queries you may find already determined by an experiment in my former Letter, the designe of which was to show that the length of the coloured Image proceeded not from any unevennesse in the glasse, or any other contingent irregularity in the refractions. Amongst other irregularities I know not what is more obvious to suspect then a fortuitous dilating & spreading of light after some such manner as Des-Cartes hath described in his æthereall refractions for explicating the Tayle of a Comet, or as M^{r} Hook now supposeth to be effected by the splitting & rarefying of his æthereall pulses. And to prevent the suspicion of any such irregularities, I told you that I refracted the light contrary ways with two Prisms successively, to destroy thereby the regular effects of the first Prism by the second, & to discover the irregular effects by augmenting them with iterated refractions. Now amongst other irregularities, if the first Prism had spread & dissipated every ray into an indefinite number of diverging parts, the second should in like manner have

spread & dissipated every one of those parts into a further indefinite number, whereby the Image would have been still more dilated; contrary to the event. And this ought to have happened because those linear diverging parts depend not on one another for the manner of their refraction, but are every one of them as truly & completely rays as the whole was before its incidence; as may appeare by intercepting them severally.

The reasonablenesse of this proceeding will perhaps better appear by acquainting you with this further circumstance. I sometimes placed the second Prism in a position transferse to the first, on designe to try if it would make the long Image become <22r> four-square by refractions crossing those which had drawn the round Image into a long one. For if amongst other irregularities the refraction of the first Prism did by splitting dilate a linear ray into a superficiall, the crosse refractions of that second Prism ought by further splitting to dilate & draw that superficiall ray into a pyramidall Solid. But upon tryall I found it otherwise, the image being as regularly oblong as before, and inclined to both the Prisms at an angle of 45 degrees.

I tryed also all other Positions of the second Prism by turning the ends about its middle part, & in no case could observe any such irregularity. The Image was ever alike inclined to both Prisms, its breadth answering to the Suns diameter, & its length being greater or lesse accordingly as the refractions more or lesse agreed or contradicted one another.

And by these observations since the breadth of the Image was not augmented by the crosse refractions of the second Prism, that refraction must have been performed without any splitting or dilating of the ray, & therefore at least the light incident on that Prism must be granted an aggregate of rays unequally refrangible in my sense. And since the Image was equally inclined to both Prisms, & consequently the refractions alike in both, it argues that they were performed according to some constant law without any irregularity.

[10]To determin the second Query M^{r} Hook refers to an experiment made with two wedge-like Bones, the designe of which was to produce all colours out of a mixture of two. But there is a double defect in this Instance. For it appeares not that by this Experiment all colours can be produced out of two; & if they could yet the Inference would not follow.

That all colours cannot by the Experiment be produced out of two, will appeare by considering that the tincture of Aloes which afforded one of those colours[11], was not all over of one uniform colour, but appeared yellow neare the edg of the Box, & red at other places where it was thicker: affording all variety of colours from a pale Yellow to a deep red or scarlet according to the various thicknesse of the liquor. And so the solution of Copper which afforded the other colour was of various Blues & Indicos. So that instead of two colours here is a great variety made use of for the production of all others. Thus for instance, to produce all sorts of Greens, the severall degrees of Yellow & pale Blew must be mixed, but to <22v> compound Purples, the Scarlet & deep Blew or Indico are to be the ingredients.

Now if M^{r} Hook contend that all the Reds & Yellows of the one liquor, or Blews & Indicos of the other, are onely various degrees & dilutings of the same colour, & not divers colours, that is a begging of the Question; & I should as soon grant that the two Thirds or Sixts in Musick are but severall degrees of the same sound, & not divers sounds. Certainly it is much better to beleive our senses informing us that Red & Yellow are divers colours, & to make it a Philosophicall Query, why the same Liquor doth according to its various thicknesse appear of those divers colours, then to suppose them to be the same colour because exhibited by the same liquor. For if that were a sufficient reason, then Blew & Yellow must also be the same colour, since they are both exhibited by the same Tincture of Nephritick Wood.

