Hypothesis explaining the properties of light

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***December* 9. There was produced a manuscript of Mr. NEWTON, touching his theory of light and colors, containing partly an hypothesis to explain the properties of light discoursed of by him in his former papers, partly the principal phænomena of the various colours exhibited by thin plates or bubbles, esteemed by him to be of a more difficult consideration; yet to depend also on the said properties of light.**

Of the hypothesis only the first part was read, giving an account of refraction, reflection, transparency, and opacity; the second part explaining colours being referred to the next meeting.

The first was as follows[h][1] :

Sir,

I have sent you the papers I mentioned, by JOHN STILES. Upon reviewing them, I find some things so obscure, as might have deserved a further explication by schemes; and some other things, I guess, will not be new to you, though almost all was new to me when I wrote them. But as they are, I hope you will accept of them, though not worth the ample thanks you sent. I remember, in some discourse with Mr. HOOKE, I happened to say, that I thought light was reflected, not by the parts of glass, water, air, or other sensible bodies; but by the same confine or superficies of the æthereal mediums, which refracts it, the rays finding some difficulty to get through it in passing out of the denser into the rarer medium, and a greater difficulty in passing out of the rarer into the denser; and so being either refracted or reflected by that superficies, as the circumstances they happened to be in at their incidence make them able or unable to get through it. And, for confirmation of this, I said further, that I thought the reflection of light, as its tending out of glass into air, would not be diminished or weakened by drawing away the air in an air-pump, as it ought to be, if they were the parts of air that reflected: and added, that I had not tried this experiment, but though he was not unacquainted with notions of this kind. To which he replied, that the notion was not new, and he would the first opportunity try the experiment I propounded. But upon reviewing the papers I send you, I found it there set down for tried; which makes me recollect, that about the time I was writing these papers, I had occasionally observed in an air-pump here at Christ's College, that I could not perceive the reflection of the inside of the glass diminished in drawing out the air. This I thought fit to mention, least my former forgetfulness, through having long laid aside my thoughts on these things, should make me seem to have set down for certain what I never tried.

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Sir, I had formerly purposed never to write any hypothesis of light and colours, fearing it might be a means to engage me in vain disputes: but I hope a declared resolution to answer nothing, that looks like a controversy, unless possibly at my own time upon some by-occasion, may defend me from that fear. And therefore considering, that such an hypothesis would much illustrate the papers I promised to send you; and having a little time this last week to spare, I have not scrupled to describe one, so far as I could on a sudden recollect my thoughts about it; not concerning myself, whether it shall be thought probable or improbable, so it do but render the papers I send you, and others sent formerly, more intelligible. You may see, by the scratching and interlining, it was done in haste; and I have not had time to get it transcribed, which makes me say I reserve a liberty of adding it; and desire, that you would return those and the other papers when you have done with them. I doubt there is too much to be read at one time, but you will soon know how to order that. At the end of the hypothesis you will see a paragraph to be inserted as is there directed: I should have added another or two, but I had not time, but such as it is, I hope you will accept it.

Sir, I am, &c.

Is. NEWTON.

An Hypothesis explaining the Properties of Light, discoursed of in my several Papers.

Sir,

In my answer to Mr. HOOKE, you may remember, I had occasion to say something of hypotheses, where I gave a reason, why all allowable hypotheses in their genuine constitution should be conformable to my theories; and said of Mr. HOOKE'S hypothesis, that I took the most free and natural application of it to phænomena to be this[i][2] : that the agitated parts of bodies, according to their several sizes, figure, and motions, do 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 means those of unequal bignesses be separated from one another, the largest beget a sensation of a red colour; the least, or shortest, of a deep violet; and the intermediate ones, of intermediate colours: much after the manner that bodies, according to their several sizes, shapes, and motions, excite vibrations in the air of various bignesses, which, according to those bignesses, make several tones in sound, &c. I was glad to understand, as I apprehend, from Mr. HOOKE'S discourse at my last being at one of your assemblies, that he had changed his former notion of all colours being compounded of only two original ones, made by the two sides of an oblique pulse; and accommodated his hypothesis to this my suggestion of colours, like sounds, being various, according to the various bigness of the pulses. For this I take to be a more plausible hypothesis than any other described by former authors, because I see not how the colours of thin transparent plates or skins can be handsomely explained, without having recourse to æthereal pulses: but yet I <249> like another hypothesis better, which I had occasion to hint something of in the same letter in these words[k][3] :

The hypothesis of light's being a body, had I propounded it, has a much greater affinity with the objector's own hypothesis, than he seems to be aware of; the vibrations of the æther being as useful and necessary in this as in his. 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 several depths or thicknesses, accordingly as they are excited by the said corpuscular rays of various sizes and velocities; of what use they will be for explicating the manner of reflexion and refraction; the production of heat by the sun-beams; the emission of light from burning, putrifying, or other substances, whose parts are vehemently agitated; the phænomena of thin transparent plates, and bubbles, and of all natural bodies; the manner of vision, and the difference of colours; as also their harmony and discord; I shall leave to their consideration, who may think it worth their endeavour to apply this hypothesis to the solution of phænomena.

Were I to assume an hypothesis, it should be this, if propounded more generally, so as not to determine what light is, farther than that it is something or other capable of exciting vibrations in the æther: for thus it will become so genreal and comprehensive of other hypotheses, as to leave little room for new ones to be invented. And therefore, because I have observed the heads of some great virtuosos to run much upon hypotheses, as if my discourses wanted an hypothesis to explain them by, and found, that some, when I could not make them take my meaning, when I spake of the nature of light and colours abstractedly, have readily apprehended it, when I illustrated my discourse by an hypothesis; for this reason I have here thought fit to send you a description of the circumstances of this hypothesis as much tending to the illustration of the papers I herewith send you. And though I shall not assume either this or any other hypothesis, not thinking it necessary to concern myself, whether the properties of light, discovered by me, be explained by this, or Mr.

HOOKE'S, or any other hypothesis capable of explaining them; yet while I am describing this, I shall sometimes, to avoid circumlocution, and to represent it more conveniently, speak of it, as if I assumed it, and propounded it to be believed. This I though fit to express, that no man may confound this with my other discourses, or measure the certainty of one by the other, or think me obliged to answer objections against this script: for I desire to decline being involved in such troublesome and insignificant disputes.

But to proceed to the hypothesis: First, it is to be supposed therein, that there is an æthereal medium much of the same constitution with air, but far rarer, subtler, and more strongly elastic. Of the existence of this medium the motion of a pendulum in a glass exhausted of air almost as quickly as in <250> the open air, is no inconsiderable argument. But it is not to be supposed, that this medium is one uniform matter, but compounded, partly of the main phlegmatic body of æther, partly of other various æthereal spirits, much after the manner, that air is compounded of the phlegmatic body of air intermixed with various vapours and exhalations: for the electric and magnetic effluvia, and gravitating principle, seem to argue such variety. Perhaps the whole frame of nature may be nothing but various contextures of some certain æthereal spirits, or vapours, condensed as it were by precipitation, much after the manner, that vapours are condensed into water, or exhalations into grosser substances, though not so easliy condensible; and after condensation wrought into various forms; at first by the immediate hand of the Creator; and ever since by the power of nature; which, by virtue of the command, increase and multiply, become a complete imitator of the copies set her by the protoplast. Thus perhaps may all things be originated from æther.

At least, the elastic effluvia seem to instruct us, that there is something of an æthereal nature condensed in bodies. I have sometimes laid upon a table a round piece of glass about two inches broad set in a brass ring, so that the glass might be about one eighth or one sixth of an inch from the table, and the air between them inclosed on all sides by the ring, after the manner as if I had whelmed a little sieve upon the table; and then rubbing a pretty while the glass briskly with some rough and raking stuff, till some very little fragments of very thin paper, laid on the table under the glass, began to be attracted and move nimbly to and fro; after I had done rubbing the glass, the papers would continue a pretty while in various motions; sometimes leaping up to the glass and resting there a while; then leaping down and resting there; then leaping up, and perhaps down and up again, and this sometimes in lines seeming perpendicular to the table; sometimes in oblique ones; sometimes also they would leap up in one arch and down in another, divers times together, without sensibly resting between; sometimes skip in a bow from one part of the glass to another without touching the table, and sometimes hang by a corner, and turn often about very nimbly, as if they had been carried about in the midst of a whirlwind, and be otherwise variously moved, every paper with a diverse motion. And upon sliding my finger on the upper side of the glass, though neither the glass, nor inclosed air below, were moved thereby, yet would the papers, as they hung under the glass, receive some new motion, inclining this way or that way, accordingly as I moved my finger. Now, whence all these irregular motions should spring, I cannot imagine, unless from some kind of subtil matter lying condensed in the glass, and rarefied by rubbing, as water is rarefied into vapour by heat, and in that refraction diffused through the space round the glass to a great distance, and made to move and circulate variously, and accordingly to actuate the papers till it return into the glass again, and be recondensed there. And as this condensed matter by rarefaction into an æthereal wind (for by its easy penetrating and circulating through glass I esteem it æthereal) may cause these odd motions, and by condensing again may cause electrical attraction with its returning to the glass to succeed in the place of what is there continually recondensed; so may the gravitating attraction of the <251> earth be caused by the continual condensation of some other such like æthereal spirit, not of the main body of phlegmatic æther, but of something very thinly and subtilly difused through it, perhaps of an unctuous or gummy, tenacious, and springy nature, and bearing much the same relation to æther, which the vital aereal spirit, requisite for the conservation of flame and vital motions, does to air. For, if such an æthereal spirit may be condensed in fermenting or burning bodies, or otherwise coagulated in the pores of the earth and water into some kind of humid active matter, for the continual uses of nature, adhering to the sides of those pores, after the manner that vapours condense on the sides of a vessel; the vast body of the earth, which may be every where to the very center in perpetual working, may continually condense so much of this spirit, as to cause it from above to descend with great celerity for a supply; in which descent it may bear down with it the bodies it pervades with force proportional to the superficies of all their parts it acts upon; nature making a circulation by the slow ascent of as much matter out of the bowels of the earth in an aereal form, which, for a time, constitues the atmosphere; but being continually buoyed up by the new air; exhalations and vapours rising underneath, at length (some parts of the vapours, which return in rain, excepted) vanishes again into the æthereal spaces, and there perhaps in time relents, and is attenuated into its first principle: for nature is a perpetual worker, generating fluids out of solids, and solids out of fluids, fixed things out of volatile, and volatile out of fixed, subtil out of gross and gross out of subtil; some things to ascend, and make the upper terrestrial juices, rivers, and the atmosphere; and by consequence, others to descend for a requital to the former. And, as the earth, so perhaps may the sun imbibe this spirit copiously, to conserve his shining, and keep the planets from receding further from him. And they, that will, may also suppose, that this spirit affords or carries with it thither the solary fewel and material principle of light: and that the vast æthereal spaces between us and the stars are for a sufficient repository for this food of the sun and planets. But this of the constitution of æthereal natures by the by.

In the *second* place, it is to be supposed, that the æther is a vibrating medium like air, only the vibrations far more swift and minute; those of air, made by a man's ordinary voice, succeeding one another at more than half a foot or a foot distance; but those of æther at a less distance than the hundred thousandth part of an inch. And, as in air the vibrations are some larger than others, but yet all equally swift (for in a ring of bells the sound of every tone is heard at two or three miles distance, in the same order that the bells are struck;) so, I suppose, the æthereal vibrations differ in bigness, but not in swiftness. Now, these vibrations, beside their use in reflexion and refraction, may be supposed the chief means, by which the parts of fermenting or putrifying substances, fluid liquors, or melted, burning, or other hot bodies, continue in motion, are shaken asunder like a ship by waves, and dissipated into vapours, exhalations, or smoke, and light loosed or excited in those bodies, and consequently by which a body becomes a burning coal, and smoke, flame; and, I suppose, flame is nothing but the particles of smoke turned by the access of light and heat to burning coals, little and innumerable.

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Thirdly, as the air can pervade the bores of small glass pipes, but yet not so easily as if they were wider; and therefore stands at a greater degree of rarity than in the free aereal spaces, and at so much a greater degree of rarity as the pipe is smaller, as is known by the rising of water in such pipes to a much greater hight than the surface of the stagnating water, into which they are dipped; so I suppose æther, though it pervades the pores of crystal, glass, water, and other natural bodies, yet it stands at a greater degree of rarity in those pores, than in the free æthereal spaces, and at so much a greater degree of rarity, as the pores of the body are smaller. Whence it may be, that the spirit of wine, for instance, though a lighter body, yet having subtiler parts, and consequently smaller pores, than water, is the more strongly refracting liquor. This also may be the principal cause of the cohesion of the parts of solids and fluids, of the springiness of glass, and bodies, whose parts slide not one upon another in bending, and of the standing of the mercury in the Torricellian experiment, sometimes to the top of the glass, though a much greater hight than twenty-nine inches. For the denser æther, which surrounds these bodies, must croud and press their parts together, much after the manner that air surrounding two marbles presses them together, if there be little or no air between them. Yea, and that puzzling problem; *By what means the muscles are contracted and dilated to cause animal motion, may receive greater light from hence than from any means men have hitherto been thinking on*. For, if there be any power in man to condense and dilate at will the æther, that pervades the muscle, that condensation or dilation must vary the compression of the muscle, made by the ambient æther, and cause it to swell or shrink accordingly. For though common water will scarce shrink by compression, and swell by relaxation, yet (so far as my observation reaches) spirit of wine and oil will; and Mr. BOYLE'S experiment of a tadpole shrinking very much by hard compressing the water, in which it swam, is an argument, that animal juices do the same. And as for their various pression by the ambient æther, it is plain, that that must be more or less accordingly as there is more or less æther within, to sustain and counterpoise the pressure of that without. If both æthers were equally dense, the muscle would be at liberty, as if pressed by neither: if there were no æther within, the ambient would compress it with the whole force of its spring. If the æther within were twice as much dilated as that without, so as to have but half as much springiness, the ambient would have half the force of its springiness counterpoised thereby, and exercise but the other half upon the muscle; and so in all other cases the ambient compresses the muscle by the excess of the force of its springiness above that of the springiness of the included. To vary the compression of the muscle therefore, and so to swell and shrink it, there needs nothing but to change the consistence of the included æther; and a very little change may suffice, if the spring of æther be supposed very strong, as I take it to be many degrees stronger than that of air.

Now for the changing the consistence of the æther; some may be ready to grant, that the soul may have an immediate power over the whole æther in any part of the body, to swell or shrink it at will: but then how depends the <253> muscular motion on the nerves? Others therefore may be more apt to think it done by some certain æthereal spirit included within the *dura mater*, which the soul may have power to contract or dilate at will in any muscle, and so cause it to flow thither through the nerves. But still there is a difficulty, why this force of the soul upon it does not take off the power of its springiness, whereby it should sustain, more or less, the force of the outward æther. A third supposition may be, that the soul has a power to inspire any muscle with this spirit, by impelling it thither through the nerves. But this too has its difficulties, for it requires a forcible intending the spring of the æther in the muscles, by pressure exerted from the parts of the brain: and it is hard to conceive, how so great force can be exercised amidst so tender matter as the brain is. And besides, why does not this æthereal spirit, being subtil enough, and urged with so great force, go away through the dura mater and skins of the muscle; or at least so much of the other æther to go out to make way for this, which is crouded in? To take away these difficulties is a digression; but seeing the subject is a deserving one, I shall not stick to tell you how I think it may be done.

First then, I suppose, there is such a spirit; that is, that the animal spirits are neither like the liquor, vapour, or gas of spirit of wine; but of an æthereal nature, subtil enough to pervade the animal juices, as freely as the electric, or perhaps magnetic, effluvia do glass. And to know, how the coats of the brain, nerves, and muscles, may become a convenient vessel to hold so subtil a spirit, you may consider, how liquors and spirits are disposed to pervade or not pervade things on other accounts than their subtilty. Water and oil pervade wood and stone, which quicksilver does not; and quicksilver metals, which water and oil do not: water and acid spirits pervade salts, which oil and spirit of wine do not; and oil and spirit of wine pervade sulphur, which water and acid spirits do not. So some fluids, as oil and water, though their parts are in freedom enough to mix with one another, yet by some secret principle of unsociableness they keep asunder; and some, that are sociable, may become unsociable, by adding a third thing to one of them, as water to spirit of wine, by dissolving salt of tartar in it. The like unsociableness may be in æthereal natures, as perhaps between the æthers in the vortices of the sun and planets; and the reson, why air stands rarer in the boxes of small glass-pipes, and æther in the pores of bodies, than elsewhere, may be, not want of subtilty, but sociableness. And on this ground, if the æthereal vital spirit in a man be very sociable to the marrow and juices, and unsociable to the coats of the brain, nerves, and muscles, or any thing lodged in the pores of those we suppose no great violence done to it to squeeze it out; and that it may not be altogether so subtil as the main body of æther, though subtil enough to pervade readily the animal juices, and that, as any of it is spent, it is continually supplied by new spirit from the heart.

