Letter from Newton to John Collins, dated 10 December 1672

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Cambridge.
December 10^{th} 1672.

Sir

My unwillingness to trouble you in the midst of your late buisiness made me suspend writing, though I stood obliged to thank you for the rest of M^{r} Horrox works which I received some weeks since. But by yours which I received two days since I presume the most of that trouble is over. I am heartily glad at the acceptance which our Reverend friend D^{r} Barrow's Lectures finds with forreign Mathematicians, & it pleased me not a little to understand that they are falln into the same method of drawing Tangents with me. What I guess their method to be you will apprehend by this example. Suppose CB applyed to AB in any given angle be terminated at any Curve line AC, and calling AB x & BC y let the relation between x & y be expressed by any æquation as $x^3 - 2xxy + bxx - bbx + byy - y^3 = 0$ whereby the curve is determined. To draw the tangent CD the Rule is this. Multiply the termes of the æquation by any arithmeticall progression according to the dimensions of y suppose thus $\begin{matrix} x^3 & -2xxy & +bxx & -bbx & +byy & -y^3 \\ 0 & 1 & 0 & 0 & 2 & 3 \end{matrix}$, also according to the dimensions of x suppose thus $\begin{matrix} x^3 & -2xxy & +bxx & -bbx & +byy & -y^3 \\ 3 & 2 & 2 & 1 & 0 & 0 \end{matrix}$. The first product shall be the Numerator, & the last divided by x the Denominator of a fraction which expresseth the length of BD to whose end D the tangent CD must be drawn. The length BD therefore is $\frac{-2xxy+2byy-3y^3}{3xx-4xy+2bx-bb}$.

This Sir is one particular, or rather a Corollary of a Generall Method which extends it selfe without any troublesome calculation, not onely to the drawing tangents to all curve lines whether Geometrick or mechanick or how ever related to streight lines or to other curve lines but also to the resolving other abstruser kinds of Problems about the crookedness, areas, lengths, centers of gravity of curves &c. Nor is it (as Huddens method de maximis et minimis & consequently Slusius his new method of Tangents as I presume) limited to æquations which are free from surd quantities. This method I have interwoven with that other of working in æquations by reducing them to infinite series. I remember I once occasionally told D^{r} Barrow when he was about to publish his Lectures that I had such a method of drawing Tangents but some divertisment or other hindered me from describing it to him.

Of resolving by Cardans rules Equations that have 3 possible roots there may be examples framed at pleasure, but unlesse Brasser show a direct method of performing it, which Ferguson doth not, it will not be allowed scientifick. How it is to be done directly I may possibly show upon occasion.

M^{r} Gregory is pleased to consider further the most advantageous construction of Catadioptricall Telescopes. And as his designe in his Optica promota excells that of M. Cassegrain (though they differ so sleightily that I thought it not worth the while to takeing notice of the difference) the advantage being that the little concave Ellipsis comes nearer to a sphericall figure then the convex Hyperbola; so I conceive his present proposall excells them both, of making that speculum plane. And this I conjecture is the way which Signor Salvetti one of the great Dukes Musitians mentioned in the last Transactions intends to make experiment of, excepting that instead of the convex eye glass he may probably substitute a concave one to erect the object. But yet I cannot think it the best it being liable to the 1st, 3^{d}, & last of those difficulties which I urged against M. Cassegrain, & in my Judgment not wholly capable of the advantages which M^{r} Gregory propounds. The first disadvantage was that more light is lost in direct then oblique reflexion. I am convinced by severall observations that reflexion is not made by the solid parts of a body, as is commonly presumed, but by the confine of the two Mediums whereof one is within, & the other without the body. And as stones are reflected by water when thrown obliquely which force their way into it when thrown directly downwards so the rays of light (whether corporeall like stones or not) are most easily & copiously reflected when incident most obliquely. This you may observe in the passage of light out of glass into Air which is reflected more & more copiously as the obliquity is increased untill beyond a certain degree of obliquity it be wholly reflected. Also in the reflexion of light by an imperfectly polished plate of Brass or Silver or any other metall you may observe that the images of objects which by direct reflexion appear <28v> dull & confused, appear by very oblique reflexion pretty distinct & vigorous. This advantage of oblique reflexion would be inconsiderable if metall reflected almost all the light directly incident on it, but so far as I can observe there is at least a third part if not the better half of the light lost & stifled in the metall at every reflexion ; & it is of some estimation if a third or 4th part of that can be redeemed by setting the flat speculum obliquely. As for M^{r} Gregory's insinuation that direct rays have the advantage of oblique because a direct ball is reflected more regularly from a rough wall then an oblique one, if he please to consider how different are the causes & circumstances of those reflexions, possibly upon second thoughts he may apprehend why the contrary ought to happen in light, at least the Experiment of the rudely polished plate of metall may perswade him.

