LETTERS OF

BARROW, FLAMSTEED, WALLIS, AND NEWTON,

FROM THE ORIGINALS.

CORRESPONDENCE

OF SCIENTIFIC MEN

OF THE SEVENTEENTH CENTURY,

INCLUDING LETTERS OF

BARROW, FLAMSTEED, WALLIS, AND NEWTON,

PRINTED FROM THE ORIGINALS

IN THE COLLECTION OF THE RIGHT HONOURABLE THE

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LETTERS

OF

SCIENTIFIC MEN.

CORRESPONDENCE WITH REV. THOMAS BAKER[a].

CXXX.

BAKER TO COLLINS.

Worthy Sir,

YOURS of June the 22nd I have received, and want words to express both my thanks for the communications, as also the delight and satisfaction I received, in the contemplation of the excellent and high notions therein. Dr. Pell's, Mr. Newton's, Mr. Gregory's conceptions I shall at present wave (some whereof come

[a] Of the Rev. Th. Baker an account will be found in the Biographia Britannica. He was collated, in 1681, to the living of Bishops Nympton, near South Molton, which is said, in the Episcopal Registers, to have been then "legitime vacantem." Chapple, nevertheless, supposes that "he "had been presented many years "before, during Cromwell's pro-"tectorate, and that he conform-"ed[a]." But Anthony Wood particularly describes him as having been, in early life, an active royalist. His conforming likewise, (if it had taken place,) could hardly have had any connection with what occurred twenty years after the Restoration: and his presentation must have been quite recent, if it took place before that event, since the parish records mention the burial of "Matthew Hans-"comb, Vicar," on the 15th of Sept. 1659, after the Protectorate of even Rich. Cromwell had expired. One of his letters indeed is dated from Bishops Nympton as early as July, 1676: and it is possible that he was resident curate of the parish before he became incumbent of it: or he may by some act have rendered the living void in 1681, so as to make it necessary for him to be again presented to it. But there is no record in the registers at Exeter of

[a] Lysons' Magna Britannia, Devon, p. 368.

within the sphere of my weak capacity, and others exceed my belief). At present only I shall make bold to trouble you with the problem, wherein I suppose yourself to be concerned; for in your letter to Mr. Oldenburg you are pleased to hint, that one Collins (whom I suppose to be yourself) had thought of a method for solving the problem proposed to Mr. Gregory; viz.

The sum of the squares, and the sum of the cubes, of four proportionals being given, to find the proportions themselves.

Indeed you have most ingeniously strawed the way, for its invention, by Billy's doctrine, which will not

any previous collation subsequent to that of William Tyler in 1620.

Wood says, "a little before his "death the members of the Royal "Society sent him some mathematical queries, to which he returned so satisfactory an answer, that "they gave him a medal with an "inscription full of respect. What "the queries were, I know not: "sure I am that he kept the "medal by him as a great honour "to the time of his death; and "now it is kept by his son, "James Baker, beneficed in "Somersetshire[b]." No traces, however, have been found of any such transaction in the records of the Royal Society for the time here specified, and it was therefore most probably the act of some individual members, not of the body. It may be noticed likewise, that Wood had his information from some one, who, with great show of precision, was very inaccurate in his report. For, he says, that Baker died "on Thursday, the "fifth of June, in sixteen hundred and ninety," whereas it appears from the registers of Bishops Nympton, that he was buried there on the 22nd of May, 1689. His will (which was not proved till 1690) is long; it describes very minutely the manner in which his property was to be disposed of, and we may gather from it that, besides a son who bore his own name, he left a daughter Joane, who appears to have married a gentleman of the name of Pole; he speaks of many others in 1676, (see p. 16.)

He printed a catalogue[c] of mathematical works which he was desirous of publishing. The first of which, the "Geometrical Key," came out in 1684. The Royal Society had promoted the publication[d], and were applied to, in the beginning of 1687, for assistance in bringing forward the rest; but, although they encouraged the design[e], it was never executed—probably the author's death prevented it.

b Athenæ Oxonienses, vol. IV. p. 287.
c These proposals are frequently bound up with the "Geometrical Key."
d Birch's Hist. of the Roy. Soc. vol. IV. p. 155, 6.
e Ibid. p. 527. He was not F. R. S.

ascend to such high dimensions, as Mr. Gregory proclaims him to be (to him) Magnus Apollonius, that can resolve it by an equation under thirty dimensions. You have hit the same method, which I long since (that is Michaelmas last) solved it by, when the same was proposed by Dr. Davenant to Mr. Strode and myself; which solution then of mine, whether I have advanced it by making the same demonstration, with great facility, to solve eight problems of the same bran, I leave to you to determine.

As to Mr. Billy's doctrine and book, they are both strangers to me; and though it be my hap to solve it, yet dare I not arrogate (being conscious of my own weakness) that Hogun Mogun[b] title of Magnus Apollonius, which is due to yourself.

I confess, with some unusual artifice, I have so depressed the equation, that it should not exceed seven dimensions: neither have I observed your prescribed way, but found out the equation without finding out (by the assistance of the sum of the four, and the sum of their squares) the four proportionals, and then cubifying them. My way of demonstration is short, and not tedious (as I suppose) as your way too seems to be. I should have sent you the demonstration, were it not I presumed I should but light a candle to the sun. I shall only offer to your consideration the naked problems solved, hoping you will candidly accept, and pardon the weakness, of him, that owns himself to be but a tiro in this great art, but

your most faithful and most obliged servant,

THOMAS BAKER.

Bp. Nympton, July 20th, 1676.

Sir,

I suppose that you were pleased to send me three

[b] Sic in MS.

books; viz. Slusius's Mesolabe, Gregory's Geometry, and Mercator's Astronomy; but never intimated so much in your letter. If so, I humbly desire to know the prices, that I may return the money with thanks.

Mr. Brome hath sent (unknown, I suppose, unto you) me Mercator's Astronomy, too; thus am I doubly booked, &c.

Mr. Newton's two sheets of a new doctrine by infinite series, &c., would be infinitely acceptable to your servitor

T. B.

CXXXI.

COLLINS TO BAKER.

Worthy Sir, 19th Aug. 1676.

It is now above a fortnight since I sent you Mr. Newton's two sheets; in which letter I promised to send something more of that argument, but must crave a little more of your patience. [I] have since been engaged in transcribing, &c. of many papers and arguments against tin farthings, in which argument being shipwrecked, and the matter determined for tin, and against copper farthings, my employment at the farthing office in Fenchurch Street is ceased, and now I have obtained otium prelo-bibliare, and desire for the future that you be pleased to direct your letters to me, at the house of William Austin, Esq., over against the Adam and Eve in Petty France, Westminster.

I have yours of the 24th of May, 1676, before me to answer, wherein—

1. You are pleased to acquaint me with your having

attained a more compendious way for solving of all equations, geometrically or arithmetically, proportional, though ascending to the eighth power, which I doubt not but you have used in your late solution of Dr. Davenant's problem.

2. Next you mention a treatise of cubic equations of yours in Mr. Strode's hands, with your generous offer of my perusal, upon condition of returning it by Michaelmas. I have not time yet to write to Mr. Strode, but hope to do it next week; he hath sent up to Mr. Oldenburg an exercise de arte combinatoria, with a table of figurate numbers, desiring the same might be printed apart, though I think it more proper to go with his conics, when they have received those advantages you shall think meet. Leibnitz writ a thin quarto de arte combinatoria; Pascal touches thereon in his tract du Triangle Arithmétique; so doth the author of the late French Algebra (viz. Père Malbranche's servitor) entitled Élémens Mathematiques; and I know not whether Mr. Strode hath seen these authors. One Torporley[c], long since, left a manuscript treatise in Latin in Sion College, wherein is a much more copious table of figurate numbers,

[c] Nath. Torporley left his manuscripts to Sion College, where he spent the latter years of his life; but the greater part of them was destroyed in the fire of London. Reading, in his catalogue of the library, mentions only one, "Corrector Analyticus," which is an attack on Warner for the manner in which he had edited Harriot's "Artis Analyticæ Praxis." This is a short tract, and incomplete. There is, however, another volume, A. 37–39, entitled, "Algebraica, Tabulæ Sinuum, &c." in which Torporley's hand may be certainly recognized. Wood, in the list of his works, speaks of "Congestor opus Mathematicum,—imperfect." A perfect copy of this treatise is in Lord Macclesfield's possession, and probably once belonged to Collins. It is a small folio, and a loose paper in it has the following memorandùm: "This manuscript is of great value, and should be carefully preserved. W. J"[ones]. See also Lord Teignmouth's Life of Sir W. Jones, p. 11, where it is spoken of in a letter to Cotes.

which I have caused to be transcribed, with what he says de combinationibus, to send to Mr. Strode.

3. You mention a treatise of Trigonometry done twenty-six years since, not only simple and vulgar, but such as Bartholinus pretends to have found out, that is compound, a new way of resolving two things at once.

My apprehensions concerning this are, that at two operations two quæsita may be obtained: for instance,

In an oblique spherical triangle, giving two sides, with the angle comprehended given, to find both the other angles at two operations.

The history, about the proportions for that purpose, being somewhat prolix, I shall pretermit, and only say the want of a demonstration of those proportions occasioned Mr. Oughtred to publish his Treatise of Trigonometry, which, whosoever peruseth, I presume will find prolix and intricate, and Dr. Newton's, in Trigonometria Britannica, fallacious. For my own part, though no sphinx, nor at leisure, yet about twelve years since it was not very difficult to find out two easy demonstrations of those proportions; the one from what was known in trigonometry before, viz. by aid thereof, without any scheme at all, to prove that the rectangle and ratio of the tangents of the half sum and half difference of the angles at the base are given; the other from the stereographic projection, supposing the eye at the nadir, the plane horizontal, and the spherical triangle to have its vertex in the zenith, and the two legs to reach to the parallel of latitude, which, in this case, the said legs and parallel, are projected in right lines, and the matter again easily deducible from the præcognita of trig^{a}.: but doubtless yours is more general and easy.

4. You mention a demonstration you have of Vieta's

way, the prostaphæretical way, &c. (in one sheet, geometrically done). I have not lately looked into Vieta, and so, not knowing, or not remembering, which you intend, cannot speak thereto; the demonstration of the common prostaphæretical way, in Pitiscus, Longomontanus, &c. leans on a common proportion for finding the difference of the sines of two arches.

5. Your treatise about angular sections will be a desirable pleasant argument, and no doubt but you and Dr. Wallis, (whose treatise of the same subject I have in my hands in order to the press,) writing inconsulto, have many different observations. Dr. Wallis doth not handle the habitudes of equations about tangents and secants. The following proportion I have found of use in the gradual progression of the sines, supposing the sines of the first and second degree,

As the sine of the difference of two arches
is to the difference of their sines ;
so is the sum of their sines
to the sine of the sum of those arches.

6. You mention that you have writ a Miscellany of Problems of Des Cartes, M. de Montfort, and divers others besides, with an infinite comp[a]. of your own invention.

As for Montfort, I know of no other problem of his but one. The angle that an ordinate in a known ellipsis makes with either of the axes, and the distance of the angular point from the centre, being given, to find the length of the ordinate, which hath been solved by many, particularly by myself; the problem in spherical triangles being but this, the latitude of a place with the sun's (or star's) height, at east or west, being given to find the time from noon or midnight.

7. You say you are loath to be public, as being conscious of your own weakness. Thus, sir, after you have

given us a glimpse of treasure, should not we here be accounted slothful to let it remain longer in the mine unexhausted? Wherefore the main of this letter is to excite you to a publication of them, and I shall be as careful as I can in procuring undertakers, who both here and elsewhere are backward enough as to mathematical affairs, though they have the copies gratis, or upon very mean or easy terms. Kersey's Algebra hath sold well, and doubtless by the assistance of yourself and other learned persons, who may be willing to enrich the commonwealth of learning, another volume may succeed, most of it of a different subject from the former.

8. I am much obliged for your candid offer to undertake such problems as are troublesome here. At Venice there is an algebraist, behind the curtain, who hath sent over some problems to Mr. Oldenburg about plane triangles, and hath promised upon receiving the solution thereof to become a correspondent to the Royal Society, ... recreations of his own, which prob. by another opportunity I hope to impart to you.

Lastly. . . . . . . in the solution of a problem put by Dr. Davenant, viz.

Thus of Van Ceulen's numbers to derive those of Metius, or a decimal fraction being proposed to reduce it to several vulgar fractions that shall best express it, too great or too little according to any number of figures desired.

Diameter -	1.00000
Circumference	3.14159

Pursue the vulgar rule for finding the greatest common measure of the two numbers by dividing the bigger by the lesser, and the first divisor by the first remainder, and again by the second remainder divide the second divisor, reserve the quotes, and a regress on them will discover the number sought.

Example.

Divisors.		Quotes.
100000)	314159	(3
14159)	100000	(7
887)	14159	(15 too little.
		(16 too big.

The regress on supposal nothing remaineth.

106)	333	(3	113)	355	(3
15)	106	(7	16)	113	(7
1)	15	(15	1)	16	(16

Explication.

If 1 be divisor, 16 quot. 0 rem. what is the dividend? 16
16 .... 7 ... 1 113
113 .... 3 ... 16 355

Frenicle's Book.—Bond's Theory.—French glass grinding.

The heads of another letter.

1. A better account of infinite series for roots of equations.

2. Mr. Gregory's solution of Kepler's problem with Halley's method.

3. About conics from projections, and Mr. Newton's moveable angles, and that of the foci.

4. The powers of unknown roots in species and numbers from Gregory, with keeping proportionals low.

5. The annuity problem.

6. The Brereton problem.
7. The Venetian problems.
Merry's Hudden.

In a letter of 20th Sept. sent Venetian problems, the extracting of roots in species out of Mr. Newton's letter, and the extracting the square root out of his exercise, Gregory about annuity problem, the reducibility of Davenant's problem to infinite series.

CXXXII.

BAKER TO COLLINS.

Honoured Sir,

Yours of Sept. 21, 1676, I have received, together with the copies of M. Leibnitz, M. Tschirnhaus, and Mr. Newton's two sheets. As I cannot sufficiently admire, both the excellency of your invention and skill (in the disquisition and contexture of things of so sublime a nature) and the dulness of my apprehension, (being of so thick a skull, as not capable of some of their notions) so, the indefatigable pains of yourself, not only in transcribing so much, but in interpreting some of their abstruser riddles, (especially for the use of him, immeriting so immense pains and favour from you). If providence second and smile on my present endeavours, I hope I shall (within some short tract of time) be better capacitated (in some measure) to endeavour some small requital. In the interim, let my hearty thanks for the presents pass for an apology, and for the first fruits of it: and, as for the great pains, so for the greater love, in sending Frenicle's Triangles to Henry Brome, in order to be transmitted to me, which as yet I never received, and wonder at his neglect.

Your ingenious method of resolving Dr. Davenant's problem hath its deserved praise, which (so far as to bring it to [an] equation, amidst the throng of divers methods of disquisition) I stumbled on; but being gravelled how to proceed, I desisted. You are pleased to intimate further, that Mr. Newton's way (especially that by way of division) will untie that knot, and perhaps leave a general rule to solve all others of the like kind. It is probable; but (I doubt) Mr. Newton himself must have a finger, then, in it: and it is advisable, whether so untied (by the way of infinite series) the trouble and pains be not equal to mine.

I understand also by you, that my Apollonius Magnus Gregorianus is indifferently resented, and intimate my consent for the press. I must confess, that I naturally have no prurient itch that way, as being too conscious of my own weakness, and its worthlessness. But (if any thing should prevail) as the respect, that it (though undeservedly) finds from yourself, so, the consideration (a trite, but true apology) of the importunity of some friends of mine (who seem to admire, that as yet I never appeared upon the public stage); as also the fears, lest my conceptions (having taken air) should be surreptitiously, by other men, under another's name (though perhaps drest up in a different and finer garb) exposed to public view, and so strangers should reap the harvest of other men's labours and inventions (which is not my single jealousy only, but common to others too); I say this (if any thing) should prevail with me to have them committed to the press. The whole business I refer to yourself, which if you think not convenient, my humble request is, that you would (with as much speed, as your convenience will permit) return me the original.

I make bold to send you (for a new year's gift) the perusal of the enclosed (entitled, Cardanus Promotus) occasioned by M. Tschirnhaus's letter (in which I suppose some things are not rightly copied out, which I have corrected, as is to be seen in the enclosed); into part of which I have made bold to make a short inquisition, viz., into some of those conditions, which he hath consigned to some equations, which (I suppose and suspect) not to be comprehensive enough, being only such as naturally flow from the equation; but that (which I have noted with an asterisk *) which happens in the resolution, being by him (which in many cases is absolutely necessary) not at all animadverted. This I have endeavoured to prove, both by the testimony of such (who are of irrefragable authority with me) but by example too, and demonstration mathematical (which commands my assent). But the whole I leave to your and others more critical judgment.

I have set down two valors of x to every equation; the latter of which may be accounted more simple than either that of Cardanus or my first, by how much, to the obtaining of which one single simple root is only to be extracted. I could have wished, (and it would be of no mean use in that part of mathematical learning, which concerns equations) that M. Tschirnhaus's conditions (put to his equations) had squared right with all equations (as it was necessary) of that kind; for then, his resolutions of such equations would have been so universal, as (by the same) to have easily resolved, not only all cubical equations whatsoever, but all problems almost, about angular sections; for example, these:

Pr. 1. If there be two rectangle triangles, having the same common hypotenuse, whose acute angle of the first be double, triple, &c., or any way multiple, &c., to the acute angle of the second.

Having the base of the first, and the common hypotenuse (or double radius) given, to find (by a resolution, consisting of one simple root only) the base of the simple angle—which would have resolved, not only all, what is defective in Cardan's second rule, about cubical equations, but many others, ascending to higher powers, and consequently (the resolution of this problem being found) the resolution of the third cubical equation (all cubicals being reducible, as you know, to three equations) might with equal facility be found, by the resolution of this second problem following; viz.

Pr. 2. Having the cathetus of the first and the common hypotenuse given, to find the cathetus of the simple angle. (Now the resolution of the first problem makes an easy entrance to the resolution of the second.)

The truth is, upon the first view (and taking M. Tschirnhaus's rules upon trust) I cried (with Archimedes) εὕρηκα, and thereupon composed one sheet, in order to the resolution of the aforesaid problems, &c., but, upon examination, I found myself deluded, merely upon the defect of that condition, which flows out of the bowels of the resolution; which occasioned me to write this tract here sent, which if you think worthy of a public view, I shall not dissent; if not, pray, after perusal, return it to him, whom you have eternally obliged to be

your most faithful
and humble servant,

Dec. 27, 1676, Bp. Nympton. THOS. BAKER.

CXXXIII.

COLLINS TO BAKER.

Feb. 10, 1676-7.

Mr. Baker.—Worthy Sir,

I doubt not but you have been in some anxiety about your Cardanus Promotus, it being so long since sent up, and by me received, without any answer; and till now I could not conveniently be at leisure nor able to speak to the purpose. The causes I refer to the copy of a letter I sent by the last post to Mr. Strode[d]. On Thursday I was at the Royal Society, and your Ap[e] and Cardanus Promotus having been shewed to the Lord Brounker and Sir Jonas Moore, the Society agreed to give 40*s*. to promote the printing thereof; your pains were judged to be too learned to be either of vulgar sale, or easily to find an undertaker. But, however, Sir Jonas Moore undertook to find a stationer to engage in it; for the truth of it is, Mathematical learning will not here go off without a dowry; the booksellers have lost so much by the works of Drs. Wallis and Horrox, the Optic and Geometric Lectures of Dr. Barrow, &c., though by Mr. Gregory and others esteemed the best things extant, that it is no easy task to persuade booksellers to undertake any thing but toys that are mathematical. But, however, without doubt yours will now go on speedily. I have promised to correct each sheet first at the press, and my good friend, Mr. Kersey, long confined to his bed by a

[d] See Strode's Correspondence.

[e] There is no notice in Birch's Hist. of the Royal Society of these works having been mentioned at the Royal Society on the 3d of Feb. 1676. The first, of which the name seems to have escaped Collins, was probably "Apollonius Magnus Gregorianus; or a Treatise of Four "Geometrical Proportionals," which Baker had drawn up, but which was never printed.

grievous stone in his bladder, to make a review of each sheet; so I hope it will be well corrected. And, whereas you wish me profit by correcting at the press, I assure you I never got any, but have for divers years spent much time and some money in midwifery at the press. The case is the same at Paris in France, where M. Carcavi hath in his hands the many and learned remains of Fermat, Frenicle, Roberval, Lalovera, besides the many works of which I could give you a catalogue, of Pascal and Bulliàldus, which no booksellers will undertake; and they have offered to send over hither, on condition of getting the same printed, but we have refused. I have now above twelve sheets in all of Mr. Newton and Leibnitz to impart to you, whereof four sheets of Mr. Newton are in answer to the letters of Messrs. Leibnitz and Tschirnhaus, besides somewhat of my own, and I hope speedily to write to you, and gradatim transmit the whole, which I doubt not but will remove the doubts you meet with in the new doctrine of infinite series, &c. I did not offer your Treatises to Mr. Brome, as a person improper, he being an English bookseller, little concerned in Latin books, and having no foreign trade, and a person so wedded to his own printer, that is not accustomed to mathematical works, nor furnished with proper types, that I would not run the hazard of an ill impression, whereas in truth we have but one printer, namely, Mr. Wm. Godbid in Little Britain, that is accustomed [to] and fitted for such and music work, who besides is a very worthy honest person. And, my dear friend, Mr. Brome tells me he sent Frenicle du T. R.[f] to Mr. Brockhurst, a bookseller in Exeter,

[f] Frenicle published Traité des Triangles Rectangles en nombres, 12mo. Paris, 1676, which is probably the work to which these initials refer.

where you may direct your friend to call for it. Mr. Dary, a tobacco-cutter, a good algebraist, and friend of mine, hath published a little analytical tract of Interest, with approximations for equations; which I hope next week to send by the carrier to your friend Mr. Leigh at Exeter. I rest——

CXXXIV.

BAKER TO COLLINS.

Bp. Nympton, Mar. 15, 1676-7.

Sir,

Yours both, dated Feb. 10th, and March 3d, 1676, I received, together with Frenicle's Triangles, on March 11th. For all which I return you hearty thanks, &c.

1. I cannot but admire the quaintness and curiosity of M. Frenicle's inventions through the whole; yet his second remark on propos. 24, seems to be of most use; &c., because possibly it may conduce to the solution of equations (if improved) belonging to angular sections.

2. I wonder not that so mean tracts as mine are, will not pass without a dowry, seeing more sublime ones are in the same condemnation. It is well (and I account it no small happiness) that the R. S. will give them a portion. For I have too many daughters at home, that more need portions, than those tracts; and therefore I shall be very unwilling myself to expend much that way; my engagements are therefore the deeper to those that will. If I may be so happy to know (if ever) the time, when they may chance to pass the press, I shall endeavour an epistle dedicatory to the reader, to give him an account of the swelling titles

imposed on them; and (if you think fit, if I may not be too bold in so doing) to dedicate it either to the whole body of the Society, or to such particular members of it, as have a respect to it, as my Lord Brounker, Sir Jonas Moore, Mr. Oldenburg, and yourself; concerning which I, by this, humbly crave your advice.

