Letter from Newton to Henry Oldenburg, dated 10 January 1675/6

Author: Isaac Newton

Source: MS Add. 9597/2/18/48-49, Cambridge University Library, Cambridge, UK

Published online: February 2013

<48r>

jan. 10. $167\frac{5}{6}$.

Sir

Concerning the Experiment of the glas & papers I should add these two to the former directions. One that the glas be rubd with a full handfull of stuff which may cover & rub all the glas at once: for thus its' electric vertue will be more easily & vigorously excited then if rub'd with a little only doubled up but once or twice. This rubbing with the stuff I suppose rarefies & diffuses the electric effluvia from the glas into the air, & the knocking or rubbing with the finger ends puts the diffused effluvia into irregular motions: The other thing I would note is that the papers may perhaps be too little as well as too great. Too small ones will be apter to stick to the glas or table. If the Experiment be tryed with a glas three or four inches broad set about $\frac{1}{6}$ of an inch from the table, & the papers of a thin sort of paper cut into triangular pieces, the sides of those triangles may not unfitly be about the 20^{th} or 25^{t} part of an inch, more or less. It may be best tryed with bits of several sizes put in at once, & if there be put in a piece or two of the wing of a fly, those, I find, will move more easily though scarce so variously. These & the former directions observed, I cannot imagin how you should miss, though I cannot promis al things will appear justly to you as they did to me, there being unaccountable circumstances which may make a difference.

I am obliged to you Sir for your candor in acquainting me with M^{r} Hook's insinuations. It's but a reasonable piece of justice I should have an opportunity to vindicate my self from what may be undeservedly cast on me: & therefore since you have been pleased to be my representative there, & I have no means of knowing what's done but by you, I hope you will continue that equitable candor; though I think the present buisines of no great moment as to me, not imagining that the Royal Society are to be imposed on in a thing so plain, or that M^{r} Hook himself will persist in mistake when he hears the difference stated. The only thing I said he could pretend taken from his Hypothesis was the disposition of æther to vibrate, & yet whilst he grasps at all he is likely to fall short of this too. That æthereal vibrations are light is his; but that æther may vibrate (which is all I suppose) is to be had from a higher fountain: for that æther is a finer degree of air & air a vibrating Medium are old notions & the principles I go upon. I desire M^{r} Hook to shew me therefore, I say not only the summ of the Hypothesis I wrote, which is his insinuation, but any part of it taken out of his Micrographia: but then I expect too that he instance in what's his own. It's most likely he'l pretend I had from him the application of vibrations to the solution of the Phænomena of thin plates: & yet all the use I make of vibrations is to strengthen or weaken the reflecting power of the æthereal superficies, which is so far from being in his Micrographia, that the last Spring when I told him of the reflecting power of the æthereal superficies, he took it for a new notion, having till then supposed light to be reflected by the parts of gross bodies . To the things

that he has from Des Cartes, pray add this, that the parts of solid bodies have a vibrating motion least he should say I had from him what I say about heat. And his having from Des Cartes the reduction of all colours to two you may if need be explain further for me thus <48v> That as Des Cartes puts every globulus to be urged forward on one side by the illuminated Medium & impeded on the other by the dark one, so M^{r} Hook puts every vibration to be promoted at one end & retarded at the other by those Mediums, & thence both alike derive two modifications of light on the two sides of the refracted beam for the production of all colours.