In the next place, for knowing how this spirit may be used for animal motion, you may consider, how some things unsociable are made sociable by the <254> mediation of a third. Water, which will not disolve copper, will do it, if the copper be melted with sulphur: aqua fortis, which will not pervade gold, will do it by addition of a little sal armoniac, or spirit of salt: lead will not mix in melting with copper, but if a little tin or antimony be added, they mix readily, and part again of their own accord, if the antimony be wasted by throwing saltpeter or otherwise: and so lead melted with silver quickly pervades and liquefies the silver in a much less heat than is requsite to melt the silver alone; but, if they be kept in the test till that little substance, that reconciled them, be wasted or altered, they part again of their own accord. And, in like manner, the æthereal animal spirit in a man may be a mediator between the common æther and the muscular juices, to make them mix more freely; and so, by sending a little of this spirit into any muscle, though so little as to cause no sensible tension of the muscle by its own force; yet, by rendering the juices more sociable to the common external æther, it may cause that æther to pervade the muscle of its own accord in a moment more freely and copiously than it would otherwise do, and to recede again as freely, so soon as this mediator of sociableness is retracted. Whence, according to what I said above, will proceed the swelling or shrinking of the muscle, and consequently the animal motion depending thereon.

Thus may therefore the soul, by determining this æthereal animal spirit or wind into this or that nerve, perhaps with as much ease as air is moved in open spaces, cause all the motions we see in animals: for the making which motions strong, it is not necessary, that we should suppose the æther within the muscle very much condensed or rarified by this means, but only that its spring is so very great, that a little alteration of its density shall cause a great alteration in the pressure, And what is said of muscular motion, may be applied to the motion of the heart, only with this difference, that the spirit is not sent thither, as into other muscles, but continually generated there by the fermentation of the juices, with which its flesh is replenished, and as it is generated, let out by starts into the brain through some convenient ductus to perform those motions in other muscles by impression, which it did in the heart by its generation. For I see not, why the ferment in the heart may not raise as subtil a spirit out of its juices, to cause these motions, as rubbing does out of a glass, to cause electric attraction, or burning out of fewel, to penetrate glass, as Mr. BOYLE has shewn, and calcine by corrosion metals melted therein.

Hitherto I have been contemplating the nature of æther and æthereal substances by their effects and uses; and now I come to join therewith the consideration of light.

In the fourth place therefore, I suppose light is neither æther, nor its vibrating motion, but something of a different kind propagated from lucid bodies. They, that will, may suppose it an aggregate of various peripatetic qualities. Others may suppose it multitudes of unimaginable small and swift corpuscles of various sizes, springing from shining bodies at great distances one after another; but yet without any sensible interval of time, and continually urged forward by a <255> principle of motion, which in the beginning accelerates them, till the resistence of the æthereal medium equal the force of that principle, much after the manner that bodies let fall in water are accelerated till the resistance of the water equals the force of gravity. God, who gave animals self-motion beyond our understanding, is, without doubt, able to implant other principles of motion in bodies, which we may understand as little. Some would readily grant this may be a spiritual one; yet a mechanical one might be shewn, did not I think it better to pass it by. But they, that like not this, may suppose light any other corporeal emanation, or any impulse or motion of any other medium or æthereal spirit diffused through the main body of æther, or what else they can imagine proper for this purpose. To avoid dispute, and make this hypothesis general, let every man here take his fancy: only, whatever light be, I suppose, it consists of rays differing from one another in contingent circumstances, as bigness, form, or vigour; like as the sands on the shore, the waves of the sea, the faces of men, and all other natural things of the same kind differ; it being almost impossible for any sort of things to be found without some contingent variety. And further, I would suppose it diverse, from the vibrations of the æther, because (besides, that were it these vibrations, it ought always to verge copiously in crooked lines into the dark or quiescent medium, destroying all shadows; and to comply readily with any crooked pores or passages, as sounds do,) I see not how any superficies (as the side of a glassprism, on which the rays within are incident at an angle of above forty degrees) can be totally opake. For the vibrations beating against the refracting confine of the rarer and denser æther must needs make that pliant superficies undulate, and those undulations will stir up and propagate vibrations on the other side. And further, how light, incident on very thin skins or plates of any transparent body, should, for many successive thicknesses of the plate in arithmetical progression, be alternately reflected and transmitted, as I find it is, puzzles me as much. For, though the arithmetical progression of those thicknesses, which reflect and transmit the rays alternately, argues, that it depends upon the number of vibrations between the two superficies of the plate, whether the ray shall be reflected or transmitted: yet I cannot see, how the number should vary the case, be it greater or less, whole or broken, unless light be supposed something else than these vibrations. Something indeed I could fancy towards helping the two last difficulties, but nothing which I see not insufficient.

Fifthly, it is to be supposed, that light and æther mutually act upon one another, æther in refracting light, and light in warming æther; and that the densest æther acts most strongly. When a ray therefore moves through æther of uneven density, I suppose it most pressed, urged, or acted upon by the medium on that side towards the denser æther, and receives a continual impulse or ply from that side to recede towards the rarer, and so is accelerated, if it move that way, or retarded, if the contrary. On this ground, if a ray move obliquely through such an unevenly dense medium (that is, obliquely to those imaginary superficies, which run through the equally dense parts of the medium, and may be called the refracting superficies) it must be incurved, as it <256> is found to be, by observation in water[1][4] , whose lower parts were made gradually more salt, and so more dense than the upper. And this may be the ground of all refraction and reflexion: for as the rarer air within a small glass-pipe, and the denser without, are not distinguished by a meer mathematical superficies, but have air between them, at the orifice of the pipe, running through all intermediate degrees of density: so I suppose the refracting superficies of æther, between unequally dense mediums, to be not a mathematical one; but of some breadth, the æther therein, at the orifices of the pores of the solid body, being of all intermediate degrees of density between the rarer and denser æthereal mediums; and the refraction I conceive to proceed from the continual incurvation of the ray all the while it is passing the physical superficies. Now, if the motion of the ray be supposed in this passage to be increased or diminished in a certain proportion, according to the difference of the densities of the æthereal mediums, and the addition or detraction of the motion be reckoned in the perpendicular from the refracting superficies, as it ought to be, the sines of incidence and refraction will be proportional according to what DES CARTES has demonstrated.

The ray therefore, in passing out of the rarer medium into the denser, inclines continually more and more towards parallelism with the refracting superficies; and if the differing densities of the mediums be not so great, nor the incidence of the ray so oblique, as to make it parallel to that superficies before it gets through, then it goes through and is refracted; but if, through the aforesaid causes, the ray become parallel to that superficies before it can get through, then it must turn back and be reflected. Thus, for instance, may be observed in a triangular glass-prism O E F, that the rays A *n*, that send out of the glass into the air, do, by inclining them more and more to the refracting superficies, emerge more and more obliquely till they be infinitely oblique; that is, in a manner parallel to the superficies, which happens when the angle of incidence is about forty degrees; and then, if they be a little more inclined are all reflected, as at A V λ, becoming, I suppose, parallel to the superficies before they can get through it. Let A B D C represent the rarer medium; E F H G the denser, C D F E the space between them, or refracting physical superficies, in which the æther is of all intermediate degrees of density, from the rarest æther at C D, to the densest, at E F; A *m n* L a ray, A *m* its incident part, *m n* its incurvation by the refracting superficies, and *n* L its emergent part. Now, if the ray A *m* be so much incurved as to become at its emergence *n*, as nearly as may be, parallel to C D, it is plain, that if that ray had been incident a little more obliquely, <257> it must have become parallel to C D, before it had arrived at E F,the further side of the refracting superficies; and so could have got no nearer to E F, but must have turned back by further incurvation, and been reflected, as it is represented at A μ V λ. And the like would have happened, if the density of the æther had further increased from E F to P Q; so that P Q H G might be a denser medium than E F H G was supposed; for then the ray, in passing from *m* to *n*, being so much incurved, as at *n* to become parallel to C D or P Q, it is impossible it should ever get nearer to P Q, but must at *n* begin by further incurvation to turn back, and so be reflected. And because, if a refracted ray, as *n* L, be made incident, the incident, A *m*, shall become the refracted; and therefore, if the ray A μ V, after it is arrived at V, where I suppose it parallel to the refracting superficies, should be refracted perpendicularly back, it would return back in the line of incidence V μ A. Therefore going forward, it must go forward in such another line, V π λ, both cases being alike, and so be reflected at an angle, equal to that of incidence.

This may be the cause and manner of reflection, when light tends from the rarer towards the denser æther: but to know, how it should be reflected, when it stands from the denser towards the rarer, you are further to consider, how fluids near their superficies are less pliant and yielding than in their more inward parts; and, if formed into thin plates, or shells, they become much more stiff and tenacious than otherwise. Thus, things, which readily fall in water, if let fall upon a bubble of water, they do not easily break through it, but are apt to slide down by the sides of it, if they be not too big and heavy. So, if two well polished convex glasses, ground on very large spheres, be laid one upon another, the air between them easily recedes, till they almost touch; but then begins to resist so much, that the weight of the upper glass is too little to bring them together so as to make the black, mentioned in the other papers I send you, appear in the midst of the rings of colours: and, if the glasses be plain, though no broader than a two-pence, a man with his whole strength is not able to press all the air out from between them, so as to make them fully touch. You may observe also, that insects will walk upon water without wetting their feet, and the water bearing them up; also motes falling upon water will often lie long upon it without being wetted: and so, I suppose, æther in the confine of two mediums is less pliant and yielding than in other places, and so much the less pliant by how much the mediums differ in density: so that in passing out of denser æther into rarer, when there remains but a very little of the denser æther to be past through, a ray finds more than ordinary difficulty to get through; and so great difficulty, where the mediums are of very differing density, as to be reflected by incurvation, after the manner described above; the parts of æther on that side, where they are less pliant and yielding, acting upon the ray much after the manner that they would do were they denser there than on the other side: for the resistance of the medium ought to have the same effect on the ray, from what cause soever it arises. And this, I suppose, may be the cause of the reflection of quicksilver, and other metalline bodies. It must also concur to increase the reflective virtue of the superficies, when rays tend out of the rarer medium into the <258> denser: and, in that case therefore, the reflection having a double cause, ought to be stronger than in the æther, as it is apparently. But in refraction, this rigid tenacity or unpliableness of the superficies need not be considered, because so much as the ray is thereby bent in passing to the most tenacious and rigid part of the superficies, so much it is thereby unbent again in passing on from thence through the next parts gradually less tenacious.

Thus may rays be refracted by some superficies, and reflected by others, be the medium they tend into, denser or rarer. But it remains further to be explained, how rays alike incident on the same superficies (suppose of crystal, glass, or water) may be at the same time some refracted, others reflected. And for explaining this, I suppose, that the rays, when they impinge on the rigid resisting æthereal superficies, as they are acted upon by it, so they react upon it and cause vibrations in it, as stones thrown into water do in its surface; and that these vibrations are propagated every way into both the rarer and the denser mediums; as the vibrations of air, which cause sound, are from a stroke, but yet continue strongest where they began, and alternately contract and dilate the æther in that physical superficies. For it is plain by the heat, which light produces in bodies, that it is able to put their parts in motion, and much more to heat and put in motion the more tender æther; and it is more probable, that it communicates motion to the gross parts of bodies by the mediation of æther than immediately; as for instance, in the inward parts of quicksilver, tin, silver, and other very opake bodies, by generating vibrations, that run through them, than by striking the outward parts only, without entering the body. The shock of every single ray may generate many thousand vibrations, and by sending them all over the body, move all the parts, and that perhaps with more motion than it could move one single part by an immediate stroke; for the vibrations, by shaking each particle backward and forward, may every time increase its motion, as a ringer does a bell by often pulling it, and so at length move the particles to a very great degree of agitation, which neither the simple shock of a ray, nor any other motion in the æther, besides a vibrating one could do. Thus in air shut up in a vessel, the motion of its parts caused by heat, how violent soever, is unable to move the bodies hung in it, with either a trembling or progressive motion: but if air be put into a vibrating motion by beating a drum or two, it shakes glass-windows, the whole body of a man, and other massy things, especially those of a congruous tone: yea I have observed it manifestly shake under my feet a cellared free-stone floor of a large hall, so as, I believe, the immediate stroke of five hundred drumsticks could not have done, unless perhaps quicky succeeding one another at equal intervals of time. Æthereal vibrations are therefore the best means by which such a subtile agent as light can shake the gross particles of solid bodies to heat them: and so supposing that light, impinging on a refracting or reflecting æthereal superficies, puts it into a vibrating motion, that physical superficies being by the perpetual appulse of rays always kept in a vibrating motion, and the æther therein continually expanded and compressed by turns; if a ray of light impinge upon it, while it is much compressed, I suppose it is then too dense and stiff to let the ray <259> pass through, and so reflects it; but the rays, that impinge on it at other times, when it is either expanded by the interval of two vibrations, or not too much compressed and condensed, go through and are refracted.

These may be the causes of refractions and reflections in all cases; but, for understanding how they come to be so regular, it is further to be considered, that in a heap of sand, although the surface be rugged, yet if water be poured on it to fill its pores, the water, so soon as its pores are filled, will evenly overspread the surface, and so much the more evenly, as the sand is finer: so, although the surface of all bodies, even the most polished, be rugged, as I conceive, yet where that ruggedness is not too gross and coarse, the refracting æthereal superficies may evenly overspread it. In polishing glass or metal, it is not to be imagined, that sand, putty, or other fretting powders, should wear the surface so regularly, as to make the front of every particle exactly plain, and all those plains look the same way, as they ought to do in well polished bodies, were reflection performed by their parts: but that those fretting powders should wear the bodies first to a coarse ruggedness, such is sensible, and then to a finer and finer ruggedness, till it be so fine that the æthereal superficies evenly overspreads it, and so makes the body put on the appearance of a polish, is a very natural and intelligible supposition. So in fluids, it is not well to be conceived, that the surfaces of their parts should be all plain, and the plains of the superficial parts always kept looking all the same way, notwithstanding that they are in perpetual motion. And yet without these two suppositions, the superficies of fluids could not be so regularly reflexive as they are, were the reflexion done by the parts themselves, and not by an æthereal superficies evenly overspreading the fluid.

Further, concerning the regular motion of light, it might be suspected, whether the various vibrations of the fluid, through which it passes, may not much disturb it: but that suspicion, I suppose, will vanish, by considering, that if at any time the foremost part of an oblique wave begin to turn it awry, the hindermost part, by a contrary action, must soon set it straight again.

Lastly, because without doubt there are, in every transparent body, pores of various sizes, and I said, that æther stands at the greatest rarity in the smallest pores; hence the æther in every pore should be of a differing rarity, and so light be refracted in its passage out of every pore into the next, which would cause a great confusion, and spoil the body's transparency. But considering that the æther, in all dense bodies, is agitated by continual vibrations, and these vibrations cannot be performed without forcing the parts of æther forward and backward, from one pore to another, by a kind of tremor, so that the æther, which one moment is in a greater pore, is the next moment forced into a less; and on the contrary, this must evenly spread the æther into all the pores not exceeding some certain bigness, suppose the breadth of a vibration, and so make it of an even density throughout the transparent body, agreeable to the middle sort of pores. But where the pores exceed a certain bigness, I suppose the æther suits its density to the bigness of the pore, or to the medium within it; and so being of a diverse density from the æther that surrounds it, refracts <260> or reflects light in its superficies, and so make the body, where many such interstices are, appear opake.

Some of the members taking particular notice, among other things, of an experiment mentioned in this hypothesis, desired, that it might be tried; viz. that having laid upon a table a round piece of glass, about two inches broad, in a brass ring; so that the glass might be one third part of an inch from the table; and then rubbing the glass briskly, till some little fragments of paper laid on the table under the glass began to be attracted, and move nimbly to and fro; after he had done rubbing the glass, the papers would continue a pretty while in various motions, sometimes leaping up to the glass, and resting there a while, then leaping down, and resting there, and then leaping up and down again, and this sometimes in lines seeming perpendicular to the table, sometimes in oblique ones; sometimes also leaping up in one arch, and leaping down in another divers times together, without sensibly resting between; sometimes skipping in a bow from one part of the glass to another, without touching the table, and sometimes hanging by a corner, and turning often about very nimbly, as if they had been carried about in the middle of a whirlwind; and being otherwise variously moved, every paper with a different motion. And upon sliding his finger upon the upper side of the glass, though neither the glass not the inclosed air below were moved thereby, yet would the papers, as they hung under the glass, receive some new motion, inclining this or that way, according as he moved his finger.

This experiment Mr. NEWTON proposed to be varied with a larger glass placed farther from the table, and to make use of bits of leaf gold instead of papers; thinking, that this would succeed much better, so as perhaps to make the leaf gold rise and fall in spiral lines, or whirl for a time in the air, without touching either the table or glass.

It was ordered, that this experiment should be tried at the next meeting; and Mr. HOOKE promised to prepare it for that meeting.