3. That I understand in part M. Leibnitz's method of working, though not well how to apply it; I make bold to give you this account.

The admirable M. Leibnitz writes thus: rectius initio scripsissem,

1. $a + bx + cy + dxy + exx + fyy + f = 0$, æquatio data (I refer you to his letter). According to this equation expressing the relation between x and y, will be

2. $\dfrac{b + dy + 2ex}{c + dx + 2fy} = z$, æquatio inventa ad tollendum y, sic:

3. ex æquatione datâ, $y = \dfrac{-a - bx - exx - f}{c + dx + fy}$;

4. ex æquatione inventâ, $y = \dfrac{b + 2ex - cz - dxz}{2fz - d}$;

5. ponendo compendii causâ,

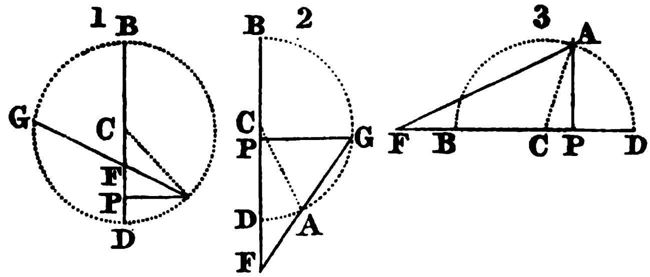
$$\left.\begin{aligned} -a - bx - exx - f = p, \quad &\text{et } c + dx = q \\ b + 2ex - cz - dxz = r, \quad &\text{et } 2fz - d = s \end{aligned}\right\} \text{erit.}$$

6. (per 3.) $y = \dfrac{p}{q + fy}$; et $y =$ (per 4.) $\dfrac{r}{s}$; quare,

7. $ps = qr + fry$; et $\dfrac{ps - qr}{fr} = y = (6.)\, \dfrac{r}{s}$; ideoque,

8. $pss - qrs = frr$; vel $pss = frr + qrs$; et in loco literarum p, q, r, s, substituendo valores assumptos, æquationemque ordinando prodibit æquatio quæsita, exprimens relationem z ad solam x; &c. I have not time to calculate the equation, &c. Thus far I understand; but (as I have said) I know not how to apply

it. If an example, in numbers, were done, exhibiting the ratios of these &c., it were possible then I might understand the application of it in time, which yet you have intimated much of already.



Sir, Mr. Strode sent me some papers (by you sent to him) of Dr. Barrow's, in which (as he says) the geometrical constructions of all cubic, and quadrato-quadratic equations are exhibited. I much suspect that all cubics are not by him there demonstrated. I might instance in two or three places (of which perhaps more hereafter); I shall at present give you one (even at the first start).

Page 1ª. 1. $a^3 \ * -lla + m^3$
2. $a^3 \ * +lla - m^3$
3. $a^3 \ * -lla - m^3$
} Each of these equations (secundum Doctorem Barrow) hath respect to its proper figure.

It is evident, that the 1st and 2d figures both refer to the first equation, whereof the first gives the lesser root, and the second the greater root, and the third figure has respect only to the third equation; so that the second equation is widowed of its geometrical construction. I suppose I need not trouble myself, nor you, to demonstrate it. I have sent you, enclosed[h], an extract of equations, whose roots are in arithmetical

[h] No enclosure was found in the letter.

proportion, the demonstrations of which I have, but not ranged into that form and order as necessary. Sir, pardon the hastiness of my pen in scribbling so rudely; being, in haste,

tuissimus,

THOS. BAKER.

CXXXV.

BAKER TO COLLINS.

Mr. Collins,

I sent you a letter dated March 15, 1676-7, wherein I acquainted you of the reception of yours of Feb. 10, and March 3, 1676-7, with Frenicle's Triangles, for which (as then so now) I return you thanks: as also I mentioned some errors (as I conceived) in Dr. Barrow's geometrical constructions of all cubic, and quadrato-quadratic equations, of which I therein gave you a taste: as also I sent you an extract of equations, whose roots are arithmetically progressive, &c. I suspect the letter never came to your hands, by reason I have not heard from you since. I humbly desire you to let me know whether those papers of mine (Apoll. Mag. Greg., and Cardanus promotus) keep up that estimation (upon review), as formerly you represented (though unworthily) they had received from the R. S., and whether the undertaker of them intends the press or no (if not, to return the papers); if he does, to acquaint me with the time when he means to expose them to the public; because I think it fit to preface them with an epistle dedicatory to the reader, and to dedicate them to such members of the R.S. as you shall think fit, which in my last to you I mentioned; thinking it most equal for those, that are fathers to the giving them a dowry, should be godfathers too (if they please to own the brats).

Sir, the carrier waits for me, in haste

tuissimus,

THOS. BAKER.

April 10th, 1677,
Bp. Nympton.

CXXXVI.

COLLINS TO BAKER.

Mr. Baker—Sir, 24th April, 1677.

By divers of your letters, I perceive you are in some anguish till you know what will become of your papers. Sir Jonas Moore hath them, and he lately returned from Portsmouth, and is now gone to Newmarket, with an intent to return within ten days. He hath a great esteem of them, so likewise hath Dr. Croone, a physician of the Royal Society, who was a late Professor of Rhetoric of Gresham College, and is a learned mathematician; he is desirous to propound you a member of the Royal Society[1], which shall not be any thing to your cost, and it is accounted a dignity to any man to have his name inserted in the yearly printed lists of that honourable body; and if you signify your consent, doubtless it will come to pass, and whatsoever is sent up is recorded in their register, so that if any plagiarism should happen, which I would not have you suspect, a public vindication will appear in the Transactions. Now as to the printing your learned labours, I have the following considerations to offer:—

[1] Thomas Baker was elected F.R.S. in Nov. 1684.

1. That Dr. Wallis and Dr. Barrow, &c. use to commit their writings to the Royal Society to get printed when opportunity offers. Divers of Dr. Wallis's papers I have had some years in my hands, and could not get printed, yet now a stationer desires them, namely, one Pitt, who having bought a remain of above two hundred of Horrox's Astronomy, (which, though a very good book, proved very damageable to the undertaker,) to revive the sale thereof, desires eight or ten sheets to add to it, and accordingly there will be so much of the Doctor's[k] subjoined, which hitherto hath not seen light. I forbear the particulars, hoping when done to send you these accessions.

2. That, even of the best mathematical books that come out beyond sea, the stationers do not import above a dozen or twenty, or some small number, affirming they cannot sell more, at least till the book hath got fame and is much desired.

3. That, if an author, or any person in his behalf, will pay a stationer for one hundred books, ready money, at a rate he sells them in his shop, there are stationers, [who] will undertake any book proposed, and even the books so bought may, within a few years, be bartered away with some money for other books, that little loss may ensue.

4. Sir Jonas tells me that, as to yours in particular, though the society have agreed to give forty shillings, which is more than they have done towards printing of any author save Horrox's Astronomy, with which there was five pounds given, yet a stationer will not undertake yours without five pounds encouragement, but in regard he hath things of another nature to offer to the said stationer, who will readily embrace them,

[k] Wallis,—see further about Horrox's Opera Posthuma in Flamsteed's letters.

but are not yet quite ready, he saith they shall go off together: but possibly this may require time.

5. Stationers have lost much by Dr. Wallis's works de Cycloide, de Calculo centri gravitatis, and his *Letter Commerce*, but more especially by Dr. Barrow's Optic, and Geometric Lectures: both which books, with the plates, as I compute it, did cost the first undertaker 4*s*. 8*d*. or near 5*s*., and yet at last a great number of both books, together with the plates, were sold by Sir Thomas Davis, Lord Mayor, formerly a stationer, the heir of the rich Audley, into whose hands they came for a debt, at the rate of 1*s*. 6*d*. a pair, or both of them, to Mr. Scot a bookseller in Little Britain, who drives a foreign trade, or otherwise they would have turned to waste paper; and yet Dr. Barrow's works, in the judgment of Mr. Gregory, and other learned persons, do *infinitely transcend* any thing of the like kind.

6. I cannot say how soon your papers may be extant, but if these considerations do not induce you to leave them here, you may signify your pleasure to have them remanded, and it shall be forthwith complied with. I have kept a copy of this letter to shew Sir Jonas Moore, who, upon his return, intends to write to you.

7. Any thing you mislike, in the letters I send you, you may freely write or print against, provided you say it is the assertion of a learned man, and the like, without naming the person in particular, and to do otherwise it is our sentiment would be disobliging to the correspondents of the Royal Society. I lately sent copies of three papers of Leibnitz' to you, and have nothing further at present to add but that I am——

CXXXVII.

COLLINS TO BAKER.

Worthy Sir, 23d May, 1677.

I have yours of the 23d of April, and have been dilatory in answering. The truth of it is, it hath been my misfortune to be concerned in public employments, as in the Council of Plantations, &c., wherein I have not been paid, and have great arrears due to me, for want whereof I am almost ruined; and having a numerous family to maintain, to wit, a wife, and seven small children, I am forced to undertake such occasional business as offers, and by consequence to neglect a correspondence with the learned, which, though unworthy, I much covet. Sir Jonas Moore being a surveyor of the Stores and Ordnance, and now naval preparations going on, he is much in journeys and absent, so that I can give you no account as yet of your labours going to the press, though I much wish I could, and shall omit no endeavours to hasten. I further add that I begin to enter into a small employment, as to part of my time, under the Company of the Royal Fishery, yet in its infancy, who meet on Wednesday and Friday nights at Stationers' Hall, which is not remote from the printing house, and it is my sentiment that Sir Jonas Moore is recommending to one Mr. Scott, a stationer in Little Britain, a new canon (digested by Mercator) of 100,000 Logarithms, with some treatise of Navigation, for the use of forty hospital boys that are fitted to go to sea; and that at the same time he will likewise engage the said bookseller to put yours into the press, which will not be long first[1].

[1] Baker's Geometrical Key was not published till 1684, and then by R. Clavel, at the Peacock in St. Paul's Churchyard.

As to what you mention of requiting me, when Mr. Oliver comes up, pray first let there be a quantum meruit, and next stay till it be demanded. Such time as I spend in midwifery at the press, is such as I can spare, and consequently I account it redounds not to my damage, and I shall be very ready to usher the pains of any learned man into the world, without any other inducement than the public benefit like to ensue.

Lamentation makes the next paragraph. The most learned and pious Dr. Barrow, Master of Trinity College in Cambridge, coming up to make the customary Easter election of some Westminster boys, to be admitted into the University, did here fall sick of a malignant fever, which ended his days, to the very great grief of all, that either knew or heard of his worth. Some mathematical remains there are of his, not yet printed, particularly one about Archimedes' methods of invention, which he takes to have been analytic, and conjunct with the method Indivisibilium vel innumerabilium; twenty-five lectures about mathematical sciences, partly historical, partly doctrinal, about the several methods of invention, argumentation, and demonstration, with discourses of paralogisms, &c.; a Treatise in English about Perspective, and Projections of the Sphere, being public lectures by him read in Gresham College: all which I shall endeavour with his relations to get preserved and printed. And whereas you have, or have seen a little tract of his about constructions for equations, but imperfect, would you vouchsafe its amendment, it might possibly be recommended to the press by Dr. Tillotson and Mr. Hill of the Royal Society, who have the dispose of his papers. And since him we have likewise lost the industrious Mr. Kersey[m], who hath long been afflicted with the

[m] P. 37. This connects the statement there given from Aubrey.

stone in the bladder, which in the event hath cut the thread of life.

I am next to speak of your enclosed paper about the qualifications of such cubic equations, which you say solutionem subeunt; I presume you mean may be solved by aid of canons in surds. You begin thus:

1ª. Formula $x^3 - pxx + qx - r = 0$

Si $pp = 3q$; et $\frac{p^3}{27} \begin{smallmatrix}= \\ \sqsubset \\ \sqsubset\end{smallmatrix} \frac{p^3}{27}$; vel $pp \begin{smallmatrix}= \\ \sqsubset \\ \sqsubset\end{smallmatrix} 3q$.

Pardon me if I desire your explication.

I take the case to be thus. If the triple of the coefficient of the roots be equal to the square of the coefficient of the squares, then upon taking away one of these terms, and new forming the equation, both march off together, and the root sought is found by extracting the pure cube root.

Si $\frac{p^3}{27} \begin{smallmatrix}= \\ \sqsubset \\ \sqsubset\end{smallmatrix} \frac{p^3}{27}$: how the same quantity can be both equal to, and greater than, itself, I understand not, and both these signs $\begin{smallmatrix}\sqsubset \\ \sqsubset\end{smallmatrix}$ or $\begin{smallmatrix}\sqsubset \\ \sqsubset\end{smallmatrix}$ signify that the precedent quantity is greater than the subsequent. If you intend that it may likewise be less, the sign should be $\sqsupset$ or $\sqsupset$.

Your design, I presume, is to give notice of the several kinds of cubic equations, that are disguised: I will mention some, that I have observed.

2. If you put $x^3 = 0$, and increase the root of this equation, all the terms will be full, and as they came in together, so march off together; in which case the root is found by division.

3. If you put $x^3 \pm pxx = 0$, upon increasing the root you will draw in the two last terms, which will

come in with a pair of equal roots, which may be found by division.

4. If you put $x^3 \pm qx = 0$, and increase the root, you draw in the second and fourth terms; and consequently an equation so disguised, when distinguished by proper symptoms, may be solved by division and extracting the square root.

5. If a negative root be numericè equal to an affirmative root, the coefficients and resolvend are directly proportional, and one root is the coefficient of the second term, and the square root of the coefficient of the third term or penultimate, which may be either negative or affirmative, are the other roots sought, as in this example:

$$-a^3 + 7aa + 25a = 175.$$

The proportionals are $1:7::25:175$;

the roots being $+7-5+5$.

To these might be added divers others; but you may ask, whereto doth this tend? I answer, to submit to your consideration, whether such cases as these, and more, mentioned in your letter, ought not only to be intimated in words, but likewise have canons suited to them for their solution.

The twelve problems, that came from Venice, are now, it seems, pretended to have been first proposed by a Professor at Leyden in Holland, and are solved by one at Naples, in an ill-printed sheet of paper; a transcript whereof I here send you, as an excuse till I can get leisure to transmit what will be more pleasing.

CXXXVIII.

BAKER TO COLLINS.

Worthy Sir, June 15, —77, Bp. Nympton.

Yours of May 24, and June 4, —77, I have received, by the former of which I understand that you are not more dignified with public employments than ruined by them ; the defect of the deserved pay proving damageable to your family and correspondents. I shall not complain, though with equal reason, I doubt, so to do, but sympathise with you, having a just equal number of chargeable olive-branches, and being in the same predicament and blessed condemnation with you, not more preaching than unpaid, and preaching the art of contentment to others, am forced to practise it. However, I hope, such disadvantages shall not intercept or interrupt our mutual correspondence.

The surprising news of the death of two such mathematical phœnixes (of our age), as Dr. Barrow and Mr. Kersey, extracted tears from those eyes of mine, which need no additions to render them more dim, as needing rather some optic glasses to advantage them, (and I wish you could procure large ones for me,) that so I might continue in my trade too, the giving over of which, (and give over I must, without some such supplies,) how ruinous it may prove to me and my family, a blind man may easily see.

(1.) Sir, you desire an explication both of my symbols and of those qualifications of cubic equations sent you in my last.

My symbols are these, $\sqsubset^{-}$ greater ; $\sqsubset_{-}$ lesser.

Suppose an equation $x^3 - pxx + qx - r = 0$.

I say generally, either $pp =$, or $\overline{\sqsubset}$, or $\underline{\sqsubset}$ $3q$, which the analyst must try to see which it is:

If he finds $pp = 3q$, then is there an universal rule; for $\frac{p^3}{27}$ must needs be $=$, or $\overline{\sqsubset}$, or $\underline{\sqsubset}\, r$.

1. If $\frac{p^3}{27} = r$, then $\frac{p}{3} = x$, (a direct cubic equation).

2. If $\frac{p^3}{27} \overline{\sqsubset}\, r$, then $\frac{p}{3} - \sqrt[3]{\frac{p^3}{27} - r} = x$.

3. If $\frac{p^3}{27} \underline{\sqsubset}\, r$, then $\sqrt[3]{r - \frac{p^3}{27}} + \frac{p}{3} = x$.

2. If he finds $pp \overline{\sqsubset}\, 3q$, then he must try other conditions; as,

1. If $\frac{pq}{3} - \frac{2p^3}{27} = r$, then are the roots in arithmetical progression; viz. $\frac{p}{3} \pm_{0} \sqrt{\frac{pp}{9} - \frac{3r}{p}} = x$, or

$$\frac{p}{3} \pm_{0} \sqrt{\frac{pp}{3} - q} = x = \left\{ \begin{array}{c} \text{great} \\ \text{less} \\ \text{middle} \end{array} \right\} \text{propor.}$$

2. If $\frac{q^3}{p^3} = r$, then are the roots in geometrical progression; viz. $\frac{q}{p} = x$ middle, and

$$\frac{p}{2} - \frac{q}{2p} \pm \sqrt{\frac{pp}{4} - \frac{q}{2} - \frac{3qq}{4pp}} = x = \left\{ \begin{array}{l} \text{great.} \\ \text{less.} \end{array} \right.$$

With many others of the like bran.

3. If he finds $pp \underline{\sqsubset}\, 3q$, then he must try other conditions; as,

1. If $\frac{q^3}{p^3} = r$, then will $\frac{q}{p} = x$, or $\sqrt[3]{r} = x$, &c.

With divers others, whose roots are obtained by extracting the simple cubic, or square, root only, &c.

The tract of these things is imperfect, or at least needs a licking over before I should expose it to view; however, as it is, I shall send it (as you direct me in your last) to Mr. Enoch Wyre; and if after the perusal it may be thought fit to have it printed, I shall remand it from him, and give it another dress, perhaps, according to the more vendible mode.

(2.) Sir, as (upon your desire) I have given you an explication of my cubics, &c., so I desire you to do the like for me on your observations sent, viz.

1. If you put $x^3 = 0$
2. .... $x^3 \pm pxx = 0$
3. .... $x^3 \pm qx = 0$

} I see not your use.

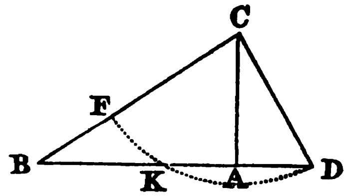
(3.) As for the twelve problems solved by Montfort, I might (if I would) have prevented him, within one week after you first sent them me, naked, or only proposed. I forbore to communicate my answer of them to you then, upon your suggestion in yours to me, of Sept. 21,—76, wherein you wrote, that those problems coming over a year since, and not having been yet answered, it was not very material whether any answer were given to them. However, I (here enclosed) send you a taste of some of them, differing in method but little from Mr. Montfort's; only I work all by species, he in a mixed way; I by analogism, bringing them to be wrought geometrically, he only arithmetically (as it were). I have several ways, he but one; his solution (as in the first problem) adapted but to one side of the triangle, mine to both, which differ from one another, and therefore I suppose mine (in that) more universal; but I leave this to better judgments.

(4.) I have by me (in a tract thirty years since by me composed) the geometrical construction of these subsequent problems, unknown to the ancient analysts, as appears by Regiomontanus' ingenuous confession, Vieta in Appendicula Gall. Apollon., &c.

Symbola eadem [n] quæ Oughtredi.

Lat.	BC $= a$	$Z = \text{BC} + \text{CD}$
	CD $= e$	$X = \text{BC} - \text{CD}$
Bas.	BD $= b$	$Z' = aa + ee$
	CA $= p$	$X' = aa - ee$
	BA $-$ DA $= k$	$Æ = ae$

In triangulo BCD.



1.	Data	b, p, ang. C	
2.		b, k, ang. C	
3.		b, X', ang. C	
4.		k, X', ang. C	
5.		$Æ$, p, ang. C	ad verticem. Requiritur
6.		$Æ$, b, ang. C	triang. geometricè.
7.		b, Z', ang. C	
8.		Z, b, ang. C	
9.		X, b, ang. C	

10.	X, b, ang. B	
11.	Z, b, ang. B	
12.	b, X', ang. B	ad basem. Requiritur
13.	$Æ$, p, ang. B	triang. geometricè.
14.	b, p, ang. B	
15.	b, Z', ang. B	

[n] Z', X', $Æ$, have been substituted for three symbols, which modern types do not supply.

I confess it will create great trouble to transcribe them, there being several ways of operation, and about thirty schemes; and in so small a hand, that I can scarce read them myself.

Sir, I have sent by the bearer, an ingenious gentleman of our parish, Mr. Edmond Gibbons, a solicitor in chancery, (well known to all chancery men,) twenty shillings, as a token to you; desiring you to accept of it, as a small taste from

yours,

THOS. BAKER.

Pray, be not too hasty with Mr. Strode, for his Conics. They are with me, and are not yet finished; which will require more time to do it, than I can well spare as yet.

I thought to have transcribed more of these problems enclosed; but (inter transcribendum) Mr. Gibbons (the mercury of these) waits on horseback at the door.

CXXXIX.

BAKER TO ——[o].

Sir,

I have sent three letters to Mr. Peter Perkins, to which all I have received no answer; the first of which had the first sheet of my Apollonius printed, and by me corrected. I desire you to give me notice, whether any ever came to his hands, and what pro-

[o] The address of this letter is lost, but it was most probably written to Collins.

gress is made in the printing, or whether there be a stoppage on the press, or no; in which you will much oblige

your faithful friend
and servant,
THOS. BAKER.

Bp. Nympton,
Sept. 4, —78.

Major-General Lambert, prisoner at Plymouth, hath sent me these problems to be solved. I desire the solutions of them (having sent mine to him).

Prob. 1.

$$\left.\begin{array}{l} a : b :: c : d \\ aa + bb + cc + dd = 250 \\ b + 5 = c \\ a + 9 = d \end{array}\right\} \text{Qu. } a, b, c, d?$$

Prob. 2.

$$\left.\begin{array}{l} aa + bb + cc + dd = 756 \\ b + 6 = c \\ b - 9 = a \end{array}\right\} \text{Qu. } a, b, c, d?$$

CORRESPONDENCE WITH DR. ISAAC BARROW.

CXL.

BARROW TO COLLINS.

Honoured Sir,

Your excessive goodness doth continue to plunge me into such depths of obligation, which I shall never be able to get out of. All the requital I can make is heartily to thank you. I received the book of Snellius and that of your own composition, which you were pleased to bestow upon me, and for which I thank you, (though I must confess it doth somewhat surpass my capacity,) who have little acquainted myself with that kind of practics, and indeed hardly with any; that little study I have employed upon mathematical businesses, being never designed to any other use than the bare knowledge of the general reasons of things, as a scholar, and no further; so that if you propound any thing to me, I pray please to do it in the most general and abstract terms, as near to the geometrical style as you can; otherwise I shall hardly understand the questions.