But that they are divers colours you will more fully understand by the reason of them; which is this. The Tincture of Aloes is qualified to transmit most easily the rays indued with red, most difficultly the rays indued with violet, & with intermediate degrees of facility the rays indued with intermediate colours. So that where the liquor is very thin it may suffice to intercept most of the Violet, & yet transmit most of the other colours; all which together must compound a middle colour, that is, a faint Yellow. And where it is so much thicker as also to intercept most of the Blew & Green , the remaining Green Yellow & Red must compound an Orang. And where the thicknesse is so great that scarce any rays can passe through it besides those indued with Red, it must appeare of that colour, & that so much the deeper & obscurer by how much the liquor is thicker. And the same may be understood of the various degrees of Blew exhibited by the solution of Copper by reason of its disposition to intercept Red most easily & transmit a deep Blew or Indico colour most freely.

But supposing that all colours might according to this experiment be produced out of two by mixture, yet it follows not that those two are the onely originall colours, & that for a double reason. First because those two are not themselves originall colours but compounded o others; there being no liquor nor any other body in nature whose colour in day-light is wholly uncompounded. And then because though those two were originall & all others might be compounded of them, yet it follows not that they cannot be otherwise produced. For I sayd <23r> they had a double origin, the same colours to sense being in some case compounded & in others uncompounded; & sufficiently declared in my third & fourth Propositions & in the Conclusion by what Properties the one might be known & distinguished from the other. But because I suspect by some circumstances that the Distinction might not be rightly apprehended, I shall once more declare it, & further explaine it by examples.

That colour is primary or originall which cannot by any art be changed, & whose rays are all alike refrangible; & that compounded which is changeable into other colours, & whose rays are not alike refrangible. For instance to know whether the colour of any Green Object be compounded or not, view it through a Prism, & if it appear confused, & the edges tinged with blew yellow or any variety of other colours, then is that Green compounded of such colours as at its edges emerge out of it. But if it appeare distinct, & well defined, & intirely Green to the very edges without any other colours emerging, it is of an originall & uncompounded Green. In like manner if a refracted beam of light being cast on a white wall exhibit a Green colour, to know whether that he compounded, refract the Beam with an interposed Prism; & if you find any difformity in the refractions, & the Green be transformed into Blew Yellow or any variety of other colours, you may conclude that it was compounded of those colours which emerge. But if the refractions be uniforme, & the Green persist without any change of colour; then is it originall & uncompounded. And the reason why I call it so, is because a Green indued with such properties cannot be produced by any mixing of other colours.

Now if two Green Objects may to the naked eye appeare of the same colour, & yet one of them through a Prism seem confused & variegated with other colours at the edges, & the other distinct & intirely Green; or if there may be two beames of light which falling on a white wall do to the naked eye exhibit the same green colour, & yet one of them when transmitted through a Prism be uniformely & regularly refracted, & retain its colour unchanged, & the other be irregularly refracted & made to divaricate into a multitude of other colours: I suppose these two Greens will in both cases be granted of a different origine & constitution. And if by mixing colours a Green cannot be compounded with the properties of the unchangeable Green; I think I may call that an uncompounded colour, especially since its rays are all alike refrangible, & uniform in all respects.

The same rule is to be observed in examining whether Red, Orange, Yellow, Blew, or any other colour be compounded or <23v> not. And by the way since all white Objects through the Prism appeare confused & terminated with colours; whitenesse must according to this distinction be ever compounded, & that the most of all colours, because it is the most confused & changed by refractions.

From hence I may take occasion to communicate a way for the improvement of Microscopes by refraction; which I doe the more willingly because M^{r} Hook hath made such excellent use of that Instrument, & I shall be glad if it will contribute any thing to the promotion of those his ingenious endeavours, or add to his Inventions of that kind. The way is by illuminating the Object in a darkened Room with light of any convenient colour not too much compounded. For by that meanes the Microscope will with distinctnesse beare a deeper Charge & larger Aperture; especially if its construction be such as I may hereafter describe. For the advantage in ordinary Microscopes will not be so sensible.

