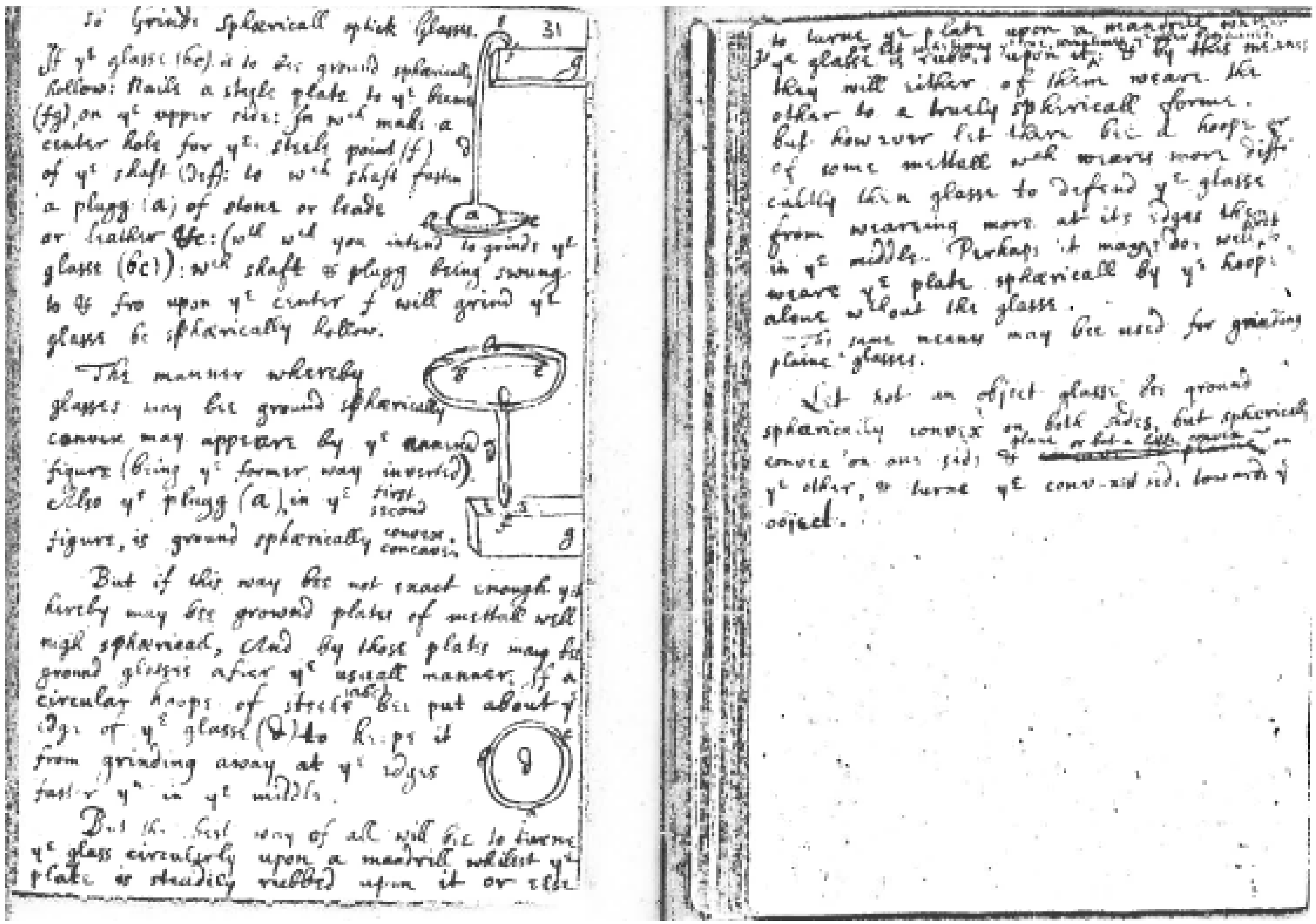
From Newton's College Notebook (ULC Add. 4000)



To grinde sphæricall optick glasses. 31

If ye glasse (bc) is to bee ground sphærically hollow: Naile a steele plate to ye beame (fg), on ye upper side: In wch make a center hole for ye steele point (f) of ye shaft (de): to wch shaft fasten a plugg (a) of stone or leade or leather &c: (wth wch you intend to grinde ye glasse (bc)): wch shaft & plugg being swung to & fro upon ye center f will grind ye glasse bee sphærically hollow.

The manner whereby glasses may bee ground sphærically convex may appeare by ye second figure (being ye former way inverted). Also ye plugg (a), in ye first / second figure, is ground sphærically convex / concave.

