# Letter to Oldenburg on the lengths and angles of prism images, dated 18 August 1676

**Author:** Isaac Newton

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M^r Newtons Answer to the precedent Letter,
sent to the Publisher.

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Sir The things opposed by M^r Line being upon tryalls found true & granted me; I begin with the new question about the proportion of the length of the Image to it's breadth. This I call a new one, for though M^r Line in his last letter spake against so great a length as I assigne, yet, as it seems to me, it was not to grant any transvers length shorter then that assigned by me (for in his first letter he absolutely denyed that there would be any such length;) but to lay the greater emphasis upon his discours whilst in defence of common Optiques he was disputing in general against a transvers Image: & therefore in my answer I did not prescribe the just quantity of the refracting angle with which I would have the experiment repeated : which would have been a necessary circumstance had the dispute been about the just proportion of the length to the breadth. Yet I added * **{***}** In my 1st letter **{**in**}** Phil. Trans. **{**N.**}** 121. p. 500. this note, that the bigger the angle of the Prism is, the greater will be the length in proportion to the breadth: not imagining but that when he had found in any Prism the length of the Image transvers to the axis, he would easily thence conclude that a Prism with a greater angle would make the Image longer, & consequently that by using an angle great enough he might bring it to equal or exceed the length assigned by me: as indeed he might, for by taking an angle of 70 or 75 degrees, or a little greater, he might have made the length not only 5 but 6 or 8 times the breadth & more. No wonder therefore that M^r Lucas found the Image shorter then I did, seing he tryed the experiment with a less angle.

The angle indeed which I used was but about $63^{degr.}$, $12^{min}$, & his is set down $60^{degr}$: the difference of which from mine being but $3^{degr.}$, $12^{min}$, is too little to reconcile us, but yet it will bring us considerably nearer together. And if his angle was not exactly measured, but the round number of $60^{degr.}$ set down by guess or by a less accurate measure (as I suspect by the conjectural measure of the refraction of his prism by the ratio of the sines 2 to 3 set down at the same time instead of an experimental one,) then might it be two or three degrees less then 60, if not still less: & all this, if it should be so, would take <53v> **{**away**}** the greatest part of the difference between us.

But however it be, I am well assured my own observation was exact enough. For I have repeated it divers times since the receipt of M$^{r}$ Lucas's letter, & that without any considerable difference of my observations either from one another or from what I wrote before. And that it might appear experimentally how the increase of the angle increases the length of the Image, & also that no body who has a mind to try the experiment exactly might be troubled to procure a Prism which has an angle just of the bigness assigned by me; I tryed the experiment with divers angles; & have set down my trialls in the following table; where the first column expresses the six angles of two Prisms which I used, which were measured as exactly as I could by applying them to the angle of a Sector; & the second column expresses in inches the length of the image made by each of those angles; its breadth being two inches: its distance from the Prism 18 feet & 4 inches, & the breadth of the hole in the window shut $\frac{1}{4}$ of an inch.

| | The angles of degr min | The lengths of y$^{e}$ Image |
|---|---|---|
| y$^{e}$ 1$^{st}$ Prism | 56 . 10 | $7\frac{3}{4}$ |
| | 60 . 24 | $9\frac{1}{2}$ |
| | 63 . 26 | $10\frac{1}{3}$ |
| y$^{e}$ 2$^{d}$ Prism | 54 . 0 | $7\frac{1}{3}$ |
| | 62 . 12 | $10\frac{1}{8}$ |
| | 63 . 48 | $10\frac{3}{4}$ |

[1]

You may perceive that the lengths of the images in respect of the angles that made them, are something greater in the second Prism then in the first: but that was because the glass of which the second Prism was made, had the greater refractive power.