[12]There remaines now the third Query to be considered, & that is, whether whitenesse be an uniform colour or a dissimilar mixture of all colours. The experiment which I brought to decide it M^{r} Hook thinks may be otherwise explaned, & so concludes nothing. But he might easily have satisfied himselfe by trying what would be the result of a mixture of all colours. And that very Experiment might have satisfied him if he had examined it by the various circumstances. One circumstance I there declared of which I see no notise taken: & it is that if any colour at the Lens be intercepted, the whitenesse will be changed into the other colours. If all the colours but red be intercepted, that red alone in the concourse or crossing of the rays will not constitute whitenesse, but continues as much red as before; & so of the other colours. So that the businesse is not onely to shew how rays which before the concours exhibit colours, doe in the concours exhibit white: But to show how in the same place where the severall sorts of rays apart exhibit severall colours, a confusion of all together make white. For instance if Red alone be first transmitted to the paper at the plce of concourse, &

then the other colours be let fall on that Red, the question will be whether they convert it into white by mixing with it onely, as Blew falling on Yellow light is supposed to compound Green; or whether there be some further change wrought in the colours by their mutuall acting on one another, untill, like contrary Peripatetic Qualities, they become assimilated. And he that shall explicate this last case Mechanically must conquer a double impossibility. He must first show that many unlike motions in a fluid can be clashing so act on one another & change each other as to <24r> become one uniform motion; & then that an uniform motion can of it selfe without any new unequall impressions, depart into a great variety of motions regularly unequall. And after this he must further tell me why all objects appeare not of the same colour, that is why their colours in the Air where the rays that convey them every way are confusedly mixed, doe not assimilate one another & become uniforme before they arrive at the Spectators eye.

But if there be yet any doubting, tis better to put the event on further circumstances of the experiment, then to acquiesce in the possibility of any Hypotheticall Explication. As for instance by trying what will be the apparition of these colours in a very quick consecution of one another. And this may be easily performed by the rapid gyration of a wheel with many spoakes or coggs in its perimeter, whose interstices & thicknesses may be equall & of such a largenesse that if the wheel be interposed between the Prism & the white concourse of the colours, one half of the colours may be intercepted by a spoak or cogg & the other halfe passe through an interstice. The wheel being in this posture you may first turne it slowly about to see all the colours fall successively on the same place of the paper held at their afforesaid concours, & if you then accelerate its gyration untill the consecution of those colours be so quick that you cannot distinguish them severally, the resulting colour will be a whitenesse perfectly like that which an unrefracted beam of light exhibiteth when in like manner successively interrupted by the spoakes or Coggs of that circulating Wheel. And that this whitenesse is produced onely by a successive intermixture of the colours without their being assimilated or reduced to any uniformity, is certainly beyond all possibility of doubting, unlesse things that exist not at the same time may notwithstanding act on one another.

There are yet other circumstances by which the truth might have been decided, as by viewing the white concourse of the colours through another Prism placed close to the eye, by whose refraction that whitenesse may appeare again transformed into colours. And then to examin their origin, if an Assistant intercept any of the colours at the Lens before their arrivall at the whitenesse, the same colours will vanish from amongst those into which that whitenesse is converted by the second Prism, Now if the rays which disappeare be the same with those that are intercepted, then it must be acknowledged that the second Prism makes no new <24v> colours in any rays which were not in them before their concours at the paper. Which is a plane indication that the rays of severall colours remain distinct from one another in the whitenesse, & that from their previous dispositions are derived the colours of the second Prism. And by the way what is said of their colours may be applyed to their refrangibility.

The aforesaid wheel may be also here made use of; And if its gyration be neither too quick nor too slow, the succession of the colours may be discerned through the Prism, whilst to the naked eye of a By-stander they exhibit whitenesse.

