

Copy of an extract from Newton to John Collins, dated 10 December 1672

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Extract of M^r Newtons Letter to M. Collins. Dec. 10. 1672.

I am heartily glad at the acceptance, which D. Barrow's Lectures finds with forrein Mathematicians; and it pleased me not a little to understand that they are falne into the same method of drawing Tangents with me. What I guess their method to be, you will apprehend by this example;

Suppose CB applied to AB in any given angle be terminated at any Curve line AC, and calling AB x and BC y, let the relation between x and y be exprest 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 Arithmetical progression according to the dimensions of y, suppose thus

$x^3 - 2xxy + bxx - bbx + byy - y^3$; also according to the dimensions of x suppose thus

0	1	0	0	2	3
x^3	$-2xxy$	$+bxx$	$-bbx$	$+byy$	$-y^3$

 . The first product shall be the Numerator, and the last divided by x the

Denominator of a fraction which expresses the length of BD, to whose end D the tangent CD must be drawn.

The length BC therefore is $\frac{-2xxy+2byy-3y^3}{3xx-4xy+2bx-bb}$.

This is one particular, or rather a Corollary of a general Method which extends itself, without any troublesome calculation, not only to the drawing tangents to all Curve lines, whether Geometric, or Mechanic, or however 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, and 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 infinit 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 divertisemt or other hindered me from describing it to him.

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

<43r>

Ex eadem {epta} Newtoni ad Collinium
d. 10. Dec. 1672.

M^r Gregory is pleased to consider further the most advantageous construction of Cata-dioptrical Telescopes. And as his dessein in his Optica promota excels that of M. Cassegrain (though they differ so slightly, that I thought it not worth the while to take notice of the difference,) the advantage being, that the little concave Ellipsis comes nearer to a Spherical figure, than 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 Musicians, mentioned in the last Transactions, intends to make Experiment of, excepting that instead of the Convex Ey-glas glass he may probably substitut a Concave one to erect the object. But yet I cannot think it the best, it being liable to the first, third and last of those difficulties, I urged against M. Cassegrain, and in my Judgement not wholly capable of the advantages, which M. Gregory propounds. The first disadvantage was, that more light is lost in direct than oblique reflection. I am convinced by several 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, and 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 Corporeal like stones, or not,) are most easily and copiously reflected when incident most obliquely. This you may observe in the passage of Light out of Glass into Air, which is reflected more and 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 polishet plate of Brass or Silver or any other metall, you may observe that the Images of objects, which by direct reflexion appear dull and confused, appear by very oblique reflexion pretty distinct and 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 and stifled in the metal at every reflexion; and it is of some estimation if a third or fourth 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, than an oblique one; if he please to consider, how different are the causes and 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 polishet plate of metall may persuade him.

The next dis-advantage arising from the distance of the litle speculum from the Ey-glass, being allowed, I pass to the last, which is to this effect; That; if <43v> to diminish the magnifying virtue of the instrument the litle speculum be made of a larger sphere, (as it is in M. Gregory's dessein, a plane being equivalent to a sphere whose center is indefinitely distant,) that would cause too many of the best rays to be intercepted. And tho in his designe scarce a forth part of the whole light be intercepted, yet those rays seem to me of more value than 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 advantagious, as, that unles the speculum F be made so broad as to intercept more than a quarter, or perhaps than a third part of the whole Light, it will be difficult to enlarge the aperture as is requisite for viewing dull and obscure objects. That the Ey-glas, if placed at the bottom, will scarcely be well defended from the unusefull glaring 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 midle; for the metal near that hole will be apt to weare away too fast, as it doth near the exterior limb. And tho the hole may be made after it is polishet, yet if the figure happen to be less true, or if afterwards the metal chance to tarnish, it must be polishet again.

