Chapter I
"New Theory about Light and Colours"

19 February 1672

Sir,

To perform my late promise to you, I shall without further ceremony acquaint you, that in the beginning of the year 1666 (at which time I applied myself to the grinding of optic glasses of other figures than *spherical*) I procured me a triangular glass prism, to try therewith the celebrated *phenomena* of *colours*. And in order thereto having darkened my chamber, and made a small hole in my window shuts, to let in a convenient quantity of the sun's light, I placed my prism at its entrance, that it might be thereby refracted to the opposite wall. It was at first a very pleasing divertisement, to view the vivid and intense colours produced thereby; but after a while applying myself to consider them more circumspectly, I became surprised to see them in an *oblong* form; which, according to the received laws of refraction, I expected should have been *circular*.

They were terminated at the sides with straight lines, but at the ends, the decay of light was so gradual, that it was difficult to determine justly, what was their figure; yet they seemed *semicircular*.

Comparing the length of this coloured *spectrum* with its breadth, I found it about five times greater; a disproportion so extravagant that it excited me to a more than ordinary curiosity of examining, from whence it might proceed. I could scarce think, that the various *thickness* of the glass, or the termination with shadow or darkness, could have any influence on light to produce such an effect; yet I thought it not amiss to examine first these circumstances, and so tried, what would happen by transmitting light through parts of the glass of diverse thicknesses, or through holes in the window of diverse bignesses, or by setting the prism without so that the light might pass through it, and be refracted before it was terminated by the hole: but I found none of these circumstances material. The fashion of the colours was in all these cases the same.

Then I suspected, whether by any *unevenness* in the glass, or other contingent irregularity, these colours might be thus dilated. And to try this, I took another prism like the former, and so placed it, that the light, passing through them both, might be refracted contrary ways, and so by the latter returned into that course, from which the former had diverted it. For, by this means I thought, the *regular* effects of the first prism would be destroyed by the second prism, but the *irregular* ones more augmented, by the multiplicity of refractions. The event was, that the light, which by the first prism was diffused into an *oblong* form, was by the second reduced into an *orbicular* one with as much regularity, as when it did not at all pass through them. So that, what ever was the cause of that length, 'twas not any contingent irregularity.

I then proceeded to examine more critically, what might be effected by the difference of the incidence of rays coming from diverse parts of the Sun; and to that end, measured the several lines and angles, belonging to the image. Its distance from the hole or prism was 22 foot; its utmost length 13¼ inches; its breadth 2⅝ inches; the diameter of the hole ¼ of an inch; the angle, which the rays, tending towards the middle of the image, made with those lines, in which they would have proceeded without refraction, 44 deg. 56'. And the vertical angle of the prism, 63 deg. 12'. Also the refractions on both sides of the prism, that is, of the incident, and emergent rays, were as near, as I could make them, equal, and consequently about 54 deg. 4'. And the rays fell perpendicularly upon the wall. Now subtracting the diameter of the hole from the length and breadth of the image, there remains 13 inches the length, and $2^{3/8}$ the breadth, comprehended by those rays, which passed through the centre of the said hole, and consequently the angle at the hole, which that breadth subtended, was about 31', answerable to the Sun's diameter; but the angle, which its length subtended, was more than five such diameters, namely 2 deg. 49'.

Having made these observations, I first computed from them the refractive power of that glass, and found it measured by the *ratio* of the sines, 20 to 31. And then, by that *ratio*, I computed the refractions of two rays flowing from opposite parts of the Sun's *discus*, so as to differ 31' in their obliquity of incidence, and found, that the emergent rays should have comprehended an angle of about 31', as they did, before they were incident.

But because this computation was founded on the hypothesis of the proportionality of the *sines* of incidence, and refraction, which though by my own experience I could not imagine to be so erroneous, as to make that angle but 31', which in reality was 2 deg. 49'; yet my curiosity caused me again to take my prism. And having placed it at my window, as before, I observed, that by turning it a little about its *axis* to and fro, so as to vary its obliquity to the light, more than by an angle of 4 or 5 degrees, the colours were not thereby sensibly translated from their place on the wall, and consequently by that variation of incidence, the quantity of refraction was not sensibly varied. By this experiment therefore, as well as by the former computation, it was evident, that the difference of the incidence of rays, flowing from diverse parts of the Sun, could not make them after intersection diverge at a sensibly greater angle, than that at which they before converged; which being, at most, but about 31 or 32 minutes, there still remained some other cause to be found out, from whence it could be 2 deg. 49'.