Mr. OLDENBURG was desired to enquire by letter of Mr. NEWTON, whether he would consent, that a copy might be taken of his papers, for the better consideration of their contents.

Mr. OLDENBURG presented from Mr. MARTYN, the printer to the Society, Mr. WILLUGHBY'S *Ornithologia*, printed at London, 1676, in fol.

December 16. Mr. NEWTON'S experiment of glass rubbed to cause various motions in bits of paper underneath, was tried, but did not succeed in those circumstances, with which it was tried. This trial was made upon the reading of a letter of his own to Mr. OLDENBURG, dated at Cambridge, 14th December, 1675[m][5] , in which he gives some more particular directions about that experiment.

The letter was as follows:

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The notice you gave me of the Royal Society's intending to see the experiment of glass rubbed, to cause various motions in bits of paper underneath, put me upon recollecting myself a little further about it; and then remembring, that, if one edge of the brass hoop was laid downward, the glass was as near again on the table as it was when the other edge was laid downward, and that the papers played best when the glass was nearest to the table; I began to suspect, that I had set down a greater distance of the glass from the table than I should have done; for in setting down that experiment, I trusted to the idea I had of the bigness of the hoop, in which I might easily be mistaken, having not seen it of a long time. And this suspicion was increased by trying the experiment with an object glass of a telescope, placed about the third part of an inch from the table; for I could not see the papers play any thing near so well as I had seen them formerly. Whereupon I looked for the old hoop with its glass, and at length found the hoop, the glass being gone; but by the hoop I perceived, that, when one edge was turned down, the glass was almost the third part of an inch from the table, and when the other edge was down, which made the papers play so well, the glass was scarce the eighth part of an inch from the table. This I thought fit to signify to you, that, if the experiment succeed not well at the distance I set down, it may be tried at a less distance, and that you may alter my paper, and write in it the eighth part of an inch instead of $\frac{1}{2}$ or $\frac{1}{3}$ of an inch. The bits of paper ought to be very little, and of thin paper; perhaps little bits of the wings of a fly, or other light substances, may do better than paper. Some of the motions, as that of hanging by a corner and twirling about, and that of leaping from one part of the glass to another, without touching the table, happen but seldom; but it made me take the more notice of them.

Pray present my humble service to Mr. BOYLE, when you see him, and thanks for the favour of the converse I had with him at Spring. My conceit of trepaning the common æther, as he was pleased to express it, makes me begin to have the better thoughts on that he was pleased to entertain with a smile. I am apt to think, that when he has a set of experiments to try in his air-pump, he will make that one, to see how the compression or relaxation of a muscle will shrink or swell, soften or harden, lengthen or shorten it.

As for registring the two discourses, you may do it; only I desire you would suspend till my next letter, in which I intend to set down something to be altered, and something to be added in the hypothesis.

It was ordered, that Mr. OLDENBURG should again write to Mr. NEWTON, and acquaint him with the want of success of his experiment, and desire him to send his own apparatus, with which he had made it: as also to enquire, whether he had secured the papers being moved from the air, that might somewhere steal in.

Hereupon the sequel of his hypothesis, the first part of which was read at the preceding meetings, was read to the end.

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Thus much of refraction, reflection, transparency, and opacity; and now to explain colours; I suppose, that as bodies of various sizes, densities, or sensations, do by percussion or other action excite sounds of various tones, and consequently vibrations in the air of various bigness; so when the rays of light, by impinging on the stiff refracting superficies, excite vibrations in the æther, those rays, whatever they be, as they happen to differ in magnitude, strength or vigour, excite vibrations of various bigness; the biggest, strongest, or most potent rays, the largest vibrations; and others shorter, according to their bigness, strength, or power: and therefore the ends of the capillamenta of the optic nerve, which pave or face the retina, being such refracting superficies, when the rays impinge upon them, they must there excite these vibrations, which vibrations (like those of sound in a trunk or trumpet) will run along the aqueous pores or crystalline pith of the capillamenta through the optic nerves into the sensorium (which light itself cannot do) and there, I suppose, affect the sense with various colours, according to their bigness and mixture; the biggest with the strongest colours, reds and yellows; the least with the weakest, blues and violets; the middle with green, and a confusion of all with white, much after the manner, that in the sense of hearing, nature makes use of aereal vibrations of several bignesses to generate sounds of divers tones; for the analogy of nature is to be observed. And further, as the harmony and discord of sounds proceed from the proportions of the aereal vibrations, so may the harmony of some colours, as of golden and blue, and the discord of others, as of red and blue, proceed from the proportions of the æthereal. And possibly colour may be distinguished into its principal degrees, red, orange, yellow, green, blue, indigo, and deep violet, on the same ground, that sound within an eighth is graduated into tones. For, some years past, the prismatic colours being in a well darkened room cast perpendicularly upon a paper about two and twenty foot distant from the prism, I desired a friend to draw with a pencil lines cross the image, or pillar of colours, where every one of the seven aforenamed colours was most full and brisk, and also where he judged the truest confines of them to be, whilst I held the paper so, that the said image might fall within a certain compass marked on it. And this I did, partly because my own eyes are not very critical in distinguishing colours, partly because another, to whom I had not communicated my thoughts about this matter, could have nothing but his eyes to determine his fancy in making those marks. This observation we repeated divers times, both in the same and divers days, to see how the marks on several papers would agree; and comparing the observations, though the just confines of the colours are hard to be assigned, because they pass into one another by insensible gradation; yet the *differences* of the observations were but little, especially towards the red end, and taking means between those differences, that were, the length of the image (reckoned not by the distance of the verges of the semicircular ends, but by the distance of the centres of those semicircles, or length of the strait sides as it ought to be) was divided in about the same proportion that a string is, between the end and the middle, to sound the tones in the eighth. You will understand me best by viewing the annexed figure, in which A B and C D represent the strait sides, about ten inches long, A B C and B T D the semicircular ends, X and <263> Y the centres of those semicicles, X Z the length of a musical string double to X Y, and divided between X and Y, so as to sound the tones expressed at the side (that is X H the half, X G and G I the third part, Y K the fifth part, Y M the eighth part, and G E the ninth part of X Y) and the intervals between these divisions express the spaces which the colours written there took up, every colour being the most briskly specific in the middle of those spaces.

Now for the cause of these and such like colours made by refraction, the biggest or strongest rays must penetrate the refracting superficies more freely and easily than the weaker, and so be less turned awry by it, that is, less refracted; which is as much as to say, the rays, which make red, are least refrangible, those, which make blue and violet, most refrangible, and others otherwise refrangible according to their colour: whence, if the rays, which come promiscuously from the sun, be refracted by a prism, as in the aforesaid experiment, these of several sorts being variously refracted, must go to several places on an opposite paper or wall, and so parted, exhibit every one their own colours, which they could not do while blended together. And, because refraction only severs them, and changes not the bigness or strength of the ray, thence it is, that after they are once well severed, refraction cannot make any further changes in their colour.

On this ground may all the phænomena of refractions be understood: but to explain the colours made by reflections, I must further suppose, that, though light be unimaginably swift, yet the æthereal vibrations, excited by a ray, move faster than the ray itself, and so overtake and outrun it one after another. And this, I suppose, they will think an allowable supposition, who have been inclined to suspect, that these vibrations themselves might be light. But to make it the more allowable, it is possible light itself may not be so swift, as some are apt to think; for, notwithstanding any argument, that I know yet to the contrary, it may be an hour or two, if not more, in moving from the sun to us. This celerity of the vibrations therefore supposed, if light be incident on a thin skin or plate of any transparent body, the waves, excited by its passage through the first superficies, overtaking it one after another, till it arrive at the second superficies, will cause it to be there reflected or refracted accordingly as the condensed or expanded part of the wave overtakes it there. If the plate be of such a thickness, that the condensed part of the first wave overtake the ray at the second superficies, it must be reflected there; if double that thickness, that the following rarified part of the wave, that is, the space between that and the next <264> wave, overtake it, there it must be transmitted; if triple the thickness, that the condensed part of the second wave overtake it, there it must be reflected, and so where the plate is five, seven, or nine times that thickness, it must be *reflected* by reason of the third, fourth, or fifth wave, overtaking it at the second superficies; but when it is four, six, or eight times that thickness, so that the ray may be overtaken there by the dilated interval of those waves, it shall be *transmitted*, and so on; the second superficies being made able or unable to reflect accordingly as it is condensed or expanded by the waves. For instance, let A H Q represent the superficies of a spherically convex glass laid upon a plain glass A I R, and A I R Q H the thin plane-concave plate of air between them, and B C, D E, F G, H I, &c. thicknesses of that plate, or distances of the glasses in the arithmetical progression of the numbers 1. 2. 3. 4. &c. whereof B C is the distance, at which the ray is overtaken by the most condensed part of the first wave: I say, the rays incident at B, F, K, and O, ought to be *reflected* at C, G, L, and P, and those incident at D, H, M, and Q, ought to be *transmitted* at E, I, N, and R; and this, because the ray B C arrives at the superficies A C, when it is condensed, by the first wave that overtakes it; D E, when rarified by the interval of the first and second; F G when condensed by the second wave; H I, when rarified by the interval of the second and third; and so on for an indeterminate number of successions; and at A, the center or contact of the glasses, the light must be *transmitted*, because there the æthereal mediums in both glasses are continued as if but one uniform medium. Whence, if the glasses in this posture be looked upon, there ought to appear at A, the contact of the glasses, a black spot, and about that many concentric circles of light and darkness, the squares of whose semidiameters are to sense and arithmetical progression. Yet all the rays, without exception, ought not to be thus reflected or transmitted: for sometimes a ray may be overtaken at the second superficies, by the vibrations raised by another collateral or immediately succeeding ray; which vibration, being as strong or stronger than its own, may cause it to be reflected or transmitted when its own vibration alone would do the contrary. And hence some little light will be reflected from the black rings, which makes <265> them rather black than totally dark; and some transmitted at the lucid rings, which makes the black rings, appearing on the other side of the glasses, not so black as they would otherwise be. And so at the central black spot, where the glasses do not absolutely touch, a little light will be reflected, which makes the spot darkest in the middle, and only black at the verges. For thus I have observed it to be, by tying very hard together two glass prisms, which were accidentally (one of them at least) a very little convex, and viewing by divers lights this black spot at their contact. If a white paper was placed at a litle distance behind a candle, and the candle and paper viewed alternately by reflection from the spot, the verges of the spot, which looked by the light of the paper as black as the middle part, appeared by the stronger light of the candle lucid enough, so as to make the spot seem less than before; but in the middle part continued as absolutely black in one case as in the other, some specks and streaks in it only excepted, where I suppose the glasses, through some unevenness in the polish, did not fully touch. The same I have observed by viewing the spot by the like reflection of the sun and clouds alternately.

But to return to the lucid and black rings, those rings ought always to appear after the manner described, were light uniform. And after that manner, when two contiguous glasses A Q and A R have been illustrated, in a dark room, by light of any uniform colour made by a prism, I have seen the lucid circles appear to about twenty in number, with many dark ones between them, the colour of the lucid ones being that of the light, with which the glasses were illustrated. And if the glasses were held between the eye and prismatic colours, cast on a sheet of white-paper, or if any prismatic colour was directly trajected through the glasses to a sheet of paper placed a little way behind, there would appear such other rings of colour and darkness (in the first case between the glasses, in the second, on the paper) oppositely corresponding to those, which appeared by reflection: I mean, that, whereas by reflected light there appeared a black spot in the middle, and then a coloured circle; on the contrary, by transmitted light there appeared a coloured spot in the middle, and then a black circle, and so on; the diameters of the coloured circles, made by transmission, equalling the diameters of the black ones made by reflection.

Thus, I say, the rings do and ought to appear when made by uniform light; but in compound light it is otherwise. For the rays, which exhibit red and yellow, exciting, as I said, larger pulses in the æther than those, which make blue and violet, and consequently making bigger circles in a certain proportion, as I have manifestly found they do, by illuminating the glasses successively by the aforesaid colours of prism in a well darkened room, without changing the position of my eye or of the glasses; hence the circles, made by illustrating the glasses with white light, ought not to appear black and white by turns, as the circles made by illustrating the glasses; for instance, with red light, appear red and black; but the colours, which compound the white light, must display themselves by being reflected, the blue and violet nearer to the center than the red and yellow, whereby every lucid circle must become violet in the inward verge, red in the outward, and of intermediate colours in the intermediate <266> parts, and be made broader than before, spreading the colours both ways into those spaces, which I call the black rings, and which would here appear black, were the red, yellow, blue, and violet, which make the verge of the rings, taken out of the incident white light, which illustrates the glasses, and the green only left to make the lucid rings. Suppose C B, G D, L F, P M, R N, S X, represent quadrants of the circles made in a dark room by the very deepest prismatic *red* alone; and Y β, γ δ, λ φ, π μ, ρ ν, σ ξ, the quadrants of like circles made also in a dark room, by the very deepest prismatic *violet* alone: and then, if the glasses be illuminated by open day light, in which all sorts of rays are blended, it is manifest, that the first lucid ring will be Y β B C; the second γ δ D G, the third, λ φ F L, the fourth π μ M P, the fifth ρ ν N R, the sixth σ ξ X S, &c. in all which the deepest violet must be reflected at the inward edges represented by the pricked lines, where it would be reflected were it alone, and the deepest *red* at the outward edges represented by the black lines, where it would be reflected, were it alone; and all intermediate colours at those places, in order, between these edges, at which they would be reflected were they alone; each of them in a dark room, parted from all other colours by the refraction of a prism. And because the squares of the semidiameters of the outward verges AC, A G, A L, &c. as also of A Y, A γ, A λ, &c. the semidiameters of the inward are in arithmetical progression of the numbers 1, 3, 5, 7, 9, 11, &c. and the squares of the inward are to the squares of the outward (A Y^9 to A C^9, A γ^9 to A G^9, A λ^9 to A L^9, &c.) as 9 to 14, (as I have found by measuring them carefully and often, and comparing the observations:) therefore the outward *red* verge of the second ring, and inward *violet* one of the third, shall border upon one another (as you may know by computation, and see them represented in the figure) and the like edges of the third and fourth rings shall interfere, and those of the fourth and fifth interfere more, and so on. Yea, the colours of every ring must spread themselves something more both ways than is here represented, because the quadrantal arcs here described represent not the verges, but the middle of the rings made in a dark room by the extreme violet and red; the *violet* falling on both sides the pricked arches, and *red* on both sides the black line arches. And hence it is, that these rings or circuits of colours succeed one another continually, without any <267> intevening black, and that the colours are pure only in the three or four first rings, and then intervening and mixing more and more, dilute one another so much, that after eight of nine rings they are no more to be distinguished, but seem to constitute an even whiteness; whereas, when they were made in a dark room by one of the prismatic colours alone, I have, as I said, seen above twenty of them, and without doubt could have seen them to a greater number, had I taken the pains to make the primatic colours more uncompounded. For by unfolding these rings from one another, by certain refractions expressed in the other[s][6] papers I send you, I have, even in day-light, discovered them to above an hundred; and perhaps they would have appeared innumerable, had the light or colour illustrating the glasses been absolutely uncompounded, and the pupil of my eye but a mathematical point; so that the rays, which came from the same point of the glass might have gone into my eye at the same obliquity to the glass.

What has been hitherto said of the rings, is to be understood of their appearance to an unmoved eye: but if you vary the position of the eye, the more obliquely you look on the glass, the larger the rings appear. And of this the reason may be, partly that an oblique ray is longer in passing through the first superficies, and so there is more time between the waving forward and backward of that superficies, and consequently a larger wave generated, and partly, that the wave in creeping along between the two superficies may be impeded and retarded by the rigidness of those superficies, bounding it at either end, and so not overtake the ray so soon as a wave, that moves perpendicularly cross.

The bigness of the circles made by every colour, and at all obliquities of the eye to the glass, and the thickness of the air, or intervals of the glasses, where each circle is made, you will find expressed in the other papers I send you; where also I have more at large described, how much these rings interfere, or spread into one another; what colours appear in every ring, where they are most lively, where and how much diluted by mixing with the colours of other rings; and how the contrary colours appear on the back side of the glasses by the transmitted light, the glasses transmitting light of one colour at the same place, where they reflect that of another. Nor need I add any thing further of the colours of other thinly plated mediums, as of water between the aforesaid glasses, or formed into bubbles, and so encompassed with air, or of glass blown into very thin bubbles at a lamp furnace, &c. the case being the same in all these, excepting that, where the thickness of the plate is not regular, the rings will not be so; that in plates of denser transparent bodies, the rings are made at a less thickness of the plate (the vibrations, I suppose, being shorter in rarer æther than in denser) and that in a denser plate, surrounded with a rarer body, the colours are more vivid than in the rarer surrounded with the denser; as, for instance, more vivid in a plate of glass surrounded with air, than in a plate of air surrounded with glass; of which the reason is, that the reflection of the second superficies, which causes the colours, is, as was said above, stronger in the for <268> mer case than in the latter: for which reason also the colours are most vivid, when the difference of the density of the medium is greatest.