I have also received Bettinus, whereof the rate is very moderate. Concerning the books you mention, I have most of them, particularly Hugenius de magnitudine circuli, (which I would gladly have compared with Snellius,) but his treatise de quadratura circuli, &c. I have not, but would gladly see and have it to myself, if procurable, for I exceedingly esteem his writings; and if true that he hath set out an Algebra, I should be glad to have it, though I ken no Dutch, and would try what I could divine out of it. As for Mengolus (in whom I never read any thing) and Stifelius his Algebra, and whatever other books, I refer it wholly to your discretion, and shall be glad to have what you shall think good, and though my employments allow me but little leisure to peruse them thoroughly, yet I shall so far look into them as to give you my judgment of them, if you so require. If Cavallerius his Trigonometry come in your way, you may please to purchase it for me. I was familiarly acquainted with Renaldinus at Florence, and he was then working upon his Algebra. We may expect a collection of what is in former writers, but whether much new I cannot tell.

I cannot tell where may be found an entire table of versed sines, and forbear to make inquisition about it, because, I suppose, if there be any, you may much easier find it by advantage of acquaintance (whereof I have not one here much versed in these things) and books; only in Torricellius, p. 207, you have a table of versed sines (as I take it) for each degree.

What you speak concerning the parabolical conoids I do not understand, whether it is by way of inquiry, or offer. If you ask me, I answer that I think I have sufficient reason to pronounce, that generally the proportions between segments made by planes parallel to the axis (or otherwise) cannot geometrically be found out; because they cut all the circles parallel to the base in different inexplicable proportions, so that it were but a vain labour to endeavour the invention of them. However, I am now employed in thoughts so different, that I cannot well compose my mind to think upon it. If you remember, Mersennus and Torricellius do mention a general method of finding the tangents of curve lines by composition of motions, but do not tell it us. Such a one I have sometime found out, and did think to send it to you, it being only one theorem very easily and simply demonstrated; but wanting leisure to dress it, I will attend till you call for it, if you think such a curiosity worth your regarding. I shall no longer trouble you, but wishing you all health and welfare, with my hearty thanks for all your kindness, rest

your very much obliged

and thankful friend and servant,

Is. Barrow.

I enclose these, that you may be reimbursed in what you have laid out for me. Pray, Sir, excuse my backwardness and hastiness in writing, and impute them partly to my business and partly to my idleness, which I will confess is very great in this kind.

The address of this letter is lost; but there can be little doubt that it was written to Collins. As Barrow mentions his method of tangents, it is particularly to be regretted that we have not the exact date of it. The mention, however, of the parabolical conoid shews that it was earlier than that of the next letter.

This letter is printed in the Gen. Dict. vol. II. p. 708.

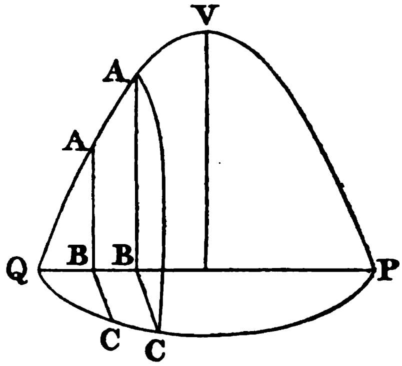
CXLI.

BARROW TO COLLINS.

Honoured Sir,

I have received, and heartily thank you for the book you sent me, but I pray put it upon my account, for it is too much in conscience to put you upon so much trouble and expense too. Your resentments of those small testimonies of my respect do v[ery] much exceed their desert, nor can any thing in my power a[cquit] my obligations to you: as do also those opinions of me [you] express much surpass the truth, and I cannot in any wise own them, being conscious to myself of much defect, and requesting you to have more moderate thoughts of me. I entreat you to procure that Apollonius for me you mention; the price doth not displease me. I find that the Bettinus you sent me (his Ærarium) hath not much more in it than references to his Apiarium; that book, I suppose, is very rare and dear, yet if casually you shall meet with one, I would willingly have it.

Since the term I thought a little concerning the business you proposed about the segments of a parabolical conoid, and I can give the dimension of them. Let ACQ be one of the segments, and L the rectum latus of the parabola QVP, then will L in AB be equal to BC^2, and L in AB in $BC = BC^3$; and AB in $BC = \frac{BC^3}{L}$, and so every-



where (the lines AB being parallel to the axis, and BC being perpendicular to QP the basis of the parabola QVP, or ordinatim applicata in the semicircle QCP) whence the half segment ACQ will be $\frac{2}{3}$ of all the cubes upon the lines BC, divided by L. The sum of these cubes I can give, though the calculation be somewhat long, and therefore I now forbear it.

The case is different in other parabolical conoids; for there the section by planes parallel to the axis do not yield like lines or figures to that which generates the conoid, but altogether of another kind and nature; as in the section of a cone by a plane parallel to the axis, not a triangle but an hyperbola is produced, &c. Wherefore I would gladly hear an intimation in two words, how you apply your reasoning about them. I shall no further trouble you at present, but hoping shortly to kiss your hands at London, in the mean while, wishing you all welfare and happiness, rest in haste,

your most affectionate friend and obliged servant,

Trin. Coll.
Jan. 9, 1664.

IS. BARROW.

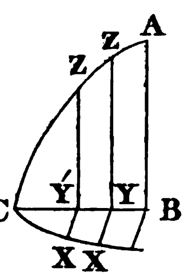
CXLII.

BARROW TO COLLINS.

Honoured Sir,

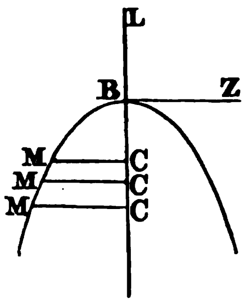
I have received and heartily thank you for the Apollonius, for which, and the other books formerly sent, you may please to reimburse yourself from my father. The Bettinus you sent therewith I have cast my eye upon, but find little but repetitions and trivialities, so that I approve your advice concerning the rest, and shall not be desirous of them further till I shall hap to see them. Your conjecture concerning the proportionality of the lines in the semi-cubic parabola to the cubes of the bases of the parabolas in the quadratic conoid hath something in it of mistake, (which is very excusable, for the conjecture, till examined, might well seem probable).

Let ABC be a cubic semi-parabola; for more easiness let the axis AB and the base BC be equal, and call them r, and call BY a; the line ZY will be $r-\frac{a^3}{rr}$. Call BY′ b, and the line ZY′ will be $r-\frac{b^3}{rr}$, and so always. Multiply these by rr, and they will have themselves as r^3-a^3, r^3-b^3, r^3-c^3, &c. or as $\sqrt{r^6-2r^3a^3+a^6}$, &c. but the cubes of YX will proceed as $\sqrt{r^6-3r^4aa+3rra^4-a^6}$: $\sqrt{r^6-3r^4bb+3rrb^4-b^6}$; so that you see the difference. And indeed if you will find a plane, in which parallel lines proceed as the cubes of the ordinatim applicata in a circle, it will not be like any of the parabolas, nor convex one way.



For let ABC (Plate 4. fig. 1.) be the quadrant of a circle, BQQA a cubical parabola whose axis [is] AC, which will cut the circumference in ZQ, (I can find and determine the point where) consequently, supposing the radius BC to be an unit, and therefore that the lines PG in the triangle ABC are the linear cubes of the lines QG, (the ordinates in the cubical parabola,) the lines YG, (the cubes of the lines ZG,) will be greater than the lines PG, till you come to the line ZQNPYG; but above that line the lines PG will exceed the lines YG, so that the curve BYYA will not be uniform, nor convex or concave one way.

Further, in a cubical parabolical conoid, the lines that are made by a section parallel to the axis, in the superficies of the conoid, are altogether of a different nature from the cubical parabola, and are a kind of hyperbola, whose property is this.



Let LC be the axis of this line, and let LB, BZ be two determinate lines; and $LB = b$, $BZ = c$, and the ordinate $MC = x$, and the intercepted portion $BC = y$. The nature of this line will be expressed by this equation, $x^6 + 3ccx^4 + 3c^4xx = \frac{c^6}{bb}yy + \frac{2c^6}{b}y$; or by this, *Cube* $\overline{xx + cc} =$ *Quad.* $\overline{\frac{c^3}{b}y + c^3}$.

These things I have formerly thought of and write them now extempore; so that I fear you will hardly perceive my meaning, without you think it worth the while a little to consider them. I shall no further trouble you but with the assurance that I shall ever remain

your most affectionate friend,

and faithful obliged servant,

Is. Barrow.

This letter is again without date, but what is said in the beginning about Bettinus seems to refer to something subsequent to letter CXLI; for he there desires to have Apollonius, here returns thanks for it, and mentions Bettinus as received with it. Again the discussion of the conoid was a reason for placing it subsequent to that letter.

There is a little confusion introduced by the expressions semi-cubic, and cubic semi-parabola, but the curve to which the demonstration belongs is obvious. A trifling error in the original has been corrected.

CXLIII.

BARROW TO COLLINS.

Worthy Sir,

I hope you did receive my answer to your very kind letter with the paper enclosed, which I sent immediately upon the receipt of yours, before I had time to peruse it. I am afraid you will esteem me uncivil in not sooner replying further; but the truth is, I am so encumbered with business, that I had scarce any leisure to consider the particulars. However, I have thought a little upon that pretty theorem you mentioned out of Viviani, and the deductions from it, and the results of my thoughts (hastily set down) I send you in the enclosed paper. Were I to compute the portions of a sphere or spheroid, I should only use these rules, out of Archimedes; $\frac{\pi}{\delta}$ or $\frac{355}{113} \times \overline{rnn - \frac{n^3}{3}}$ is the portion of the sphere, putting r for the radius, and n for the axis of the portion, and $\frac{\pi}{\delta} \times \overline{rnn - \frac{r}{3t} n^3}$ is the portion of the spheroid, putting r for the latus rectum and t for the transverse, and n for the axis of the portion. I should be very glad to see Dettonville's book, and Viviani's too, were I not unwilling to deprive you of the use and put you to trouble, for the offer of which, and all your kind and courteous expressions, I acknowledge myself very much obliged to you, and shall, upon all occasions, so far as lies in my power, endeavour to approve myself

your obliged faithful servant,

ISAAC BARROW.

Trin. Coll. 5th Sept. 1664.

Part of this letter is printed in the Gen. Dict. vol. II. p. 709. The expression for the solidity of the spheroid is written in the original $\frac{\pi}{\delta} \times \overline{\frac{rnn}{2} - \frac{r}{3t}} n^3$. The error is manifest, but it is nevertheless printed without correction in the Gen. Dict. See Archimedes, De Con. et Sphær. Prop. XXXI.

CXLIV.

BARROW TO COLLINS.

Honoured Sir,

I have received and thank you for the Mengolus. I shall not have leisure for awhile to consider him seriously; but casting my eye upon him, I do not wonder at Mr. Kersey's not having patience to peruse him. For I perceive he doth affect to use abundance of new definitions and uncouth terms, so that one must, as it were, learn new languages to attain to his meaning, though it may be only somewhat ordinary is couched under them. I esteem this a great fault in any writer, for much time is spent, and labour employed, to less purpose than needed, since there is little in any science but may be sufficiently explaind in the usual manner of speaking; as particularly M. Des Cartes his Geometry doth plainly shew, where so many useful rules are delivered without any new words or definitions at all. But I begin to prate. However, be pleased to put this book upon my account, (seeing you can furnish yourself with another,) for I love to have by me divers books, which I do not much esteem, upon which score you need not scruple at your discretion to send me any book that I have not. I never matter the point of money in this case, and shall take any willingly and thankfully from you: 'tis hard if there be not one thing at least to be learned out of any new book, and that satisfies me more than the expense of a few shillings can displease me.

I send you the catalogue of my mathematical books, with the prices of them as they cost me. There be few or none but common ones and well known to you, but I shall be glad if any of them may ever accommodate you, as likewise of any opportunity to approve and express myself, what I am exceedingly obliged to be, that [I am],

your most affectionate friend,

and faithful servant,

ISAAC BARROW.

Trin. Coll. Nov. 12, 1664.

If you chance to meet with an Alhazenus and Vitello his Optics, please to purchase it for me, I request you.

This letter is printed in the Gen. Dict. vol. II. p. 709.

CXLV.

BARROW TO COLLINS.

Honoured Sir,

I have received and render you many thanks for the book, and especially for your catalogue, which will afford me much direction, and do me great service. You have begot in me a great desire of seeing Lalovera's book, and there be divers others, which I should gladly have, did not my business so take me up that I could hardly allow them any inspection (particularly those of Angeli, and Hugenius, to complete what of them I have already); I am sorry that I cannot furnish you with the Fourniers you desire, for Mr. Story hath not one of them left, (he talks of making another impression,) nor is one of them to be found among our booksellers, so that I am so unhappy as not to be capable therein to serve you.

Alsted's Admiranda Mathematica is nothing but a very short comprisal of the chief mathematical sciences; containing small systems of Arithmetic, Geometry, Astronomy, Geography, Optics, Music, Architecture, according to the methodical or Ramistical way. It is done, I think, well enough according to the design, but may well be wanted.

I cannot very well describe to you Bartschius his Planisphere. It treats more or less concerning most parts of Astronomy, but mainly concerning the fixed stars and their asterisms, giving verbal descriptions, catalogues with longitude and latitude, and three or four maps or delineations of them in plano. At the end are subjoined Ephemerides of the planets from the year 1662 to 1686, with some other astronomical tables. Whether there be any thing extraordinary in the book I cannot tell, for I have looked very little upon it; but if you please I will send it you, that you may inform yourself.

Thomas Albii (that is, in plain English, Mr. Thomas White) his Chrysaspis is a very small tract pretending to the quadrature of the circle (but most easily confutable), as also to prove the equality of the spiral line to the semi-circumference of the circle, to which it appertains: both which errors he hath recanted publicly.

Whether Dibuadus hath commented upon the last books[a] of Euclid I cannot tell: mine is only upon the first six.

I must request your pardon for so hasty and imperfect an attempt: my much business, and present obligation to endeavour, in some sort, the content of a numerous and learned auditory, doth indispose me with that fulness and freedom I desire to express myself, what I am so much obliged to be,

your most affectionate friend
and faithful servant,
ISAAC BARROW.

Trin. Coll. 29 Novemb. 64.

Please to present my service to Mr. Howes when you see him.

Part of this letter is printed in the Gen. Dict. vol. ii. p. 709.

[a] In the margin of this letter Collins has written in this place, "the other four books," and against Thomas Albii he has written, "Bachon."

CXLVI.

BARROW TO COLLINS.

Dear Sir,

I could have wished that you had taken money for the Dettonville; but since you will have it thus, I shall be content with thanks to add this to the score of your innumerable obligations upon me. If you meet with that other piece of his, which you mentioned, please to continue your intention of furnishing me therewith. General rules for the sum of the terms themselves, of the squares, and the cubes in any arithmetical progression, (which I set down because you seem to intimate particular ones,) are these: if the first term be named *a*, the common excess *d*, the number of the terms *n*;

the sum of the terms is $na + \left.\begin{matrix} \frac{nn}{2} \\ -\frac{n}{2} \end{matrix}\right\} d$;

the sum of the squares is $naa + \left.\begin{matrix} nn \\ -n \end{matrix}\right\} ad + \left.\begin{matrix} \frac{n^3}{3} \\ -\frac{nn}{2} \\ +\frac{n}{6} \end{matrix}\right\} dd$;

the sum of the cubes is

$$na^3 + \left.\begin{matrix} \frac{3nn}{2} \\ -\frac{3n}{2} \end{matrix}\right\} aad \left.\begin{matrix} +n^3 \\ -\frac{3nn}{2} \\ +2n \end{matrix}\right\} add \left.\begin{matrix} +\frac{n^4}{4} \\ -\frac{n^3}{2} \\ +\frac{nn}{4} \end{matrix}\right\} d^3.$$

For your proposition concerning Archimedes and Apollonius, I cannot well tell what to answer. I have been offered, by a friend, to be at charges of printing them for me, which would yield me, I suppose, a considerable benefit; for I think I could put off many here. But till I be necessitated by some engagement, I shall hardly ever induce myself to take the pains and spend the time requisite for the reviewal of them; although within two or three months I think I could perform that. If the stationer, you mention, should make me a round offer, and propose fair conditions, I might perhaps be moved: till such occasion I am likely to supersede. I have also been urged to review that little Euclid, which ten or eleven years ago I writ very hastily; I would however gladly have it in my disposal, and therefore would know whether Mr. Neeland (who published it) his widow do make any account thereof. He got (as I have been told from himself) some hundreds of pounds by it, and did not keep conditions with me in printing it so well as he did promise me; so that I might presume to right myself. But I would not have any controversy, nor do the shadow of an injury to any; wherefore if you have opportunity of inquiry, and could inform me, whether Mr. Neeland's relict would consent that I dispose thereof at my pleasure, you would favour me therein. Indeed, if I should [re]solve about Archimedes or Apollonius, I should willingly have that book go along with them, (corrected and enlarged and polished somewhat,) by reason of some short scholiums, that might be conveniently interserted, as lemmatical and preparatory to their demonstrations: as also I should add Theodo[sius] his Spherics and some other elementary things. But this spoken in way of supposition &c.

We have Kepler's Comment. de Stellâ Martis in our library.

I have no more at present, but with my best wishes and prayers for your welfare, rest

your most affectionate friend
and obliged servant,
ISAAC BARROW.

Trin. Coll. March 3. 65.

Part of this letter is printed in the Gen. Dict. vol. ii. p. 703.

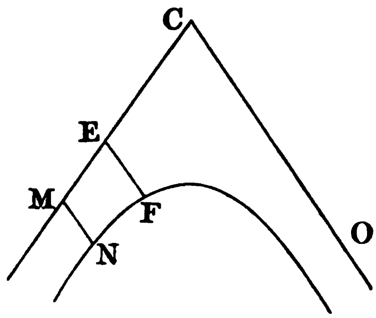
CXLVII.

BARROW TO COLLINS.

Dear Sir,

I have been long guilty in omission of writing to you, although I have been always intending it: partly your desiring an account concerning Mengolus his performances, and partly a design to send you some of my lectures to peruse, (that at least you may see what I, have been doing in those matters,) have retarded; which last I shall do when I meet with a safe opportunity by any of our fellows going up. But as for Mengolus I have been once or twice looking into him; but his language is so uncouth and ambiguous, his definitions so many and so obscure, that I think it were easier, toward the understanding any matter, to learn Arabic than his dialect. So that (beside that I do very much dislike such kind of writing, and hope very little from those that use it) having business enough, (which the last year hath been increased by divers gentlemen being committed to my care,) I can hardly allow leisure, and indeed have not patience enough to pierce into the depth of his obscurities. I see that he propounds many ordinary things involved in his way, but what he hath performed new I cannot guess. The matter itself, you mention, is of manifold use, and I have sometimes thought of it, but what I have forgot, and I think to little purpose: only concerning the dimension of the hyperbola I find this written in a note book, where I used to cast some things that came into my head.

Sint CM, CO, asymptoti hyperbolæ NF; et EF, MN ad CO parallelæ; oportet spatium EFNM, computationi subjicere.



Sint CM, CE commensurabiles (sin minus, aliqua saltem minime differens a CM ipsi CE commensurabilis erit) item sit α ad β ratio quam habent termini sibi proxime succedentes in illâ proportione geometricâ, cui logarithmi adaptantur (quam rationem nonnulli vocant elementum logarithmicum) differentia vero logarithmorum respondentium numeris, quibus exprimitur ipsarum CE, CM proportio, vocetur L;

$$\text{Regula 1. } \overline{\frac{L\beta}{\alpha} - L}\,.\,CE \times EF \sqsubset \text{Hyp. Spat. EMNF,}$$

$$\text{2. } \overline{L - \frac{L\alpha}{\beta}}\,.\,CE \times EF \sqsupset \quad \text{Spat. EMNF.}$$

$$\text{Not. } \frac{L\beta}{\alpha} - L : L - \frac{L\alpha}{\beta} :: \beta : \alpha.$$

Whether this consideration be of any use or no I know not; but having lost too much time upon thoughts of this kind, and despairing of any great success in them, I confess I have little mind to think on them.

I do heartily thank you for your advertisements, and for the book you sent me of optics, which I hope will be of some use to me, and I shall hereafter acquaint you with my thoughts concerning it; for it lies now in my way, and I hardly look on any other, than such as do. Be pleased, I pray, to put it on my account, and in your next to let me know what you have expended for me, that I may reimburse you.

With my best wishes, I remain
your most obliged
and very affectionate
friend and servant,
I. BARROW.

Trinity Coll. Feb. 1, 66–7.

Part of this letter is published in the Gen. Dict. Vol. II. p. 709.

CXLVIII.

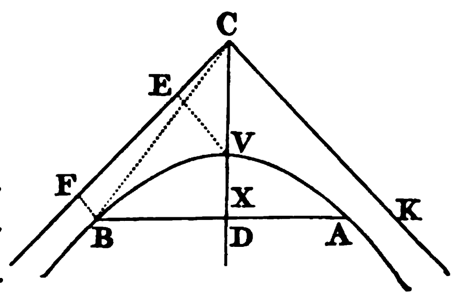
BARROW TO COLLINS.

Dear Sir,

The rules I sent you concerning the hyperbola, I cannot well exemplify, not knowing exactly the proportion, which the arms immediately succeeding have in that rank, according to which the usual tables of logarithms are computed; and having not leisure now to find it out. But the ground of them is this. The asymptotes ZH, ZK making a right angle, let ZA, ZB, ZC, ZD &c. be in continual proportion (Plate 4. Fig. 2.) viz. that of α to β; therefore drawing the lines DM, EN, FP &c. parallel to ZK, and completing the rectangles

EM, Eμ, and FN, Fν &c. all the rectangles EM, FN, GP &c (as also the inscribed ones Eμ, Fν, Gπ, &c.) will be equal each to other. Therefore EM taken so often as there be parts (DE, EF, &c.) in DH, doth exceed the hyperbolical space DHSM; and Eμ, so often taken, is less than the same space; but the number of those parts in DH is equal to the number in ZH, subtracting the number in ZD (that is to the logarithm of ZH, subducting the logarithm of ZD) which difference I call L; and because DE = ZE − ZD = $\frac{\beta}{\alpha}$ZD − ZD = $\frac{\beta-\alpha}{\alpha}$ZD; therefore the parallelogram EM = $\frac{\beta-\alpha}{\alpha}$ ZD × DM and consequently $L \times \frac{\beta-\alpha}{\alpha}$ZD × DM exceeds the space DHSM. In like manner EN = $\frac{\alpha}{\beta}$ DM, and therefore DE × EN = $\frac{\beta-\alpha}{\alpha}$ ZD × $\frac{\alpha}{\beta}$DM = $\frac{\beta-\alpha}{\beta}$ ZD × DM, and thence L × $\frac{\beta-\alpha}{\beta}$ ZD × DM is less than the space DHSM. The use of these rules, which I never well considered, I did ever since I thought of them suspect. But I shall impart some others now, which I take to be somewhat better, which I lately reduced from the like concerning the circle, that I long since thought of; the which I shall all briefly set down.

Sit C Centrum, CD axis, V vertex, BA basis segmenti circularis vel hyperbolici, et X centrum gravitatis utriusvis segmenti. Accipiantur numeri quilibet m, n (quorum minor m) et sit $\frac{m}{n}$ in hyperbola ma-



jor, in circulo minor quam $\frac{1}{2}$. Item in Hyperb. sit $VD = \text{vel} \sqsupset \frac{2m-n}{n-m}$, et in circulo $VD = \text{vel} \sqsupset \frac{n-2m}{n-m}$

Erit 1. Reg. in Hyperb. $\frac{n}{m+n} VD \times BA \sqsupset$ segm. BVA.