Then I began to suspect, whether the rays, after their trajection through the prism, did not move in curve lines, and according to their more or less curvature tend to diverse parts of the wall. And it increased my suspicion, when I remembered that I had often seen a tennis ball, struck with an oblique racket, describe such a curve line. For a circular as well as a progressive motion being communicated to it by that stroke, its parts on that side, where the motions conspire, must press and beat the contiguous air more violently than on the other, and there excite a reluctancy and reaction of the air proportionably greater. And for the same reason, if the rays of light should possibly be globular bodies, and by their oblique passage out of one medium into another acquire a circulating motion, they ought to feel the greater resistance from the ambient aether, on that side, where the motions conspire, and thence be continually bowed to the other. But notwithstanding this plausible ground of suspicion, when I came to examine it, I could observe no such curvature in them. And besides (which was enough for my purpose) I observed, that the difference between the length of the image, and diameter of the hole, through which the light was transmitted, was proportionable to their distance.

The gradual removal of these suspicions at length led me to the *experimentum crucis* [crucial experiment], which was this: I took two boards, and placed one of them close behind the prism at the window, so that the light might pass through a small hole, made in it for that purpose, and fall on the other board, which I placed at about 12 foot distance, having first made a small hole in it also, for some of that incident light to pass through. Then I placed another prism behind this second board, so that the light, trajected through both the boards, might pass through that also, and be again refracted before it arrived at the wall. This done, I took the first prism in my hand, and turned it to and fro slowly about its *Axis*, so much as to make the several parts of the image, cast on the second board, successively pass through the hole in it, that I might observe to what places on the wall the second prism would refract them. And I saw by the variations of those places, that the light, tending to that end of the image, towards which the refraction of the first prism was made, did in the second prism suffer a refraction considerably greater than the light tending to the other end. And so the true cause of the length of that image was detected to be no other, than that *light* consists of *rays differently refrangible*, which, without any respect to a difference in their incidence, were, according to their degrees of refrangibility, transmitted towards diverse parts of the wall.

When I understood this, I let off my aforesaid glass works; for I saw, that the perfection of telescopes was hitherto limited, not so much for want of glasses truly figured according to the prescriptions of optics authors (which all men have hitherto imagined) as because that light itself is a *heterogeneous mixture of differently refrangible rays*. So that, were a glass so exactly figured, as to collect any one sort of rays into one point, it could not collect those also into the same point, which having the same incidence upon the same medium are apt to suffer a different refraction. Nay, I wondered, that seeing the difference of refrangibility was so great, as I found it, telescopes should arrive to that perfection they are now at. For measuring the refractions in one of my prisms, I found, that supposing the common *sine* of incidence upon one of its planes was 44 parts, the *sine* of refraction of the utmost rays on the red end of the colours, made out of the glass into the air, would be 68 parts, and the *sine* of refraction of the utmost rays on the other end, 69 parts: so that the difference is about a 24th or 25th part of the whole refraction. And consequently, the object glass of any telescope cannot collect all the rays, which come from one point of an object, so as to make them convene at its *focus* in less room than in a circular space, whose diameter is the 50th part of the diameter of its aperture; which is an irregularity, some hundreds of times greater, than a circularly figured *lens*, of so small a section as the object glasses of long telescopes are, would cause by the unfitness of its figure, were light *uniform*.

This made me take *reflections* into consideration, and finding them regular, so that the angle of reflection of all sorts of rays was equal to their angle of incidence; I understood, that by their mediation, optics instruments might be brought to any degree of perfection imaginable, provided a *reflecting* substance could be found, which would polish as finely as glass, and *reflect* as much light, as glass *transmits*, and the art of communicating to it a parabolic figure be also attained. But these seemed very great difficulties, and I almost thought them insuperable, when I further considered, that every irregularity in a reflecting superficies makes the rays stray 5 or 6 times more out of their due course, than the like irregularities in a refracting one: so that a much greater curiosity would be here requisite, than in figuring glasses for refraction.

Amidst these thoughts I was forced from Cambridge by the intervening plague, and it was more than two years, before I proceeded further. But then having thought on a tender way of polishing, proper for metal, whereby, as I imagined, the figure also would be corrected to the last; I began to try, what might be effected in this kind, and by degrees so far perfected an instrument (in the essential parts of it like that I sent to *London*) by which I could discern Jupiter's 4 concomitants, and showed them diverse times to two others of my acquaintance. I could also discern the Moon-like phase of *Venus*, but not very distinctly, nor without some niceness in disposing the instrument.

From that time I was interrupted till this last Autumn, when I made the other. And as that was sensibly better than the first (especially for day objects) so I doubt not, but they will be still brought to a much greater perfection by their endeavours, who, as you inform me, are taking care about it at *London*.