Of the colours of natural bodies also I have said enough in those papers, shewing how the various sizes of the transparent particles, of which they consist, is sufficient to produce them all, those particles reflecting or transmitting this or that sort of rays, according to their thickness, like the aforesaid plates, as if they were fragments thereof. For, I suppose, if a plate of an even thickness, and consequently of an uniform colour, were broken into fragments of the same thickness with the plate, a heap of those fragments would be a powder much of the same colour with the plates. And so, if the parts be of the thickness of the water in the black spot at the top of a bubble described in the seventeenth of the observations I send you, I suppose the body must be black. In the production of which blackness, I suppose, that the particles of that size being disposed to reflect almost no light outward, but to refract it continually in its passage from every part to the next; by this multitude of refractions, the rays are kept so long straggling to and fro within the body, till at last almost all impinge on the solid parts of the body, and so are stopped and stifled; those parts having no sufficient elasticity, or other disposition to return nimbly enough the smart shock of the ray back upon it.

I should here conclude, but that there is another strange phænomenon of colours, which may deserve to be taken notice of. Mr. HOOKE, you may remember, was speaking of an odd straying of light, caused in its passage near the edge of a razor, knife, or other opake body in a dark room; the rays, which pass very near the edge, being thereby made to stray at all angles into the shadow of the knife.

To this Sir WILLIAM PETTY, then president, returned a very pertinent query, Whether that straying was in curve lines? and that made me, having heard Mr. HOOKE some days before compare it to the straying of sound into the quiescent medium, say, that I took it to be only a new kind of refraction, caused perhaps by the external æther's beginning to grow rarer a little before it came at the opake body, than it was in free spaces; the denser æther without the body, and the rarer within it, being terminated not in a mathematical superficies, but passing into one another through all intermediate degrees of density: whence the rays, that pass so near the body, as to come within that compass, where the outward æther begins to grow rarer, must be refracted by the uneven denseness thereof, and blended inwards toward the rarer medium of the body. To this Mr. HOOKE was then pleased to answer, that though it should be but a new kind of refraction, yet it was a new one. What to make of this unexpected reply, I knew not; having no other thoughts, but that a new kind of refraction might be as noble an invention as any thing else about light; but it made me afterwards, I know not upon what occasion, happen to say, among some that were present to what passed before, that I thought I had seen the experiment before in some Italian author. And the author is HONORATUS FABER, in his dialogue De Lumine, who had it from GRIMALDO; <269> whom I mention, because I am to describe something further out of him, which you will apprehend by this figure: supppose the sun shine through the little hole H K into a dark room upon the paper P Q, and with a wedge M N O intercept all but a little of that beam, and you will see upon the paper six rows of colours, R, S, T, V, X, Y, and beyond them a very faint light spreading either way, such as rays broken, like H N Z, must make. The author describes it more largely in divers schemes. I have time only to hint the sum of what he says.

Now for the breaking of the ray H N Z, suppose, in the next figure M N O be the solid wedge, A B C the inward bound of the uniform rarer æther within, beween which bounds the æther runs through all the intermediate degrees; and it is manifest, that, if a ray come between B and N, it must in its passage there bend from the denser medium towards C, and that so much the more, by how much it comes nearer N. Further, for the three rows of colours V X Y, those may perhaps proceed from the number of vibrations (whether one, two, or three) which overtake the ray in its passage from G, till it be about the mid-way between G and H; that is, at its nearest distance to N, so as to touch the circle described about N, with that distance; by the last of which vibrations, expanding or contracting the medium there, the ray is licensed to recede again from N, and go on to make the colours; or further bent about N, till the interval of the next wave overtake it, and give it liberty to go from N, very nearly in the line it is then moving, suppose toward Z, to cause the faint light spoken of above, you will understand me a little better, by comparing this with what was said of the colours of thin transparent plates, comparing the greatest distance that the ray goes from G B H towards N, to the thickness of one of those plates. Something too there is in DES CARTES'S explication of the rainbow's colours, which would give further light in this. But I have no time left to insist further upon particulars; nor do I propound this without diffidence, having not made sufficient observation about it.

After reading this discourse, Mr. HOOKE said, that the main of it was contained in his *Micrographia*, which Mr. NEWTON had only carried farther in some particulars.

The Society adjourned till December 30.

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December 30. There was read a letter to Mr. OLDENBURG from Mr. NEWTON, dated at Cambridge 21st December, 1675[t][7] , in answer to what had been written to him by Mr. OLDENBURG concerning the want of success of his experiment made with a glass rubbed &c. This letter was as follows:

Upon your letter I took another glass four inches broad, and one fourth of an inch thick, of such glass as telescopes are made of, and placed it a one sixth part of an inch from the table. It was set in such a piece of wood, as the object-glasses of telescopes use to be set in: and the experiment succeeded well. After the rubbing was still, and all was still, the motion of the papers would continue sometimes while I counted a hundred, every paper leaping up about twenty times more or less, and down as often. I tried also with two other glasses that belong to a telescope, and it succeeded with both; and I make no question but any glass will do that, if it be excited to electric virtue, as I think any may. If you have a mind to any of these glasses, you may have them; but I suppose, if you cannot make it do in other glass, you will fail in any I can send you. I am apt to suspect the failure was in the manner of rubbing; for I have observed, that the rubbing variously, or with various things, alters the case. At one time I rubbed the aforesaid great glass with a napkin, twice as much as I used to do with my gown, and nothing would stir; and yet presently rubbing it with something else, the motions soon began. After the glass has been much rubbed too, the motions are not so lasting; and the next day I found the motions fainter and difficulter to excite than the first. If the Society have a mind to attempt it any more, I can give no better advice than this: to take a new glass not yet rubbed (perhaps one of the old ones may do well enough after it has lain still a while) and let this be rubbed, not with linen, nor soft nappy woolen, but with stuff, whose threads may rake the surface of the glass, suppose tamerine, or the like, doubled up in the hand, and this with a brisk motion as may be, till an hundred or an hundred and fifty may be counted, the glass lying all the while over the papers. Then, if nothing stir, rub the glass with the finger ends half a score of times to and fro, or knock your fingerends as often upon the glass; for this rubbing or knocking with your fingers, after the former rubbing, conduces most to excite the papers. If nothing stir yet, rub again with the cloth till sixty or eighty may be counted, and then rub or knock again with your fingers, and repeat this till the electric virtue of the glass be so far excited as to take up the papers, and then a very little rubbing or knocking now and then will revive the motions. In doing all this, let the rubbing be always done as nimbly as may be; and if the motion be circular, like that of glass-grinding, it may do better. But if you cannot make it yet succeed, it must be let alone till I have some opportunity of trying it before you. As for the suspicion of the papers being moved by the air, I am secure from that; yet in the other, of drawing leaf-gold to above a foot distance, which I never went about to try myself till the last week, I suspect the air might raise the gold, and then a small attraction might determine it towards the glass; for I could not make it succeed.

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It was ordered, that Mr. NEWTON'S directions in this letter should be observed in the experiment to be made at the next meeting of the Society.

Mr. OLDENBURG read a letter to himself from Mr. JOHN GASCOIGNE, dated at Liege, 15th December, 1675[u][8] , acquainting him with the death of Mr. LINUS of the epidemical disease, which then raged through so many countries, and with the resolution of Mr. LINUS'S disciples, to try Mr. NEWTON'S experiment concerning light and colours more clearly and carefully, and before more witnesses, according to the directions given them by Mr. NEWTON'S last letter: intimating withal, that if the said experiment be made before the Royal Society, and be attested by them to succeed, as Mr. NEWTON affirmed, they would rest satisfied.

It was ordered, that when the sun should serve, the experiment should be made before the Society.

Mr. AUBREY presented the Society with his observations made in Wiltshire, which being read, he was desired to endeavour to procure some of the iron-ore of Sein in that county, said to be so rich, that the smith could melt it in his forge: as also to procure from Easton-Peires in Malmesbury hundred, some of the blue clay, free from sand, and almost of the colour of ultramarine; which clay Mr. DOIGHT supposed to be very fit for porcelane.

The Society adjourned till the 13th of January following.

January 13. Captain HENRY SHEERES, JOHN MAPLETOFT, M. D.[x][9] , and Signor FRANCISCO TRAVAGINI were proposed candidates, the first in the name of Sir JOSEPH WILLIAMSON, the second by Mr. HOOKE, and the third by Mr. OLDENBURG.

Mr. NEWTON'S experiment of glass rubbed, to cause various motions in bits of paper underneath, being made according to his more particular directions, succeeded very well. The rubbing was made both with a scrubbing brush, made of short hog's bristles, with a knife, the haft of the knife made of whalebone, and with the nail of one's finger. It appeared, that touching many parts at once with a hard and rough body, produced the effect expected.

It was ordered, that Mr. NEWTON should have the thanks of the Society, for giving himself the trouble of imparting to them such full instructions for making the experiment.

Mr. OLDENBURG produced and read a Latin letter of Mr. FLAMSTEAD to Sir JONAS MOORE, dated at Greenwich, 24th December, 1675[y][10] , containing an account of his observations made of the late eclipse of the moon on the 21st December, *p, m*.

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It was ordered, that Mr. OLDENBURG should be desired, according to the motion made by Mr. FLAMSTEAD, to impart these obesrvations to Signor CASSINI at Paris, and to desire him to communicate to the Society his observations on the same eclipse.

Mr. OLDENBURG produced likewise some papers of Mr. AUBREY, containing his observations of the county of Surry. But the time being elapsed, these papers were referred to the next meeting

January 20. Mr. AUBREY'S papers of observation on Surrey were read.

There was also read the beginning of Mr. NEWTON'S discourse, containing such observations, as conduce to further discoveries for completing his theory of light and colours, especially as to the constitution of natural bodies, on which their colours or transparency depend: in which he describes first the principal of his observations, and then considers and makes use of them.

At this time there were read the first fifteen of those observations as follow[z][11] :

I suppose you understand, that all transparent substances (as glass, water, air &c.) when made very thin by being blown into bubbles, or otherwise formed into plates, do exhibit various colours, according to their various thinness, although at a greater thickness they appear very clear and colourless. In my former discourse about the constitution of light, I omitted these colours, because they seemed of a difficult consideration, and were not necessary for the establishing of the doctrine, which I propounded; but because they may conduce to further discoveries for compleating that theory, especially as to the constitution of the parts of natural bodies, on which their colours or transparency depend, I have now sent you an account of them. To render this discourse short and distinct, I have first described the principal of my observations, and then considered and made use of them. The observations are these:

Obs. 1. Compressing two prisms hard together, that their sides (which by chance were a very little convex) might somewhere touch one another, I found the place, in which they touched, to become[a][12] absolutely transparent, as if they had been there one continued piece of glass; for when the light fell so obliquely on the air, which in other places was between them, as to be all reflected, in that place of contact it seemed wholly transmitted; insomuch that when looked upon, it appeared like a black or dark spot, by reason of no sensible light was reflected from thence, as from other places; and when looked through, it seemed, as it were, a hole in that air, that was formed into a thin <273> plate by being compressed between the glasses; and through this hole objects, that were beyond, might be seen distinctly, which could not at all be seen through other parts of the glasses, where the air was interjacent. Although the glasses were a little convex, yet this transparent spot was of a considerable breadth, which breadth seemed principally to proceed from the yielding inwards of the parts of the glasses by reason of their mutual pressure; for by pressing them very hard together, it would become much broader than otherwise.

Obs. 2. When the plate of air, by turning the prisms about their common axis, became so little inclined to the incident rays, that some of them began to be transmitted, there arose in it many slender arcs of colours, which at first were shaped almost like the conchoid, as you see them here delineated. And by continuing the motion of the prisms, these arcs increased and bended more and more about the said transparent spot, till they were compleated into circles or rings incompassing it, and afterwards continually grew more and more contracted.

These arcs, at their first appearance, were of a violet and blue colour, and between them were white arcs of circles, which presently became a little tinged in their inward limbs with red and yellow, and to their outward limbs the blue was adjacent; so that the order of these colours from the central dark spot, was at that time white, blue, violet, black, red, orange, yellow, white, blue, violet, &c. but the yellow and red were much fainter than the blue and violet.

The motion of the prisms about their axis being continued, these colours contracted more and more, shrinking towards the whiteness on either side of it, until they totally vanished into it; and then the circles in those parts appeared black, and white, without any other colours intermixed; but by further moving the prisms about, the colours again emerged out of the whiteness, the violet and blue at its inward limb, and at its outward limb the red and yellow; so that now their order from the central spot was white, yellow, red, black, violet, blue, white, yellow, red &c. contrary to what it was before.

Obs. 3. When the rings or some parts appeared only black and white, they were very distinct and well defined, and the blackness seemed as intense as that of the central spot; also, in the borders of these rings, where the colours began to emerge out of the whiteness, they were pretty distinct, which made them visible to a very great multitude. I have sometimes numbered above thirty successions (reckoning every black and white ring for one succession) and seen more of them, which by reason of their smallness I could not number. But in other positions of the prisms, at which the rings appeared of many colours, I could not distinguish above eight or nine of them, and the exterior of those too were confused and dilute.

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In these two observations, to see the rings distinct, and without any other colour but black and white, I found it necesary that I held my eye at a good distance from them. For by approaching nearer, although in the same inclination of my eye, yet there emerged a bluish colour out of the white, which by dilating itself more and more into a black, rendered the circles less distinct, and left the white a little tinged with red and yellow. I found also, that by looking through a slit or oblong hole, which was narrower than the pupil of my eye, and held close to it parallel to the prisms, I could see the circles much distincter and visible to a far greater number than otherwise.

Obs. 4. To observe more nicely the order of the colours, which arose out of the white circles, as the rays became less and less inclined to the plate of air; I took two object-glasses, the one a plane-convex for a fourteen foot telescope, and the other a large double convex for one of fifty foot; and upon this laying the other with its plane side downwards, I pressed them slowly together, to make the colours successively emerge in the middle of the circles, and then slowly lifted the upper glass from the lower, to make them successively vanish again in the same place, where being of a considerable breadth, I could more easily discern them. And by this means I observed their succession and quantity to be as followeth.

Next to the pellucid central spot made by the contact of the glasses, succeeded violet, blue, white, yellow, and red. The violet and blue were so very little in quantity, that I could not discern them in the circles made by the prisms; but the yellow and red were pretty copious, and seemed about as much in extent as the white, and four or five times more than the blue and violet. The next circuit or order of colours immediately encompassing these was violet, blue, green, yellow, and red. And these were all of them copious and vivid, excepting the green, which was very little in quantity, and seemed much more faint and dilute than the other colours. Of the other four the violet was least, and the blue less than the yellow or red. The third circuit or order was also purple, blue, green, yellow, and red, in which the purple seemed more reddish than the violet in the former circuit, and the green was much more conspicuous, being as brisk and copious as any of the other colours except the yellow; but the red began to be a little faded, inclining very much to purple. After these succeeded green and red: the green was very copious and lively, inclining on the one side to blue, and the other to yellow. But in this fourth circuit there was neither violet, blue, nor yellow, and the red was very imperfect and dirty. Also the succeeding colours became more and more imperfect and dilute, till after three or four more revolutions they ended in perfect whiteness.

Obs. 5. To determine the interval of the glasses, or thickness of the interjacent air, by which each colour was produced; I measured the diameter of the first six rings at the most lucid part of their orbits, and squaring them I found their squares to be in arithmetical progression of the odd numbers, 1. 3. 5. 7. 9. 11. And since one of the glasses was plane and the other spherical, their <275> intervals at those rings must be in the same progression. I measured also the diameters of the dark or faint rings between the more lucid colours, and found their squares to be in arithmetical progression, of the even numbers 2, 4, 6, 8, 10, 12; and it being very nice and difficult to take these measures exactly, I repeated them divers times, at divers parts of the glasses, that by their agreement I might be confirmed in them; and the same method I used in determining some others of the following obseravtions.

Obs. 6. The diameter of the first ring, at the most lucid part of its orbit, was $\frac{58}{100}$ parts of an inch, and the diameter of the sphere, on which the double convex object-glass was ground, was an hundred and two foot, as I found by measuring it; and consequently the thickness of the air, or aereal interval of the glasses at that ring, was $\frac{1}{14554}$ of an inch. For as the diameter of the said sphere (an hundred and two foot, or twelve hundred and twenty-four inches) is to the semidiameter of the ring $\frac{29}{100}$, so very nearly is that semidiameter to $\frac{1}{14554}$, the said distance of the glasses. Now, by the precedent observations, the eleventh part of this distance ($\frac{1}{160094}$) is the thickness of the air at that part of the first ring, where the yellow would be most vivid, were it not mixed with other colours in the white; and this doubled gives the difference of its thickness at the yellow in all the other rings, viz. $\frac{1}{80047}$, or, to use a round number, the eighty thousand part of an inch.