[1] By M^{r} Gascoin's letter one might suspect that M^{r} Linus tryed the experiment some other way then I did, & therefore I shall expect till his friends have tried it according to my late directions: in which trial it may possibly be a further guidance to 'em to acquaint 'em that the Prism casts from it several images. One is that oblong one of colours which I mean & this is made by two refractions only. Another there is, made by two refractions & an intervening reflexion, & this is round & colourless if the angles of the Prism be exactly equal, but if the angles at the reflecting base be not equal it will be coloured, & that so much the more by how much unequaller the angles are, but yet not much unround unles the angles be very unequal. A third image there is made by one single reflexion & this is always round & colourles. The only danger is in mistaking the second for the first. But they are distinguishable not only by the length & lively colours of the first, but by its' different motion too: for whilst the Prism is turned continually the same way about its' axis, the second & third move swiftly, & go always on the same way till they dissappear, but the first moves slow & grows continually slower till it be stationary & then turns back again & goes back faster & faster till it vanish in the place where it began to appear. If without darkning their Room they " hold the prism at their window in the sun's open light in such a posture that " its' axis be perpendicular to the sun beams & then turn it about its' axis, they " cannot miss of seing the first image: which having found, " they may double up a paper once or twice, & make a round hole " in the middle of it about $\frac{1}{2}$ or $\frac{3}{4}$ of an inch broad, & hold the paper immedi" ately before the Prism " that the sun may shine on the Prism through that hole; & the " Prism being stayed & held steddy in that posture which makes the image stationary, if " the image then fall directly on an opposite wall, or on a sheet of paper placed " at the wall suppose 15 or 20 foot from the prism or further off, " they will see that image in such an oblong figure as I have described, with the red at " one end, the violet at the other & a blewish green in the middle; & if they ob" scure their Room as much as they can by drawing curtains or otherwise, it " will make the colours the more conspicuous. This direction I have set down that no body into whose hands a Prism shall happen may find difficulty or trouble in trying it. But when M^{r} Linus's friends have tryed it thus, they may proceed to repeat it in a dark room with a less hole made in their window shut. And then I shal desire that they will send you a full & clear description how they tryed it, expressing the length breadth & angles of the Prism, its position to the incident rays & to the window shut, the bignes of the hole in the shut through which the sun shined on the Prism, what side of the Prism the sun shine**{**d**}** on & at what side the light came out of it again, the distance of the Prism from the opposite paper or wall on which the refracted light was cast perpendicularly, & the length breadth & figure of the space there illuminated by that light, & the situation of each colour within that figure: & if they please to illustrate their description with a scheme or two it will make the buisines plainer. By this means if there be any difference in our way of experimenting, I shall be the better enabled to discern it & give 'em notice where the failure is & how to rectify it. I should be glad too if they would favour me with a description of the Experiment as it has been hitherto tryed by M^{r} Linus, that I <49r> may have an opportunity to consider what there is in that which makes against me. And because M^{r} Gascoin seems to suspect that my directions sent M^{r} Linus differ from what I have printed, I desire also that he would signify wherein he thinks they may differ so as to need reconciling. Fuller they are, but not different, nor any other then I have followed above these seven years. As for my suspicion that M^{r} Linus might possibly rely on old Experiments, his quoting Sir Kenelm Digby for a By-stander might have made any other stranger to his way as well as me suspect it: but I wonder most at M^{r} Gascoin's insinuation as if I influenced the Press in what concerns M^{r} Linus & me. You know Sir I never spake nor hinted a syllable to you concerning printing or not printing any thing of M^{r} Linus, nor so much as knew of the printing his first letter till it was out in the Transactions. When you sent it to me I out of a great desire to avoid controversies (which as you know I had enterteined long before) wrote back to you that I had no mind to meddle with it: but as I was ready to seal that letter, I added a Post-script to this purpose: That seing M^{r} Linus was designing somthing about light for the Pres, to prevent publishing his mistake you might if you thought fit signify to him (but not from me) that the Experiment was tryed otherwise then he suggested & that in such & such respects which I there named. And the substance of this Postscript was that you

published at the end of his first letter on which M^r^ Gascoin here animadverts, but was so far from being designd for the Press by me, that the first sight of it together with his letter in the Transactions made me say to one, that I wisht they had been suppress for I doubted the printing them would make M^r^ Linus unquiet & so in the end create me trouble. As for his 2^d^ letter which you shewed me at London I returnd it again to you so soon as I had read it & never saw it since, persisting in my desire to avoid the controversy. And at my returning it you moved me for an answer with this argument that if I wavd it M^r^ Linus was like to make the more stir: to which I replied that the buisines being about matter of fact was not proper to be decided by writing but by trying it before competent witnesses. Whereupon at your motion I told you what was requisite, & by your procurement preparations were accordingly made for it's tryall at the next Assembly of the Royal Society as I understood by M^r^ Hook: But the day provd cloudy & before another Assembly I returnd to Cambridge, & from that time never enquired after nor regarded the matter further till you sent me M^r^ Linus's third letter. This is the history of M^r^ Linus buisines so far as I know't: which I have set down that his friends may see he has not been dealt with obliquely as they seem to apprehend. All I think that they can object to you is that you were at a stand becaus you could not ingage me in the controversy, & to me that I had no mind to be ingag'd: a liberty every body has a right to & may gladly make use of, sometimes at least, & especialy if he want leisure or meet with prejudice or groundles insinuations. But I hope to find none of this in M^r^ Gascoin. The hansome genius of his present Letter makes me hope it for the future. In the mean time I desire with him that you would publish M^r^ Linus's letters as soon as you can conveniently to prevent further misapprehensions.

Sir I am

Your obliged & humble Servant

Is. Newton.

< insertion from the bottom of the page >

Pray Sir let not my papers
go out of your hands till you hear
from me about registring them.

In printing my former letter to M^r^ Linus you
may leave out what I mention of M^r^ Hill & M^r^ Hook, or
at least put letters for their names: for I beleive they had rather not be mentiond. If you have opportunity pray present my service to M^r^ Hook, for I suppose there is nothing but misapprehension in what has lately happend.

< text from f 49r resumes >
<49v>

Newtons Letter
Rec. jan. 12. $\frac{75}{76}$.
Answ. jan. 15. 75/76.

[1] Philosophical Transactions N. 121. p. 503.