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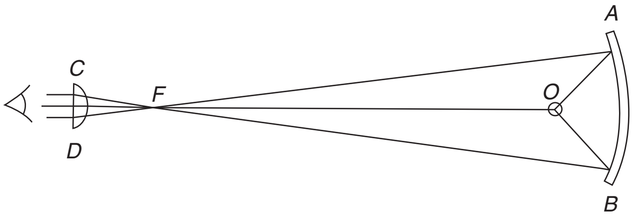

Figure 1.1

I have sometimes thought to make a microscope, which in like manner should have, instead of an object glass, a reflecting piece of metal. And this I hope they will also take into consideration. For those instruments seem as capable of improvement as *telescopes*, and perhaps more, because but one reflective piece of metal is requisite in them, as you may perceive by the annexed diagram, where *A B* represent the object metal, *C D* the eye glass, *F* their common focus, and *O* the other focus of the metal, in which the object is placed.

But to return from this digression, I told you, that light is not similar, or homogeneous, but consists of *difform* [diverse forms of] rays, some of which are more refrangible than others: so that of those, which are alike incident on the same medium, some shall be more refracted than others, and that not by any virtue of the glass, or other external cause, but from a predisposition, which every particular ray hath to suffer a particular degree of refraction.

I shall now proceed to acquaint you with another more notable difformity in its rays, wherein the *origin of colours* is unfolded: concerning which I shall lay down the *doctrine* first, and then, for its examination, give you an instance or two of the *experiments*, as a specimen of the rest.[1]

[1] In Newton's February 6 (1672) letter to Henry Oldenburg, Secretary of the Royal Society, which was the basis for the publication of "A New Theory" in the Society's *Philosophical Transactions*, the following passage was included (but removed for publication): "A naturalist would scarce expect to see the science of those become mathematical, & yet I dare affirm that there is as much certainty in it as in any other part of opticks. For what I shall tell concerning them is not an hypothesis but most rigid consequence, not conjectured by inferring 'tis thus because not otherwise or because it satisfies all phenomena (the philosophers' universal topic), but evinced by the mediation of experiments concluding directly & without any suspicion of doubt. To continue the historical narration of these experiments would make discourse too tedious & confused, & therefore I shall lay down the doctrine first . . ." See *Correspondence*, vol. 1, 96–7.

The doctrine you will find comprehended and illustrated in the following propositions.

1. As the rays of light differ in degrees of refrangibility, so they also differ in their disposition to exhibit this or that particular colour. Colours are not *qualifications of light*, derived from refractions, or reflections of natural bodies (as 'tis generally believed) but *original* and *connate properties*, which in diverse rays are diverse. Some rays are disposed to exhibit a red colour and no other, some a yellow and no other, some a green and no other, and so of the rest. Nor are there only rays proper and particular to the more eminent colours, but even to all their intermediate gradations.
2. To the same degree of refrangibility ever belongs the same colour, and to the same colour ever belongs the same degree of refrangibility. The *least refrangible* rays are all disposed to exhibit a *red* colour, and contrarily those rays, which are disposed to exhibit a *red* colour, are all the least refrangible: so the most *refrangible* rays are all disposed to exhibit a deep *violet colour*, and contrarily those which are apt to exhibit such a violet colour, are all the most refrangible. And so to all the intermediate colours in a continued series belong intermediate degrees of refrangibility. And this analogy between colours, and refrangibility, is very precise and strict, the rays always either exactly agreeing in both, or proportionally disagreeing in both.
3. The species of colour, and degree of refrangibility proper to any particular sort of rays, is not mutable by refraction, nor by reflection from natural bodies, nor by any other cause, that I could yet observe. When any one sort of rays has been well parted from those of other kinds, it hath afterwards obstinately retained its colour, notwithstanding my utmost endeavours to change it. I have refracted it with prisms and reflected it with bodies, which in daylight were of other colours; I have intercepted it with the coloured film of air interceding two compressed plates of glass; transmitted it through coloured mediums, and through mediums irradiated with other sort of rays, and diversely terminated it, and yet could never produce any new colour out of it. It would by contracting or dilating

become more brisk, or faint, and by the loss of many rays, in some cases very obscure and dark; but I could never see it changed *in specie*.

4. Yet seeming transmutations of colours may be made where there is any mixture of diverse sorts of rays. For in such mixtures, the component colours appear not, but by their mutual allaying each other, constitute a middling colour. And therefore, if by refraction, or any other of the aforesaid causes, the difform rays, latent in such a mixture, be separated, there shall emerge colours different from the colour of the composition. Which colours are not new generated, but only made apparent by being parted; for if they be again entirely mixed and blended together, they will again compose that colour, which they did before separation. And for the same reason, transmutations made by the convening of diverse colours are not real; for when the difform rays are again severed, they will exhibit the very same colours, which they did before they entered the composition; as you see, blue and yellow powders, when finely mixed, appear to the naked eye green, and yet the colours of the component corpuscles are not thereby really transmuted, but only blended. For, when viewed with a good microscope, they still appear blue and yellow interspersedly.
5. There are therefore two sorts of colours. The one original and simple, the other compounded of these. The original or primary colours are, *red*, *yellow*, *green*, *blue*, and a *violet-purple*, together with orange, indigo, and an indefinite variety of intermediate gradations.
6. The same colours in *specie* with these primary ones may be also produced by composition: For, a mixture of *yellow* and *blue* makes *green*; of *red* and *yellow* makes *orange*; of *orange* and *yellowish green* makes *yellow*. And in general, if any two colours be mixed, which in the series of those, generated by the prism, are not too far distant one from another, they by their mutual alloy compound that colour, which in the said series appeareth in the mid-way between them. But those, which are situated at too great a distance, do not so. *Orange* and *indigo* produce not the intermediate green, nor scarlet and green the intermediate yellow.