Obs. 7. These dimensions were taken, when my eye was placed perpendicularly over the glasses, in or near the axis of the rings; but when I viewed them obliquely, they became bigger, continually swelling as I removed my eye farther from their axis; and partly by measuring the diameter of the same circle at several obliquities of my eye, partly by other means; as also by making use of the two prisms for very great obliquities, I found its diameter, and consequently the thickness of the air at its perimeter in all those obliquities, to be very nearly in the proportions expressed in this table.

Incidence on the air.	Refraction into the air.	Diameter of the ring.	Thickness of the air.
gr. min.	gr. min.		
00 00	00 00	. 10	. 10
6 26	10 00	. $10\frac{1}{13}$	. $10\frac{2}{15}$
12 45	20 00	. $10\frac{1}{3}$	. $10\frac{2}{3}$
18 49	30 00	. $10\frac{3}{4}$	. $11\frac{1}{2}$
24 30	40 00	. $11\frac{2}{5}$	. 13
29 37	50 00	. $12\frac{1}{2}$	. $15\frac{1}{2}$
33 58	60 00	. 14	. 20
35 47	65 00	. $15\frac{1}{4}$	. $23\frac{1}{3}$
37 19	70 00	. $16\frac{4}{5}$	. $28\frac{1}{2}$
38 33	75 00	. $19\frac{1}{4}$	. 37
39 27	80 00	. $22\frac{6}{7}$	. $52\frac{1}{4}$
40 00	85 00	. 29	. 84
40 11	90 00	. 35	. $122\frac{1}{2}$

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In the first two columns are expressed the obliquities of the rays to the plate of air; that is, their angles of incidence and refraction. In the third column, the diameter of any coloured ring of those obliquities is expressed in parts, of which ten constitute that diameter, when the rays are perpendicular. And in the fourth column the thickness of the air at the circumference of that ring is expressed in parts, of which also ten constitute that thickness, when the rays are perpendicular.

Obs. 8. The dark spot in the middle of the rings increased also by that obliquation of the eye, although almost insensibly. But, if instead of the object-glasses, the prisms were made use of, its increase was more manifest, when viewed so obliquely, that no colours appeared about it. It was least, when the rays were incident most obliquely on the interjacent air, and increased more and more, until the coloured rings appeared, and then decreased again, but not so much as it increased before. And hence it is evident, that the transparency was not only at the absolute contact of the glasses, but also where they had some little interval. I have sometimes observed the diameter of that spot to be between half and two fifth parts of the diameter of the exterior circumference of the red in the first circuit or revolution of colours, when viewed almost perpendicularly; whereas, when viewed obliquely, it hath wholly vanished, and become opake and white, like the other parts of the glass. Whence it may be collected, that the glasses did then scarcely, or not at all, touch one another; and that their interval of the perimeter of that spot, when viewed perpendicularly, was about a fifth or sixth part of their interval at the circumference of the said red.

Obs. 9. By looking through the two contiguous object-glasses, I found, that the interjacent air exhibited rings of colours, as well by transmitting light as by reflecting it. The central spot was now white, and from it the order of the colours were yellowish, red, black, violet, blue, white, yellow, red; violet, blue, green, yellow, red, &c. but these colours were very faint and dilute, unless when the light was trajected very obliquely through the glasses; for by that means they became pretty vivid, only the first yellowish red, like the blue in the fourth observation, was so little and faint as scarcely to be discerned. Comparing the coloured rings made by reflection with these made by transmission of the light, I found, that white was opposite to black, red to blue, yellow to violet, and green to a compound of red and violet; that is, those parts of the glass were black when, looked through, which when looked upon appeared white, and on the contrary; and so those, which in one case exhibited blue, did in the other case exhibit red; and the like of the other colours.

Obs. 10. Wetting the object-glass a little at their edges, the water crept in slowly between them, and the circles thereby became less, and the colours more faint; insomuch that, as the water crept along, one half of them, at which it first arrived, would appear broken off from the other half, and contracted into a less room. By measuring them I found the proportion of their diameters to the diameters of the like circles made by air, to be about seven to eight; <277> and consequently the intervals of the glasses at like circles, caused by these two mediums, water and air, are as about three to four. Perhaps it may be a general rule, that if any other medium, more or less dense than water, be compressed between the glasses, their interval at the rings, caused thereby, will be to their interval, caused by interjacent air, as the sines are, which measure the refraction made out of that medium into air.

Obs. 11. When the water was between the glasses, if I pressed the upper glass variously at its edges to make the rings move nimbly from one place to another, a little bright spot would immediately follow the center of them, which, upon creeping in of the ambient water into that place, would presently vanish. Its appearance was such, as interjacent air would have caused, and it exhibited the same colours; but it was not air, for where any aereal bubbles were in the water they would not vanish. The reflection must rather have been caused by a subtiler medium, which could recede through the glass at the creeping in of the water.

Obs. 12. These observations were made in the open air. But further, to examine the effects of coloured light falling on the glasses, I darkened the room, and viewed them by reflection of the colours of a prism cast on a sheet of white paper; and by this means the rings became distincter, and visible to a far greater number than in the open air.

I have seen more than twenty of them, whereas in the open air I could not discern above eight or nine.

Obs. 13. Appointing an assistant to move the prism to and fro about its axis, that all its colours might successively fall on the same place of the paper, and be reflected from the circles to my eye whilst I held it immoveable; I found the circles, which the red light made, to be manifestly bigger than those, which were made by the blue and violet; and it was very pleasant to see them gradually swell or contract, accordingly as the colour of the light was changed. The interval of the glass at any of the rings, when they were made by the utmost red light, was to their interval at the same ring, when made by the utmost violet, greater than three to two, and less than thirteen to eight. By the most of my observations it was as nine to fourteen. And this proportion seemed very nearly the same in all obliquities of my eye, unless when two prisms were made use of instead of the object-glasses: for then, at a certain great obliquity, the rings made by the several colours seemed equal; and, at a greater obliquity, those made by the violet would be greater than the same rings made by the red.

Obs. 14. While the prism was turned about uniformly, the contraction or dilation of a ring made by alll the several colours of the prism successively reflected from the object-glasses, was swiftest in the red, slowest in the violet, and in intermediate colours it had intermediate degrees of celerity. Comparing the extent, which each colour obtained by this contraction or **{**dilation,**}** I found, <278> that the blue was sensibly more extended than the violet, the yellow than the blue, and the red than the yellow. And, to make a juster estimation of their proportions, I observed, that the extent of the red was almost double to that of the violet, and that the light was of a middle colour between yellow and green at that interval of the glasses, which was an arithmetical mean between the two extremes; contrary to what happens in the colours made by the refraction of a prism, where the red is most contracted, the violet most expanded, and in the midst of them is the confine of green and blue.

Obs. 15. These rings were not of various colours, like those in the open air, but appeared all over of that prismatic colour only, with which they were illuminated: and, by projecting the prismatic colours immediately upon the glasses, I found, that the light, which fell on the dark spaces, which were between the coloured rings, was transmitted through the glasses without any variation of colour. For, on a white paper placed behind, it would paint rings of the same colour with those, which were reflected, and of the bigness of their intermediate spaces. And from hence the origin of these rings is manifest, namely, that the aereal interval of the glasses, according to its various thickness, is disposed in some places to reflect, and in others to transmit, the light of any colour; and, in the same place to reflect one colour, where it transmits another.

These observations so well pleased the Society, that they ordered Mr. OLDENBURG to desire Mr. NEWTON to permit them to be published, together with the rest; which, they presumed, did correspond with those, that had been now read to them.

Besides, there was read a passage of Mr. NEWTON'S letter to Mr. OLDENBURG, of 21 December, 1675, stating the difference between his hypothesis and that of Mr. HOOKE. Which passage was as follows:

As for Mr. HOOKE'S insinuation, that the sum of the hypothesis I sent you had been delivered by him in his Micrography, I need not much be concerned at the liberty he takes in that kind: yet, because you think it may do well, if I state the difference I take to be between them, I shall do it as briefly as I can, and that the rather, that I may avoid the favour of having done any thing unjustifiable or unhandsome towards Mr. HOOKE. But, for this end, I must first (to see what is his) cast out what he has borrowed from DES CARTES, or others, viz. that there is an æthereal medium; that light is the action of this medium; that this medium is less implicated in the parts of solid bodies, and so moves more freely in them, and transmits light more readily through them, and that after such a manner, as to accelerate the rays in a certain proportion; that refraction arises from this acceleration, and has sines proportional; that light is at first uniform; that its colours are some disturbance or new modification of its rays by refraction or reflection; that the colours of a prism are made by means of the quiescent medium, accelerating some motion of the rays on one side, where red appears, and retarding it on <279> the other side, where blue appears; and, that there are but these two original colours, or colour-making modifications of light, which by their various degrees, or, as Mr. HOOKE calls it, dilutings, produce all intermediate ones. This rejected, the remainder of his hypothesis is, that he has changed DES CARTES'S pressing or progressive motion of the medium to a vibrating one, the rotation of the globuli to the obligation of pulses, and the accelerating their rotation on the one hand, and retarding it on the other, by the quiescent medium, to produce colours, to the like action of the medium on the two ends of his pulses for the same end. And having thus far modified his by the Cartesian hypothesis, he has extended it further, to explicate the phænomena of thin plates, and added another explication of the colours of natural bodies, fluid and solid.

This, I think, is in short the sum of his hypothesis; and in all this I have nothing in common with him, but the supposition, that æther is a susceptible medium of vibrations, of which supposition I make a very different use; he supposing it a light itself, which I suppose it is not. This is as great a difference as is between him and DES CARTES. But besides this, the manner of refraction and reflection, and the nature and production of colours in all cases (which takes up the body of my discourse) I explain very differently from him; and even in the colours of thin transparent substances, I explain every thing after a way so differing from him, that the experiments I ground my discourse on, destroy all he has said about them; and the two main experiments, without which the manner of the production of those colours is not to be found out, were not only unknown to him, when he wrote his Micrography, but even last spring, as I understood, in mentioning them to him. This therefore is the sum of what is common to us, that æther may vibrate; and so, if he thinks fit to use that notion of colours, arising from the various bigness of pulses (without which his hypothesis will do nothing) *his* will borrow as much from my answer to his objections, as that I send you does from his Micrography.

But, it may be, he means, that I have made use of his observations, and of some I did; as, that of the inflection of rays, for which I quoted him; that of opacity, arising from the interstices of the parts of bodies, which I insist not on; and that of plated bodies exhibiting colours, a phænomenon, for the notice of which I thank him. But he left me to find out and make such experiments about it, as might inform me of the manner of the production of those colours, to ground an hypothesis on; he having given no further insight to it than this, that the colour depended on some certain thickness of the plate; though what that thickness was at every colour, he confesses in his Micrography, he had attempted in vain to learn; and therefore, seeing I was left to measure it myself, I suppose he will allow me to make use of what I took the pains to find out. And this I hope may vindicate me from what Mr. HOOKE has been pleased to charge me with.

The reading of the rest of Mr. NEWTON'S discourse was referred to the next meeting.

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January 27. Mr. OLDENBURG produced from his highness prince RUPERT a piece of marble, having several pictures of boys and trees painted upon it in such a manner, that all the out-lines of the pictures were exactly defined without any flowing of the colours abroad, and the colours fixed by the fire, and afterwards so polished, that they would be permanent, and last as long as the marble.

This was acknowledged by the members to be a very great improvement of what had been done at Oxford by a certain stone-cutter there; and that all, that had been performed before in this art, was not comparable to this degree of improvement.

Mr. HOOKE remarked, that he conceived, that there were but two colours in this piece; and that he had a method of doing it with most colours, and to paint with them upon marble almost as curiously as with a pencil.

Mr. NEWTON'S letter of January 25, 167$\frac{5}{6}$ c[13] , in which he acknowledged the favour of the Society in their kind acceptance of his late papers; and declared, that he knew not how to deny any thing, which they desired should be done: but he requested, that the printing of his observations about colours might be suspended for a time, because he had some thoughts of writing such another set of observations for determining the manner of the production of colours by the prism: which observations, he said, ought to precede those now in the Society's possession, and would be most proper to be joined with them.

There was also read a letter of Mr. PASCALL of Somersetshire to Mr. AUBREY, dated 18 January 167$\frac{5}{6}$, containing some natural observations of that county, viz. concerning the nature of the lead-mines in Mendip-Hills; a well resembling the sulphur-well near the Spaw in Yorkshire; a spring petrifying far more than the dropping-well at Knaresborough in the north; the motion of some underground waters in the parishes of ZOLANDE, formerly recovered from the sea &c.

It was ordered, that the reading of Mr. NEWTON'S observations about colours be continued at the next meeting.

February 3. There was presented from Dr. WALLIS his edition of ARCHIMEDES'S *Arenarius*, with a new translation of his and notes, printed at Oxford, in 1676.

The reading of Mr. NEWTON'S observations on colours was continued, viz. that part, wherein he explains by the simplest of colours the most recompounded; as follows:

Obs. 16. The squares of the diameters of these rings, made by prismatic colour, were in arithmetical progression, as in the fifth observation. And the diameter of the sixth circle, when made by the yellow, and viewed almost perpendicularly, was about $\frac{58}{100}$ parts of an inch, agreeable to the sixth observation.

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The precedent observations were made with a rarer thin medium terminated by a denser, such as was air or water compressed betwixt two glasses. In those, that follow, are set down the appearances of a denser medium thinned within a rarer; such as are the plates of Muscovy-glass, bubbles of water, and some others thin substances terminated on all sides with air.

Obs. 17. If a bubble be blown with water, first made tenacious by dissolving a little soap in it, it is a common observation, that after a while it will appear tinged with a great variety of colours. To defend these bubbles from being agitated by the external air (whereby their colours are irregularly moved one among another, so that no accurate observation can be made of them) as soon as I had blown any of them, I covered it with a clear glass, and by that means its colours emerged in a very regular order, like so many concentric rings incompassing the top of the bubble. And as the bubble grew thinner by the continual subsiding of the water, these rings dilated slowly, and overspread the whole bubble, descending in order to the bottom of it, where they vanished successively. In the mean while, after all the colours were emerged at the top, there grew in the center of the rings a small, round, black spot, like that in the first observation, which continually dilated itself, till it became sometimes more than one half or three fourths of an inch in breadth, before the bubble broke. At first I thought there had been no light reflected from the water in that place; but observing it more curiously, I saw within it several smaller, round spots, which appeared much blacker and darker than the rest, whereby I knew, that there was some reflection at the other places, which were not so dark as those spots. And by further trial I found, that I could see the images (as of a candle or the sun) very faintly reflected, not only from the great black spot, but also from the little darker spots, which were within it.

Besides the aforesaid coloured rings, there would often appear small spots of colours ascending and descending up and down the side of the bubble, by reason of some inequalities in the subsiding of the water; and sometimes small black spots generated at the sides, would ascend up to the larger black spot at the top of the bubble, and unite with it.

Obs. 18. Because the colours of these bubbles were more extended and lively than those of air thinned between two glasses, and so more easy to be distinguished, I shall here give you a further description of their order, as they were observed in viewing them by reflection of the skies, when of a white colour, whilst a black substance was placed behind the bubble: and they were these; red, blue, red, blue; red, blue; red, green; red, yellow; green, blue, purple; red, yellow, green, blue, violet; red, yellow, white, blue, black.

The three first successions of red and blue were very dilute and dirty, especially the first, where the red seemed in a manner to be white. Amongst these there was scarcely any other colour sensible, only the blues (and principally the second blue) inclined a little to green.

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The fourth red was also dilute and dirty, but not so much as the former three: after that succeeded little or no yellow, but a copious green, which at first was inclined a little to yellow, and then became a pretty brisk and good willow green, and afterwards changed to a blueish colour; but there succeded neither blue nor violet.

The fifth red at first was very much inclined to purple, and afterwards became more bright and brisk, but yet not very pure. This was succeeded with a very bright and intense yellow, which was but little in quantity, and soon changed to green; but that green was copious, and something more pure, deep, and lively, than the former green. After that followed an excellent blue of a bright sky colour; and then a purple, which was less in quantity than the blue, and much inclined to red.

The sixth red was at first of a very fair and lively scarlet, and soon after of a brighter colour, being very pure and brisk, and the best of all the reds. Then, after a lively orange, followed an intense, bright, and copious yellow, which was also the best of all the yellows; and this changed, first to a greenish yellow, and then to a greenish blue; but the green between the yellow and blue was very little and dilute, seeming rather a greenish white than a green. The blue, which succeeded, became very good, and of a fair, bright, sky colour; but yet something inferior to the former blue: and the violet was intense and deep, with little or no redness in it, and less in quantity than the blue.