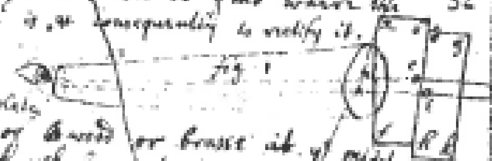
But if this way bee not exact enough yet hereby may bee ground plates of mettall well nigh sphæricall, And by those plates may bee ground glasses after ye usuall manner. If a circular hoope of steele (&c) bee put about ye edge of ye glasse (d) to keepe it from grinding away at ye edges faster yn in ye middle.

But the best way of all will bee to turne ye glasse circularly upon a mandrill whilst ye plate is steadily rubbed upon it or else to turne ye plate upon a mandrill whilst ye glasse is rubbed upon it; [illegible] by this meanes they will either of them weare the other to a truely sphæricall forme. But however let there bee a hoope of some mettall wch weares more difficultly than glasse to defend ye glasse from wearing more at its edges then in ye middle. Perhaps it may doe well first to weare ye plate sphæricall by ye hoope alone wthout the glasse.

The same meanes may bee used for grinding plaine glasses.

Let not an object glasse bee ground sphærically convex on both sides, but sphærically convex on one side & plane or but a little concave on ye other, & turne ye convex side towards ye object.

If the Glasses of a Telescope bee not truely ground how to find where the fault is, & consequently to rectify it.



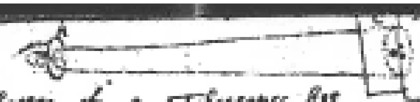
Take two plates abfh, dgkh, of wood or brasse at ye midst of ye sides of wch draw two very small holes (whose diameters are about ye 20th or 30th pte of an inch, that they may transmit but so small light as may serve to see ye edge of ye sunne or a starre of ye first magnitude)

Also make two other plates rs, & be like ye former but ye holes in ym must be as small as can bee (viz: ye 100th pte of an inch in diameter or lesse). for ye small end of ye tube.

Also make another plate wxzy, wth a hole in it about ye 5t or sixt pte of an inch in diameter (viz equall to ye diameter of ye pupill of ye eye, or eye glasse). make ye [illegible] wth a very small hole) for ye eyeglasse.

First cover ye object glasse wth ye plates [illegible] distant about ye holes in ym being distant about ye [illegible] parte of an inch, & placed neare ye center of ye object glasse. Also cover ye eye glasse wth ye plate A so yt its hole exactly respect ye center of ye eye glasse then turne ye tube to a starre wch will appeare like two starres if ye tube bee too long or short, wch bee shortned or lengthned untill there appeare but one. And then is ye Tube of a good ~~distance~~ length for ye vertices of ye glasses.

Secondly remove those plates, and instead thereof cover ye object glasse wth ye plate wxz, its hole exactly respecting ye center of ye glasse. (Fig 2)



If ye Glasses of a Telescope bee not truely ground. Theire errors ~~[illegible]~~ may bee thus found.

Because an error is much more easily discernable in ye object glasse yn in ye eye glasse let us first suppose ye eye glasse to bee ground true towards its center, (tis exact enough if it be sphericall, & not Hyperbolicall), & so wee may find & rectifie ye errors of ye object glasse.

First make a thin plate (A) of brasse & in the center of it a small hole (whose diameter perhaps may bee about ye 50th or 100th parte of an inch. With wch plate cover ye eye glasse ye center of it respecting ye center of ye glasse.

Secondly make two other plates the one B with two holes as neare to its edge as may bee their distance being about ye 8t pte of an inch or lesse, & ye other C wth one hole close to ye midst of its edge. Let ye diameters of these 3 holes bee about a 20th pte of an inch or lesse. And their edges must bee so thin that they may slide one upon another, & yet not let any rays passe through, to wch purpose make ym oblique. With these two plates cover the object glass (first stopping ye hole of C) & look at a starre (or ye edge of ye sunne &c) & if ye object appeare double (like two starres) make ye Tube longer or shorter untill it appeare single. Then open ye hole of C, & ye plate B being fixed, slide ye plate C up & downe still looking at ye starre, When their appear

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parts of the Sun, could not make them after decuſſation 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 degr. 49′.