There is something still remaining to be said of this Experiment. But this I conceive is enough to enforce it, & so to decide the controversy. However I shall now proceed to sho some other ways of producing whitenesse by mixtures, since I perswade my selfe that this assertion above the rest appeares Paradoxicall, & is with most difficulty admitted. And because M^{r} Hook desires an instance of it in bodies of divers colours, I shall begin with that. But in order thereto it must be considered that such coloured bodies reflect but some part of the light incident on them, as is evident by the 13th Proposition; & therefore the light reflected from an aggregate of them will be much weakened by the losse of many rays. Whence a perfect & intense whitenesse is not to be expected, but rather a colour between those of light & shaddow, or such a grey or dirty colour as may be made by mixing white & black together.

And that such a colour will result may be collected from the colour of dust found in every corner of a house, which hath been observed to consist of many coloured particles. There may be also produced the like dirty colour by mixing severall Painters colours together. And the same may be effected by painting a Top (such as Boys play with) of divers colours; for when it is made to circulate by whipping it will appear of such a dirty colour.

Now the compounding of these colours is proper to my purpose because they differ not from whitenesse in the species of colour but onely in degree of luminousnesse. Which (did not M^{r} Hook concede it) I might thus evince. A beam of the Suns light being transmitted into a darkened Room if you illuminate a sheet of white paper by that light reflected from a body of any colour the paper will always appeare of the colour of that body by whose reflected light it is illuminated. If it be a red body, the paper will be red; if a green body, it will be <25r> green; & so of the other colours. And the reason is that the fibers or threds of which the paper consists are all transparent & specular, & such substances are known to reflect colours without changing them. To know therefore to what species of colour a Grey belongeth, place any Grey body, (suppose a mixture of Painters colours) in the said light, & the Paper being illuminated by its reflexion shall appear white. And the same thing will happen if it be illuminated by reflexion from a black substance.

These therefore are all of one species, but yet they seem to be distinguished not onely by degrees of luminousnesse, but also by some other inequalities whereby they become more harsh or pleasant. And the distinction seems to be that Greys & perhaps Black are made by an uneven defect of light, consisting as it were of many little veines or streames which differ either in luminousnesse or in the unequall distribution of diversly coloured rays; such as ought to be caused by reflexion from a mixture of white & black or of diversly coloured corpuscles. But when such imperfectly mixed light is by a second reflexion from the paper more evenly & uniformely blended, it becomes more pleasant, & exhibits a faint or shaddowed whitenesse. And that such little irregularities as these may cause these differences is not improbable if wee consider how much variety may be caused in sounds of the same tone by irregular & uneven jarrings. And besides, these differences are so little that I have sometimes doubted whether they be any at all, when I have considered that a black & white body being placed together, the one in a strong light & the other in a very faint light so proportioned that they might appear equally luminous; it hath been difficult to distinguish them when viewed at distance, unlesse when the Black seemed more blewish, & the white body in a light still fainter hath in comparison of the black body it selfe appeared black.

This leads me to another way of compounding whitenesse; which is, that if four or five Bodies of the more eminent colours, or a paper painted all over in severall parts of it with those severall colours in a due proportion, be placed in the said beam of light, the light reflected from those colours to another white paper held at a convenient distance shall make that paper appeare white. If it be h eld too neare the colours, its parts will seem of those colours which are nearest them, but by removing it further that all its parts may be equally illuminated by all the colours, they will be more & more diluted untill they <25v> become perfectly white. And you may further observe that if any of the colours be intercepted, the paper will no longer appeare white, but of the other colours which are not intercepted. Now that this whitenesse is a mixture of the severally coloured rays falling confusedly on the Paper, I see no reason to doubt of, because if the light became uniforme & similar before it fell on the paper, it must much more be uniforme when at a greater distance it falls on the spectators eye; & so the rays which come from severall colours would in no qualities differ from one another, but all of them exhibit the same colour to the Spectator, contrary to what he sees.