2. Reg. in Hyperb. $\frac{n}{m+2n} VD \sqsupset DX$.

Universim autem

3. Reg. in Hyper. est $\frac{2}{3} VD \times BA \sqsubset$ segm. BVA.

4. Reg. in Hyper. est $\frac{2}{5} VD \sqsubset DX$.

In circulo 1. Reg. $\frac{n}{m+n} VD \times BA \sqsubset$ segm. BVA.

2. Reg. $\frac{n}{m+2n} VD \sqsubset DX$.

Universim vero est

3. Reg. $\frac{2}{3} VD \times BA \sqsupset$ segm. BVA.

4. Reg. $\frac{2}{5} VD \sqsupset DX$.

Exempl. 1. In Hyp. sit $\frac{m}{n} = \frac{3}{4}$, adeoque (justa præmissam cautionem), $VD = \text{vel} \sqsupset 2\ CV$.

Erit segm. $BVA \sqsubset \frac{4}{7} \left(\text{vel } \frac{12}{21}\right) VD \times BA$,

$\sqsupset \frac{2}{3} \left(\text{vel } \frac{14}{21}\right) VD \times BA$.

item $DX \sqsubset \frac{4}{11} \left(\text{vel } \frac{20}{55}\right) VD$,

$\sqsupset \frac{2}{5} \left(\text{vel } \frac{22}{55}\right) VD$.

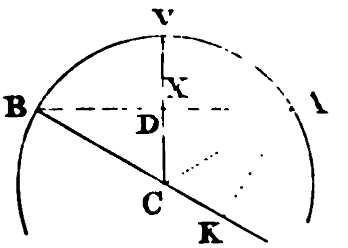
Exempl. 2. In Hyp. sit $\frac{m}{n} = \frac{1001}{2001}$, adeoque $VD = \frac{1}{1000} CV$,

$$\text{Erit segm. } BVA \sqsubset \frac{2001}{3002} VD \times BA \sqsupset \frac{2}{3} VD \times BA;$$

$$\text{et } DX \sqsubset \frac{2001}{5003} VD, \text{ at } \sqsupset \frac{2}{5} VD.$$

ac ita progredi licet quousque placuerit[m]. Minimæ vero cujusvis hyperbolicæ particulæ dimensione supputatâ, difficile non fuerit ampliora spatia dimetiri, &c.

Exemp. 3. In Circulo, sit $\frac{m}{n} = \frac{1}{3}$, adeoque (juxta cautionem præstitutam) $VD =$ vel $\sqsupset \frac{1}{2} CV$;



$$\text{Erit segm. } BVA \sqsupset \frac{3}{4} \left(\text{vel } \frac{9}{12}\right) VD \times BA,$$

$$\sqsubset \frac{2}{3} \left(\text{vel } \frac{8}{12}\right) VD \times BA;$$

$$\text{et } DX \sqsupset \frac{3}{7} \left(\text{vel } \frac{15}{35}\right) VD,$$

$$\sqsubset \frac{2}{5} \left(\text{vel } \frac{14}{35}\right) VD.$$

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[m] The position of the words in the original is perplexing, but this must be their connection.

Exemp. 4. In Circ. sit $\frac{m}{n} = \frac{5}{11}$, adeoque VD = vel $\sqsupset$ $\frac{1}{6}$ CV. Puta sit arcus BVA = 60 gr.

$$\text{Erit segm. BVA} \sqsupset \frac{11}{16}\left(\text{vel } \frac{33}{48}\right) VD \times BA,$$

$$\sqsubset \frac{2}{3}\left(\text{vel } \frac{32}{48}\right) VD \times BA;$$

$$\text{et DX} \sqsupset \frac{11}{27}\left(\text{vel } \frac{55}{135}\right) VD,$$

$$\sqsubset \frac{2}{5}\left(\text{vel } \frac{54}{135}\right) VD.$$

Coroll. In Hyperbola facile accommodantur hæc ad spatia (qualia EVBF) posito CX, CF esse asymptotos hyperbolæ, et VE, BF ad CK fore parallelas. Nam spatium quinquangulum EVDBF æquatur triangulo CDB = $\frac{CD \times DB}{2}$, adeoque spatium EVBF $\sqsupset \frac{CD \times DB}{2} - \frac{n}{m+n} VD \times DB$, $\sqsubset \frac{CD \times DB}{2} - \frac{2}{3} VD \times BD$.

Coroll. In Circulo, sit AK (= s) sinus rectus arcûs BA, et BA = c, et CD = t, et radius CV = r.

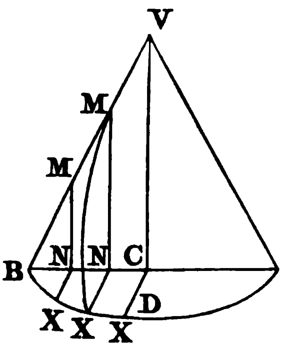
$$\left.\begin{array}{l} \text{1. Erit } AK + \frac{2n}{m+n}\overline{AB - AK} \sqsubset \text{arc BVA} \\ \text{at } AK + \frac{4}{3}\overline{AB - AK} \sqsupset \text{arc BVA} \end{array}\right\} \text{e 1\&2 reg}$$

$$\left.\begin{array}{l} \text{2. Erit } \dfrac{\left.\begin{array}{l}2m\\+4n\end{array}\right\} rc \left.\begin{array}{l}+3nr\\+mt\\-nt\end{array}\right\} s}{3nr \left.\begin{array}{l}+3m\\+3n\end{array}\right\} t} \sqsupset \text{arc BVA} \\ \text{at } \dfrac{10\,rc \left.\begin{array}{l}+6r\\-t\end{array}\right\} s}{6r + 9t} \sqsubset \text{arc BVA} \end{array}\right\} \text{e 3\&4 reg}$$

What you mention out [of] Mr. Warner's paper I find not to be true, that the spaces comprehended by equally distant parallels are in a musical progression, which I thus shew.

Sint ZD, DE, EH, HI, æquales (Pl. 4. Fig. 3.) erit spatium ZDMK infinitum, ergo non universe verum est quod asseritur. Porro dico spatium DN minus esse duplo spatii EO; nam inter FO, DM proportione media sit XY, unde spatium $DY = \frac{1}{2} DO$, et $FO = \frac{DM}{3}$, adeoque $XY = \sqrt{\frac{DM^2}{3}} = \sqrt[4]{\frac{DM^4}{9}}$. Item inter EN, DM trium proportione mediarum prima sit TS; unde spatium $TN = \frac{1}{4}$ spat. DN, et $TS = \sqrt[4]{\frac{DM^4}{8}}$, ergo $TS \sqsubset XY$, et spatium $TN \sqsubset XN$, hoc est $\frac{1}{4} DN \sqsubset XN$; quare $\frac{3}{4} DN \sqsupset DN - XN = DY = \frac{1}{2} DO$; ergo $3\ DN \sqsupset 2\ DO$, vel $\frac{3}{2} DN \sqsupset DO$, hoc est $DN + \frac{1}{2} DN \sqsupset DN + EO$; quare $\frac{1}{2} DN \sqsupset EO$. Q. E. D. Simili discursu similes hujusmodi assertiones refellere licet &c.

As to your quære, whether, supposing the triangle VCB be equal to the quadrant BCD, the hyperbolas MNX will be equal to the segments BNX (each to each). I answer negatively. For if each be equal to each, then the sum of the hyperbolas will be equal to the sum of the segments: that is the quadrant of the cone BVA will be equal to all the segments BNX, which I thus disprove.



Suppose the quarter of the cone be equal to all the segments BNX. Now because CV is equal to BD (which I call π), a quadrant of the cone will be equal to $\frac{r\pi}{2} \times \frac{\pi}{3} = \frac{r\pi\pi}{6}$, but all the segments BNX are equal to $\frac{r}{2}$ into all BX, $-\frac{r}{2}$ into all NX, that is (because all the arches BX are equal to the square of the radius, and all the lines NX to the quarter of the circle) to $\frac{r^3}{2} - \frac{rr\pi}{4}$; therefore $\frac{r\pi\pi}{6} = \frac{r^3}{2} - \frac{rr\pi}{4}$, or $\frac{\pi\pi}{3} = rr - \frac{r\pi}{2}$, or $\pi\pi + \frac{3r\pi}{2} = 3rr$. Wherefore $\sqrt{\frac{9}{16}rr + 3rr} - \frac{3r}{4} = \pi$, or (quadruplating both sides) $\sqrt{9rr + 48rr} - 3r$, or $\sqrt{57rr} - 3r = 4\pi$; wherefore $5r$ is greater than the whole circumference, which is very absurd.

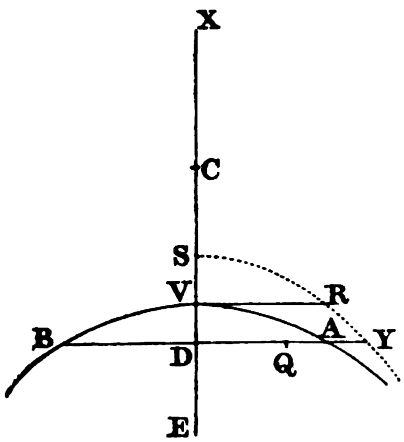
Now my hand is in, I will add briefly these theorems concerning the superficies of conoids and spheroids, which, for all that I know, are new.

Sit BVA (Pl. 4. Fig. 4.) sectio quævis conica, cujus axis VD, basis BA, vertex V, centrum C (modo centrum habeat) et VR (ipsi VD perpendicularis) semissis parametri, quæ dicatur *P*.

1. In parabola fiat SV $= \frac{1}{2}$ VR (vel $\frac{1}{4}$ *P*) et vertice S, axe SD, parametro *P*, describatur parabola SRY; erit spatium VRYD ad superficiem conoidis parabolici BVA ut circuli radius ad circumferentiam.

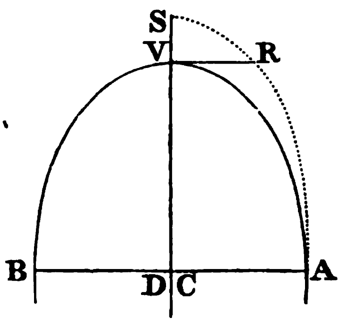
Nam sit EF pars aliquota minima lineæ parabolicæ VB, et ducatur ET tangens parabolam, ac sint EMH, GFK ad VD perpendiculares, et EP tangenti ET perpendicularis. Estque EG : NM :: ET : TM :: EP : EM, unde EG $\times$ EM $=$ NM $\times$ EP. Est autem EP $=$ MH (quod non difficile demonstratur); ergo EG $\times$ EM $=$ NM $\times$ MH $=$ rectang. MK. Hinc patet spatium VDYR æquari sinubus rectis omnibus simul sumptis ad arcus VB pertinentibus; adeoque propositum liquet.

2. In Hyperbola (latus ejus transversum dicatur T), fiat $DE = \frac{P}{T} VD + \frac{P}{2}$, et ducatur EB, et fiat $DY = EB$, tum fiat $\sqrt{DY^2 - VR^2} : VR :: \sqrt{CD^2 - CV^2} : L$, et fiat $\sqrt{CV^2 - L^2} = SC = CX$. Et centro C, vertice S, diametro XS per punctum R (vel Y) describatur hyperbola SRY. Erit spatium VRYD ad superficiem conoidis ut radius circuli ad circumferentiam.



Demonstratur eodem plane modo, quo præcedens.

3. In Ellipse, fiat $\sqrt{CA^2 - VR^2} : CA :: CV : CS$, et centro C, vertice S, semiaxibus CS, CA describatur ellipsis SRA; erit spatium VRA ad spheroidis superficiem itidem ut radius circuli ad circumferentiam.



Demonstratur itidem hoc, ut in parabola.

Cor. In Circulo, quoniam est $CA - VR = 0$, vertex S infinite distat a centro C; unde VRAC est quadratum super CA.

I shall only add that I am

your most affect. fr.

and obliged servant,

I. B.

March 6. 1667–8.

I have not yet had opportunity to send the books I spake of; and I think it better to reserve them now till the next term is past, when I shall have somewhat more, and more considerable, and intend then myself for London.

I cannot think any thing of mine to deserve so public a notification; but otherwise you may impart as you think good what, upon consideration, you do approve. Only I desire no mention be made of me, or intimation whence it comes, &c.

CXLIX.

BARROW TO COLLINS.

Dear Sir,

Our term beginning next week, I cannot set myself fully to satisfy your desire; however by this little you may discern somewhat concerning the reason of those rules I sent you.

Sit (Pl. 4. Fig. 5.) circuli vel hyperbolæ æquilateræ (cujus centrum C) segmentum $\beta EV\alpha A$, communique axe $V\delta$, vertice V, base $\beta\alpha$, descripta censeatur paraboliformis $\beta HVH\alpha$, cujus exponens $\frac{m}{n}$ (hic autem in circulo minor esto, major autem in hyperbola quam $\frac{1}{2}$). Notentur hæc.

1. Si fuerit axis $V\delta =$ vel $\sqsupset \frac{n-2m}{n-m}$ CV, tota linea paraboliformis $\beta HVH\alpha$ (ad partes verticis) extra circulum cadet.

2. Si fuerit $V\delta$ = vel $\sqsupset \frac{2m-n}{n-m}$ CV, tota paraboliformis intra hyperbolam cadet, ad partes verticis.

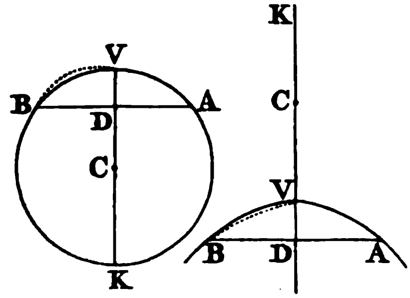
3. Si fuerit $VD = \frac{n-2m}{n-m}$ CV, et $V\delta \sqsupset$ VD, paraboliformis $\beta HVH\alpha$ producta (puta βM, αN) intra circulum excurret, usque ad BA.

4. Si fuerit $VD = \frac{2m-n}{n-m}$ CV, et $V\delta \sqsupset$ VD, paraboliformis $\beta HVH\alpha$ producta (ut βM, αN) usque ad BA extra hyperbolam excurret.

5. Si fuerit $V\delta = \frac{n-2m}{n-m}$ CV, circuli et paraboliformis una ad β communis erit tangens, puta βT.

6. Itidem si fuerit [$V\delta =$] $\frac{2m-n}{n-m}$ CV, hyperbola et paraboliformis communem ad β tangentem habebunt, ut βT.

7. Iisdem positis, paraboliformium parametri ad β in circulo omnium ad ejusdem generis figuras circulo adscriptibiles pertinentium maximæ erunt; in hyperbola vero omnium minimæ.



8. Parametrum appello rectam datam, quæ ipsa, vel cujus potestas ducta in potestatem ipsius VD ab *m* denominatam, ipsius D B potestatem efficit, ab *n* denominatam. Ut si $DB^4 =$

$P \times VD^3$; aut si $DB^3 = P^2 \times VD$, erit P parameter. Et brevioris examinis gratiâ notentur hæ regulæ, (VK diameter).

In circulo, $p = \sqrt{2n - 2m \,.\, KD^n \times VD^{n-2m}}$.

In hyperbola, $p = \sqrt{2n - 2m \,.\, \frac{KD^n}{VD^{2m-n}}}$.

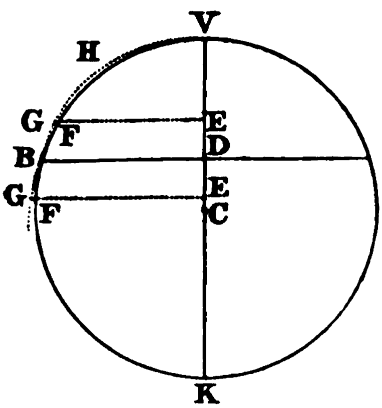
9. Parabola (Apollonianam intelligo) communes cum circulo et hyperbola verticem, axem et basin habens, tota (supra basin) intra circulum versatur; infra vero protracta extra circulum transcurrit. Contra vero in hyperbola, pars supra basin extrorsum jacet, pars infra illam introrsum excurrit.

10. Adscisco quoque de lineis paraboliformibus ista jam (opinor) satis pervulgata; quòd nempe sit spatium $\beta V\alpha = \frac{n}{m+n} V\delta \times \beta\alpha$, et $\delta T = \frac{n}{m} V\delta$, et, posito X centro gravitatis spatii $\beta V\alpha$, fore $\delta X = \frac{n}{m+2n} V\delta$.

Ex hisce deducuntur istæ regulæ, quarum demonstratio multa deposceret verba, plura certe quam quæ nunc iis mihi vacat impendere. Nihilominus unum aut alterum e facilioribus exemplum proponam.

Exemp. 1. In circulo sumatur $\frac{1}{4}$ pro $\frac{m}{n}$, adeoque $\frac{n-2m}{n-m} = \frac{2}{3}$. Ponatur $VD = \frac{2}{3} VC$, (unde $VD = \frac{1}{2} DK$); estque $\sqrt{2n - 2m \,.\, KD^n \times VD^{n-2m}} = \sqrt{6KD^4 \times VD^2} = \sqrt{6} \,.\, KD^2 \times VD = p$.

Accipiatur in VK punctum quodvis E. Dico fore $KD^2 \times VD$ (hoc est $4VD^3$) $\sqsubset$ $KE^2 \times VE$; id autem ex calculo facillime colligitur; nam $KE^2 \times VE = 4VD^3 - 3VD \times DE^2 \pm DE^3$.

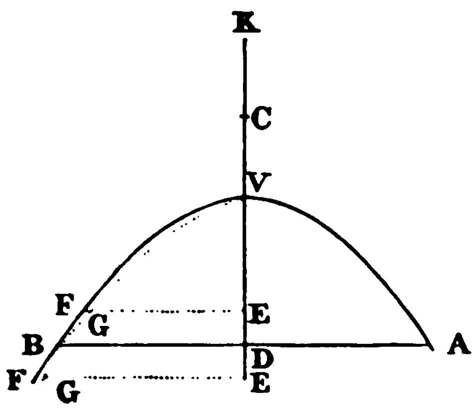


Ductâ jam EFG ad DB parallelâ, sit punctum F in circulo, et punctum G in paraboliformi (cujus index $\frac{1}{4}$) estque $EG^4 = KD^2 \times VD \times VE \sqsubset KE^2 \times VE \times VE = EF^4$: quare $EG \sqsubset EF$. Ergo punctum G est extra circulum. Et idem de quibusvis aliis punctis constat. Adeoque tota paraboliformis extra circulum cadit.

Not. Quo punctum E ad D propius accedit, eo $KE^2 \times VE$ majus est, ut facile monstratur: et inde propos. 3 (quæ supra) deducitur.

Exemp. 2. In hyperbola sumatur $\frac{m}{n} = \frac{3}{4}$, adeoque $\frac{2m-n}{n-m} = 2$. Ponatur ergo $VD = 2CV = KV$; item est

$$\sqrt{\frac{2n-2m \,.\, KD^{n}}{VD^{2m-n}}} = \sqrt{\frac{2KD^4}{VD^2}} = \sqrt{2}\frac{KD^2}{VD}.$$



Dico (sumpto quovis in KD puncto E) fore $\frac{KD^2}{VD} \sqsupset \frac{KE^2}{VE}$, hoc est $4VD \sqsupset \frac{KE^2}{VE}$, vel $4VD \times VE \sqsupset KE^2$; nam $KE^2 = \overline{KV+VE}^2 = \overline{VD+VE}^2 = VD^2 + VE^2 + 2VD \times VE$; at $VD^2 + VE^2 \sqsubset 2VD \times VE$, (sunt enim VD^2, $VD \times VE$, VE^2 proportionales,) ergo liquet propositum.

Ductâ jam EF ad DB parallelâ, quæ secet hyperbolam in F, et paraboliformem (cujus index $\frac{3}{4}$) in G, erit $EG^4 = \frac{KD^2}{VD} \times VE^3 \sqsupset \frac{KE^2}{VE} \times VE^3 = KE^2 \times VE^2 = EF^4$; ergo $EG \sqsupset EF$; quare punctum G est intra hyperbolam, et tota proinde paraboliformis VGB.

Not. Quo punctum E propius est ipsi D, eo minus est $\frac{KE^2}{VE}$, indeque pendet prop. 4. supra.

Ita sufficiet utcunque regularum fontes indicâsse.

That these rules may be better accommodated to practice, and that it may be known what numbers for $\frac{m}{n}$ are fit in each particular case to be taken, I add these two rules.

In circulo si sit $VD = \frac{s}{t} CV$, sumatur $m = t - s$, et $n = 2t - s$.

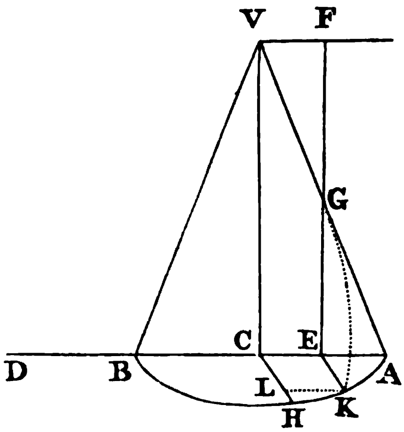
In hyperbola si sit $VD = \frac{s}{t} CV$, sumatur $m = t + s$, et $n = 2t + s$.

And to avoid multiplicity, the former rules may be altered, putting these equal terms every where in the place of m and n respectively, which I leave to your pleasure.

To give some experiment of those rules, I shall by them examine that method, which you say the gaugers use; taking an easy case to that purpose.

Conus rectus VBA, cujus axis VC, secetur planis parallelis VCH, FEK, et sit BD = BC, conoque circumscribatur cylindrus &c.

Bisecetur CA in E, et sit axis CV = DE, et quia frustum conicum VCHKEGV æquatur duobus pyramidalibus, quorum unius basis est hyperbola GEK, altitudo EC, et alterius basis est circulare segmentum CHKE, altitudo VC; erit illud frustrum conicum æquale τῷ $\frac{\text{CE} \times \text{hyp. GKE} + \text{VC} \times \text{CHKE}}{3}$. Cylindri vero segmentum FVCHKEFV = VC × CHKE; est autem juxta methodum propositam, DE : CA (hoc est in præsenti casu 5 : 2) : : segm. cyl. : frust. con. : : VC × CHKE : $\frac{\text{CE} \times \text{GKE} + \text{VC} \times \text{CHKE}}{3}$: : 3VC × CHKE : CE × GKE + VC × CHKE; ergo 6VC × CHKE = 5CE × GKE + 5VC × CHKE, vel VC × CHKE = 5CE × GKE; et quia VC = 5CE, hinc CHKE = GKE. Hoc est segmentum circulare CHKE hyperbolicum spatium GKE adæquabit: hoc ad regulas nostras exigamus.



Sumatur $\frac{m}{n} = \frac{2}{3}$, ergo $\frac{2m - n}{n - m} = 1$, est autem F centrum hyperbolæ EGK, et FG = GE: ergo cautioni nostræ hac in parte satisfactum est quoad hunc casum.