Then I began to ſuſpect, whether the Rays, after their trajection through the Priſme, did not move in curve lines, and according to their more or leſs curvity tend to divers parts of the wall. And it increaſed my ſuſpition, when I remembred 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 ſtroak, 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 Æther, on that ſide, where the motions conſpire, and thence be continually bowed to the other. But notwithſtanding this plauſible ground of ſuſpition, when I came to examine it, I could obſerve no ſuch curvity in them. And beſides (which was enough for my purpoſe) I obſerved, that the difference 'twixt 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 ſuſpitions, at length led me to the *Experimentum Crucis*, which was this: I took two boards, and placed one of them cloſe behind the Priſme at the window, ſo that the light might paſs through a ſmall hole, made in it for the purpoſe, and fall on the other board, which I placed at about 12 feet diſtance, having firſt made a ſmall hole in it alſo, for ſome of that Incident light to paſs through. Then I placed another Priſme 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 Priſme 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 Priſme would refract them. And

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And I ſaw by the variation of thoſe places, that the light, tending to that end of the Image, towards which the refraction of the firſt Priſme was made, did in the ſecond Priſme ſuffer a Refraction conſiderably greater then the light tending to the other end. And ſo the true cauſe of the length of that Image was detected to be no other, then 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 divers parts of the wall.

When I underſtood this, I left 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 Optick Authors, (which all men have hitherto imagined,) as becauſe that Light it ſelf 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 Priſmes, 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 24*th* or 25*th* 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 then in a circular ſpace, whoſe diameter is the 50*th* part of the Diameter of its Aperture; which is an irregularity, ſome hundreds of times greater, then 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 Optick inſtruments might be brought to any degree of perfection imaginable, provided a *Reflecting* ſubſtance could be found,

A Passage from Newton to Oldenburg, 6 Feb 1672
Removed from the Published Letter

I shall now proceed to acquaint you with another more notable difformity in its Rays, wherein the *Origin of Colours* is unfolded. **A naturalist would scearce expect to see ye science of those become mathematicall, and 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 conseqence, not conjectured by barely inferring 'tis thus because not otherwise or because it satisfies all phenomena (the Philosophers universall Topick,) but evinced by ye mediation of experiments concluding directly and without any suspicion of doubt. To continue the historicall narration of these experiments would make a discourse too tedious and confused, and therefore I shall rather** 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.

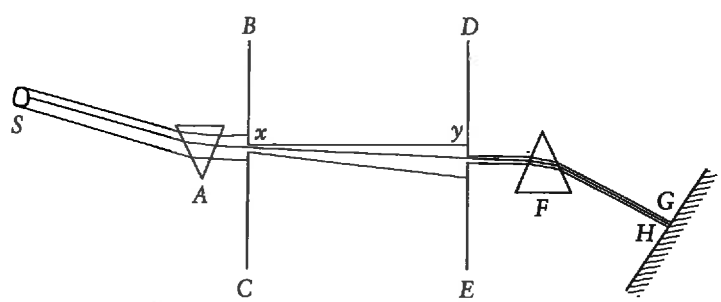

Fig. 9. The "crucial experiment", 1672. *A*, the first prism, *F* the second prism. *BC* the first diaphragm pierced at *x*; *DE* the second diaphragm pierced at *y*, about 12 feet from the former. *S* the hole in the shutter admitting the beam. As *A* is slowly turned axially, a succession of pure colours appears at different points on the screen *GH* [about twenty feet from *S*] such as red at *G*, violet at *H*, others in turn falling between. From Newton to Oldenburg for Pardies, 11 June 1672; *Philosophical Transactions* no. 85, 15 July 1672, p. 5016.

Mr. NEWTON's Answer to the foregoing Letter. № 85, *p.* 5014. *Translated from the Latin.*

In the observations of the Rev. F. Pardies, one can hardly determine whether there is more of humanity and candour, in allowing my arguments their due weight, or penetration and genius in starting objections. And doubtless these are very proper qualifications in researches after truth. But to proceed, F. Pardies says, that the length of the coloured image can be explained, without having recourse to the divers refrangibility of the rays of light; as suppose by the hypothesis of F. Grimaldi, viz. by a diffusion of light, which is supposed to be a certain substance put into very rapid motion; or by Mr. Hook's hypothesis, by a diffusion and expansion of undulations; which, being formed in the æther by lucid bodies, is propagated every way. To which may be added the hypothesis of Descartes, in which a similar diffusion of *conatus*, or pression of the globules, may be conceived, like as is supposed in accounting for the tails of comets. And the same diffusion or expansion may be devised according to any other hypotheses, in which light is supposed to be a power, action, quality, or certain substance emitted every way from luminous bodies.