The days in which I made these trialls were pretty clear but not so clear as I desired, & therefore afterward meeting with a day as clear as I desired, I repeated the experiment with the second Prism, & found the lengths of the image made by its severall angles to be about $\frac{1}{4}$ of an inch greater then before, the measures being those set down in this table.

| | The angles of degr min | The lengths of y$^{e}$ Image |
|---|---|---|
| y$^{e}$ 2$^{d}$ Prism | 54 . 0 | $7\frac{2}{3}$ . |
| | 62 . 12 | $10\frac{1}{2}$ . |
| | 63 . 48 | 11 . |

The reason of this difference I apprehend was that in the clearest days, the light of the white skies which dilutes & renders invisible the faintest colours at the ends of the Image is a little diminished in a clear day & so gives leave to the colours to appear to a greater length; the Sun's light at the same time becoming brisker & so strengthening the colours & making the faint ones at the two ends <53ar> more conspicuous. For I have observed that in days something cloudy, whilst the Prism has stood unmoved at the window, the image would grow a little longer or a little shorter accordingly as the Sun was more or less obscured by thin clouds which passed over it; the image being shortest when the cloud was brightest & the suns light faintest. Whence it is easy to apprehend that if the light of the clouds could be quite taken away, so that the Sun might appear surrounded with darkness, or if the suns light was much more stronger then it is the colours would still appear to a greater length.

In all these observations the breadth of the Image was just two inches. But observing that the sides of the two Prisms I used were not exactly plain but a little convex, (the convexity being about so much as that of a double convex glass of a sixteen or eighteen foot Telescope), I took a third Prism whose sides were as much concave as those of the other were convex; & this made the breadth of the Image to be two inches & a third

part of an inch: the angles of this Prism & the lengths of the Image made by each of those angles being those exprest in this Table

| The angles of the Prism degr. | The lengths of of y$^{e}$ Image in inches. |
|---|---|
| 58 | $8\frac{1}{2}$ . |
| $59\frac{1}{2}$ | 9 . |
| $62\frac{1}{2}$ | $10\frac{1}{3}$ . |

In this case you see the concave figure of the sides of the Prism by making the rays diverge a little, causes the breadth of the Image to be greater in proportion to its length then it would be otherwise. And this I thought fit to give you notice of, that M$^{r}$ Lucas may examin whether his Prism have not this fault. If a Prism may be had with sides exactly plain, it may do well to try the experiment with that: but it's better if the sides be about so much convex as those of mine are, because the Image will thereby become much better defined. For this convexity of the sides does the same effect as if you should use a Prism with sides exactly plain, & between it & the hole in the window shut, place an object-glass of an 18 foot Telescope to make the round image of the sun appear distinctly defined on the wall when the Prism is taken away, & consequently the long image made by the Prism to be much more distinctly defined (especially at it's streight sides) then it would be otherwise.

One thing more I shall add: That thee utmost length of thee Image from the faintest red at one end to the faintest blew at the other, must be measured. For in my first letter about colours where I set down the length to be five times the breadth, I called that length the utmost length of the image; & I measured the utmost length, because I account all that length to be caused by the imedat light of the sun, seing <53av> the colours (as I noted above) become visible to the greatest length in the clearest days, that is, when the light of the Sun transcends most the light of the clouds. Sometimes there will happen to shoot out from both ends of the Image a glaring light a good way beyond these colours, but this is not to be regarded, as not apperteining to the Image. If the measures be taken right the whole length will exceed the length of the streight sides by about the breadth of the Image.

By these things set down thus circumstantially, I presume M$^{r}$ Lucas will be enabled to accord his trials of the Experiment with mine; so nearly at least that there shall not remain any very considerable difference between us. For if some little difference should still remain, that need not trouble us any further seeing there may be many various circumstances which may conduce to it; such as are not only the different figures of prisms, but also the different refractive power of glasses, the different diameters of the Sun at divers times of the year, & the little errors that may happen in measuring lines & angles, or in placing the Prism at the window: though for my part I took care to do these things as exactly as I could. However M$^{r}$ Lucas may make sure to find the Image as long or longer then I have set down, if he take a Prism whose sides are not hollow grownd, but plain, or (which is better) a very little convex, & whose refracting angle is as much greater then that I used as that he has hitherto tryed it with is less; that is, whose angle is about 66 or 67 degrees, or (if he will) a little greater.

Concerning M$^{r}$ Lucas's other experiments, I am much obliged to him that he would take these things so far into consideration, & be at so much pains for examining them; & I thank him so much the more because he is the first that has sent me an experimental examination of them. By this I may presume he really desires to know what truth there is in these matters. But yet it will conduce to his more speedy & full [2][3]satisfaction if he a little change the method which he has propounded, & instead of a multitude of things try only the Experimentum Crucis. For it is not number of Experiments, but weight to be regarded; & where one will do, what need many?