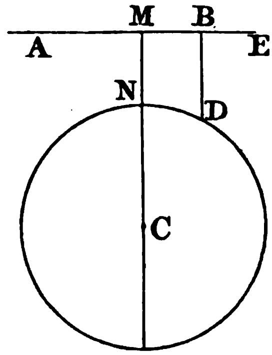
Est proinde hyperb. GKE $\sqsubset \frac{n}{m+n}\left(\frac{3}{5}\right)$ GE $\times$ EK, hoc est (CA $= r$) $\sqsubset \frac{3}{8}rr\sqrt{3}$ (nam GE $= \frac{VC}{2} = \frac{5}{4}r$, et EK $= \frac{r}{2}\sqrt{3}$).

Rursus adsumatur $\frac{m}{n} = \frac{1}{3}$, et (ductâ KL ad HC perpendiculari) est $\frac{n}{m+n}\left(\frac{3}{4}\right)$ HL $\times$ KL $\sqsubset$ Segm. circ. KHL, hoc est $\frac{3}{4} \times \overline{r - \frac{r}{2}\sqrt{3}} \times \frac{r}{2}$ (hoc est $\frac{3rr}{8} - \frac{3rr}{16}\sqrt{3}$) $\sqsubset$ segm. KHL; adjungatur rectang. CLKE $= \frac{rr}{4}\sqrt{3} = \frac{4rr}{16}\sqrt{3}$, erit $\frac{3rr}{8} + \frac{1}{16}rr\sqrt{3} \sqsubset$ HKEC. Hactenus nil occurrit absurditatis aut repugnantiæ. Itaque rursus tentemus.

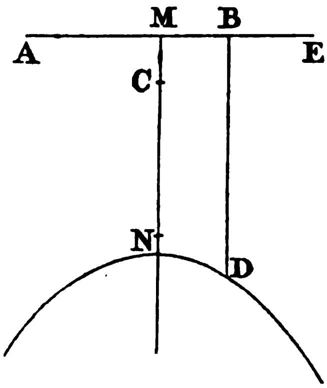
Accipe $\frac{m}{n} = \frac{5}{11}$, eritque juxta nostras regulas $\frac{11}{16}$ VD $\times$ BA $\sqsubset$ HKL, hoc est $\frac{11rr}{32} - \frac{11}{64}rr\sqrt{3} \sqsubset$ HKL; ergo, adjungendo rectangulum CHKE $= \frac{rr}{4}\sqrt{3} = \frac{16rr}{64}\sqrt{3}$, erit $\frac{11rr}{32} + \frac{5}{64}rr\sqrt{3} \sqsubset$ HKEC; est autem $\frac{3}{8}rr\sqrt{3}$ (hoc est $\frac{24}{64}rr\sqrt{3}$) majus quam $\frac{11rr}{32} + \frac{5rr}{64}\sqrt{3}$, vel $\frac{19}{64}rr\sqrt{3} \sqsubset \frac{11}{32}rr$, vel $19\,rr\sqrt{3} \sqsubset 22\,rr$. Hinc quiddam minus hyperbolico spatio GKE majus est quodam quod segmentum circulare excedit. Et proinde spatia hæc inæqualia sunt, quod adversatur præmonstratis. Methodus igitur indigitata peccare deprehenditur in hoc casu; ac proinde in aliis aberret oportet.

Concerning the construction of solid problems, to which your inclosed refers, I have sometime, among other things, observed that all, when the equations are quadrato-quadratical, may be referred to these two problems, and be constructed as they, without any reduction or alteration of the terms.

1. Dato circulo, et recta AE positione data, et in hac assignato puncto A; reperiatur in ea punctum B, a quo ducendo BD ipsi AE perpendicularem, quæ circulum secet in D, sit rectangulum ABD æquale dato plano (P^2).

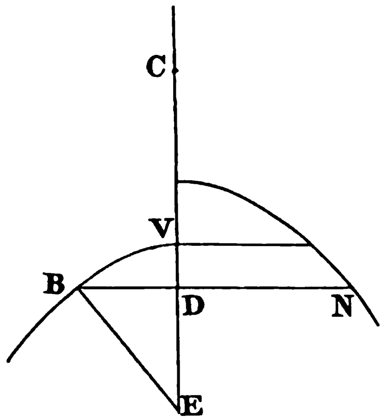


2. Itidem data hyperbola et recta AE positione, punctoque A in hac; reperiatur punctum B, a quo si ducatur BD ad AE perpendicularis, sit rect. ABD = dato P^2.



Prius ad æquationes spectat, quales a^4, la^3, $mmaa$, n^3a, $+ p^4$, in quibus habetur $+ p^4$; alterum ad illas a^4, la^3, $mmaa$, n^3a, $- p^4$ in quibus p^4 negatur. Varia positio lineæ AB et puncti A in ea, et quantitas datarum &c. cunctas æquationum varietates efficit. Etiam cubicæ æquationes hic se offerunt, modo detur una radix, ceu MN; vel, modo planum datum æquetur rectangulo AMN.

Concerning the imparting those things, although I persist in my opinion, yet I shall leave it to your pleasure, on condition they be first well examined. I writ in haste, and, had I thought of such a thing, might have considered and polished them better: and perhaps there may be divers errors in them; as for instance, after I had sent my last, I presently recollected



an error in the second theorem about the surface of the conoids, where, if I do not forget, I took the line, as may be $DN = \frac{P}{T} VD + \frac{P}{2}$, when as I should have taken $DE = \frac{P}{T} VD + \frac{P}{2}$, have drawn EB, and made $DN = EB$, &c. which error I pray you to correct, that came from my haste and slip of memory. And others, I suppose, there may be the like; wherefore I would not that any of those things be communicated before they be tried. If that honourable person, whom you mentioned, should think it worth his regard, and condescend to try them, I should be content to have them disposed as he should approve, &c. But I see no need of mentioning names. A member of the society to another inquiring about the dimension of, &c. would be abundantly, to my seeming, sufficient.

With my best wishes, hearty love, and service, I rest

Your most affectionate friend

and obliged servant,

Isaac Barrow.

Trin. Coll. March 28, 1668.

On the subject of this and the following letter, see Barrow's Lectiones Geometricæ, and particularly the Appendicula to Lect. XI.

CL.

BARROW TO COLLINS.

Dear Sir,

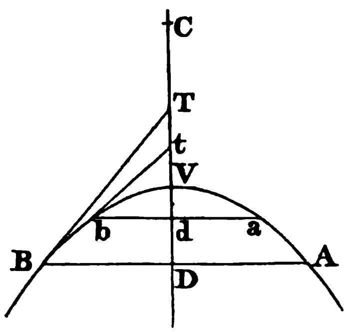
I have received the remainder of D. P. [Dr. Pell?] his book, for which I return you many thanks.

If you so much differ in opinion from me as to think any of those things fit to be published, I would, however, gladly be first informed what, and how. I shall now briefly and cursorily set down a kind of general demonstration, by which the truth of those rules concerning [the] hyperbola and circle will sufficiently, I take it, appear. But if my Lord vouchsafe to examine them, I doubt not but some more handsome demonstration of them will offer itself to his perspicacious mind.

1. (Pl. 4. Fig. 6.) In circulo, (cujus centrum C,) si fuerit $VD = \frac{n-2m}{n-m}$ CV, et DB ad CV perpendicularis, ac BT circulum tangat; etiam BT tanget paraboliformem, cujus exponens $\frac{m}{n}$ (axe nimirum et base iisdem descriptam).

Nam quia BT circulum tangit, erit CT : CV : : CV : CD; quare CT − CV : CV − CD : : CV : CD, hoc est TV : DV : : CV : CD; componendoque TD : DV : : CV + CD : CD. Item quia (ex hypothesi) est CV : VD : : $n-m$: $n-2m$, erit per conversionem rationis CV : CD : : $n-m$: m, quare componendo erit CV + CD : CD : : n : m. Quapropter erit TD : DV : : n : m; unde liquet propositum.

2. In hyperbola, cujus centrum C, axis CVD, si fuerit $VD = \frac{2m-n}{n-m}$, et BT tangat hyperbolam; etiam hæc eadem paraboliformem tanget, cujus exponens $\frac{m}{n}$, (axe VD, base BD descriptam). Etenim quia



rursus CD : CV : : CV : CT, erit CD − CV : CV − CT : : CD : CV, hoc est VD : TV : : CD : CV; quamobrem inverse componendo erit TD : VD : : CV + CD : CD. Verum est CV : VD : : $n-m$: $2m-n$, ideoque inverse componendo CV : CD : : $n-m$: m, iterumque componendo CV + CD : CD : : n : m. Quare TD : VD : : n : m; adeoque patet propositum.

LEMMA.

In ellipse, vel circulo, vel hyperbola (quibus centrum C, axis CD, vertex V,) rectæ BT, bt tangant, et sint BD, bd perpendiculares axi; ac VD $\sqsubset$ Vd; dico in ellipse vel circulo fore TD : VD $\sqsubset$ td : Vd, sed in hyperbola contrarie fore TD : VD $\sqsupset$ td : Vd.

Nam est TV : VD : : CV : CD : : CT : CV, item (simili de causa) tV : Vd : : Ct : CV; ergo quum sit in ellipse CT : CV $\sqsubset$ Ct : CV, erit quoque TV : VD $\sqsubset$ tV : Vd; componendoque [&c.]. Item quum sit in hyperbola CT : CV $\sqsupset$ Ct : CV, erit etiam TV : VD $\sqsupset$ tV : Vd.

PROPOSITIO.

1. Recta BT (Pl. 4. Fig. 6.) circulum (vel ellipsin) et paraboliformem communi base BA, axe VD descriptam

Plate 1.

Fig 1 p. 37.

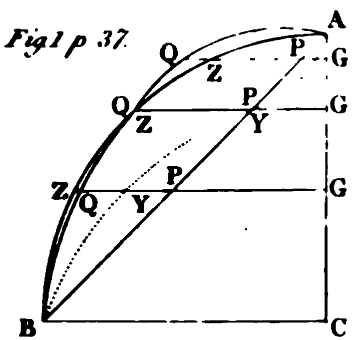
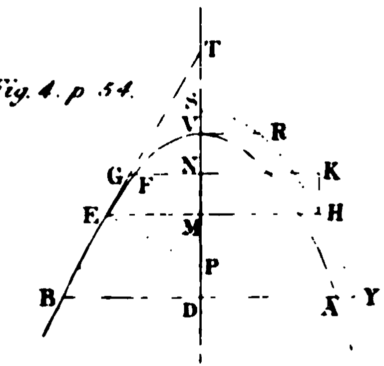


Fig. 4. p. 54.



tangat; et sit Vd —⊐ VD, et base bd, axe Vd concipiatur ejusdem generis paraboliformis describi; hæc, inquam, tota cadet extra circulum vel ellipsin.

Nam recta bt circulum tangat ad b; est ergo (per lemma præcedens) td : Vd —⊐ TD : VD. Ergo bt paraboliformem bVa secat, puta in E, ergo saltem paraboliformis pars quam subtendit recta bE est extra circulum. Concipiatur per E transiens ellipsis ad semiaxem VC descripta, quam tangat ES, et ducatur EK ad BD parallela. Est rursus SK : VK —⊐ TD : VD, adeoque ES paraboliformem EVA secat, puta in F; ergo ejus pars EF extra dictam ellipsin jacet, adeoque magis extra circulum. Similem discursum continuando tota paraboliformis extra circulum sita demonstrabitur.

Persimili discursu (præcedentis lemmatis alteram partem adhibendo, et hyperbolas ad communem semiaxem CV descriptas adscribendo) constabit paraboliformes intra hyperbolam totas jacere. Verbis parco, negotiis plenus, inops temporis.

In haste, your most affectionate friend
and obliged servant,
I. Barrow.

Trin. Coll. May 14, 68.

CLI.

BARROW TO COLLINS.

Dear Sir,

Those Lectures, having (as you may see in the beginning) got a warrant to go abroad, fly to you, and to your protection I commit them. In the fourteenth lecture I have tore out some leaves, concerning the determination of images in all kinds of lenses, for all cases, which I shall send you somewhat more exacted.

I have also a lecture or more behind, which shall be sent in due time; also somewhat of preface. In the mean time, being entered, I could wish expedition.

I have nothing yet to answer concerning the matters of your last; indeed, for the reason, touched upon formerly, I shall presume upon you so as to forbear thinking of them until the term comes, and I set my thoughts upon such things; when perhaps I shall be better disposed thereto, although I do not hope much to hit upon any thing satisfactory to those purposes.

I did forget to answer concerning somewhat you formerly proposed about M. Slusius his books. If any store of them lie upon your hands, you may please to deliver half a score (or so many as you shall think convenient for you) to this bearer John Stiles, a carrier of this place, receiving of him the price: however, I pray, spare me one or two, which I would dispose of to friends.

Please to present my service to Mr. More. So with my best wishes, I rest

Your most affectionate and obliged servant,

Isaac Barrow.

Trin. Coll. Feb. 23, 1668-9.

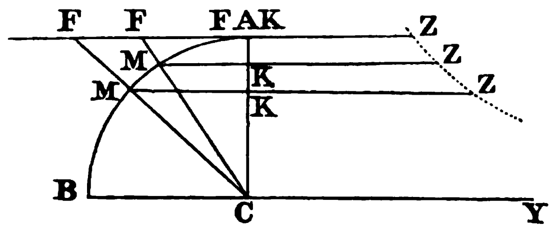
CLII.

BARROW TO COLLINS.

Dear Sir,

I write now only to save you the trouble of calling upon the carrier; for it will about be the end of the next week before I can send my book; and I shall take care that it come safely to your hands, without your further trouble. I leave the agreeing with Mr.

Pulleyn[n] wholly to you; only rather desiring you not to ask more of him than he is very willing to allow. For I would have no other benefit to myself than some copies for you to dispose of, and a few to present to my friends. I am not only willing that Mr. More peruse the book, but shall be very thankful to him for any care he shall bestow upon it, leaving it wholly to you and him to alter what you shall think fit. The Greek letters, I suppose, may be easily changed into the small letters of the Latin alphabet corresponding; but then the text must be also suitably changed. If it could be, I should think it best that the schemes were placed with the text in the book. I leave all to your ordering and pleasure.



What you ask concerning my papers is easily resolved; viz. If the ordinates KZ be equal to the secants CF, the line ZZZ will be an hyperbola, whose asymptotes [are] CK, [o]CY ; because CK : CA : : $\left\{\begin{matrix} CM : CF \\ CA : KZ \end{matrix}\right\}$, wherefore every where $CK \times KZ = CA^2$; which is the most notorious property of an hyperbola.

[n] Barrow's Lectiones Opticæ were published in 1669, and it is said in their titlepage that præstant venales apud Johannem Dunmore et Octavianum Pulleyn Juniorem. The writers in the Gen. Dict. and the Biographie Universelle, are clearly ignorant of this edition of 1669, as also of that of the Lectiones Geometricæ of 1670 (see note q, p. 72). From the different accounts, which they give of the titlepages, there must have been three editions, namely, those alluded to in these notes, that of 1672, (Gen. Dict. vol. II. p. 702.) and that of 1674.

[o] CZ in MS.

The alteration of ZZ into VV came not, I dare say, from my hand, but from somebody that misapprehended the business; and the space AZZZK is equal to the sum of (or space made by) the tangents AF applied perpendicularly to a straight line equal to the arch AM, &c. as, to my seeming, in that paper was very clearly demonstrated.

For the other questions I must desire some respite; they requiring more consideration than I can at present afford, my mind being indeed unhooked from these things and employed upon other meditations. So I rest

Your most affectionate friend
and obliged servant,
Is. Barrow.

Trin. Coll. March 13, 1668-9.

I desire you would make this agreement with Mr. Pulleyn, that the printing may be speedily dispatched, within as small a time as may be.

CLIII.

BARROW TO COLLINS.

Dear Sir,

I have received all yours. The first, if it had been delivered to me in time, might have prevented you some trouble, for had I known M. Huygens had been printing his Optics, I should hardly have sent my book. He is one that hath had considerations a long time upon that subject, and is used to be very exact in what he does, and hath joined much experience with his speculations. What I have done is only what, in a small time, my thoughts did suggest, and I never had opportunity of any experience. So that I have great reason to believe what he hath done, with so much advantage in all respects, will be much more perfect; yet seeing perhaps there may be in mine some things which have not occurred to him, or which he did not intend to consider, you may, if you think good, proceed in ordering the impression, the manner of which I wholly refer to your discretion. The stationer's offer is more than I expected, and doth abundantly satisfy me. Expedition will be to his advantage. The schemes being altered in quantity (retaining due proportion) will not be any prejudice. The three last lectures, if, when the book is broken up for the printers, they should be sent me, I should transcribe them more fairly; and I have not, indeed, yet read them in the schools, so that they would ease me of making new ones. I have several new (I suppose) geometrical theorems of a general importance, which perhaps I may put together and add as an appendix, having digested them into lectures, &c. I intended to send you some of them, but my business hath hindered me, which (besides pupils and other ordinary employments) hath been imposed upon me by the college; 'tis to make Theological Discourses [p] (as our statutes order) upon the chief points of [the] Catechism, (the Creed, Decalogue, Lord's Prayer, Sacraments, &c.) which out of term so takes up my thoughts that I cannot easily apply them to any other matter. For I have that imperfection, as not to be able to draw my thoughts easily from one thing to another. This hath made me so backward in correspondence upon what you propounded, together with presumption in your forbearance and readiness to excuse me. I thank

[p] These were printed after his death, and will be found in the sixth volume of the collection of his Theological Works, published at Oxford in 1830.

you for M. Slusius his books, but wish you had received the price for them. I am so deep in your debt upon all accounts, that I am even ashamed to think of it, and in no hope to get out of it. With my best wishes I rest

Your most affectionate and obliged
friend and servant,
I. BARROW.

Easter Eve, 1669.

Mr. Jonas Moore was the other day here with me, and will (he said) be here again.

Part of this letter is printed in the Gen. Dict. vol. II. p. 702.

CLIV.

BARROW TO COLLINS.

Dear Sir,

I was aware of your objection about your spiral. What you say is true, but not inconsistent with what I say; for I do not refer to the vulgar logarithms, but to such as are accommodated to the construction I there propound, which increase as their correspondent proportionals decrease (such as my Lord Napier first pitched upon), but you respect the common logarithms. However, to avoid mistake, you may insert these words:

Vel retro, (prout vulgares logarithmi procedunt,) si DI sit numerus in serie geometrica exorsa a DO, et desinente in D[B] ac 0 sit logarithmus ipsius DO, et arcus LK ipsius DB, erit arcus LZ logarithmus ipsius DI [q].

What you speak concerning Dr. Wallis his remarks

[q] See Barrow's Lectiones Geometricæ (1670), p. 124.

on the said spiral I knew well, and therefore omitted to say more about it; you may therefore, if you please, after the problem, write down these words:

— Hujusce spiralis naturam ac dimensionem (ut et spatii BDA dimensionem) luculente prosecutus est præclarissimus D. Wallisius: quapropter de illa plura reticeo [r].

The inclosed paper please to insert among those which I last sent unto you, putting numbers to the proble[ms] in sequel to the former.

I have, to satisfy your desire, scribbled over and se[nt] unto you what concerns the curves for equations, which, if you please, you may a[dd] to the rest; but if they do not please, you must undergo the blame. I am sorry you are so much out of purse; but I hope you will provide for your indemn[ification]. I shall rather undergo the loss, than you shall suffer thereby. I have had twenty sup[er]numerary copies of the Optics; the price of them, if you please, I shall send unto you: I pray acquaint me what it is. I pray rather put paper upon my acco[unt] for what remains of the impression, rather than there should be more stops. [I] will, if you please, send you five pounds for that purpose. I shall in time send [you] a Bible, with some little prologue to the reader. I pray now quicke[n the] printer; he might now, if he pleases, soon dispatch: therein you will ad[d t]o the innumerable obligations of

Your most affectionate friend
and obliged servant,

ISAAC BARROW.

Trin. Coll. March 29, 1670.

[r] Ibid. p. 124.

CLV.

BARROW TO COLLINS.

Dear Sir,

I received your last, and thereby perceive that your honest printer is yet somewhat slow in performance. I request you to blot out those four verses inscribed Juventuti Academicæ; for a friend, whose advice I asked, hath persuaded me that they are not proper. For the 13th Lecture, being I took the pains to exscribe it, and prepare it, (as well as ever I should do it, as I think,) I had rather it should pass, but am content you should do therein as you please. If you do not like these words in the epistle[s] (in which I meant to jest with you, no more) concerning it, I pray expunge them, and substitute these:

Ultimam apposui, materia quidem a reliquis abjunctam, at scopo non ita dissitam, nonnullis eam haud inutilem arbitratus.

Concerning the character, which you speak of, of my books, I shall esteem myself obliged to you, if you will effect that there be nothing said of them in the Philosophical Reports, beyond a short and simple account of their subject. I pray let there be nothing in commendation or discommendation of them; but let them take their fortune or fate, pro captu lectoris. Any thing more will cause me displeasure, and will not do them or me any good.

[s] The following seems to be the passage which is here alluded to in the Epistola ad Lectorem prefixed to the Lectiones Geometricæ. Ultimam amicus (vir sane cum primis probus, ast in hujusmodi negotiis flagitator improbus) extorsit, aut certe, pro jure quod merito obtinet suo, exegit.

I have no more, but that I am your most affectionate friend and obliged servant,

ISAAC BARROW.

Trin. Coll. 23 April, 1670.

This letter is printed in the Gen. Dict. vol. II. p. 703.

CLVI.

BARROW TO COLLINS.

Dear Sir,

Since my being in London I have been at Audley Inn (End?), waiting there upon the Queen's family, for one of my friends. Being thence returned, I have bethought myself of my promise to you concerning my Perspective Lectures; the which I now send you, although I see part of them written so ill, and so confusedly, that I fear you will hardly be able to make any thing of them. I have also with it sent you my Apollonius, which yet is little worth your looking in, having in it nothing considerable, but its brevity. It hath lied by me without looking on for many years. If I had any better things I should more gladly impart them in return for those many favours, which have rendered me

Your most obliged and affectionate
friend and servant,

ISAAC BARROW.

Trin. Coll. Oct. 11, 1670.

I do need ten of the Geometrical Lectures, to give to my friends. Please to favour me in procuring them, and to send me the price of them, and of the twenty Optic Lectures for which I am indebted, that I may discharge myself of the debt. I hope, as they promised, they will be favourable in the rate.

CORRESPONDENCE OF FLAMSTEED.

CLVII.

FLAMSTEED TO LORD BROUNKER, PRESIDENT, AND THE OTHER MEMBERS OF THE ROYAL SOCIETY[t].

Nov. 24, 1669.

Amongst those many illustrious, noble, and generous wits, who have had the honour to subscribe themselves of the Royal Society, I find some astronomers numbered, whom our age accounts its glory, our arts their honour and supporters; which induces me to believe, that among those ingenuous arts, which have thought themselves under their patronage, and hope an improvement from their industry, the heavenly sciences are not to be accounted either as least or last. Their excellent history shews that they have not neglected the heavens, and their endeavours for the improvement of optic glasses, and encouragement of such as labour in them, do more than obscurely demonstrate those high respects they have for the sublimest of human sciences and arts—Astronomy. These, with some slighter considerations, induced me to apply myself, with these not slubbered supplications, to your Honour, whom fame reports to be inflamed with these

[t] It may be doubted whether this can properly be considered as a part of epistolary correspondence, like the rest of the present collection; but it was thought that a little latitude might be indulged in this instance to the first effort of this great astronomer to obtain his share of public notice: especially as the paper was only partially printed in the Philosophical Transactions.

celestial fires, to burn with no ordinary love of science, and to bear no mean place in that illustrious body, of which you have stood a primitive member. You know how much it may conduce to the limation of astronomy, and the correction of our canons, to have the celestial phænomena accurately observed; and how much it concerns the observer to have notice of what appearances the heavens exhibit convenient for his observation. I have endeavoured, in these following pages, to accommodate him with the calculations of such of the more notable phænomena of the year 1670, as will be conspicuous in the English horizon, if the heavens be clear; and shall, God willing, perform the like for the future years, if I may but be encouraged by the acceptation of these.