In answer to this, it is to be observed that the doctrine which I explained concerning refraction and colours, consists only in certain properties of light, without regarding any hypotheses, by which those properties might be explained. For the best and safest method of philosophizing seems to be, first to inquire diligently into the properties of things, and establishing those properties by experiments and then to proceed more slowly to hypotheses for the explanation of them. For hypotheses should be subservient only in explaining the properties of things, but not assumed in determining them; unless so far as they may furnish experiments. For if the possibility of hypotheses is to be the test of the truth and reality of things, I see not how certainty can be obtained in any science; since numerous hypotheses may be devised, which shall seem to overcome new difficulties. Hence it has been here thought necessary to lay aside all hypotheses, as foreign to the purpose, that the force of the objection should be abstractedly considered, and receive a more full and general answer.

By light therefore I understand, any being or power of a being, (whether a substance or any power, action, or quality of it, which proceeding directly from a lucid body, is apt to excite vision. And by the rays of light I understand its least or indefinitely small parts, which are independent of each other; such as are all those rays which lucid bodies emit in right lines, either successively or all together. For the collateral as well as the successive parts of light are inde-

from Shapiro, Optical Lectures, Vol I
Lecture 3, pp. 86 ff

Verùm ne videar officij limites excessisse dum naturam colorum pertrectare aggredior, qui nihil ad Mathesin attinere censeantur: non abs re erit si de ratione incepti hujus iterum commonefaciam. Nimirum tanta est inter proprietates refractionum et colorum affinitas, ut seorsim explicari nequeant. Qui alterutras ritè velit cognoscere, ut alteras cognoscat necesse est. Et praeterea si de refractionibus non agerem, et earum disquisitio non esset in causâ quòd negotium de coloribus simul explicandis inceptarem: tamen generatio colorum tantam Geometriam complectitur, et eorum cognitio tantâ firmatur evidentiâ, ut vel ipsorum gratiâ possem aggredi, sic limites Mathesis nonnihil ampliaturus. Quemadmodum enim Astronomia, Geographia, Navigatio, Optica, et Mechanica pro scientijs mathematicis habentur, licèt in ijs agatur de rebus Physicis, Caelo, Terra, Navibus, luce et motu locali: Sic etiamsi colores ad Physicam pertineant, eorum tamen scientia pro Mathematicâ habenda est, quatenus ratione mathematicâ tractantur. Imò verò cùm horum accurata scientia videatur ex difficillimis esse quae Philosophus desideret; spero me quasi exemplo monstraturum quantùm Mathesis in Philosophiâ naturali valeat; et exinde ut homines Geometras ad examen Naturae strictiùs aggrediendum, & avidos scientiae naturalis ad Geometriam priùs addiscendam horter: ut nè priores suum omninò tempus in speculationibus humanae vitae nequaquam profuturis absumant, neque posteriores operam praeposterâ methodo usque navantes, a spe suâ perpetuò decidant: Verùm ut Geometris philosophantibus & Philosophis exercentibus Geometriam, pro conjecturis et probabilibus quae venditantur ubique, scientiam Naturae summis tandem evidentijs firmatam nanciscamur.(24)

But lest I seem to have exceeded the bounds of my position while I undertake to treat the nature of colors, which are thought not to pertain to mathematics, it will not be useless if I again recall the reason for this pursuit. The relation between the properties of refractions and those of colors is certainly so great that they cannot be explained separately. Whoever wishes to investigate either one properly must necessarily investigate the other. Moreover, if I were not discussing refractions, my investigation of them would not then be responsible for my undertaking to explain colors; nevertheless, the generation of colors includes so much geometry, and the understanding of colors is supported by so much evidence, that for their sake I can thus attempt to extend the bounds of mathematics somewhat, just as astronomy, geography, navigation, optics, and mechanics are truly considered mathematical sciences even if they deal with physical things: the heavens, earth, seas, light, and local motion. Thus although colors may belong to physics, the science of them must nevertheless be considered mathematical, insofar as they are treated by mathematical reasoning. Indeed, since an exact science of them seems to be one of the most difficult that philosophy is in need of, I hope to show—as it were, by my example—how valuable mathematics is in natural philosophy. I therefore urge geometers to investigate nature more rigorously, and those devoted to natural science to learn geometry first. Hence the former shall not entirely spend their time in speculations of no value to human life, nor shall the latter, while working assiduously with an absurd method, perpetually fail to reach their goal. But truly with the help of philosophical geometers and geometrical philosophers, instead of the conjectures and probabilities that are being blazoned about everywhere, we shall finally achieve a natural science supported by the greatest evidence.(24)