In the last red appeared a tincture of scarlet next the violet, which soon changed to a brighter colour, inclining to an orange: and the yellow, which followed, was at first pretty good and lively, but afterwards it grew more and more dilute, until by degrees it ended in perfect whiteness: and this whiteness, if the water was very tenacious and well tempered, would slowly spread and dilate itself over the greatest part of the bubble, continually growing paler at the top, where at length it would crack, and those cracks, as they dilated, would appear of a pretty good, but yet obscure and dark, sky-colour; the white between the blue spots diminishing, until it resembled the threads of an irregular net-work, and soon after vanished and left all the upper part of the bubble of the said dark blue colour; and this colour, after the aforesaid manner, dilated itself downwards, until sometimes it hath overspread the whole bubble. In the mean while, at the top, which was of a darker blue than the bottom, and appeared also of many round blue spots, something darker than the rest, there would emerge one or more very black spots, and within those, other spots of an intenser blackness, which I mentioned in the former observation; and those continually dilated themselves until the bubble broke.

If the water was not very tenacious, the black spots would break forth in the white, without any sensible intervention of the blue: and sometimes they would break forth within the precedent yellow, or red, or perhaps within the blue of the second order, before the intermediate colours had time to display themselves.

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By this description you may perceive, how great an affinity these colours have with those of air, described in the fourth observation, although set down in a contrary order, by reason that they begin to appear, when the bubble is thickest, and are most conveniently reckoned from the lowest and thickest part of the bubble upwards.

Obs. 19. Viewing, at several oblique positions of my eye, the rings of colours emerging on the top of the bubble, I found, that they were sensibly dilated by increasing the obliquity, but yet not so much by far, as those made by thinned air in the seventh observation. For there they distended so much, as, when viewed most obliquely, to arrive at a part of the plate more than twelve lines thicker than that where they appeared, when viewed perpendicularly; whereas in this case the thickness of the water, at which they arrived when viewed most obliquely, was, to that thickness, which exhibited them by perpendicular rays, something less than eight to five. By the best of my observations, it was between fifteen and fifteen and a half to ten, an increase about twenty-four times less than in the other case.

Sometimes the bubble would become of an uniform thickness all over, except at the top of it near the black spot, as I knew, because it would exhibit the same appearance of colours in all positions of the eye; and then the colours, which were seen at its apparent circumference by the obliquest rays, would be different from those, that were seen in other places by rays less oblique to it. And divers spectators might see the same part of it by differing colours, by viewing it at very differing obliquities. Now, observing how much the colours at the same place of the bubble, or at divers places of equal thickness, were varied by the several obliquities of the rays, by assistance of the fourth, fourteenth, sixteenth, and eighteenth onservations, as they are hereafter explained, I collected the thickness of the water, requisite to exhibit any one the same colour at several obliquities, to be very nearly in the proportion expressed in this table.

Incidence on the water.	Refraction into the water.	Thickness of the water.
degr. min.	degr. min.	
00 00	00 00	10
15 00	11 11	$10\frac{1}{4}$
30 00	22 1	$10\frac{4}{5}$
45 00	32 2	$11\frac{4}{5}$
60 00	40 30	13
75 00	46 25	$14\frac{1}{2}$
90 00	48 35	$15\frac{1}{5}$

In the two first columns are expressed the obliquities of the rays to the superficies of the water; that is, their angles of incidence and refraction; where, I suppose, that the lines, which measure them, are in round numbers, as three to four, though probably the dissolution of soap in the water may a little alter its refractive virtue. In the third column the thickness of the bubble, <284> at which any one colour is exhibited in those several obliquities, is exprest in parts, of which ten constitute that thickness, when the rays are perpendicular.

I have sometimes observed of the colours, which arise on polished steel by heating it, or on bell metal and some other metalline substances, when melted and poured on the ground, where it may cool in the open air, that they have, like those of water-bubbles, been a little changed by viewing them at divers obliquities; and particularly, that a deep blue or violet, when viewed very obliquely, hath been changed to a deep red. But the changes of these colours are not so sensible as of those made by water; for the scoria, or vitrified part of the metal, which most metals, when heated or melted, continually protrude to their surface, where, by covering them in form of a thin glassy skin, it causes these colours, is much denser than water, and I find, that the change made by the obliquation of the eye, is least in colours of the densest thin substances.

Obs. 20. As in the ninth observation, so here, the bubble, by transmitted light appeared of a contrary colour to that, which it exhibited by reflection.Thus, when the bubbles, being looked on by the light of the clouds reflected from it, seemed red at its apparent circumference, if the clouds at the same time, or very suddenly, were viewed through it, the colour at its circumference would be blue. And, on the contrary, when by reflected light it appeared blue, it would appear red by transmitted light.

Obs. 21. By wetting plates of Muscovy-glass, whose thinness made the like colours appear, the colours became more faint, especially by wetting the plates on that side opposite the eye; but I could not perceive any variation of their species. So that the thickness of a plate requisite to produce any colour, depends only on the density of the plate, and not of the ambient medium. And hence, by the tenth and sixteenth observations, may be known the thickness of bubbles of water or plates of Muscovy-glass, or of any other substances, which they have at any colour produced by them.

Obs. 22. A thin transparent body, which is denser than its ambient medium, exhibits more brisk and vivid colours than that, which is so much rarer; as I have particularly observed in air and glass: for, blowing glass very thin at a lamp furnace, those plates encompassed with air did exhibit colours much more vivid than those of air made thin between two glasses.

Obs. 23. Comparing the quantity of light reflected from the several rings, I found it was most copious from the first or inmost, and in the exterior rings became gradually less and less. Also the whiteness of the first ring was stronger than that reflected from those parts of the thinned medium, which were without the rings, as I could manifestly perceive by viewing at distance the rings made by the two object glasses; or by comparing two bubbles of water blown at distant times, in the first of which the whiteness appeared, which succeeded the colours, and the whiteness, which preceded them, in the other.

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Obs. 24. When the two object-glasses were laid upon one another, so as to make the rings of colours appear, though with my naked eye I could not discern above eight or nine of those rings, yet, by viewing them through a prism, I have seen a far greater multitude, insomuch, that I could number more than forty, besides many others, that were so very small and close together, that I could not keep my eye so steady on them severally as to number them: but by their extent I have sometimes estimated them to be more than a hundred. And, I believe, the experiment may be improved to the discovery of far greater numbers; for they seem to be really unlimited, though visible only so far as they can be separated by the refraction, as I shall hereafter explain.

But it was but one side of these rings, namely, that, towards which the refraction was made, which by that refraction was rendered distinct; and the other side became more confused than to the naked eye, insomuch that there I could not discern above one or two, and sometimes none of those rings, of which I could discern eight or nine with my naked eye. And their segments, or arcs, which on the other side appeared so numerous, for the most part exceeded not the third part of a circle. If the refraction was very great, or the prisms very distant from the object-glasses, the middle part of those arcs became also confused, so as to disappear and constitue an even whiteness, whilst on either side their ends, as also the whole arcs farthest from the center, became distincter than before, appearing in the form you see them here designed.

The arcs, where they seemed distinctest, were only white and black successively, without any other colours intermixed. But in other places there appeared colours whose order was inverted by the refraction, in such manner, that, if I first held the prism very near the object-glasses, and then gradually removed it farther off towards my eye, the colours of the second, third, fourth, and following rings shrunk towards the white, that emerged between them, until they wholly vanished into it at the middle of the arcs, and afterwards emerged again in a contrary order: but at the end of the arcs they retained their order unchanged.

I have sometimes so laid one object-glass upon the other, that, to the naked eye, they have all over seemed uniformly white, without the least appearance of any of the coloured rings; and yet, by viewing them through a prism, great multitudes of those rings have discovered themselves. And, in like manner, plates of Muscovy glass, and bubbles of glass blown at a lamp furnace, which were not so thin, as to exhibit any colours to the naked eye, have through the prism exhibited a great variety of them, ranged irregularly up and down, in the form of waves. And so bubbles of water, before they began to exhibit their colours to the naked eye of a by-stander, have appeared, through a prism, girded about with many parallel and horizontal rings; to produce which effect, it was necessary to hold the prism parallel, or very nearly parallel, to the horizon, and to dispose it so, that the rays might be refracted upwards.

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Having given my observations of these colours, before I make use of them to unfold the causes of the colours of natural bodies, it is convenient, that, by the simplest of them, I first explain the more compounded; such as are the second, third, fourth, ninth, twelfth, eighteenth, twentieth, and twenty-fourth.

And first, to show how the colours in the fourth and eighteenth observations are produced, let there be taken in any right line the lengths Y Z, Y A, and Y H, in proportion as four, nine, and fourteen; and between Z A and Z H eleven mean proportionals, of which let Z B be the second, Z C the third, Z D the fifth, Z E the seventh, Z F the ninth, and Z G the tenth. And at the points A, B, C, D, E, F, G, H, let perpendiculars A α, B β, &c. be erected, by whose intervals, the extent of the several colours set underneath against them, is to be represented. Then divide the line A α in such proportion as the numbers 1, 2, 3; 5, 6, 7; 9, 10, 11, &c. set at the point of division denote. And through those divisions from Y draw lines 1 I, 2 K, 3 L; 5 *m*, 6 *n*, 7 *o*, &c.

Now, if A 2 be supposed to represent the thickness of any thin transparent body, at which the utmost violet is most copiously reflected in the first ring or series of colours, then, by the thirteenth observation, H K will represent its thickness, at which the utmost red is most copiously reflected in the same series. Also, by the fifth and sixteenth observations, A 6, and H *n*, will denote the thickness at which those extreme colours are most copiously reflected in the second series, and so on. And the thickness, at which any of the intermediate colours are reflected most copiously, will, according to the fourteenth observation, be defined by the in <287> termediate parts of the lines 2 K, 6 *n*, &c. against which the names of those colours are written below.

But farther, to define the latitude of these colours in each ring or series, let A 1 design the least thickness, and A 3 the greatest thickness, at which the extreme violet in the first series is reflected; and let H I and H L design the like limit for the extreme red, and the intermediate colours be limited by the intermediate parts of the lines, 1 I and 3 L; against which the names of those colours are written. And in the second series, let those limits be the lines 5 M and 7 O; and so on: but yet with this caution, that the reflections be supposed strongest at the intermediate spaces, 2 K, 6 N, 10 R, &c. and to decrease gradually towards these limits, 1 I, 3 L; 5 M, 7 O, &c. on either side, where you must not conceive them to be precisely limited, but to decay indefinitely. And whereas I have designed the same latitude to every series, I did it, because, although the colours in the first series seem to be a little broader than the rest, by reason of a stronger reflection there; yet that inequality is so insensible as scarcely to be determined by observation.

Now, according to this description, conceiving, that the rays, in which several colours in here, are by turns reflected at the space 1 K, 3 L, 5 M, O 7, 9 P, R 11, &c. and transmitted at the spaces A H I 1, 3 L, M 5, 7 O, P 9, &c. it is easy to know what colour in the open air must be exhibited at any thickness of a transparent thin body. For, if a ruler be applied parallel to A H, at that distance from it by which the thickness of the body is represented, the alternate spaces 1 I, L 3, 5 M, O 7, &c. which it crosseth, will denote the reflected original colours, of which the colour exhibited in the open air is compounded. Thus, if the constitution of the green in the third series of colours be desired; apply the ruler, as you see, at π ρ σ φ, and by its passing through some of the blue at π, and yellow at σ, as well as through the green ρ, you may conclude, that green, exhibited at that thickness of the body, is principally constituted of original green, but not without a mixture of some blue and yellow. By this means you may know, how the colours from the center of the rings outwards ought to succeed in order, as they were described in the fourth and eighteenth observations: for, if you move the ruler gradually from A H through all distances, having past over the first space, which denotes little or no reflection to be made by thinnest substances, it will first arrive at 1, the violet, and then very quickly at the blue and green, which, together with that violet compounded blue, and then at the yellow and red, by whose further addition, that blue is converted into whiteness, which whiteness continues during the transit from I to 3; and after that, by the successive deficience of its component colours, turns first to compound yellow, and then to red, and last of all the red ceaseth at L Then begin the colours of the second series, which succeed in order between 5 and O, and are more lively than before, because more expanded and severed. And, for the same reason, instead of the former white, there intercedes between the blue and yellow a mixture of orange, yellow, green, blue and indico, all which together ought to exhibit a dilute and imperfect green. So the colours of the third series all succeed in <288> order; first the violet, which a little interferes with the red of the second order, and is thereby inclined to a redish purple; then the blue and green, which are less mixed with other colours, and consequently more lively than before, especially the green. Then follows the yellow, some of which towards the green is distinct and good; but that part of it towards the succeeding red, as also that red, is mixed with the violet and blue of the fourth series, whereby various degrees of red, very much inclining to purple, are compounded. The violet and blue, which should succeed this red, being mixed with, and hidden in it, there succeeds a green; and this at first is much inclined to blue, but soon decomes a good green; the only unmixed and lively colour in this fourth series: for as it verges towards the yellow, it begins to interfere with the colours of the fifth series, by whose mixture the succeeding yellow and red are very much diluted, and made dirty, especially the yellow, which being the weaker colour, is scarce able to shew itself. After this the several series interfere more and more, and their colours become more and more intermixed, till after three or four revolutions (in which the red and blue predominate by turns) all sorts of colours are in all places pretty equally blended, and compound one even whiteness.

And since, by the fifteenth observation, the rays indued with one colour are transmitted, where those of another colour are reflected, the reason of the colours made by the transmitted light, in the ninth and twentieth observations, is also from hence evident.

If not only the order and species of these colours, but also the precise thickness of the plate, or thin body, at which they are exhibited, be desired in parts of an inch, that may be also performed by assistance of the sixth or sixteenth observation. For, according to those observations, the thickness of the thinned air, which, between two glasses, exhibited the orange or bright red of the sixth order, was $\frac{1}{14554}$ parts of an inch. Now, suppose this thickness be represented by G τ, and the eleventh part of it, G λ, will be about $\frac{1}{160000}$ of an inch. And so G μ, G V, G ξ, G o, will be $\frac{3}{160000}$, $\frac{5}{160000}$, $\frac{7}{160000}$, and $\frac{9}{160000}$. And this being known, it is easy to determine what thickness of air is represented by G φ, or any other distance of the ruler from A H.

But further, since, by the tenth observation, the thickness of air was to the thickness of water, which between the same glasses exhibited the same colour, as four to three; and, by the twenty-first observation, the colours of thin bodies are not varied by varying the ambient medium; the thickness of a bubble of water exhibiting any colour will be three fourths of the thickness of air producing the same colour. And so, according to the same tenth and twenty-first observations, the thickness of a plate of glass, whose refraction is measured by the proportion of the sines thirty-one to twenty, may be $\frac{20}{31}$ of the thickness of air producing the same colours: and the like of other mediums. On these grounds I have composed the following table; wherein the thickness of air, water, and glass, at which each colour is most intense and specific, is expressed in parts of an inch divided into ten hundred thousand equal parts.

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Now, if this table be compared with the third scheme, you will there see the constitution of each colour, as to its ingredients, or the original colours, of which it is compounded, and thence be enabled to judge of its intenseness or imperfection, which may suffice in explication of the fourth and eighteenth <290>

observations, unless it be further desired to delineate the manner, how the colours appear, when the two object-glasses are laid upon one another: to do which let there be described a large arc of a circle and a strait line, which may touch that arc; are parallel to that tangent several occult lines at such distances from it, as the numbers set against the several colours in the table denote. For the arc and its tangent will represent the superficies of the glasses, terminating the interjacent air, and the places, where the occult lines cut the arc, will show at what distances from the center, or point of the contact, each colour is reflected.

There are also other uses for this table; for by its assistance the thickness of the bubble, in the nineteenth observation, was determined by the colours, which it exhibited. And so the bigness of the parts of natural bodies may be conjectured at by their colours, as shall be hereafter shown. Also, if two or more very thin plates be laid one upon another, so as to compose one plate, equalling them all in thickness, the resulting colour may be hereby determined. For instance, Mr. HOOKE, in his Micrographia, observes, that a faint yellow plate of Muscovy glass, laid upon a blue one, constituted a very deep purple. The yellow of the first order is a faint one, and the thickness of the plate exhibiting it, according to the table, is $5\frac{1}{4}$, to which add $9\frac{1}{2}$, the thickness exhibiting blue of the second order, and the sum will be $14\ \frac{3}{4}$, which most nearly approaches $14\ \frac{4}{5}$, the thickness exhibiting the purple of the third order.

To explain, in the next place, the circumstances of the second and third observations, that is, how the colours (by turning the prisms about their common axis the contrary way to that expressed in those observations) may be converted into white and black rings, and afterwards into colours again in an inverted order; it must be remembered, that those colours are dilated by obliquation of the rays to the air, which intercedes the glasses; and that, according to the table in the seventh observation, their dilation or reflection from the common center is most manifest and speedy when they are obliquest. Now, the rays of yellow being more refracted by the first superficies of the said air than those of red, are thereby made more oblique to the second superficies, at which they are reflected, to produce the coloured rings; and consequently, the yellow in each ring will be more dilated than the red; and the excess of its dilation will be so much the greater, by how much the greater is the obliquity of the rays, until at last it become of equal extent with the red of the same ring. And, for the same reason, the green, blue, and violet, will be also so much dilated by the still greater obliquity of their rays, as to become all very nearly of equal extent with the red; that is, equally distant from the center of the rings. And then all the colours of the same series must be coincident, and by their mixture exhibit a white ring; and these white rings must have black or dark rings between them, because they do not spread and interfere with one another as before; and, for that reason also, they must become distincter, and visible to far greater numbers. But yet the violet, being <291> obliquest, will be something more dilated in proportion than the other colours; and so very apt to appear at the exterior verges of the white.