I was excited to this task by perusing the Mercurius *sub* Sole Visus of the excellent Hevelius, who hath obliged astronomy by communicating his accurate observations of the moon's transits by and over the planet Saturn, and her occultations of Spica Virginis and the Clara in fronte Scorpii [u]. I saw nothing to hinder us from performing as much, (since we have, ni fama fallax, as, if not more, accurate instruments,) if we had by the like spirit, industry, and notice when the appearances would present themselves. Though I was not possessed of fitting instruments, my best perspective being but of two feet, and my quadrant, which yet might serve sufficiently well, of the same length radius, yet hoping that I might do somewhat worth my labour, I revolved Mr. Wing's Ephemerides for the year, to find what stars she might cover, and how often in its revolution. Those occultations, which I collected might be conspicuous, I recalculated from the

[u] These are the observations published by Hevelius, in his Mercurius in Sole Visus, p. 16–20.

exactest tables in being, the Caroline, and these you have presented in these pages at large. The other[s], which could not appear, I have only noted, but bestowed no more accurate calculations upon them.

These calculi when I first framed, I fitted to the meridian of my habitation, intending them only for my own private use; but upon deeper considerations, finding how much the observations, if rightly and accurately performed, might conduce to the better stating the dimensions of the moon's orbit, and solving the irregularities of several phænomena, I resolved to communicate them to the ingenuous, and to desire their utmost care in the observing these appearances, which I had not inaccurately calculated. To this purpose I composed them in an ephemeris, which lying too long in the hands of a friend, was refused the press, but not returned into my hands till the middle of this instant November, when communicating them to an astronomical friend, he urged me yet to print the predictions of these appearances, which the world would, scarce any other ways, have notice of. I could not refuse to commit them to the considerations of the ingenuous, none of whom, if they have any sparks of the celestial fire unextinguished in them, can refuse the heavens their eyes, and the art their endeavours for the observing these appearances, which will conduce abundantly to the correction of our numbers, if dexterously applied, as I shall make appear when I lay down the calculations; nor could I think these worthy the public, which had been once refused it. At last, considering your care and respects for all ingenuous arts and sciences, and your engagements for the improvement of them, and likewise that several astronomers were members of that illustrious body with you, who were not only possessed of curious instruments fit for such observations, which I wanted, but also of ingenuities apt to improve them, I resolved to take such a course as might neither expose them, nor deny them the public, without its approbation; and therefore humbly prostrate them to your perusal, desiring that, if they seem worthy, you will be pleased to impart them, either by the press or otherwise, to such knowing and industrious persons, either of the Royal Society, or others, as will be accurate in their observations, and not less willing to communicate them: if less, one dash of your pen may silence, or your private desire smother these papers, and at once correct the author for daring to interrupt the quiet course of your other studies, and aspiring so high with these (if you esteem them no other) astronomical trifles.

I hope you will not account me culpable for having adapted the calculations to the meridian of a place no more famous than Derby. You have the occasion before; to which I may add, that, though London be the seat of the wits, yet the country is the seminary; that the meridian passing over Derby is nearer the middle of England than that of London; and that its latitude bisects it nearer than any yet stated. So that this town, which is seated in umbilico quasi regni, must needs be the most convenient place that can be elected whereon to fix our calculations: for the distance of any place, within the kingdom of England, from it will not much vary the manner of the appearance in any of the phænomena, except the eclipse of the sun. For in the occultations the stars will appear to incede nearly under the same angle and spots of the moon; nor will the times of the phases, or the moræ sub luna, differ much in any place of the kingdom, nor at all, if a due consideration be had of the differences of meridians and the laws of parallaxes.

I desire your pardon if I seem to have exceeded in the Epistle. I come now to the phænomena you expect. Mr. Wing's Ephemerides, from which our vulgar calendars and almanacs are derived, will tell us that we shall have but one eclipse, and that of the moon, conspicuous this year; but with the peace of that industrious deceased astronomer, and his sectators, I dare affirm that a part of one of the sun will, if the air be serene, be likewise conspicuous, which I have therefore calculated from the Caroline Tables to this meridian of Derby, whose longitude from London I have used in this eclipse, 6 minutes to the west, and latitude 52°, 57′. By which Tables on Saturday, April the ninth, at the time of sun-set supposed at 7h. 3′. P.M.

The letter then gives the calculated particulars of the eclipse, as they are printed in the Philosophical Transactions, vol. IV. p. 1103, 4. There is likewise a scheme drawn of this and the other eclipse of the sun: but all these particulars, and others of the same kind, are now no longer of such interest as to require insertion in the present publication.

Let me therefore invite the ingenuous to the contemplation of this noble appearance; for the observing of which I would prescribe them to cast the species of the sun through a telescope of at least two feet, free from spots and colours, on an extended paper placed at least six feet behind the eye-glass, or so far as that his species may appear at least six inches over, whose periphery ought to be divided into 360 degrees, for the better observing the inclination of the cusps of each phasis, and his diameter into digits and their parts by concentric circles, for measuring the quantities of the observed parts [1]. For the measuring of time it is requisite that the observer be possessed of good quadrants, clocks or pendulums, and that in all things, as near as he can, he follow the accurate method of the excellent Hevelius, whose praiseworthy curiosity in all his observations ought to be the emulation of all those who aspire to the name of artists, or the repute of astronomers.

We have this year a visible eclipse of the moon on Sunday night the 18th of September, of which our Ephemerides may give some short notice. I shall give you larger, with the times of each phasis, in a method not vulgar, from the Caroline Tables, and also fuller heads of the Wingian calculus from his Ephemeris.

Flamsteed here inserts all the particulars of the eclipse, as he had calculated them from Wing and Streete; but it was thought useless to print his numbers, although the whole of this part of his letter, on the lunar eclipse, has been omitted in the Transactions.

The ingenuous observer, if furnished with good and convenient instruments, ought carefully and accurately the same night, both before and after the eclipse, to observe the moon's apparent diameter, either by trajecting her species through the same telescope, in which he usually receives the sun, and so by comparing hers with his visible diameter, or by any other means, which may seem equally or more exact unto him. In the eclipse he ought to observe the beginning both of the spurious and the true shades, and their evasions from the moon's superficies, with a large and clear perspective: as also, during her transit through the shades, over what spots the arches both of the umbra

[1] Inter observandum diametro lunari sub sole depictæ ratio aliqua habenda est; an scilicet excedit, an minor est, et quanto, diametro solari. FLAMSTEED.

and penumbra pass, in such several phases as he shall think most convenient for his contemplation. It is always required both in this and all other observations that the time be measured with exact and rectified Automas. I once intended to have shewn over what spots of the moon the shadow would have passed in the phasis of each digit; but it would have been too large a labour for these sheets, and therefore I have superseded. To the ingenuous the annexed scheme of this eclipse will give sufficient light for the understanding the manner of the observation. It will be highly worth the labour of the ingenuous to bestow their utmost pains and care to acquire the exact measures of the moon's and umbra's diameter, to which I would with all seriousness excite them. For this defect falling exactly on the moon's perigæon, we may, by comparing it with observations made on her apogæon, be enabled hereby to define more accurately, not only of the section of the eccentricity by the Caroline author, but also of the proportions of her orb and latitude, in which the constitutions of astronomers, for want of good telescope observations, are found to be various, imperfect, and seldom, but by chance, responding [to] the heavens.

Here let me stick; and now a word, with humble submission, to our virtuosi. The famous Hevelius, and, with him, our classical astronomers, think it the best method of measuring the digits in a lunar defect, to observe over what spots in the moon's surface the arch of the perfect umbra passes, and likewise what parts stand in straight lines or perpendicular to the cusps. But this cannot be observed without a large and clear telescope and an exact selenography, in which the librations of the moon may be perfectly stated and limb described. For telescopes, if we may credit the modest boasts of our mechanical artists, the world affords no better than the English possess: which yet the thrice ingenuous Mr. Hooke persuades us may be promoted to a greater perfection. I cannot but acknowledge that this acute author hath done his country singular service by his Micrographia, and describing the engine for grinding glasses[y]; it is only desired that he had acquainted us what sands and powders to use for the wearing and polishing of them. He affirms to know several secrets for the meliorating and improving of optics, of which yet we have had no treatise that I know of, published either by Englishmen, or in the English tongue. He may do well to accommodate his country, which expects it from him, with something of them and the dioptrics in her vernacular tongue, and to afford her what he professes to know, and yet conceals, of such optical inventions. Why burns this lamp in secret?

> Quo didicisse —— nisi quod semel intus
> Innatum est, rupto jecore exierit? Pers. Sat.

And,

> Quid te scire prodest, nisi te scire hoc sciat alter?

The Selenography of the excellent Hevelius hath for its neatness attracted the eyes and applause of the ingenuous; which yet not only Ricciolus (whose description and delineation of the moon's superficies, though badly cut, I find to be more accurate) finds fault with, but myself have with no large glass observed it to present the moon's face amiss. I have formerly understood from the writings of Dr. Ward, that some of [the] virtuosi have laboured in this task, and if I be not deceived, the history of the illustrious Royal Society mentions it to be the case of some of her members. Truly this task is worthy a generous spirit, a noble ingenuity, and might well merit the

y Phil. Trans. Vol. I. p. 63.

recommendations of that honourable fellowship to some person indued with a celestial wit and a graphical hand not unemulous of it. I confess this labour would require an indefatigable industry, some watchful nights, careful days, curious calculations, a Lynceus' eyes, Apelles' hands, and Kepler's ingenuity, to delineate exactly the moon's superficies in her various phases and to solve her intricate librations; but if esteem and honour paid to such works, such truly Herculean labours, and champions of the celestial sciences, would richly recompense the pains and diligence of the undertaker, (and if we may believe the eyes of one who hath seen it through his celestial tubes, there are yet abundant laurels to be won in the Selenian games,) up! generous English spirits! run and strive to obtain those prizes, which the excluded world endeavours surreptitiously to deprive you of. Shall we, who have supplied ourselves in all other knowledge, yet in Dioptrics and the Selenography borrow from them their faulty systems and delineations? As if our clime afforded not as good wits, curious instruments, fair encouragements, and generous rewards for invention, arts, and knowledge, as foreign countries; no, those (I may speak it to our shame) we have we exceed them in. What then hath cast us behind them? not our want of wits, but loathe of pains. What hath made them so far outstrip us? not their acuteness, but industry. Hear me, then, you illustrious stars, whose united rays have framed that constellation, whose light hath caused the admiration of the ingenuous world; from whose milder aspects arts, knowledge, sciences, artists (all which have thrust themselves under your patronage) hope benigner influences will distil upon them. If you please to assist in upholding these sciences, if you will thrust your shoulders under to support them, whose weight would overbear a single bearer, they will stand up adorned trophies and colosses to your honour. But if you yield and derelinquish them, they'll fall at last besmeared with dust amongst you, and they will be your foil and shame from other hands, which, had they stood by yours, had been your crown and honour.

Thus far, I fear beyond your patience, this heat for the concerns of science hath caused me to excur. I return to beg your pardon, or buy it, if you please, with the present of some novel phenomena, of which, except from me, you are like to have but few or slight advertisements. You have seen the moon passive under the sun, in the shades of the earth: I now exhibit her active and triumphing over the lesser lights.

Those stars which have not above 6° 20′ latitude from the ecliptic, may all of them some time or other be covered by the moon in her passing through the zodiac, of which sort I have numbered, from the Tychonic catalogues, the stars so lying, as you see in the annexed table; and I find in the zodiac 189, with 6 of the Pleiades, 12 in the northern and 6[1] in the southern constellations, placed out of the ecliptic. So that the total numbered of those that are found in the Tychonic catalogue, which the moon may cover in her course, is 212.

		Brought forward	140	
In Ariete	12	In Capricorno	20	
Tauro	21	Aquario	16	
Geminis	21	Piscibus	13	
Cancro	12	For. Aurigæ	5	bor.
Leone	22	Ophiucho	7	bor.
Virgine	18	Ceto	3	aust.
Libra	12	Orione	2	aust.
Scorpio	8	Pleiad.	6	bor.
Sagittario	14			
	140	toto	212	

[1] This should be 5, or the sum will be 213; see also the table.

Of these she frequently absconds one or other, yet no notice [is] taken of it by the Ephemerides of our astrologers. Indeed it would be too large a labour for one man to undertake the calculation of all the occultations visible in the year, and perhaps not altogether needful; for the stars of the fifth and sixth magnitude are more numerous, not so accurately (as it is feared) rectified, and the moon approaching them within 4 or 5 degrees absconds them to the naked eye; when, if the observation be undertaken with a telescope, other smaller stars may be mistaken for those the calculus intends, and so the observer not only deluded, but the art detrimented by his observation. The moon will several times, this year, cover two noted fixed stars. That I may pleasure the ingenuous, and excite the industrious artist to the like endeavours, I shall give the calculations of the occultations and transits from the Caroline Tables.

Flamsteed then gives the account of the occultations of η Virginis on the 2d of January and 25th of February, of Antares on the 3d of March and 23d of May, and the near approach of the Moon to η Virginis on the 25th of March; with diagrams to illustrate the appearances. All which (excepting the diagrams) may be seen in the Phil. Trans. Vol. IV. p. 1106—1112. He then enumerates nine other occultations, and he says that, "not being visible to us, I have only given you some short notes of them from the Ephemerides of Mr. Vincent Wing."

The moon covers no other of the fixed stars but what are less than of the fourth magnitude, that I know of, this year. As for those, which will be observable, I have given you the heads of my calculations in each appearance. I once thought to have delivered them more at large with an account of my method, which, as it is not usual, so is it more rigid and accurate than ordinary. [illegible] too much [illegible] have [illegible] in [illegible] containing a [illegible] views [illegible] especially the [illegible] Mercury [illegible] some [illegible] at least I [illegible] the [illegible] sub [illegible] corrections. [illegible] the [illegible] whom [illegible] No—my [illegible] I have found [illegible] observations [illegible] with Tables [illegible] selves are [illegible] faulty, to [illegible] more accurate Caroline Tables [illegible] the planet Mercury [illegible] he hath [illegible] more rigid and [illegible] by numbers, [illegible] to the rough-hewn [illegible] call them, than calculations. [illegible] the liberty [illegible] prostrate to the most [illegible] whom, as the excellent Hevelius [illegible] always shall submit my [illegible] and endeavours.

I have further to advert that the moon's apogeon being this year in the midst of Virgo, and her ascending node of Aries, her greatest librations should happen in the middle of Gemini and Sagittarius, and her greatest latitudes in Cancer and Capricornus. And therefore let me desire those, who are possessed of good quadrants, to observe the moon's meridional altitudes in Cancer, and, if they have friends in the southern countries, and instruments, to get them taken with all possible accuracy in Capricornus, that from thence we may be the better informed how to state the variations of her greatest latitudes, in which astronomy is yet something dubious, and the astronomers dissent. Let me also request the ingenuous to note the phases of the moon in Gemini and Sagittarius, and to draw schemes of each appearance by the help of good telescopes for the better stating the irregularities of her libration, and reducing that anomalous motion to the ties of numbers.

I need not urge further (after what I have said before) how much we need a Selenography, nor how great an honour it would be to our country to have a more exact one framed and delineated by a virtuoso of her own. We have excellent glasses and many able artists, who have chosen to hug themselves in their recluded knowledge, whom the honourable Royal Society might do well to stir up to such endeavours, and force to live more public. Our exactest Ephemerides (Mr. Wing's) are within one year expired, those of Argolus respond not [to] the heavens; it were to be wished that some laborious person would undertake the composing of new ones for future years. My daily distempers and affairs afford me not time for this task, nor have I the confidence to rely on any tables till my own, whose foundations are but laying, can be perfected, nor adventure on such a task with so small a talent of health and time. I esteem Mr. Streete's numbers the exactest of any extant; and, in my opinion, he is obliged (that he may not come short of his deceased antagonist) to present us with Ephemerides from his tables, than which nothing can be more acceptable to the ingenuous. I earnestly therefore entreat and conjure him not to neglect this opportunity of endearing himself to them, and obliging his country. He will not want the assistance of astronomers, and if I understand that I may any wise be useful to him, he shall command my best endeavours. He affirms he can proceed to a greater exactness in the moon. I am certain his Tables had need of it; for on the eclipse of October 25, 1668, they erred 16 minutes of time, and the Philolaic more. By this means he may let the world see he can make good his assertion, and yet (if he think convenient so to reserve himself) conceal the numbers, whence they are derived. Nor needs the moon alone this limation. I have observed Saturn near the stars in the tail of Capricornus erring very much from his Tables, but more from Mr. Wing's Ephemerides. I had intended to have delivered here my observations of Saturn and Mars, with some of Hevelius, but fearing to be too prolix, reserving them till some other opportunity I supersede.

I may not omit to certify that, whilst I am writing, on November 30th in the evening, I discovered the new star in pectore Ceti (on which Hevelius hath a peculiar tract) as big as that in ore Ceti, or in nodo lini Piscium, but of a more languid and quiet light. Our astronomers may do well to give some heed to it, and to observe when it shall extinguish again, for it is no common appearance.

I have done, and now submit all to the rod of your censure: desiring your pardon for my bold application unto you and my frequent apostrophes in it. Excuse, I pray you, this juvenile heat for the concerns of science, and want of better language from one, who, from the sixteenth year of his age to this instant, hath only served one bare apprenticeship in these arts, under all other discouragements, except his better genius. I crave the liberty to conceal my name, not to suppress it. I have composed the letters of it written in Latin in this sentence—

In Mathesi a sole fundes[z].

I had many material things to add, but they would have swelled my letter beyond its prescribed limits. If I may understand that you accept of these, or think them worth your notice, you shall ere long hear more from

your, and the illustrious Royal Society's,
most devoted
and humble servant,

J. F.

The former part of this letter, as far as p. 86, l. 28, is published in the Philosophical Transactions (No. 55, Jan. 17, 1670); many verbal alterations and corrections are there introduced, and at the end there is the following notice : " So far this diligent author for this time. The other particulars contained in his papers may perhaps be published hereafter. We were to reserve a part of this tract for some other communications." The publication however was not resumed.

It may be added, that notwithstanding the concealment of the anagram, the paper is said (p. 1099) to be " written by the learned and industrious Mr. John Flamsteed."

[z] An anagram of Johannes Flamsteedius.

lation. I suppose it cannot be hid to those, who are versed in trigonometry: it is the accuratest I could choose, and the numbers were twice, some thrice, repeated, for more certainty. If any desire to be more fully satisfied, I hope I shall answer their expectations in an Epistle to the excellent Hevelius, containing a commentary on, and a correction of, several particulars in his Mercurius sub sole visus, and commentary on Mr. Horrox his Venus sub sole visa, which I have lying by me, written in Latin about half a year since; but my style not pleasing some of my more judicious friends, I have since then given myself to the perusal of the classic old Roman and some modern authors, that so I may bestow a better language on it before I expose it to the view of such severities as I shall be sure to meet with. I intend to present it, with an Epistle De æquatione temporis, to the view of the Royal Society, before I commit it to the public; but by reason of my frequent distempers, my parents' affairs, and the coldness of the season, I shall be forced to protract the time I had set myself for the reperusal of my papers, which yet I hope to present you with completed within these six months.

I have solar Tables by me composed above two years since, to wit before I was twenty-one years of age, which I intend to expose with my Epistle De æquatione temporis directed to Mr. Streete, whom on occasion you may let know that I shall write to him before the term be over by my kinsman, an attorney; but I shall be forced to trouble you with the letter, because I know not the place of his habitation. I desire to transact things fairly with him, as I have done with his deceased antagonist Mr. Wing, with whom I had a fair correspondence, and though we differed de parallaxi, et æquationibus systematis solaris,

[illegible]

[illegible]

[illegible] Sir, [illegible] and may [illegible] [illegible] describes [illegible] intend to [illegible] poison [illegible] better [illegible] that would [illegible]

[illegible] with [illegible]

concerning the usual method or necessaries for grinding and polishing them. If you know any thing, which you may freely impart, I should be much obliged by a communication.

I thank you for the catalogue of mathematical pieces. I desire you inform me of Hecker's Ephemerides, on what tables[b] they are framed, at what year they begin and how long continue, to what meridian they are fitted, where they may be had, and at what rates; and likewise where and at what rates I may have the Urania propitia Mariæ Cunitiæ.

Sir, I have received from you, by the carrier, Mr. Barrow's Optics and some pieces of Mr. Dary's, and your own. I know not how I have merited so much kindness from you, as a favour so much beyond my deserts, as it was far from my expectation; which yet I had received with less reluctancy, if I enjoyed but a conveniency to recompense it. The country is barren of ingenuities, and affords me nothing worthy your acceptation. Sir, I desire you, that if you know any thing wherein I may pleasure you, you would be pleased to let me understand it. Your desires shall be commands, to which I will readily obey. I desire you, therefore, use freely and command boldly the best endeavours of one, who is obliged to serve you and desires to do so, whilst he is

yours to command,

JOHN FLAMSTEED, Derb.

This letter is printed in the Gen. Dict. vol. V. p. 250.

[b] "Rudolphine Tables, Kepler's form for Uraniburg where Tycho lived." "From 1666 to 1680." "Tabulæ Solares."—COLLINS.

CLIX.

FLAMSTEED TO COLLINS.

July 16, 1670.

Mr. Collins,

This is to let you understand that I have received from you Fabri's Optics and Barrow's Geometria, for both which I am your debtor to account: I thank you for your care. As for Fabri, I have read a part of him, and am very much pleased with his endeavours. He inclines much to Mr. Hugenius his system of Saturn, rather than of Hevelius. I have not seen Hugenius, therefore pray when you write next let me know whether his Systema Saturnium be to be had in London, and at what rates. I have received another letter from you since, with an account of Gassendus his observation and an information of Mengolus his book, for which I am obliged to you. Pray, when it is known, let me understand his opinion of the quantity of the sun's horizontal parallaxes, and the refractions, and what refraction he allows the sun in the altitude of 20gr. I suspect, and have good reason for it, that in inland places it is nothing, or not sensible, in that altitude, for by some of Mr. Wright's observations the sun at noon had no refraction the 15th of December in altit. 15gr. Pray let me know when any ephemerides come forth, for I am yet to furnish myself for future years. I wrote to you to inquire for a treatise of clockwork for me, which I desire you would do, when you come in any likely place. Mr. Jonas Moore shewed us one in his study in 4to. I have forgotten the rate of it; but I would be willing to bestow an angel, or more, to get the like or any other good piece, for I want a good clock, and having the conveniences and necessaries for making one, I had rather be at pains to make one for myself, than buy one at hazard whether it be good or bad; and I had rather buy other men's experience in their works than pay for my own in making experiments. I thank you for your advice of writing to Mr. Townley. I am at present calculating the moon's appulses for the next year, which, when I have finished, I will send him a specimen of, to make a way for our own future correspondence. I desire you not any ways to incommodate your own affairs, by taking care of mine, but do them for me at your best leisure and conveniency. As for my glasses, I desire you, when you write any thing about them, direct your letter to Mr. William Litchford, ironmonger in Derby, so will they come to my hands safe, for letters come often to me on my father's business, which he commonly opens in my absence. And, therefore, I desire you, when you write any thing of my glasses, superscribe your letter as I have requested, but you may write within as to me, for my friend, who is my sole adjutor, knows your hand and will deliver them unopened.

