

Letter from Newton to Henry Oldenburg, dated 21 December 1675

Author: Isaac Newton

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<46r>

M^r Newton to M^r Oldenburg containing a more particular direction for trying the Experiment of glas rubbed to cause various motions, mentioned in his former letter of Dec. 14. 1675.

Sir^[1]

Upon your letter I took another glas 4 inches broad & $\frac{1}{4}$ of an inch thick, of such glas as Telescopes are made of & placed it a $\frac{1}{6}$ 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 & all was still the motion of the papers would continue sometimes while I counted a hundred, every paper leaping up about 20 times more or les & down as often. I tryed it also with two other glasses that belong to a Telescope & it succeeded with both, & I make no question but any glas will do that 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 glas you will faile in any I can send you. I am apt to suspect the failure was in the manner of rubbing: for I have observd that the rubbing variously or with various things alters the case. At one time I rubbed the afforesaid great glass with a napkin twice as much as I used to do with my gown & nothing would stir, & yet presently rubbing it with something els the motions soon began. After the glass has been much rubd too, the motions are not so lasting, & the next day I found the motions fainter & difficulter to excite then the first. If the Society have a mind to attempt it any more, I can give no better advice then 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,) & let this be rubbed not with linnen nor soft nappy woollen but with stuff whose threds may rake the surface of the glass, suppose Tamerine or the like doubled up in the hand, & this with a brisk motion as may be till a hundred or 150 may be counted, the glass lying all the while over the papers, then if nothing stirr rub the glas with your finger ends half a score times to & fro or knock your finger ends as oft upon the glas; 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 60 or 80 may be counted & then rub or knock again with your fingers, & repeat this till the electric virtue of the glass be so far excited as to take up the papers, & then a very little rubbing or knocking now & then will revive the motions. In doing all this let the rubbing be always done as nimbly as may be, & if the motion be circular like that of glas grinders 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 bein 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 my self till the last week, I suspect the Air might raise the gold & then a small attraction might determin it toward the glas. For I could not make it succeed.

As for M^r Hook's insinuation that the summ of the Hypothesis I sent you had been delivered by him in his Micrography, I need not be much concerned <46v> 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 shal do it as briefly as I can, & that the rather that I may avoyd the savour of having done any thing unjustifiable or unhansome towards M^r 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 & so moves more freely in them & transmits light more readily through them & that after such a manner as to accelerate the rays in a certain proportion. That refraction arises from this acceleration & has sines proportional. That light is at first uniform. That its colours are some disturbance or new modification of its rays by refraction or reflexion, 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 & retarding it on the other side where blew appears, & that there are but these two original colours or colour-making modifications of light, which by their various degrees, or, as M^r Hook call's it, dilutings, produce all intermediate ones. This rejected, the remainder of his Hypothesis is that he has changed Des-Cartes pressing or progressive motion of the Medium to a vibrating one, the rotation of the globuli to the obliquation of pulses, & the accelerating their rotation on the one hand & 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 & added another explication of the colours of natural bodies fluid & solid.

This I think is in short the summ of his Hypothesis & in all this I have nothing common with him but the supposition that æther is a Medium susceptible of vibrations of which supposition I make a very different use: he supposing it light it self which I suppose it is not. This is as great a difference as is between him & Des Cartes. But besides this the manner of refraction & reflexion & the nature & production of colours in all cases (which takes up the body of my discours) I explain very differently from him; & even in the colours of thin transparent substances I explain every thing after a way so differing from him, that the experiments I grownd my discours on destroy all he has said about them, & 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's common to us, that æther may vibrate, & so if he think fit to use that notion of colours arising from the various bignes of pulses (without which his Hypothesis will do nothing) his will borrow as much from my answer to his Objections as that I sent you does from his Micrography.

But it may be he means that I have made use of his Observations & of some I did; as that of the inflexion of rays, for which I quoted him; that of opacity arising from the interstices of the parts of bodies, which I insist not on; & that of plated bodies exhibiting colours, a Phænomenon for the notice of which I thank him: But he left me to find out & make such experiments about it as might inform me of the <47r> manner of the production of those colours, to ground an Hypothesis on; he having given no further insight into it then this that the colour depended on some certain thicknes of the plate: though what that thicknes was at every colour he confesses in his Micrographia he had attempted in vain to learn & therefore seing I was left to measure it my self I suppose he will allow me to make use of what I tooke the pains to find out. And this I hope may vindicate me from what M^r Hook has been pleasd to charge me with. Sir I doubt I have already troubled you with too long a letter, & so break of abruptly

Your humble Servant

Is. Newton.

Decemb. 21.
1675.

<47v>

For Henry Oldenburg Esquire
at his house about the middle of the
old Pal-mall in

Westminster

London.

Rec. Dec. 23. 75.

Answ. Dec. 30. 75.

[¹] Dec. 21. 1675.