The next disadvantage arising from the distance of the little speculum from the eye-glass being allowed I pass to the last which is to this effect, That if to diminish the magnifying virtue of the instrument the little speculum be made of a larger sphære, (as it is in M^{r} Gregories designe, a plane being equivalent to a sphære whose center is infinitely distant,) that would cause too many of the best rays to be intercepted. And though in his designe scarce a fourth part of the whole light be intercepted yet those rays seem to me of more value then twice their number next the circumference of the Tube, because they principally conduce to distinct vision. Their loss will be judged considerable by those that have thought the loss of scarce the fortieth part of the light in my way worthy of being objected by reason that they were the best of the rays.

There are yet other considerations by which M^{r} Gregory's Tube may perhaps be thought less advantageous, as that unless the speculum F be made so broade as to intercept more then a quarter or perhaps then a third part of the whole light, it will be difficult to inlarge the aperture as is requisite for viewing dull & obscure objects. That the eye-glass if placed at the bottom will scarcely be well defended from the unusefull glareing light which in the day time comes from objects on all sides the flat speculum, at least not so well as by setting it at the side. And that an Artificer can scarcely polish the great concave so truly when perforated in the middle. For the metall neare that hole will be apt to weare away too fast as it doth neare the exterior limb. And though the hole may be made after tis polished, yet if the figure happen to be less true, or if afterwards the metall chance to tarnish, it must be polished again.

As for the advantages propounded by M^{r} Gregory I see not why the first should be recconed for one, viz: That the distance EF groweth almost the one half less & therefore the errors of the concave CD are also diminished upon the plane F by one half. For how much those errors of the concave CD are increased or diminished is to be estimated by the prevarication of the rays not at the plane F but at the Focus of that concave CD. And there the errors in both cases will be alike, provided the speculum F be accurately plane, but if there be any irregularities in the figure of that speculum F they will cause errors so much greater in one case then in the other, as that speculum is remoter from the eye-glass, which in large Telescopes may be more then 15 or 20 times.

The other advantage, viz: That his Tube will be little more then half the length of mine I should allow to be very considerable, if I thought that with equall art in the mechanism it could be made to doe the same effect. The greatest difficulty is in forming the great Concave, which when once well done, perhaps it may be thought most advantageous to make the best use of it though with a longer Tube.

The supposed advantage of Telescopes with convex or concave speculums in that they may have any desirable charge by altering the distances of the eye-glass & specula, agrees more conveniently to my design of the instrument if that speculum be made use of which I described in a letter to M^{r} Oldenburg in answer to Monsieur Auzouts considerations on these instruments, which possibly you may have seen. For instance to double the charge the eye-glass in the other way must be drawn out almost as far behind the great concave as the little speculum is before it, whereby the length of the Tube will be almost doubled whereas in my way it need be drawn out no farther from the side of the Tube then a quarter of the Tubes diameter. The charge may be also conveniently varied by having two or three eye-glasses of severall depths set in a girdle, any of which may be adjusted to the metall F by sliding that girdle about the Tube or by sliding the ring within the Tube to which that metall F is fastened.

That Telescopes by convex or concave speculums should be overcharged is not necessary but yet it is not avoydable without running upon one of the other two inconveniences described in the 7th particular of my considerations on M. Cassegrains Tube, as I there intimated.

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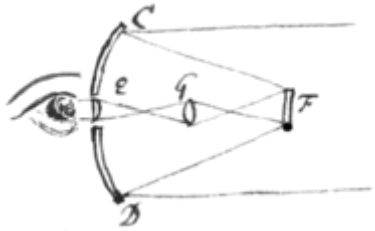
To diminish some of the afforesaid disadvantages there may be still new variations or additions to these designes. As for instance by using two eye-glasses. Suppose CD represent the great Concave, F the little speculum E the eye-glass & G another double-convex glass between E & F on both sides of which the rays cross. This way of redoubling these Tubes seems not inferior to the rest. For thus the object appeares erect, the speculum F intercepts less light, & the charge may be varied at pleasure onely by changing the positions of G & F. But yet this is not without its imperfections, & particularly (besides those common with the other Designes) the glass G will intercept many of the best rays in their passage from the concave CD to the little speculum F, unless it be made less then is consistent with some other conveniences. And by the iterated decussations of the rays objects will be rendered less distinct, as is manifest in Dioptrick Telescopes where two or three eye-glasses are applyed to erect the object.