Afterwards, by a greater obliquity of the rays, the violet and the blue become sensibly more dilated than the red and yellow; and so being further removed from the center of the rings, the colours must emerge out of the white in an order contrary to that which they had before, the violet and blue at the exterior limbs, and the red and yellow at the interior. And the violet, by reason of the greatest obliquity of its rays, being, in proportion, most of all expanded, will soonest appear at the exterior limb of each white ring, and become more conspicuous than the rest. And the several series of colours, by their unfolding and spreading, will begin again to interfere, and thereby render the rings less distinct, and not visible to so great numbers.

If, instead of the prisms, the object-glasses be made use of, the rings, which they exhibit, become not white and distinct by the obliquity of the eye, by reason, that the rays, in their passage through that air, which interceded the glasses, are very nearly parallel to themselves, when first incident on the glasses; and consequently, those indued with several colours are not inclined one more than another to that air, as it happens in the prisms.

There is yet another circumstance of these experiments to be considered; and that is, why the black and white rings, which, when viewed at a distance, appear distinct, should not only become confused by viewing them near at hand, but also yield a violet colour at both the edges of every white ring: and the reason is, that the rays, which enter the eye at several parts of the pupil, have several obliquities to the glasses, and those, which are most oblique, if considerd apart, would represent the rings bigger than those, which are the least oblique. Whence the breadth of the perimeter of every white ring is expanded outwards by the obliquest rays, and inwards by the least oblique. And this expansion is so much the greater, by how much the greater is the difference of the obliquity; that is, by how much the pupil is wider, or the eye nearer to the glasses: and the breadth of the violet must be most expanded, because the rays, apt to excite a sensation of that colour, are most oblique to the second or further superficies of the thinned air, at which they are reflected; and have also the greatest variation of obliquity, which makes that colour soonest emerge out of the edges of the white. And, as the breadth of every ring is thus augmented, the dark intervals must be diminished, until the neighbouring rings become continuous, and are blended, the exterior first, and then those nearer the center; so that they can no longer be distinguished a-part, but seem to constitute an even and uniform whiteness.

Amongst all the observations there is none accompanied with so odd circumstances as the twenty-fourth. Of those the principal are, that in thin plates, which, to the naked eye, seem of an even and uniform transparent whiteness, the refraction of a prism should make the rings of colours appear; whereas it usually makes objects to appear coloured only, where they are terminated with <292> shadows, or have parts unequally luminous; and that it should make those rings exceedingly distinct and white, although it usually renders those objects confused and coloured. The cause of these things you will understand by considering, that all the rings of colours are really in the plate, when viewed by the naked eye, although, by reason of the great breadth of their circumferences, they so much interfere, and are blended together, that they seem to constitute an even whiteness. But, when the rays pass through the prism to the eye, the orbits of the several colours in every ring are refracted, some more than others, according to their degree of refrangibility; by which means the colours on one side of the ring become more unfolded and dilated, and on the other side more complicated and contracted. And where, by a due refraction, they are so much contracted, that the several rings become narrower than to interfere with one another, they must appear distinct, and also white, if the constituent colours be so much contracted as to be wholly coincident: but on the other side, where every ring is made broader by the further unfolding its colours, it must interfere more with other rings than before, and so become less distinct.

To explain this a little further; suppose the concentric circles, A B and C D, represent the red and violet of any order, which, together with the intermediate colours, constitute any one of these rings. Now, these being viewed through a prism, the violet circle, B C, will, by a greater refraction, be further translated from its place than the red, A D, and so approach nearer to it on that side towards which the refractions are made. For instance, if the red be translated to *a d*, the violet may be translated to *b c*, so as to approach nearer to it at *c* than before; and, if the red be further translated to a d, the violet may be so much further translated to b c, as to convene with it at c, and, if the red be yet further translated to α δ, the violet may be still so much further translated to β γ, as to pass beyond it at γ, and convene with it at *e* and *f*. And this being understood, not only of the red and the violet, but of all the intermediate colours; and also of every revolution of those colours, you will easily perceive, how these of the same revolution or order, by their narrowness at *c d*, and δ γ, and their coincidence at *c d*, *e* and *f*, ought to constitute pretty distinct arcs of circles, especially at *c d*, or at *e* and *f*, and that they will appear several at *c d*, at c d exhibit whiteness by their coincidence, and again appear several at δ γ, but yet in a contrary order to that which they had before, and still retain beyond *e* and *f*. But, on the other side, at *a b*, a b, or α β, these colours must become much more confused by being dilated, and spread so as to interfere with those of other orders. And the same confusion will happen at δ γ between *e* and *f*, if the refraction be <293> very great, or the prism very distant from the object-glasses; in which case no parts of the ring will be seen, save only two little arcs at *e* and *f*, whose distance from one another will be augmented by removing the prism still further from the object-glasses. And these little arcs must be distinctest and whitest at their middle; and at their ends, where they grow confused, they must be coloured; and the colours at one end of every arc must be in a contrary order to those at the other end, by reason that they cross in the intermediate white; namely, their ends, which verge towards δ γ, will be red, and yellow on that side next the center, and blue and violet on the other side. But their other ends, which verge from δ γ, will, on the contrary, be blue and violet on that side towards the center, and on the other side red and yellow.

For confirmation of all this, I need to alledge no more, than that it is mathematically demonstrable from my former principles. But I shall add, that they, which please to take the prisms, may be the testimony of their senses be assured, that these explications are not hypothetical, but infallibly true and genuine: for in a dark room, by viewing these rings through a prism, by reflection of the several prismatic colours, which an assistant causes to move to and fro upon a wall or paper, from whence they are reflected, whilst the spectator's eye, the prism, and object-glasses (as in the thirteenth observation) are placed steddy, the position of the circles, made successively by the several colours, will be found such, in respect of one another, as I have described at *a b c d*, or a b c d, or α β γ δ. And by the same method the truth of the explications of the other observations is to be examined.

By what hath been said, the like phænomena of water-bubbles and thin plates of glass may be understood. But in small fragments of those plates, there is this further observable, that, if they, lying flat upon a table, be turned about their center, whilst they are viewed through a prism, some of them exhibit waves in one or two positions only; but the most of them do in all positions exhibit those waves, and that for the most part appearing almost all over the glass. The reason is, that the superficies of such plates are not even, but have many cavities and swellings, which, how shallow soever, do a little vary the thickness of the plate; and by the several sides of those cavities there must be produced waves in several postures of the prism. Now, though it be but some very small and narrow parts of the glass, by which these waves for the most part are caused, yet they may seem to extend themselves over the whole glass, because from the narrowest of those parts there are colours of several orders confusedly reflected, which by refraction of the prism are unfolded, and dispersed to several places, so as to constitute so many several waves as there were divers orders of the colours promiscuously reflected from that part of the glass.

These are the principal phænomena of thin plates or bubbles, whose explications depend on the properties of light, that I have heretofore delivered: and these, you see, do necessarily follow from them, and agree with them even to their least circumstances; and not only so, but do very much tend to <294> their proof. Thus, by the twenty-fourth observation, it appears, that the rays of several colours, made, as well by thin plates or bubbles, as by the refractions of a prism, have several degrees of refrangibility, whereby those of each order, which, at their reflection from the plate or bubble, are intermixed with those of other orders, are separated from them by refraction, and associated together, so as to become visible by themselves, like arcs of circles. For, if the rays were all alike refrangible, it is impossible, that the whiteness, which to the naked sense appears uniform, should by refraction have its parts transposed, and ranged into those black and white arcs.

It appears also, that the unequal refractions of difform rays proceed not from any contingent irregularities, such as are veins, an uneven polish, or fortuitous position of the pores of glass, unequal motions in the air or æther, spreading, breaking, or dividing the same ray into many diverging parts, or the like. For, admitting any such irregularities, it would be impossible for refractions to render those rings so very distinct and well defined, as they do in the twenty-fourth observation. It is necessary therefore, that every ray have its proper and constant degree of refrangibility connate with it; according to which its refraction is ever justly and regularly performed, and that several rays have several of those degrees.

And what is said of their refrangibility may be understood of their reflexibility; that is, of their dispositions to be reflected, some at a greater, and others at a less thickness of thin plates or bubbles, namely, that those dispositions are also connate with the rays, and immutable, as may appear by the thirteenth, fourteenth, and fifteenth observations, compared with the fourth and eighteenth.

By the precedent observations it appears also, that the whiteness is a dissimilar mixture of all colours, and that light is a mixture of rays endowed with all those colours. For, considering the multitude of the rings of colours in the third, twelfth, and twenty-fourth observations, it is manifest, that, although in the fourth and eighteenth observations there appear more than eight or nine of those rings, yet there are really a far greater number, which so much interfere and mingle with one another, as, after those eight or nine revolutions, to dilute one another wholly, and constitute an even and sensible uniform whiteness. And consequently, that whiteness must be allowed a mixture of all colours, and the light, which conveys it to the eye, must be a mixture of rays endued with all those colours.

But further, by the twenty-fourth observation it appears, that there is a constant relation between colours and refrangibility, the most refrangible rays being violet, the least refrangible red, and those of intermediate colours having proportionally intermediate degrees of refrangibility. And, by the thirteenth, fourteenth, and fifteenth observations, compared with the fourth or eighteenth, there appears to be the same constant relation between colour and refrangibility; the violet being on equal terms reflected at least thickness of any thin <295> plate or bubble; the red at greatest thickness, and the intermediate colours at intermediate thicknesses: whence it follows, that the colorific dispositions of rays are also connate with them, and immutable; and by consequence, that all the productions and appearances of colours in the world are derived, not from any physical change caused in light by refraction of reflection, but ony from the various mixtures or separations of rays, by virtue of their different refrangibility. And, in this respect it is, that the science of colours becomes a speculation more proper for mathematicians than naturalists.

This being read, occasion was taken to discourse of Mr. NEWTON's theory itself, and to debate, whether the rays of light, which, though alike incident in the same medium, yet exhibit different colours, may not reasonably be said to owe that exhibition of different colours to the several degrees of the velocity of pulses, rather than, as Mr. NEWTON thought, to the several connate degrees of refrangibility in the rays themselves?

Mr. HOOKE was of opinion, that the former of these ways was sufficient to give a good account of the diversity of colours

February 10. Dr. MAPLETOFT was elected and admitted.

Capt. SHEERES, Mr. HALL, and Signor TRAVAGINO were elected.

Mr. BERCHENSHAW presented himself to the Society, and shewed them his scale of music, wherein were contained,

1. A table of all consonant and dissonant intervals suitable to musical harmony, which are practicable, and may be expressed by the voice and other instruments. To these respective intervals apt and proper numbers were assigned, by which their ratio's and proportions were demonstrated.

2. A system of all the keys, by which the aforesaid intervals were completed; of which keys some were natural; some intended to the first degree of acuteness; some remitted to the first degree of gravity; some twice spissated; some twice asperated.

3. In this scale the magnitude, dimension, and proportion of the said keys were exactly demonstrated according to the proportional parts of a chord, the chord being supposed thirty-six inches long.

If it were demanded, whether there was any thing in this table and system, that was not to be found in the scales and writings of other musicians? he answered,

1. That the intervals in this table were perfect and complete. There was not one too many, nor one wanting, which might conduce to the making of harmony.

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2. That the sounds or musical numbers contained in this system arose out of the unison, and from one another, according to the reason of figurate, not simple numbers, (as, he said, he could demonstrate by numbers assigned to the respective intervals in the table) for that so the reason of the state of music required.

3. That there are neither more or less keys in this system, than would complete the aforesaid intervals.

4. That in this scale all the tones are of the same ratio, and that so are all the semitones, semiditones, ditones, and other intervals.

5. That the true magnitude and dimension of every one of the said keys are demonstrated according to the proportional parts of a chord.

6. That the natural, genuine, and true reason of the excellency and fullness of the harmony of three, four, frive, six, and seven parts, may clearly be discerned by the system of seven parts.

He added, that many other things were to be found in this table and scale, of which little or no mention is made in the scales and writings of either modern or antient musical authors; which, he said, he intended to discover, and to write of them at large, as he should be enabled thereunto.

He was exhorted to finish this work, or at least to publish this system with an explanation thereof.

After this was read the last part of Mr. NEWTON'S *observations*, wherein he considered in nine propositions, how the phænomena of thin transparent plates stand related to those of all other natural bodies: of which bodies having before mentioned, that they appear of divers colours, according as they are disposed to reflect most copiously the rays indued with these colours, he now inquires after their constitutions.

Here, among many other considerable things, he shews, how the bigness of the component parts of natural bodies may be conjectured by their colours: as also, that the cause of reflexion is not the impinging of light on the solid and impervious parts of bodies, as was commonly supposed.

This last part was as follows:

I am now come to the last part of this design; which is, to consider, how the phænomena of thin transparent plates stand related to those of all other natural bodies. Of these bodies I have already told you, that they appear of divers colours, according as they are disposed to reflect most copiously the rays endued with those colours. But their constitutions, whereby they reflect some rays <297> more copiously than others, remains to be inquired after. And this I shall endeavour in the following propositions.

Prop. 1. Those superficies reflect the greatest quantity of light, which have the greatest refracting power; that is, which interceeds mediums, that differ most in their refracting densities; and in the confines of equally dense mediums there is no reflection.

The analogy between reflection and refraction will appear by considering, that when light passeth obliquely out of one medium into another, which refracts from the perpendicular, the greater is the difference of their density, the less obliquity is requisite to cause a total reflection; because as the sines are, which measure the refraction, so is the sine of incidence, at which the total reflection begins, to the radius of the circle; and consequently that incidence is least, where there is the great difference of the sines. Thus in the passing of light out of water into air, where the refraction is measured by the ratio of the sines, 3 to 4, the total reflection begins, when the angle of the incidence is about forty- eight degrees and thirty-five minutes. In passing out of glass into air, where the refraction is measured by the ratio of the sines 20 to 31, the total reflection begins, when the angle of incidence is forty degress and ten minutes: and so, in passing out of crystal, or more strongly refracting mediums, into air, there is still a less obliquity requisite to cause a total reflection. Superficies therefore, which refract most, do soonest reflect all the light, which is incident on them, and so must be allowed most strongly reflective.

But the truth of this proposition will further appear, by observing, that in the superficies, interceeding any two of those mediums, air or water, or other liquors, common glass, crystal, and metalline glasses, the reflection is stronger or weaker accordingly as the superficies hath a greater or less refracting power. Thus, when other mediums are contiguous to air, the reflection is stronger in the superficies of glass than of water, still stronger in the superficies of crystal, and strongest in the superficies of metalline glass. So, in the confine of water and common glass, the reflection is very weak, but yet stronger than in the confine of water and oil, or almost any two liquors, and still stronger in the confine of water and crystal, or metalline glass: accordingly as those mediums differ more or less in density, so in the confine of common glass and crystal there is a weak reflection, and a stronger relection in the confine of common and metalline glass: but in the confine of two glasses of equal density, there is not any sensible reflection, as was shewn in the first observation. And the same may be understood of the superficies of two crystals or liquors, or any other substances, in which no refraction is caused: whence it comes to pass, that uniform mediums have no sensible reflexion but in their external superficies, where they are adjacent to their mediums of a different density.

Prop. 2. The least parts of natural bodies are in some measure transparent; and the opacities of those bodies arise from the multitude of reflections caused in their internal parts.

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That this is so, will easily be granted by them, that have been conversant with microscopes: and it may also be tried by applying any substance to a hole, through which the light is emitted into a dark room; for how opake soever that substance may seem in the open air, it will, by that means, appear very manifestly transparent, if it be of a sufficient thickness: only metalline bodies must be exempted, which, by reason of their excessive density seem to reflect almost all the light incident on their first superficies.

Prop. 3. Between the parts of opake or coloured bodies are many interstices, replenished with mediums of other densities, as water between the tinging corpuscles, wherewith any liquor is impregnated; air between the aqueous globules that constitute clouds or mists; and for the most part spaces void of both air and water; but yet perhaps replenished with some subtiler medium between the parts of hard bodies.

The truth of this is evinced by the two precedent propositions: for by the second proposition there are many reflections from the internal part of bodies, which by the first proposition would not happen, if the parts of those bodies were continued without any such interstices between them, because reflections are caused only in superficies, which interceed mediums of a different density.

But further, that this discontinuity of parts is the principal cause of the opacity of bodies, will appear by considering, that opake substances become transparent by filling their pores with any substance of equal, or almost equal density with their parts. Thus paper dipped in water or oil, the oculus mundi stone steeped in water, linen-cloth oiled or varnished, and many other substances soaked in such liquors, as will intimately pervade their little pores, become by that means more transparent than otherwise. So, on the contrary, the most transparent substances may, by separating their parts, be rendered sufficiently opake; as glass, by being reduced to powder, or otherwise flawed, water by being formed into many small bubbles, either alone in the form of froth, or by shaking it together with oil of turpentine, or some other convenient liquor, with which it will not incorporate, and horn by being scraped.