Had I thought more requisite, I could have added more. For before I wrote my first letter to you about colours I had taken much pains in trying experiments about them & written a Tractate on that subject wherein I had set down at large the principall of the experiments I had tryed; amongst which there happened to be the

principal of those Experiments which M^r^ Lucas has now sent me. And as for the Experiments set down in my first letter to you, they were only such as I thought convenient to select out of that Tractate.

<53br>

But suppose those had been my whole store, yet M^r^ Lucas should not have grownded his discourse upon a supposition of my want of experiments till he had examined those few. For if any of those be demonstrative, they will need no assistants nor leave room for further disputing about what they demonstrate.

The main thing he goes about to examin is the different refrangibility of light. And this I demonstrated by the Experimentum Crucis. Now if this demonstration be good, there needs no further examination of the thing; if not good the fault of it is to be shewn: for the only way to examin a demonstrated proposition is to examin the demonstration. Let that experiment therefore be examined in the first place, & that which it proves be acknowledged, & then if M^r^ Lucas want my assistance to unfold the difficulties which he fancies to be in the experiments he has propounded, he shall freely have it; for then I presume a few words may make them plain to him: whereas should I be drawn from a demonstrative experiment to begin with those, it might create us both the trouble of a long dispute, & by the multitude of words cloude rather then clear up the truth. For if it has already cost us so much trouble to agree upon the matter of fact in the first & plainest experiment, & yet we are not fully agreed: what an endless trouble might it create us, if we should give our selves up to dispute upon every argument that occurs, & what would become of truth in such a tedious dispute. [4]The way therefore that I propound being the shortest & clearest (not to say the only proper way,) I question not but M^r^ Lucas will be glad that I have recommended it, seeing he professes that it is the knowledge of truth that he seeks after. And therefore at present I shall say nothing in answer to his experimental discourse but this in general: that it has proceeded partly from some misunderstanding of what he writes against, & partly from want of due caution in trying experiments; & that amongst his experiments there is one, which when duely tryed, is, next to the Experimentum Crucis, the most conspicuous Experiment I know for proving the different refrangibility of light, which he brings it to prove against.

By the Post-script of M^r^ Lucas's letter, one not acquainted with what has passed, might think, that he quotes the Observation of the R. Society against me; whereas the relation of their Observation, which you sent to Leige, conteined nothing at all about the just proportion of the Length of the Image to it's Breadth according to the angle of the Prism, nor any thing more (so far as I can perceive by your last) than what was pertinent to the things then in dispute, viz, that they found them succeed as I had affirmed. And therefore since M^r^ Lucas has found the same success, I suppose <53bv> that when he expressed that he much rejoyced to see the trialls of the R. Society agree so exactly with his, he meant only so far as his agreed with mine.

And because I am again upon this first experiment, I shall desire that M^r^ Lucas will repeat it with all the exactness & caution that may be, regard being had to the information about it, set down in this letter; & then I desire to have the length & breadth of the Image with its distance from the Prism set down exactly in feet & inches & parts of an inch, that I may have an opportunity to consider what relation it's length & breadth have to the sun's diameter. For I know, that M^r^ Lucas's Observation cannot hold where the refracting angle of the Prism is full 60^degr:^, & the day is clear & the full length of the colours is measured, & the breadth of the Image answers to the sun's diameter: & seeing I am well assured of the truth & exactness of my own observation, I shall be unwilling to be diverted by any other experiments from having a fair end made of this in the first place.

Sir

I am

Your humble Servant

Is. Newton.

Cambridge
Aug 18. 1676

Postscript.

I had like to have forgotten to advise that the
Experimentum Crucis, & such others as shall
be made for knowing the nature of colours;
be made with Prisms which refract so much
as to make the length of the Image five times
it's breadth, & rather more then less; for,
otherwise Experiments will not succeed so plainly with
others as they have done with me.

[1] Yyyy 6.

[2] Zzzz {7} 703

[3] pray {ed}{2d} turn {this} a {Leaf}

[4] {4 Zz 704}