As to the attempt in which M^{r} Rieve was imployed, I presumed it had been done with much more accurateness then M^{r} Gregory now signifys, because M^{r} Hook, who you know is a curious & accurate experimenter, affirms in his considerations on my letter to M^{r} Oldenburg concerning refractions & colours published in the Transactions Number 80, that he made severall experiments with that Instrument. And though he lays the blame on M^{r} Rieve's encheiria, yet he says not that he blamed him then when the Experiment was made. His words are these

" I have made many tryalls both for Telescopes & Microscopes by reflexion which I have mentioned in my Micrographia, but deserted it as to Telescopes when I con sidered that the focus of a sphericall concave is not a point but a line, & that the rays are less true reflected to a point by a Concave then refracted by a Convex, which made me seek that by refraction which I found could not be ex pected from reflexion. Nor indeed could I find any effect of it by one of six foot radius which about 7 or 8 yeares since M^{r} Rieve made for M^{r} Gregory with which I made severall tryalls; but it <u>now appeares</u> that it was for want of a good encheiria; From which cause many good experiments have been lost. Both which considerations discouraged me from attempting further that way; especially since I found the Parabola much more difficult to describe then the Hyperbola or Ellipsis.

From hence I might well infer that the want of a good encheiria appeared not till now. And that M^{r} Hook was discouraged from attempting further that way onely by these two or three considerations, That a Convex (as he presumes) refracts more truly then a concave reflects, that he found no effect by one of six foot radius, which till now he attributed to some other cause then the want of a good encheiria, namely to the supposedly

less true reflexion of a sphæricall concave, & **{illeg}** he apprehended a **{**gr**}**eater difficulty of describing a Parabola then an Hyperbola or Ellipsis. Nor could I well interpret the cause **{***} * from which many **{**good**}** experiments have been lost to have been other then the want of a good encheiria which till afterwards appears not to have been wanting. I contend not that this was M^r^ Hooks meaning, but onely that his words seemed to import thus much, which gave me occasion to think there was no diligence wanting in making that experiment, especially since he expresseth that he made severall tryalls with it.

And that you may not think I strained M^r^ Gregory's sense where he spake of Hyperbolick & Elliptick Glasses & Speculums attempted in vain; I would ask to what end those Speculums were attempted if not to compose optique Instruments which is all I would infer from those words. For that those instruments if at all attempted were attempted in vain is evident by the want of success.

This Sir I have said not that I desire to discourage the tryall of any practicable way or to contend with M^r^ Gregory about so slender a difference. For I doubt not but when he wrote his Optica promota he could have described more fashions then one of these Telescopes & perhaps have run through all the possible cases of them if he had then thought it worth his paines. Because M. Cassegrain propounded his supposed invention pompously, as if the main buisiness was in the contrivance of these instruments I thought fit to signify that that was none of his contrivance, nor so advantageous as he imagined. And I have now sent you these further considerations <29v> on M^r^ Gregory's answer onely to let you see that I chose the most easy & practicable way to make the first tryalls. Others may try other ways. Nor doe I think it materiall which way these instruments are perfected so they be perfected.

You will pardon this long scribble in which I have been the more particular because M^r^ Gregory's discours looks as if intended for the Press. We are here very glad that we shall enjoy D^r^ Barrow again especially in the circumstances of a Master, nor doth any rejoyce at it more then Sir

Your obliged humble Servant

Newton.

I suppose Slusius his method of Tangents will shortly appear abroad, when it comes over I'le beg of you the trouble of transmitting a coppy to me if you will give me leave to be accountable to you for it.

< insertion from f 29v >

Scripsi ad te jam sæpius de Regula ducendarum tangentium ad curvas quaslibet geometricas absque omni Calculo quam publico dare decreveram, sed absolvere, quæ animo Conceperam, hactenus non licuit, Ejus enim usum tum in Determinatione Problema tum, tum in pluribus aliis ostendere volebam quorum aliqua ni fallor tibi antehac indicavi, Verum cum nec mihi nunc Otium sit, decrevi si ita videatur Regulam absque demonstratione et Corollarys (ne moles nimia sit) ad te mittere Transactionibus Philosophicis inserendam quo virorum doctorum censuram Subjre possit, fac igitur me certiorem quid ea de re censeas, ego enim Judicio tuo parebo Libenter, eamque cum tibi placere indicaveris statim transmittam.

< text from f 29v resumes >
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M^r^ Newton about Tellescopes with his Method of Tangents

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To Collins. **{**1**{illeg}}** Dec. 10. 1672.

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