As for what I have noted in Ricciolus, besides the frequent errors of the schemes you may note these.

Pag. 161. He applies the horizontal parallax of the sun in all altitudes, as if it was the same both in the zenith and the horizon, and so gathers the ecliptic's obliquity 20″ more than his own observations permit.

Pag. 435. He in his second example is guilty of the highest disingenuity, for he first lays down a vicious observation, by which he thrusts the sun's place above 10 minutes further than either Streete's or my Tables afford, and then derives a right ascension almost 2 minutes larger than his observation will allow, as I can easily prove, but that I have neither time nor room at present. If you desire it, I shall make it good hereafter.

Pag. 477. In his Catalogue of the fixed stars he protrudes the right shoulder of Auriga 1° 26′ forwarder than his own observed distances from Capella, the brightest of the Pleiades, and Procyon will permit, that he may make his Catalogue agree with the Tychonic, from which he was so conscious of his own defects, he scarce has dared any where to recede. So that he seems to me rather to affect the fame of having corrected our Tables, than really to have amended what was faulty in any one. I had first notice of this star's varying from the Tychonic canon in Hevelius his Mercurius sub sole, but found it confirmed absolutely by the Ricciolan observations.

In his Appendix, pag. 733, Prob. 3, he teaches how to correct the apparent time of the quadrature, as if the parallax caused the moon to be really cave, when she was apparently semi-illuminated; but this correction is needless, and all that is said in that column, and the two next, absolutely to be rejected, since that the parallax cannot vary the phasis at all, as I can easily prove, and will appear to any one, that duly considers what he there lays down.

Lastly, he is wholly inclined to the Peripatetics against all other philosophy, and on this account favours Claromontius against Tycho, Kepler, and reason itself: and though he protests against it, yet I fear he was partial to Tycho, because a Calvinist, and propense to Claromontius, as one that was a priest of his own order, and of his principles a rigid Peripatetic, and one that had left some persons, his relations, of, and well deserving from, their society.

This in haste—If you desire further information of any thing, let me know it; I shall be glad that I can serve you in any thing, whom my distance from the city forces me to trouble so often. Pray when you buy me a telescope, let me have a good one, though it cost more than I left money with you, and I shall take care to pay you as soon as I can get the money to you, after I receive the letter. This with my cordial thanks for all your favours, from

your friend and servant
to command,
JOHN FLAMSTEED.

I had something more to write, but am forced to defer till another season. My services, when you see them, to Mr. Oldenburg, Mr. Bernard, Mr. Ashmole, but in especial manner to Mr. Jonas Moore, his son, and Mr. Sherburne.

J. F.

Part of this letter is printed in the Gen. Dict. vol. V. p. 251.

CLX.

FLAMSTEED TO COLLINS.

Derby, Sept. 19, 1670.

Mr. Collins,

I have received from you a pair of glasses by the carrier, for your care of providing me which I am much obliged to you. I am sorry that I am forced to trouble you so often, but more that I live so far from you, that I cannot in presence pay you for the courtesies I owe you, nor present you with more than slender, though real, thanks. Sir, I sat up this last night waiting to observe the eclipse of the moon. The heavens were clear till midnight, after which they were, by little and little, covered over with thick whitish clouds, through which, when our clocks struck one, I saw the moon eclipsed almost a digit; but I could not find a convenient star to take its altitude, being on one side prohibited by the buildings, and the heavens all over covered with the white clouds, that none of the bigger stars, or any almost, removed from the meridian, appeared: and the moon was scarce an hour removed from the noonstead, so that it was in vain to think to take hers amongst the clouds. The heavens continued covered with these till morning, and the moon appeared very rarely through the clouds, so that despairing of any good observation, and seeing the heavens thicken, I left off and went to my rest, when I had seen the cusps parallel to our horizon, and the sinister had covered the occidental spot, or palus Mareotidis of Hevelius, but could not note the time. I wrote to Mr. Hooke about the observing this eclipse; pray, if you chance to meet him, inquire whether he observed it or not. If he did, and the heavens smiled on his endeavours, desire him [to] be pleased to communicate his observations to me; for I think the heavens anteverted our calculations some minutes, which, if so, favours an equation I have long since conceived ought to be induced into the lunar system. I could wish he would be pleased in a line or two, at his leisure, to inform me at what distance my glasses may be placed in the tube, for it would save me much trouble and labour; and likewise how I may most conveniently hang it for observations, for I saw none hung when I was at London; and likewise at what distance the brass instrument, which Mr. Moore bestowed upon me, may be placed from the eyeglass. I fear that a single eyeglass will take in but little, and I fear I must have one ground of a lesser sphere, and greater breadth, than the eyeglass, to place betwixt it and the objectglass, to enlarge the quantity of the angle it takes in; but of this when I have made trial. For the rest of the money unbestowed, pray, sir, keep it in your hands; for I question not but I shall want some things or other ere long, and I cannot tell into whose hands to dispose [of it], that will bestow it more to my advantage. I am much and sensibly obliged to yourself, and Mr. Hooke, in procuring me my glasses at so easy a rate, when, had I bought them myself, they would have cost me double and above. Pray when you see worthy Mr. Moore, Mr. Sherburne, or Mr. Oldenburg, present my services to them. I hope now speedily to present you with the phænomena of the next year; for I have as good as finished my calculations.

My services and thanks to you presented,

I rest,

yours, obliged always to serve you,

JOHN FLAMSTEED.

Part of this letter is printed in the Gen. Dict. vol. V. p. 251.

CLXI.

FLAMSTEED TO COLLINS.

Derby, Oct. 1, 1670.

Mr. Collins,

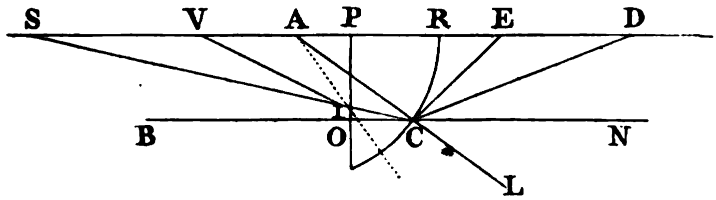
Sir,

Being something in haste when I wrote to you last, I forgot to express what I chiefly intended. You wrote to me, that you would speak to some persons of the Royal Society, that by the order of the President and Council, the Selenography of Hevelius might be bestowed on me; but, sir, I entreat you, if you have not as yet moved it, wholly to desist, for I desire not to be obliged by them to their cost. I am sufficiently indebted to them for the respects and favours they have pleased to confer on me already: if I should admit this further obligation, I know not how I should prove responsible. I know this motion proceeded from your desire to gratify me. I am obliged to you for your good-will, and services at all times, which I should esteem it an happiness if I could gain an occasion to remunerate; but in this business, I entreat you, cease to trouble yourself, for besides that I desire not to be obliged by the Society at their charges, I esteem not the author so much, but that I can willingly be without him, and having occasion to write to Mr. Oldenburg, I question not but to get an addition of time for a further perusal.

Sir, I have sent oft by our booksellers to get Hecker's New Ephemerides, but cannot hear any more of them than I had from your information. Pray, if they, or any other for future years, be to be had in London, be pleased to let me know; or if it may not be too much of trouble to you, send me one of Hecker's, or any other, that you think as good or better, and if I have not money enough left in your hands to pay, I shall speedily lodge more to serve my occasions, and pay my debts I may contract hereafter. I have not yet got a tube to my glasses; I wait till I may hear from you how Mr. Hooke approves of them, and at what distance they are to be placed, for I cannot conveniently make trial, and I had rather wait a while than have my tubes made either too long or short.

Pray, sir, if the lunar eclipse last hath been observed by any in London, if you can procure the observation without trouble, please by your next to communicate it to me. I have now finished my calculations, and am writing them out, but cannot tell what tongue to expose them in, English or Latin. Let me have your opinion; you shall rule me. The appearances are treble the number I calculated last year, and most of them someways truly notable.

I have read a part of Fabri's Optics. I was much pleased with his acuteness and accurateness at first, but coming to his 43d prop. I find it lost, for in his VI. VII. and VIII. sections he demonstrates not what he intends, but by drawing his 90th and 91st schemes too close, deceives and mocks his reader. I have reprotracted his 90th scheme in the margin[c], which



if you have the leisure to read the quoted proposition and section, and compare with his, his faults will clearly appear, and the like in the following sections upon a slight reading will be evident. Vult Fabrius angulos VIS, VSI æquales esse, ipsa quando constructione sua VIS est modo subduplus VSI: propterea quin vult AV, AI æquales esse, quando si protraheretur AI (quod minime necessarium, et cujus me latet omnino usus) facile probari potuit AV majorem

[c] Against the figure there is written, Prop. 43, Sect. 6. Synop. Opticæ Hon. Fabri, pag. 112.

esse AC, nedum AI. Error mihi Fabrii inde prosilire videt[ur], quòd ille minime perpendens secundam refractionem radii NC factam in superficie vitri in I, ita protrahit angulum VIS ac si nulla fuerit vitri crassities, quin plana cum convexa vitri superficie, punctum I scilicet cum C, coinciderit, quod impossibile. If I be not in the right, I desire you, at your leisure, in a line or two better to inform my judgment.

I am,
yours most obliged,
J. FLAMSTEED.

My respects and services to Mr. Moore, Mr. Hooke, Mr. Oldenburg, when you see them.

Part of this letter is printed in the Gen. Dict. vol. V. p. 252.

CLXII.

FLAMSTEED TO COLLINS.

Derby, Dec. 1, 1670.

Mr. Collins,
Sir,

I received your last of Nov. 19, but have not heard any thing from Mr. Hooke of what length I may frame my tube. I intend now to make trial myself, for I would not trouble him further in that business, who, I perceive, is full of employment. I hope you have received my last with my calculations of the last year's appearances; in my introduction to which if I seem to have asserted too boldly against our astronomical tables, I have now received an observation of my cousin Wilson, that will fully confirm my affirmations, which I send you enclosed. I cannot but congratulate you for your happy correspondence with P. Bertet, and I am much pleased to hear of De Mouton's book, De mensura diam. Solis et Lunæ, one of which, when they come to your hands, I desire you send me, and pay yourself out of what you have in your hands. If the piece be well done, it will conduce no little to correction of astronomical lunar tables. When I have received and perused it, I shall, according to your directions, write to Mouton, till then, not having any occasion offered, I suppose you will neither hold it convenient nor necessary.

Sir, you once, I remember, proffered to lend me Mr. Horrox's papers, which are now in your hands. If, sir, you have not procured them yet to be printed, nor any bookseller offers to undertake them, I would accept of your offer gladly, for I have libe[rty] now to peruse any author, and I would gladly try if that deviation, which is now very perceptible in the motion of Saturn, was not in his time sensible. For if I remember aright, one of his books contains the observed distances of the planets from the fixed stars, with other his observations, from which nothing as yet hath been deduced. If you please to commit them into my hands, I shall receive them on what conditions you shall prescribe, and return them when you please. I would entreat the favour of you, likewise, to let me know at what rates you receive the Italian Transactions, in which you say much mathematics are inserted, and whether they may [be] procured at any moderate prices. I should willingly undergo the trouble of learning to read that tongue, if I thought that I might but find in the Italian authors what might recompense my pains.

My coz. Wilson left 10s. with John Marke, in part of payment for an instrument he was to make him, and order to deliver it to one Mr. Bickley, and receive the rest of his money, which they agreed for. My kinsman hath not yet received nor heard of it. Pray, when you see John Marke, desire him to deliver it according to his bargain as soon as he can, for my kinsman wants it. If you have delivered my other papers to Mr. Oldenburg, pray send this after them, which comes herewith. With hearty acknowledgments of your kindness and favours done to me, I rest

yours, most obliged,

JOHN FLAMSTEED.

Pray, with the first opportunity, deliver the enclosed to Mr. Oldenburg. I would have it printed with my other papers, if they may pass the press.

By the scale of minutes in the enclosed scheme all the figures of the next year's appearances were delineated by J. F.

Part of this letter is printed in the Gen. Dict. vol. V. p. 252. None of the enclosures, to which it refers, have been met with.

CLXIII.

FLAMSTEED TO COLLINS.

Derby, Jan. 30, 1670-1.

Worthy friend,

Mr. Collins,

Not having heard from you of a long time, lest that our correspondency should be lost by intermission, or our contracted friendship freeze, I prosecute you with a letter, whose errand is chiefly to inquire of your health, and beget the like return from you, and by the by to inform you that I have lately written to Mr. Moore, from whom I have received an answer full of his natural civility and generosity. By his advice I have likewise wrote to Mr. Townley, from whom I cannot yet expect an answer, by reason my letter went but on Tuesday last, and can scarce yet reach his hands. I desire, sir, to hear whether as yet you have received your books from your French correspondents, and amongst the rest those of De Mouton and J. Ant. Payen, and whether you have so many copies, as that you can sell me one of De Mouton or Payen, if I should desire it. I would know likewise what you hear concerning the Italian Transactions, and of what nature the mathematics are, which are in them inserted. Mr. Halton, I understand, told you of my mind to have the Rudolphine Tables, and you promised to give me an account of them. Pray, sir, in your return to this let me know what you have learned of them.

I have tried my glasses by a more successful way than you gave me notice of, and I find that they will serve a tube of near fifteen feet, though I could not represent the picture of any object, not above twenty yards remote, upon a paper scene distinctly, except the scene was twenty feet from the glass; but, if the object was above two or three hundred yards, or more, removed, it appeared at fifteen feet from the glass distinct: of which, and what is the cause that an object, as the moon, being beheld through a perspective of two glasses, of which that next the eye is concave, should appear direct, and yet, permitted to pass on to a scene placed behind it, should there be painted inverse; as also why through a like perspective, whose eyeglass is convex, the object should appear inverse, but on the scene direct, are not unpleasant problems in the Optics. And though in the tube, whose glasses are both convex, the cause is easily soluble; yet supposing the eye-glass concave, it is not so facile to determine.

I have the Transaction, in which Mr. Oldenburg hath printed that short abstract of my calculations, which give the observers notice of such appearances; but the other requisites of the supputations being wanting, they will be put to a second trouble of calculating, to gain the anomalies, and other parts of the calculation, before they can be able to guess how, or where, any thing in our numbers are to be corrected. I am framing tables of oblique ascensions for the latitudes of London, Dantzic, and Derby, supposing the declination 23° 30′ 0″, to second minutes, the old ones falsely supposing it 23° 31½′; to which I intend to add the angle orient, which will no little assist calculations, and such as are minding to make astronomical predictions, or compare celestial observations with our numbers.

Craving your pardon for this trouble, and returning you thanks always for your many obliging civilities, I remain

your most obliged servitor,

JOHN FLAMSTEED.

Pray deliver the enclosed to Mr. Oldenburg. I suppose you may see him, or send to him at the meeting of the Royal Society on Thursday next.

Part of this letter is printed in the Gen. Dict. vol. V. p. 252.

CLXIV.

FLAMSTEED TO COLLINS.

Derby, March 20, 1670-1.

Mr. Collins,

I have both your letters, and the quadrant for my kinsman, for which I understand that you intend to pay Mr. Marke 15*s*. out of my money in your hands. My kinsman told me that he had ordered him to deliver the quadrant to one Mr. Bickley, if I misremember not, in Cornhill, and to receive the money of him. I desire you to inquire of John Marke why he delivered it not there. I suppose you know him to be honest, else he may be paid twice over. I am content, if he have not pilled of Mr. Bickley, that you should pay him that money, which otherwise, if you be not a weary of, I would gladly had remained in your hands. I know not how to ask you the courtesy, else I would lodge some more money in your hands next term, to pay for such books as you shall procure me at any time. I perceive you have but very little left of the money I left in your hands, else, had Gotigny's Dioptrica been to be had in London without much trouble, I would have desired you to have sent it me. However it may be soon enough hereafter, and perhaps the books may be more common with you. I can easily solve the optical problems I mentioned to you, but have not opportunity to do it now. My next, if you require the solution, shall do it.

I am exceedingly pleased to hear that Mr. Horrox's papers will be printed, and I suppose the Venus sub sole with them, to which, having well perused it, I know not what can be added. The notes of Hevelius

I find generally needless, and those on the sixth chapter, p. 124, absolutely false, (conceding his own parallax,) because he hath taken the angle of the position of Venus, which is made by a line drawn through hers and the sun's centre, instead of the angle of the vertical circle per centr. ♀ with the ecliptic, or the parallactical angle; whence he not only vitiates the truly computed parallaxes of Mr. Horrox, but also, supposing the parallax of ♀ a ☉ in altitude 1′ 57″, he falsely calculates these parallaxes.

	h.	m.	of long. ′	″	of lat. ′	″		par. long. ′	″	par. lat. ′	″
Hor.	3	15	1	20	1	27	which	0	38	1	50
	3	35	1	17	1	28	ought	0	42	1	49
	3	45	1	15	1	29	to be	0	44	1	48

So that what he hath deduced from these his vicious parallaxes ought, if his comment be printed with the original, to be corrected. Of this you may please to inform the Doctor.

You wrote, that Dr. Wallis desired my comment on the Mercurius of Heveli[us] might be printed with Horrox's papers. When I received the letter, I looked up my papers, but find that I shall scarcely have time to transcribe and fit them for the press, partly because my occasions, but more frequently my distempers, withdraw and detain me from my pen-endeavours. For the spring coming on, my blood increases, which, if I should not exercise strongly, I should spit up or receive into my stomach, with great detriment to my health: when I return from exercise, other occasions are ready to detain me. Further, I have received a letter from Mr. Oldenburg, who is not willing that I should advert too plainly on Hevelius, lest he should recede from his correspondency, and detain his observations from us, if he be disgusted. However, if I might but know by what time Mr. Horrox's papers would be ready for the printer, I would endeavour to get my notes ready to succeed them; for I am resolved not to desist if my distemper will be forced to intermit by my usual remedies, which I must question not, God permitting. In the mean time I am preparing cases for my glasses, for which I hope to raise a pole to elevate them on this evening; and then I shall endeavour, when I have leisure, to observe the heavens, that I may someways benefit the science I am enamoured of—Astronomy. No more, but that, with many thanks for your civilities, I rest

your obliged friend and servant,

JOHN FLAMSTEED.

P.S. I have received and returned letters to Mr. Townley, from whom I have an account of the solar eclipse of 1668, October 25, and the occultation of Antares on Maii 23, 1670, of which I desire you procure me Mr. Hooke's observation, if he made any; for I fear Mr. Streete's is wrested a little.

J. F.

This letter is printed in the Gen. Dict. vol. V. p. 252.

The wish expressed by Flamsteed was unfortunately not attended to. The Venus in sole visa was not inserted in the collection of Horrox's posthumous works. Hevelius had published it[d] from a manuscript which was lent him by Huygens, among whose books there appears to be a tract answering the description of it[e]; but Prof. Uylenbroek, with his accustomed care, was kind enough to examine this question for me completely, and could find no traces of the papers having been returned after the publication. This is much

[d] With his Mercurius in sole visus, 1662.

[e] Cat. Libr. Bibl. publ. Lugd. p. 352, near the top of the second column.

to be regretted, since the text, as printed by Hevelius, wants correction, especially in the punctuation. There is a manuscript at the Greenwich Observatory, which belonged to Flamsteed, (though not written by him,) which varies much from the printed text, and may suggest some probable corrections; but it is no further authority. We find in it, among the verses on the Telescope, (Hevelius, p. 114,) the two following lines, immediately after those on the satellites of Jupiter.

Et duplici, nimium cœlesti a fonte remoti
Tristia Saturni solatur lumina, flamma.

Now Horrox died in 1640, and it was not known that Saturn had any satellite till Huygens discovered one in 1655. Cassini found a second in Oct. 1671, and a third in Dec. 1672. It is clear, therefore, that the Greenwich manuscript was not written till more than thirty years after the death of Horrox, and that the transcriber felt himself at liberty to interpolate what he pleased. The 13—17 chapters of the second book are likewise deficient.

The late W. R. Whatton Esq. of Manchester, had made considerable collections for a life of Horrox, which he intended to have prefixed to a new edition of the Venus in sole visa, and he had the whole nearly ready for the press, when his sudden death, in 1835, deprived the world of the fruits of his inquiries. So little indeed is known of Horrox, whom Sir J. Herschell justly calls "the pride and boast of British Astronomy,"[f] that the following particulars seem well worthy of notice. He thus describes his famous observation, and the account is copied from the Greenwich MS. that its variations may be seen from the printed text, (Hevelius, p. 114,) especially as they do not affect the present object. "Die autem 23 "[Nov. 1639] admodum nebulosa, ne sol quidem visus. Se-"quente 24 paulo clariore, observationem institui a solis ex-"ortu ad horam usque nonam: item paulo ante decimam: "ipsoque demum meridie, et hora prima pomeridiana. Aliis "horis ad majora avocabar, quæ utique ob hæc πάρεργα neg-"ligi non decuit. At toto hoc tempore nihil penitus in sole "conspexi, excepta quadam pusilla et communi macula, par-

[f] Treatise of Astronomy, p. 86, note.

"ticulis quasi tribus a solis centro ad sinistram remota: quæ "motu omni sensibili destituta, ostendit se a Veneris levitate "alienam."

"Hora autem 3h. 15m. post meridiem, quo primum tempore "observationem repetere vacavit, discussæ penitus quæ prius "ingruerant nubes, ad oblatam veluti divinitus occasionem "invitarunt volentem. Ubi ecce gratissimum spectaculum, et "tot votorum materiem. Notavi enim maculam novam inso-"litæ magnitudinis, figuræque omnino circularis, supra limbum "solis sinistram jam toto corpore ingressam, &c. &c."

From hence it appears that he watched the sun from its first rising till 9, and then was only able to look again for the expected transit, a little before 10, at noon, at 1, and at 3h. 15m. in the afternoon. Now the sun set at 3h. 50m., and he gives no particulars of the pressing calls, which prevented his more constant attention to this important observation. This however is explained by a note in one of Hearne's memorandum books in the Bodleian (No. 102, p. 62). "Mr. Horrox," he says, "a young man, minister of Hool, a very poor pittance, "within four miles of Preston in Lancashire, was a prodigy "for his skill in Astronomy, and, had he lived, in all proba-"bility he would have proved the greatest man in the whole "world in his profession. He had a strange unaccountable "genius, and he is mentioned with great honour by Hevelius, "upon account of his discovery of Venus in the Sun upon a "Sunday, but being called away to his devotions, and duty at "church, he could not make such observations, as otherwise "he would have done." It is only necessary to add, in confirmation of this statement, that the 24th of Nov. 1639, O. S. was on a Sunday.