To the increase of the opacity of these bodies it conduces something, that by the twenty third observation, the refractions of very thin transparent substances are considerably stronger than those made by the same substances of a greater thickness. And to the reflection of solid bodies it may be further added, that the interstices of their parts are void of air. For that for the most part they are so, it is reasonable to believe, considering the ineptitude, which air hath to pervade small cavities, as appears by the ascension of water in slender glass-pipes, paper, cloth, and other such like substances, whose pores are found too small to be replenished with air, and yet large enough to admit water; and by the difficulty, wherewith air pervades the pores of a bladder, through which water find ready passage. And according to the eleventh observation, the cavities thus void of air will cause the same kind of effects as to reflection, which those do, that are replenished with it; but yet something more manifestly, be <299> cause the medium in relation to refractions is rarest, when most empty of air as Mr. HOOKE hath proved in his Micrographia; in which book he hath also largely discoursed of this and the precedent proposition, and delivered many other very excellent things concerning the colours of thin plates, and other natural bodies, which I have not scrupled to make use of so far as they were for my purpose.

Prop. 4. The parts of bodies and their interstices must not be less than of some definitive bigness, to render them opake and coloured; for the opakest bodies, if their parts be subtilly divided (as metals by being dissolved in acid menstruums, &c.) become prefectly transparent. And you may also remember, that in the eighth observation there was no reflection at the superficies of the object-glasses, where they were very near one another, though they did not absolutely touch. And in the seventeenth observation, the reflection of the water-bubble, where it became thinnest, was almost insensible, so as to cause the apparitions of very black spots.

On these grounds I conceive it is, that water, salt, glass, stones, and such like substances, are transparent; for, upon divers considerations, they seem to be as porous as other bodies, but yet their pores and parts too small to cause any opacity.

Prop. 5. The transparent parts of bodies, according to their several sizes, must *reflect* rays of one colour, and *transmit* those of another, on the same grounds, that thin plates or bubbles do reflect or transmit those rays: and this I take to be the ground of all their colours.

For, if a thinned or plated body, which being of an even thickness appears all over of one uniform colour, should be broken into fragments of the same thickness with the plate, I see no reason, why a heap of those fragments should not constitute a powder of the same colour, which the plate exhibited before it was broken. And the parts of all natural bodies, being like so many fragments of a plate, must on the same grounds exhibit the same colours.

Now, that they do so, will further appear by the affinity of their properties: as that the infusion of nephritic-wood, and many other substances reflect one colour, and transmit another, like thin bodies in the ninth and twentieth observations. That the colours of silks, cloaths, and other substances, which water or oil can intimately penetrate, become more faint and obscure by being emerged in those liquors, and recover their vigour again by being dried, much after the manner declared of thin bodies, in the tenth and twenty first observations: and that some of those coloured powders, which painters use, may have their colours a little changed, by being very elaborately and finely ground. Where I see not, what can be justly pretended for those changes, besides the breaking of their parts into less parts by that contrition, after the same manner that the colour of a plate is changed by varying its thickness. For which reason also it is, that many flowers, by being bruised, become more transparent <300> than before, or, at least in some degree or other, change their colours. Nor is it much less to my purpose, that, by mixing divers liquors, very odd and remarkable productions and changes of colours may be effected, of which no cause can be more obvious and natural, than that the saline corpuscles of one liquor do variously act upon, or unite with, the tinging corpuscles of another; so as to make them swell or shrink (whereby not only their bulk, but their density also may be changed) or to divide them into smaller corpuscles, or make many of them associate into one cluster; for we see how apt those saline menstruums are to penetrate and dissolve substances, to which they are applied; and some of them to precipitate what others dissolve. In like manner, if we consider the various phænomena of the atmosphere, we may observe, that when vapours are first raised, they hinder not the transparency of the air, being divided into parts too small to cause any reflection in their superficies: but when, in order to compose drops of rain, they began to coalesce and constitute globules of all intermediate sizes, those globules, when they become of a convenient size to reflect some colours, and transmit others, may constitute clouds of various colours, according to their sizes. And I see not what can be rationally conceived, in so transparent a substance as water for the production of these colours, besides the various sizes of its parcels, which seem to affect a globular figure most; but yet perhaps not without some instability in the smallest of them, by reason that those are most easily agitated by heat or any trembling motions in the air.

Prop. 6. The parts of bodies, on which their colours depend, are denser than the medium, which pervades their interstices.

This will appear by considering, that the colour of a body depends not only on the rays, which are incident perpendicularly or its parts, but on those also, which are incident at all other angles. And that, according to the seventh observation, a very little variation of obliquity will change the reflected colour, where the thin body or small particle is rarer than the ambient medium, in somuch that such a small particle will, at diversly oblique incidents, reflect all sorts of colours, in so great a variety, that the colour, resulting from them all confusedly reflected from a heap of such particles, must rather be a white or grey, than any other colour, or at best it must be but a very imperfect and dirty colour; whereas, if the thin body or small particle be much denser than the ambient medium, the colours, according to the nineteenth observation, are so little changed by the variation of obliquity, that the rays, which are reflected least obliquely, may predominate over the rest so much, as to cause a heap of such particles to appear very intensely of their colour.

It concludes also something to this proposition, that, according to the twenty-second observation, the colours exhibited by the denser thin body within the rarer are more brisk than those exhibited by the rarer within the denser.

Prop. 7. The bigness of the component parts of natural bodies may be conjectured by their colours.

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For since the parts of these bodies, by proposition 5. do most probably exhibit the same colours with a plate of equal thickness, provided they have the same refractive density; and since their parts seem for the most part to have much the same density with water or glass, as by many circumstances is obvious to collect: to determine the sizes of these parts, you need only have recourse to the precedent tables, in which the thickness of water or glass exhibiting any colour is expressed. Thus, if it be desired to know the diameter of a corpuscle, which being of equal density with glass, shall reflect green of the third order; the number $17\frac{1}{2}$ shows it to be about $\frac{17\frac{1}{2}}{1000000}$ parts of an inch.

The greatest difficulty is here to know, of what order the colour of any body is; and for this end we muct have recourse to the fourth and eighteenth observations, from whence may be collected these particulars.

Scarlets, and other *reds*, *oranges* and *yellows*, if they be pure and intense, are most probably of the second order. Those of the first and third order also may be pretty good; only the orange and red of the third order have too great a mixture of violet and blue.

There may be good *greens* of the fourth order, but the purest are of the third: and of this order the green of all vegetables seems to be, partly by reason of the intenseness of their colours, and partly because when they wither, some of them turn to a greenish yellow, and others to a more perfect yellow or orange, or perhaps to red; passing first through all the aforesaid intermediate colours, which changes seem to be effected by the exhaling of the moisture, which may leave the tinging corpuscles more dense, and something augmented by the accretion of the oily and earthy part of that moisture. Now the green, without doubt, is of the same order with those colours, into which it changeth, because the changes are gradual, and those colours, though usually not very pure, yet for the most part are too pure and lively to be of the fourth order.

Blues and *purples* may be either of the second or third order; but the best are of the third. Thus the colour of *violet* seems to be of that order; because their syrup, by acid liquors, turns red, and by urinous and alkalazite turns green. For since it is of the nature of acids to dissolve or attenuate, and of alcalis to precipitate or incrassate, if the purple colour of the syrup was of the second order, an acid liquor by attenuating its tinging corpuscles would tinge it to a red of the first order, and an alcali, by incrassating them, would change it to a green of the second order; which red and green, especially the green, seem too imperfect to be the colours produced by these changes. But if the said purple be supposed of the third order, its change to red of the second and green of the third may, without any inconvenience, be allowed.

If there be found any body of a deeper and less reddish purple than that of violets, its colour most probably is of the second order. But yet there being no body commonly known, whose colour is constantly more deep than theirs, <302> I have made use of their name to denote the deepest and least reddish purples, such as manifestly transcend their colour in purity.

The *blue* of the first order, though very faint and little, may possibly be the colour of some substances; and particularly the azure colour of the skies seems to be of this order. For all vapours, when they begin to condense and coalesce into small parcels, become first of that bigness, whereby such an azure must be reflected, before they can constitute clouds of other colours. And so this being the first colour, which vapours begin to reflect, it ought to be the colour of the finest and most transparent skies, in which vapours are not arrived to that grossness requisite to reflect other colours, as we find it is by experience.

Whiteness, if it be intense, is either that in the first order of colours, of which sort perhaps is the colour of white lead; or else it is a mixture of those succeeding the third or fourth order, such as is the colour of paper, linen, and most white substances. If corpuscles of various sizes, exhibiting the colours of the second and third order, be mixed, they should rather constitute an imperfect whiteness or grey, of which I have already spoken: but yet it seems not impossible for them to exhibit an intense whiteness, if they be disposed to transmit all the light, which they reflect not, and do not retain and stifle much of it. For thus I told you, that froth at a distance hath appeared very white, and yet, near at hand, the several bubbles, of which it was constituted, were seen tinged all over with rings of colours of the four or five first orders.

Lastly, for the production of *black*, the corpuscles must be less than any of those, which exhibit colours. For at all greater sizes there is too much light reflected to constitute this colour. But if they be supposed a little less than is requisite to reflect the blue of the first order, they will, according to the fourth, eighth, seventeenth, and eighteenth observations, reflect so very little light as to appear intensely black, and yet may perhaps variously refract it to and fro within themselves so long, until it happen to be stifled and lost; by which means they will appear black in all positions of the eye without any transparency. And from hence may be understood, why fire, and the more subtil dissolver, putrefaction, turn substances to black; why small quantities of black substances impart their colour very freely and intensely to other substances, to which they are applied; why glass ground very elaborately, on a copper-plate, till it be well polished, makes the sand, together with what is worn off from the glass, and copper, become very black; why black substances do soonest of all others become hot and burn, which effect may proceed, partly from the multitude of refractions in a little room, and partly from the easy commotion of so very small corpuscles; and why blacks are usually a little inclined to a bluish colour. For that they are so, may be seen by illuminating white paper by reflection from black substances, which will usually appear of a bluish white. And the reason is, that black borders on the obscure blue of the first order, described in the eighteenth observation, whence the corpuscles of black substances are most apt to reflect that colour.

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In these descriptions I have been the more particular, because it is not impossible, but that microscopes may at length be improved to the discovery of corpuscles of bodies, on which their colours depend. For if those instruments could be so far improved, as with sufficient distinctness to represent objects five or six hundred times bigger than at a foot distance they appear to our naked eyes. I should hope, that we might be able to discover some of the greatest of those corpuscles. And by one, that would magnify three or four thousand times, perhaps they might all be discovered but those, which produce blackness. In the mean while, I see nothing material, that rationally can be doubted of, excepting this position, that transparent corpuscles of the same thickness and density with a plate do exhibit the same colour. And this I would have understood not without some latitude, as well because those corpuscles may be of irregular figures, and many rays must be obliquely incident, and so have a shorter way through them than the length of their diameter; as because the straitness of the medium, pent in on all sides, may a little alter its motions, or other qualities, on which the reflexion depends. But yet I cannot much suspect the last, because I have observed of some small plates of Muscovy-glass, which were of an even thickness, that through a microscope they have appeared of the same colour at their edges and corners, where the included medium was terminated, which they appeared of in other places. However, it would add much to our satisfaction, if those corpuscles could be discovered with microscopes, which if we shall ever attain to, I fear it will be the utmost improvement of this sense; for it seems impossible to see the more secret and noble works of nature within those corpuscles, by reason of their transparency.

This may suffice concerning the constitution of natural bodies, on which their colours depend. But for further understanding the nature of reflections, I shall add these two following propositions.

Prop. 8. The cause of the reflection is not the impinging of light on the solid and impervious parts of bodies, as is commonly supposed.

This will appear by the following considerations: first, that in the passage of light out of glass into air, there is a reflection as strong or stronger than in its passage out of air into glass, and by many degrees stronger than in its passage out of glass into water. And it seems not probable, that air should have more reflecting parts than water or glass. But if that should possibly be supposed, it will avail nothing; for the reflection is as strong, if not stronger, when the air is drawn away from the glass (suppose in the air-pump invented by Mr. BOYLE) as when it is adjacent to it. Secondly, if light in its passage out of glass into air be incident more obliquely than at an angle of forty or forty-one degrees, it is wholly *reflected*; if less obliquely, it is in great measure *transmitted*. Now it is not to be imagined, that light at one degree of obliquity should meet with pores enough in the air to transmit the greater part of it, and at another degree of obliquity meet with nothing but parts to reflect it wholly; especially considering, that in its passage out of air into glass, how oblique soever be its incidence, it finds pores enough in the glass to transmit the greatest part of it. <304> If any man suppose, that it is not reflected by the air, but by the utmost superficial parts of the glass, there is still the same difficulty; besides, that such a supposition is unintelligible; and will also appear to be false, by applying water behind some part of the glass instead of air. For so in a convenient obliquity of the rays, suppose of forty-five or forty-six degrees, at which they are all *reflected*, where the air is adjacent to the glass, they shall be in great measure *transmitted*, where the water is adjacent to it; which argues, that their reflection or transmission depends on the consitution of the air and water behind the glass, and not on the parts of the glass.

Thirdly, if the colours made by a prism, placed at the entrance of a beam of light into a darkened room, be successively cast on a second prism placed at a great distance from the former, in such manner that they are all alike incident upon it; the second prism may be so inclined to the incident rays, that those, which are of a blue colour, shall be all reflected by it; and yet those of a red colour pretty copiously transmitted. Now if the reflection be caused by the parts of air or glass, I would ask, why at the same obliquity of incidence the blue should wholly impinge on those parts so as to be all reflected, and yet the red find pores enough to be in great measure transmitted. Fourthly, where two glasses touch one another, there is no sensible reflection, as was declared in the first observation; and yet I see no reason, why the rays should not impinge on the parts of glass, when contiguous to another glass, as much as when contiguous to air. Fifthly, when the top of a water-bubble (in the seventeenth observation) by the continual subsiding and exhaling of the water grew very thin, there was such a little and almost insensible quantity of light reflected from it, that it appeared intensely black; whereas, round about that black spot, where the water was thicker, the reflection was so strong as to make the water seem very white. Nor is it only at the least thickness of thin plates or bubbles that there is no manifest reflection, but at many other thicknesses continually greater and greater. For in the fifteenth observation, the rays of the same colour were by turns transmitted at one thickness, and reflected at another thickness, for an intermediate number of successions. And yet in the superficies of the third body, where it is of any one thickness, there are as many parts for the rays to impinge on, as where it is of any other thickness.

Lastly, if reflection were caused by the parts of reflecting bodies, it would be impossible for thin plates or bubbles, at the same place to reflect the rays of one colour, and transmit those of another, as they do according to the thirteenth and fifteenth observations. For it is not to be imagined, that at one place the rays, which, for instance, exhibit a blue colour, should have the fortune to dash upon the *parts*, and those, which exhibit a red, to hit upon the pores of the body; and then at another place, where the body is either a little thicker, or a little thinner, that on the contrary the blue should hit upon its *pores*, and the *red* upon its *parts*.

Prop. 9. It is most probable, that the rays, which impinge on the solid parts of any body, are not reflected but stifled and lost in that body.

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This is consentaneous to the precedent proposition, and will further appear by considering, that if all the rays should be reflected, which impinge on the internal parts of clear water or crystal, those substances should rather have a cloudy than so very clear transparency.

And further, there would be no principle of the obscurity or blackness, which some bodies have in all positions of the eye. For to produce this effect, it is necessary, that many rays be retained and lost in the body, and it seems not probable, that any rays can be stopped and retained in it, which do not impinge on its parts.

[1] [h] Register, vol. v. p. 65.

[2] [i] Transact. nº 88. p. 5088.

[3] [k] Transact. nº 88. p. 5087.

[4] [1] See Mr. HOOKE'S Micrographia, where he speaks of the inflexion of rays.

[5] [m] Letter-book, vol. vii. p. 280.

[6] [s] Obs. 24.

[7] [t] Letter-book, vol. vii. p. 284.

[8] [u] Letter-book, vol. vii. p. 282.

[9] [x] Professor of physic at Gresham College.

[10] [y] It is printed in the Philosoph. Transact. vol. x. nº 121. p. 495.

[11] [z] Register, vol. v. p. 89.

[12] [a] Note, that there is some light reflected from those parts of this black spot, where the glasses, by reason of their convexity, and some little unevenness of their surfaces, do no come to absolute contact. For by viewing the sun, by reflection from this spot, not only the verges of it become lucid, but divers lucid veins, as specks, appeared in the

midst of the blackness: but yet some parts of the spot seemed still as black as before, which parts I take to be those, where the glasses touched.

[13] c There are no letters entered from the beginning of the year 167$\frac{5}{6}$ till July 1677.