As for the Advantages propounded by M. Gregory, I see not, why the first should be reckoned for one, viz. That the distance EF grows almost the one half less, and therefore the Errors of the Concave CD are also diminishet 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

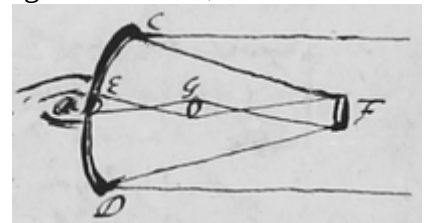
one case than in the other, as that speculum is remoter from the Ey-glass; which in large Telescopes may be more than 15 or 20 times.

The other Advantage, viz. That his Tube will be little more than half the length of mine, I should allow to be very considerable, ~~if~~ **I** thought, that with equall art in the mechanisme 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 advantagious, to make the best use of it 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 Ey-glass and specula, agrees more conveniently <42r> dessein of the Instrument if that speculum be made use of, which I described in a letter to M. Oldenburg in answer to M. Auzouts Considerations on these Instruments, which possibly you may have seen. For instance, to double the charge, the Ey-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 than a quarter of the Tube's diameter. The charge may be also conveniently varied by having 2 or 3 Ey-glasses of severall depths set in a girdle; any of which may be adjusted to the metal F, by sliding that girdle about the Tube or by sliding the ring within the Tube, to which that metal F is fastned.

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

To diminish some of the aforesaid disadvantages, there may be still new variations or additions to these designs. As, for instance, by using two Ey-glasses. Suppose CD represent the great Concave, F the little Speculum, E the Ey-glas and G another double Convex-glas between E and F on both sides of which the rays crosse. This way of redoubling these Tubes seems not inferior to the rest: for, thus the object appears erect, the speculum F intercepts less light, and the charge may be varied at pleasure, only by changing the positions of G and F. But yet this is not without its imperfections, and particularly (besides those common with the other designs,) 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 than is consistent with some other conveniences. And by the iterated decussations of the rays, objects will be rendred less distinct, as is manifest in Dioptric Telescopes, where 2 or 3 Ey-glasses are applied to erect the object.



As to the attempt in which M^r Reeves was imployed, I presumed, it had been done with much more accuratness than M^r Gregory now signifyes, because M^r Hook, who you know is a curious and accurate Experimenter, affirms in his considerations on my letter to M. Oldenburg concerning refractions & colors, publishet in the Transactions N^o 80, that he made several Experiments with that Instrument. And though he lays the blame on M. Reeve's Encheiria, yet he says not, that he blamed him then; when the Experiment was made. His words are these;

"I have made many tryals both for Telescopes and Microscopes by reflexion, which I have mentioned in my Micrography, but deserted it as to Telescopes, when I considered, that then focus of a spherical Concave is not a point but a line, and that then rays are lesse true reflected to a point by <42v> a Concave, than refracted by a Convex; which made me seek that by refraction, which I found could not be expected by reflexion. Nor indeed could I find any effect of it by one of six foot radius which about 7 or {8} years since M^r Reeve made for M. Gregory with which I made severall tryals; but it now appears, 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, than 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 only by these 2 or 3 considerations; That a Convex (as he presumes) refracts more truly, than a concave reflects; that he found no effect by one of 6 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 spherical concave; and that he apprehended a greater difficulty of describing a parabola than an Hyperbola or Ellipsis. Nor could I well interpret the cause, from which many good Experiments have been lost, to have been other than the want of a good Encheiria, which till afterwards appears not to have been wanting. I contend not, that this was M. Hooks meaning, but only 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 tryals with it.

< insertion from the left margin of f 42v >

Newton? Sept. 23. 72.

< text from f 42v resumes >

And that you may not think I strained M^r Gregorys sense, where he spake of Hyperbolic and Elliptic Glasses and Speculums attempted in vain; I would aske, to what end those Speculums were attempted if not to compose optic Instruments; which is all I would inferr from those words. For, that these 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 than one of these Telescopes and perhaps have run through all the possible cases of them, if he had thought it worth his pains. Because M. Cassegrain propounded his supposed Invention pompously, as if the main busines 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 on M. Gregory's Answer, only to let you see, that I chose the most easy and practicable way to make the first Tryals. Others may try other ways. Nor doe I think it material, which way these Instruments are perfected, so they be perfected.