Not much unlike this instance it is that if a polished piece of Metall be so placed that the colours appear in it as in a looking-glasse, & then the metall be made rough that by a confused reflexion those apparent colours may be blended together, they shall disappear, & by their mixture cause the Metall to look white.

But further to enforce this experiment, if instead of the paper any white froth consisting of small bubbles be illuminated by reflexion from the afforesaid colours, it shall to the naked eye seem white, & yet through a good Microscope the severall colours will appear distinct on the Bubbles as if seen by reflexion from so many sphericall surfaces. With my naked eye being very neare I have also discerned the severall colours on each Bubble, & yet at a greater distance where I could not distinguish them apart, the Froth hath appeared intirely white. And at the same distance when I looked intently I have seen the colours distinctly on each Bubble, & yet by straining my eyes as if I would look at something far of beyond them, thereby to render the vision confused, the Froth hath appeared without any other colour then whitenesse. And what is here said of froth may easily be understood of the Paper or Metall in the foregoing Experiments. For their parts are specular bodies like these Bubbles: & perhaps with an excellent Microscope the colours may be also seen intermixedly reflected from them.

In proportioning the severally coloured Bodies to produce these effects there may be some nicenesse, & it will be more convenient to make use of the colours of the Prism cast on a wall by whose reflexion the Paper,

Metall, Froth, & other white substances may be illuminated. And I usually made my tryalls this way because I could better exclude any scattering light from mixing with the colours to dilute them.

To this way of compounding whitenesse may be referred that other by mixing light after it hath been trajected through transparently coloured substances. For instance if no light be admitted into a Room but onely through coloured Glasse whose <26r> severall parts are of severall colours in a pretty equall proportion: all white things in the Room shall appear white if they be not held too near the Glasse. And yet this light with which they are illuminated cannot possibly be uniforme, because if the rays which at their entrance are of divers colours, do in their progresse through the Room suffer any alteration to be reduced to an uniformity: the Glasse would not in the remotest parts of the Room appear of the very same colour which it doth when the Spectators eye is very near it. Nor would the rays when transmitted into another dark Room through a little hole in an opposite Door or partition Wall, project on a Paper the species or representation of the Glasse in its proper colours

And by the way this seems a very fit & cogent instance of some other parts of my Theory, & particularly of the 13th Proposition. For in this Room all naturall bodies whatever appear of their proper colours. And all the Phænomena of colours in nature made either by refraction or without it are here the same as in the open Air. Now the light in this Room being such a dissimilar mixture as I have described in my Theory, the causes of all these Phænomena must be the same that I have there assigned. And I see no reason to suspect that the same Phænomena should have other causes in the open Air.

The successe of this Experiment may be easily conjectured by the appearances of things in a Church or Chappell whose windows are of coloured Glasse; or in the open Air when it is illustrated with clouds of various colours.

There are yet other ways by which I have produced whitenesse, as by casting severall colours from two or more Prisms upon the same place, by refracting a beam of light with two or three Prisms successively to make the diverging colours converge again, by reflecting one colour to another, & by looking through a Prism on an Object of many colours; &, which is equivalent to M^{r} Hooks way of mixing colours by concave wedges filled with coloured liquors, I have observed the shaddows of a painted glasse-window to become white where those of many colours have at a great distance interfered. But yet for further satisfaction he may try, if he please, the effects of four or five such wedges filled with liquors of as many severall colours.

Besides all these, the colours of water-bubbles, & other thin pellucid substances afford severall instances of whitenesse produced by their mixture; with one of which I shall conclude this Particular. Let some water in which a convenient quantity of Soap <26v> or wash-ball is dissolved, be agitated into a Froth, & after that Froth hath stood a whole without further agitation till you see the Bubbles of which it consists begin to break, there will appear a great variety of colours all over the top of every Bubble if you view them near at hand, but if you view them at so great a distance that you cannot distinguish the colours one from another, the froth will appear perfectly white.