7. But the most surprising and wonderful composition was that of *whiteness*. There is no one sort of rays which alone can exhibit this. 'Tis ever compounded, and to its composition are requisite all the aforesaid primary colours, mixed in a due proportion. I have often with admiration beheld, that all the colours of the prism being made to converge, and thereby to be again mixed as they were in the light before it was incident upon the prism, reproduced light, entirely and perfectly white, and not at all sensibly differing from a *direct* light of the Sun, unless when the glasses, I used, were not sufficiently clear; for then they would a little incline it to *their* colour.
8. Hence therefore it comes to pass, that whiteness is the usual colour of *light*; for, light is a confused aggregate of rays [endowed] with all sorts of colours, as they are promiscuously darted from the various parts of luminous bodies. And of such a confused aggregate, as I said, is generated whiteness, if there be a due proportion of the ingredients; but if any one predominate, the light must incline to that colour; as it happens in the blue flame of brimstone [sulphur]; the yellow flame of a candle; and the various colours of the fixed stars.
9. These things considered, the *manner*, how colours are produced by the prism, is evident. For of the rays, constituting the incident light, since those which differ in colour proportionally differ in refrangibility, *they* by their unequal refractions must be severed and dispersed into an oblong form in an orderly succession from the least refracted scarlet to the most refracted violet. And for the same reason it is, that objects, when looked upon through a prism, appear coloured. For the difform rays, by their unequal refractions, are made to diverge towards several parts of the *retina*, and there express the images of things coloured, as in the former case they did the Sun's image upon a wall. And by this inequality of refractions they become not only coloured, but also very confused and indistinct.
10. Why the colours of the *rainbow* appear in falling drops of rain, is also from hence evident. For those drops, which refract the rays, disposed to appear purple, in greatest quantity to the spectator's eye, refract the rays of other sorts so much less, as to make them pass beside it; and such are the drops on the inside of the

primary bow, and on the outside of the *second* or exterior one. So those drops, which refract in greatest plenty the rays, apt to appear red, towards the spectator's eye, refract those of other sorts so much more, as to make them pass beside it; and such are the drops on the exterior part of the *primary*, and interior part of the *secondary* bow.

11. The odd phenomena of an infusion of *lignum nephriticum*,[2] *leaf gold*, *fragments of coloured glass*, and some other transparently coloured bodies, appearing in one position of one colour, and of another in another, are on these grounds no longer riddles. For, those are substances apt to reflect one sort of light and transmit another; as may be seen in a dark room, by illuminating them with similar or uncompounded light. For then they appear of that colour only, with which they are illuminated, but yet in one position more vivid and luminous than in another, accordingly as they are disposed more or less to reflect or transmit the incident colour.

12. From hence also is manifest the reason of an unexpected experiment, which Mr *Hooke* somewhere in his *Micrographia*[3] relates to have made with two wedge-like transparent vessels, filled the one with a red, the other with a blue liquor [liquid]: namely, that though they were severally transparent enough, yet both together became opaque; for if one transmitted only red, and the other only blue, no rays could pass through both.

13. I might add more instances of this nature, but I shall conclude with this general one, that the colours of all natural bodies have no other origin than this, that they are variously qualified to reflect one sort of light in greater plenty than another. And this I have experimented in a dark room by illuminating those bodies with uncompounded light of diverse colours. For by that means

[2] *Lignum nephriticum* is nephritic wood, which was reputed in the seventeenth century to be useful in curing ailments such as kidney stones, and which would give water an unusual golden color under some circumstances.

[3] See Robert Hooke, *Micrographia: or some physiological descriptions of minute bodies made by magnifying glasses, with observations and inquiries thereupon* (London: Royal Society, 1665), 73–4; the work is available in a modern reprint, volume XX of *Historiae naturalis classica*, edited by J. Cramer and H. K. Swann (New York: Wheldon and Wesley, 1961). Hooke was the chief experimentalist at the Royal Society and had previously worked with Robert Boyle during the 1650s.