[13]Thus much concerning the designe & Substance of M^{r} Hooks Considerations. There are yet some particulars to be taken notice of; as the denyall of the Experimentum Crucis. On this I chose to lay the whole stresse of my discourse, which therefore was the principall thing to have been objected against. But I cannot be convinced of its insufficiency by a bare denyall without assigning a reason for it. I am apt to beleive it hath been misunderstood. For otherwise it would have prevented the discourses about rarefying & splitting of rays, because the designe of it is to show that rays of divers colours considered apart do at equall incidences suffer unequal refractions, without being split, rarefied, or any ways dilated.

[14]In the Considerations on my first & second Propositions M^{r} Hook hath rendered my Doctrin of unequall refrangibility very imperfect & maimed by explicating it wholly by the splitting of rays, whereas I chiefly intended it in those refractions which are performed without that supposed irregularity; such as the Experimentum Crucis might have informed him of. And in generall I find that whilst he hath endeavoured to explicate my Propositions Hypothetically, the more materiall suggestions by which I designed to recommend them, have escaped his consideration; such as are the unchangeablenesse of the degree of refrangibility peculiar to any sort of rays, the strict Analogy between the degrees of refrangibility & colours, the distinction between compounded & uncompounded colours, the unchangeablenesse of uncompounded colours, & the

assertion that if any one of the Prismatique colours be wholly intercepted, that colour cannot be new produced out of the remaining light by any further refraction or reflexion whatsoever. And of what strength & efficacy these particulars are for enforcing the Theory I desire therefore may be now considered.[15]

[16]In the last place I should take notice of a casuall expression which intimates a greater certainty in these things then I ever promised. viz: The certainty of Mathematicall Demonstrations. <27r> I said indeed that the Science of Colours was Mathematicall & as certain as any other part of Optiques; but who knows not that Optiques & many other Mathematicall Sciences depend as well on Physicall Principles as on Mathematicall Demonstrations: And the absolute certainty of a Science cannot exceed the certainty of its Principles. Now the evidence by which I asserted the Propositions of colours is in the next words expressed to be from Experiments & so but Physicall: Whence the Propositions themselves can be esteemed no more then Physicall Principles of a Science. And if those Principles be such that on them a Mathematician may determin all the Phænomena of colours that can be caused by refractions, & that by computing or demonstrating after what manner & how much those refractions doe separate or mingle the rays in which severall colours are originally inherent; I suppose the Science of Colours will be granted Mathematicall & as certain as any part of Optiques. And that this may be done I have good reason to beleive, because ever since I became first acquainted with these Principles, I have with constant successe in the events made use of them for this purpose.

Thus much I have thought fit to returne to M^{r} Hooks Considerations: which that it may bring satisfaction in this part of Optiques to the Honourable Members of the R. Society hath been the Rule of my Intensions.

Your humble Servant

I. Newton

[1] Ph. Trans. N. 88 p. 5084

[2] 1. Of the Practique part of Optiques.

[3] Of the Theoretique part.

[4] 2. Of the Theoretique part.

[5] 3. Of M^{r} Hooks Hypothesis, & that the most free & genuine constitution of that & all other mechanicall Hypotheses is conformable to my Doctrin.

[6] 4. Of M^{r} Hooks concessions, & their limitation to his Hypothesis.

[7] 5. That it is not necessary to limit or explain my Doctrin by any Hypothesis.

[8] 6. The difficulties of M^{r} Hooks discourse abstracted from Hypotheses & considered more generally.

[9] 7. That the Ray is not split or any otherwise dilated.

[10] 8 That there are more than two originall colours.

[11] Obs 10 Microgr.

[12] 9. That whitenesse is a mixture of all colours.

[13] 10. That the Experimentum Crucis is such.

[14] 11. Some particulars recommended to further consideration.

[15] This Article not Printed

[16] 12. That the Science of Colours is most properly a Mathematicall Science.