CLXV.

FLAMSTEED TO COLLINS.

Mr. Collins, Derby, May 3, 1671.

I have yours of the 27th last past, for your communications in which I heartily thank you. I am glad to hear that Mr. Horrox's papers are gone to the press: I shall, against that time they may be almost finished, have retranscribed my notes on Hevelius his Mercurius sub sole, which, lest I may offend, I shall willingly submit to the castigations and corrections of Dr. Wallis, or any other ingenuous person. My distempers and affairs of late have been so intermutually urgent, that I have performed little. I have by me a sheet or two, which I wrote some five years agone De æquatione temporis astronomica, which I have last summer made Latin, and would gladly it might see the light; it being readiest I intend first to absolve and send it to you, to be disposed of as you shall think most meet. What you write concerning Mr. Horrox's papers being in Mr. Townley's hands I am well pleased to hear of, and I hope I may do you some service in that business, for Mr. Townley corresponds with me very familiarly, and promises me all the services he can do me in my studies. But before this comes to your hands I suppose you may hear of him in London; for he wrote to me some ten days agone, that he would set forward on Monday last for the city, which I suppose he will reach by this week's end. I am to go into Lancashire, and near his house, the latter end of this month, where I intend to call, if he be returned into the country. In the mean time, if you or Mr. Jonas Moore can meet with him, you may do well to urge him in this business; and if I can do any thing when I go into Lancashire, let me have your directions, and you shall not want my endeavours, either for collecting, or methodizing, any papers, that may fall or be intrusted into my hands. I desire you, sir, to let me hear from you concerning these things within this fortnight, for I cannot know precisely what time this month I shall take my journey.

I must, before I end, acknowledge myself much obliged to you, and Mr. Hooke, for procuring me my glasses. They serve well in a tube of 13½ feet, but cast some colours, which will not be easily removed, though I put on a narrow aperture to the object glass. I have frequently seen Jove and his satellites with them, and on Saturday was a sevennight they were all in consequence of him, though not being able to judge their distances from him I have omitted it in my papers of observations: he appears of about two inches diameter, but I cannot discern any fasciæ upon him. The weather has been very foul and rainy with us these last seven days, so that I have not seen the satellites since the 26th of April last, when I saw the satellite A[g] two diameters from Jupiter, B from A four, C from B four or five, and D from Jupiter four Jovial diameters; but my glasses inverting the object, in the heavens the appearances stood the contrary way. I did, likewise, take the sun's diameter by letting his species through the object glass in my tube on a paper scene placed near the place of the removed eyeglass, where I had a perfect appearance of the sun upon the scene of white paper, with a most nitid periphery, though it was in the open air in our garden; but the limb seemed a very little coloured with such colours as appear on the scene, when his species is cast through the glasses of a small good perspective, which very thin colours would vanish if either I straitened the aperture on the object glass, or placed a red paper instead of the white one. That the visible diameter on the scene was the same with the apparent in the heavens I can prove by an optical demonstration, I think of

[g] This refers to a rough drawing which did not seem worth being engraved. A, B, C, are on the right, and D on the left hand of the planet.

mine own, though perhaps not hid to others, and which had put many thoughts into me concerning the motion of the rays of light, which I cannot here express to you, but shall remit unto some other opportunity. In the mean time pray present my services to Mr. Moore: I am reading his Algebra, and find it not very difficult unto me, and intend, when I shall have spare time, to make some progress in geometrical studies. With my respects, and tenders of my best services to yourself, I rest

your's to command,

JOHN FLAMSTEED.

This letter is printed in the Gen. Dict. vol. V. p. 253.

CLXVI.

FLAMSTEED TO COLLINS.

Derby, May 13, 1671.

Mr. Collins—Sir,

Having written to Mr. Oldenburg, I thought convenient to enclose this, to let you know that my cousin Wilson informs me, that he has ordered one Mr. Bickley to pay you forty shillings for me, and to inquire whether you have received it. I have sent Mr. Oldenburg my observation of the moon's appulse to the star in ♍ 9° 53′, whose latitude is 1° 20′ north, of the fourth light, which I saw covered by the dark side of the moon, in something less altitude than the lowest part of the Propontis, when Jupiter was elevated 32° 52′; whence I deduce the hour 9h. 16m. 30s. The precise emersion I saw not, for I was elsewhere then employed; but coming to the tube, I found that the star was the breadth of the Caspian spot, or Palus Mæotis, distant from the moon's limb, and higher than the supreme part of the said spot its whole length; whence turning to my quadrant I took the height of the moon's supreme limb 31° 54′; therefore the star's height was 31° 45′, and the hour 10h. 24m. 36s. I noted the height of Jupiter too, 22° 36′. and find the hour thence 10h. 25m. 0s. I am persuaded that the times are pretty exactly taken, for they shew partly the same duration, all things considered, as the tables, but at least ten, if not fifteen, minutes later. I desire to hear from you whether any of your acquaintance have observed this appearance, of which the calculus is with the rest in the Transactions. If you have any observations, pray favour me so far as to impart them to me. I have given Mr. Oldenburg a calculation of the moon's transit by Jupiter on the 20th of September next at sunrise, which I suppose he will, if you desire, impart to you, for I have not time to transcribe it at present, but you shall command it when you please. I desire to hear from you, and with my respects, and thanks for your continual civilities presented, I rest

your affectionate friend and servant,

JOHN FLAMSTEED.

This letter is printed in the Gen. Dict. vol. V. p. 253.

CLXVII.

FLAMSTEED TO COLLINS.

Derby, July 10, 1671.

Mr. Collins—Sir,

I have yours of the 27th last past, which put me in hopes to have seen Mr. Townley at Derby, till on

Friday last I received a letter from him, dated Manchester, July the 3d, by which he gave me to understand that he was so far forward of his journey homewards. Not having dispatched my concerns in Lancashire I am to return thither ere long, and I suppose I may set forward next Monday, and I hope I shall not fail to meet with Mr. Townley this journey. I hope, ere this come to your hands, you will have received my papers from my kinsman Mr. Wilson. I have finished my transcript of my solar tables, which I wait but for an opportunity to transmit safely unto you. In the mean-time I desire to be informed by you in what volume Mr. Horrox's works are printed, and how far the press hath proceeded in them.

Last week here I saw a boy, one Smedly, about fourteen years old, who has two perfect rows of teeth on the upper side [of] his mouth round, save that the third tooth on each side from the fore teeth is lately come forth, and has a third row cut, coming forth more inwards towards the roof of his mouth. Of these two inward extraordinary rows, the two foremost teeth of the second row came first not a year agone, the rest all within this half year or less. I thought fit to inform you of this accident, because the like happens not every day, and this may serve either to confirm the like relations, of which I think some are not wanting, or be grateful of itself for its novelty. The boy is of a good complexion and healthful, only in his childhood he was lamed with a prick in his knee. He is apprentice with a glover in town; so that if you desire further information, I can give it you at any time.

I have not of late, by reason of my occasions and the foulness of the weather, made any observations, but shall endeavour again, when leisure and fair weather shall happen. In the interim I must always acknowledge myself

your affectionate and obliged servant,

JOHN FLAMSTEED.

I spoke and wrote to my kinsman about a watch clock. I must crave your assistance and information in that business.

This letter, with the exception of the beginning, is printed in the Gen. Dict. vol. V. p. 253.

CLXVIII.

FLAMSTEED TO COLLINS.

Derby, August 1, 1671.

Mr. Collins—Sir,

With these I send you my promised solar tables, by my schoolfellow Mr. Sargeant: though I have not added the sun's diameters, yet I intend before these may be printed to send them to you, to be annexed to them, or included with the rest. Sir, I understood by Mr. Townley's uncle, that Mr. Jonas Moore has several of Mr. Gascoigne's and Mr. Crabtree's papers or letters, which, having one part of them in my hands, I would gladly see; because they often make reference to some others that I have not, and which Mr. Christopher Townley thinks are in Mr. Moore's hands. But I know not how to move Mr. Moore about them. I am an egregious debtor to him already, and such manuscripts I know to be a treasure of that nature as is not to be trusted to every one, and I cannot tell how with civility to demand them of him; but you, having given me notice of these and some information of his, I would entreat you, when you have a convenient opportunity, that you would please to speak to him about them what your own discretion may dictate to you, so that they may come safe to my hands. I desire to know whether some parts, or the sum, of them have not been excerpt by Dr. Wallis into his collection of Mr. Horrox's papers, for I find that there was a constant intercourse of mathematical dissertations betwixt Mr. Crabtree and Mr. Gascoigne, and that it reached to Mr. Horrox too, though Gascoigne and Crabtree became acquainted but some little while before Mr. Horrox's death. I esteem Mr. Gascoigne by his papers to have been as ingenuous a person as the world has bred or known, yet better versed in mechanic.inventions, and happier in them, than in his astronomy. I have his observed distances of the Pleiades, from whence I have deduced their places, and find them much different (in so small an arch as they extend) from what I have delivered them from Vin. Mutus, in the Transactions, No. 66.[h] So that those appearances will in the western stars commence later, but sooner in the eastern than I have computed. But these I may not communicate without permission from Mr. Townley, whose the papers are, and to whom I have wrote to revive these observations, and renew them with the first convenient opportunity.

Sir, my occasions are so frequent, and I am so much forced from my studies, that I fear I can scarce get any time to transcribe what I have written on M. Hevelius, it being so prolix that it will cost me more time than I can possibly spare to abbreviate it. I

[h] The reference to the 66th Number of the Phil. Trans. is for some occultations of the Pleiades, of which Flamsteed there gives the calculated times. The observations of Vinc. Mutus were published by Riccioli, in his Astronomia Reformata, (1665) p. 243.

esteem myself obliged to continue my annual preadmonitions of the lunar appearances, which is a work not only necessary, but (if I flatter not myself too much) something honourable for our countrymen, and therefore not to be discontinued. Sir, my spare hours betwixt my indispositions and affairs being now but few, and likely to be much less, I cannot possibly perform both these tasks; therefore desire you excuse me my papers upon Hevelius, by which perhaps I might disoblige him to the English Society, which he honours, and permit me only to make my new calculations, towards which I have not as yet wrote one figure. If you think them not so necessary, I shall forbear thinking of them, and perform my notes on Hevelius with all speed and s...etness possible. I shall have ere long an opportunity of sending to Dantzic, when perhaps I shall write to him and inform him civilly of his errors myself. In the mean I submit my intentions to your discretion, and resolve in this to be ruled by your advice.

Sir, I understand one Bayley a glass grinder works glasses cheap. I desire you inform my bro. Sargeant where he dwells, for I have ordered him to get me a pair of good plano-convex glasses of 2 inches broad, or 2¼, for my double convex of 1½ inch is exceedingly too narrow[i]. Inform him likewise, if you can, what they will cost, that he may not be abused. You will, in directing him, do me as great a courtesy as if you performed my business yourself, of whom I cannot civilly demand such troublesome courtesies, because I request them often, and reward you not at all for [them] with more than what would starve your cat—my thanks. I wrote to you about procuring me

[i] Against this Flamsteed has written in the margin, "For my "13 feet tube," and over it has added in pencil, "as deep as he "can —10s.—."

some information of the worth of moulds[k] for working glasses in; if you have not returned me any answer before this come to your hands, you may do well to inform Mr. Sargeant where he may inquire and be ascertained for me; though I hope an ingenuous smith we have near us may work them [for] me. I wrote by my coz. Wilson about procuring me a watch pendulum clock, but since I understand they are so dear 'tis for me no meddling with them.

Thus far I had wrote when I received your letter of the 29th: I am obliged to you for your information concerning the price of pendulum clocks. I am glad to hear they bear no bigger a rate, for I conceived by my last information from Mr. Townley that they would be of a far higher value. If I can procure my father to allow me so much money, I will not long be without one; and shall trouble either yourself or Mr. Hooke to buy me one, when I shall have either my father's allowance of the expense, or so much money of mine own extraordinary. I am sorry to hear that Mr. Long has so failed you, and for the Hamburgh loss, but glad to hear that you expect better things from Mr. Vernon. I cannot but joy with you in the knowledge of your French correspondence, but I fear you have given them an account of my abilities beyond what they will prove. I desire to know if you ever communicated any of my calculations, or my few observations, unto any of them, and what you have in return. As for the correspondence with the French astronomers, I shall gladly undertake it with the first convenient opportunity, but not over hastily. I have several observations of the moon's transits by the fixed stars, and her diameters, made by Mr. Gascoigne, and related in

[k] Here again Flamsteed has added in pencil by the side, "—10s—. Directions."

his letters, which yet (their like for accuracy not being to be had amongst the French) I think not fit to communicate, nor buy their acquaintance with them. As for my exercise De æquatione temporis, I have no perfect copy of it, so cannot send one, but shall, if you have not already, inform them of my observations of the last visible solar eclipse, and the lunar transit, and some admonitions of future appearances.

Sir, I am forced to conclude: my kinsman, Mr. Wilson, when at London, left a letter of mine to him with Mr. Oldenburg, which will inform you of my present designs and engagements, which I question not but on the least motion he will shew you, and I desire your advice on. Excuse, I pray, this length, afford your advice to my friend, and assure yourself that no one is more mindful of your favours, or more grateful for them, than

your obliged and affectionate
friend and servant,
JOHN FLAMSTEED.

Part of this letter is printed in the Gen. Dict. vol. V. p. 253.

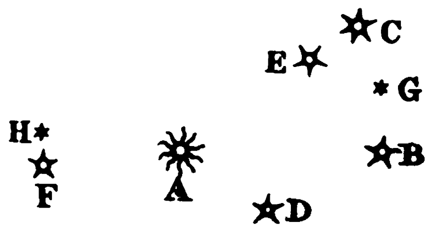
CLXIX.

FLAMSTEED TO COLLINS.

Derby, Nov. 8, 1671, die ☿.

Mr. Collins,

I hope you will pardon my silence when I shall have informed you that I am never less idle than when you hear the least of all from me. Pardon my want of good manners, I mean of thankfulness for your last favour in procuring me my seven feet glass, when I have only restrained myself that I might the more fully exhibit my gratitude, and make my thanks meritorious with you by their attendants. I have now somewhat extraordinary to furnish your correspondencies, and shall have every day more and more. Last October I observed, with my fourteen feet tube and the Townleian micrometer, the underwritten distances of the Pleiades, which I have compared with those I find in Mr. Gascoigne's papers, and those of Mutus in Ricciolus. By my own I have described the Pleiades as here.



	Mihi.		Gascoignio.[1]		Muto.
	′	″	′	″	′
AB	35	51	35	1	
AE	27	22	27	50	31
EB	19	57	19	24	22
ED	25	47	25	31	30
BD	22	0	22	43	24
AD	18	40			22
AF	22	45	22	54	27
AH	23	6			
EC	9	52			11
BC	21	40			
CG	11	31			
EG	13	42			
FH	4	39			4

These are my first essays, which therefore I cannot esteem altogether the exactest, since I have sometimes

[1] See vol. I. p. 56.

noted some little difference from these, which will not at any time amount to 20″, which yet, I think, I shall in time remove, so soon as I have so ordered my tube that it may move without shaking; my eyeglasses too, I fear, are not enough convex, which, performing well on Jupiter, I am yet loth to change till I have made some further trials. October 12, last past, I saw the anses of Saturn very exile, and almost occult in his adscititious rays and the few colours cast by my glass. Oct. 18 I saw all the satellites ad dextram Jovis in the heavens; as also October 22, when I took the distance of the remotest occidental satellite from his remotest limb 7′ 29″, or 12½ diameters, and from an oriental star, which I think was one of the fixed, 2′ 27″. Last night but one I took the moon's distance from the brightest of the Pleiades twice; and yesterday morning having seen four satellites, two on each side of Jupiter, at his altitude 38° 15′, when I returned to my tube, from which I went to warm me, I had lost one ad dextram, but whether under his face or shadow I know not; for, at first view, it was but half a diameter from his limb. Now his height was 41° 6′, so that betwixt these two times I lost it; but not having any tables of the satellites, and not thinking but that the appearance predicted by M. Cassini would be invisible as celebrated under our finitor, I gave less heed to this appearance at first; concerning which and the rest of my observations I owe an account to Mr. Oldenburg, whom pray inform of this, and that I intend to send him my now calculated occultations and transits for the next year within one week, or fortnight at furthest, after which you shall have my observations too, which I have not had time, betwixt my calculations and more necessary affairs and studies, hitherto to consider fully. Pray, at your full leisure let me hear some news of what is abroad transacted, whether you have yet received any thing from your correspondents in France, to whom, if you please, you may hint, that by several observations made yesterday morn, and the morning and night preceding, when the moon was near the Pleiades, (from which I took her distances to be accounted to you hereafter,) and in ipso perigæo, her semidiameter exceeded not 16′ 53″, nor was less than 16′ 47″, so that for divers very good reasons, too many to be rendered here, I determine 16′ 50″, which I intend to compare with some of Mr. Gascoigne's observations, but as yet cannot hope for that favourable leisure. Pardon these curt descriptions of observations, which I shall hereafter, if you require it, describe at large to you; and believe me, as of the heavens, so

your bounden observer and servant,

JOHN FLAMSTEED.

Pray let me know what you hear of the edition of Tycho's works, and whether you can procure me a Cassinus. My service to Mr. Jonas Moore, and tell him that I must thank him for these observations, which I make with the micrometer, which I am obliged to acknowledge ever to him.

J. F.

Part of this letter is printed in the Gen. Dict. vol. V. p. 253. There is the following memorandum written upon it by Collins, "Two red glasses, one of ½ inch diameter, the other ¾, "for looking on the sun."

CLXX.

FLAMSTEED TO COLLINS.

Derby, Jan. 31, 1671–2.

Mr. Collins,

I have received several informations from Cambridge that Mr. Newton's tube is now delivered into the hands of Dr. Barrow, to be by him presented and published before the Society, and that we may expect an account of it from Mr. Oldenburg, which, if he have liberty to give us, I shall hope to see in the Transactions speedily; otherwise I must request the favour of you or Mr. Oldenburg, to rectify the informations I have received concerning it. A kinsman of mine, coming lately from Cambridge, informs me that it has but one glass, opposite to which is placed a piece of mixed metal from which the distinct image of the object is reflected on a piece of brass, placed near the object glass, but under it, whereon the magnified image is viewed through a hole in the side of the tube. Now I suppose the bare reflections of the rays cannot augment the representation so extraordinarily as is related, except one of the metal plates (I suppose that near the eye) be wrought of a concave figure. I suppose you have seen this prodigy of art. Pray, if you have, let me understand its dimensions and effects from you. If you have not, pray convey this into the hands of Mr. Oldenburg, who, I doubt not, will descend so far to gratify me in this thing, and I shall endeavour to recompense [him] with such celestial observations as the heavens will allow me, when they grow clear, for since the first of January I have not had any weather for night observations, it having been either too cold for me to abide, or overcast, or both.

I have seen the Dec. Transaction, in which Mr. Oldenburg has transcribed some lines from my letters concerning the appearance of Saturn, which I am glad to find agree so well with the like of the French astronomers. You may further intimate, that I viewed the same planet, on new year's day at night, with my long tube and more convex eye-glass, but neither myself, nor a young man with me, could discern any anses, although the appearance was sufficiently clear. I have several times measured the sun's semidiameters, which none of my observations allow less in perigæo than 16′ 24″ or 25″. I think I communicated to you some measures of his diameters, when he appeared elliptical. If I have not, I shall, when you please to command them; for I owe (and esteem a good part of my endeavours) a debt to you. I am now excerpting some observations from Mr. Gascoigne's and Crabtree's reciprocal letters, in which I find exact observations of the moon's eclipse Dec. 10, 1638, and the sun's May 22, 1639, this by Mr. Gascoigne, that by Mr. Crabtree; which, if they were not in Mr. Horrox's papers, and Mr. Townley would permit, might do well if they were added to them, of which I desire to hear how many are printed by your next.

I may not conclude till I have informed you, that from Hecker's Ephemerides I find, that on Feb. 13 next following, at 6 hor. p. m. the star of Jupiter will be in the same longitude with the 24th of Leo, whose place then is ♍ 14° 8′, lat. 1° 40′ Bor., but Jupiter's latitude[m] will be only 1° 29′, so that he may be seen to

[m] Jupiter's diurnal motion but six min. so this appearance will be observable four or five days before or after.—*Note written by F. in the margin of his letter.*

pass under the star 11 min. Pray inform Mr. Hooke of it, or any such of your friends as are disposed to make observations, and accommodated with tubes and instruments. I would have given notice of this and some others to Mr. Oldenburg, had I discovered them timely enough. Now I must request you to do it for me, since I have not time to do it myself. If Mr. Jonas Moore give any attendance on the heavens I shall early enough let him know of some observable appearances, not published nor certified in the Transactions, with some observations made with the micrometer he gave me, and therefore a due debt to him. I have read De Mouton De æquatione temporis, &c., whose notes upon the equations of others might easily be redargued, but that I would spare your correspondents; of whom, if any now will agree to observe the diameters of the planets (the sun and moon especially) with the satellite-appearances of Jupiter, I shall not fail, as the weather and my distempers permit, to do my endeavour to make respondent observations. All the times I have viewed the sun I could never see any macula upon him, but his whole disc perfectly clear.

I doubt Mr. Oldenburg writes not to me, because you have let him know that you direct your letters to Mr. Litchford, which I ordered so only that my father might not see all the letters that come to me; but for his, desire him to superscribe them to myself and send them by the post, as he did formerly, except they be too cumbersome, which he may send by such friends as I procure to deliver my letters to him. I understand you live not now far from him, which makes me bold to request you [to] give him these informations. I had sent you a draught of my new micrometer, but that I lately sent one to Mr. Townley, whose sentiments I expect concerning it before I mind it should travel further. No more, but, with thanks for your former kindnesses, I rest

your obliged friend and servant,

JOHN FLAMSTEED.

The latter part of this letter is printed in the Gen. Dict. vol. V. p. 254. It is there dated 1671, but it must be 1671-2, not only from its connection with the next, in which he gives the solar diameters promised in this, but we have already had a letter of the date Jan. 30, 1670-1, at p. 105, with which this is totally unconnected. It is dated 1671-2 in the Gen. Dict.

CLXXI.

FLAMSTEED TO COLLINS.

Derby, Feb. 10, 1671-2.

Sir,

I have wrote to you lately, so have not much to write at this time, only having a convenient opportunity of conveying a letter by my schoolfellow, I thought not fit to lose it. The heavens have not favoured me of late with any serenity more than served one night, viz. Feb. 1, to observe the immersion of the first satellite into the shade of Jupiter, and the application of the third to his limb, of which I gave Mr. Oldenburg an account in a late letter, which perhaps you may have been informed of from him. If not, pray let me know, and whether you would have me write, what I communicate to him, to you; for I have supposed you so intimate that I needed not write the same things to both. I am unwilling to let this come without something of observations, therefore I here give you the sun's diameters, of which I esteem the first, third, and fourth too large, by reason of my unpractisedness in such observations at the first essays; for such they were. The rest I esteem very accurate, yet will not build upon them till I have made some further trials with an exacter micrometer. They are these:

1671–2,

		°	′				
Jan. 5,	Alt. Sol.	5	12	AB[n]	33	2	Nimis lata vereor; Sol Ellipticus.
				CD	32	11	
9,	Alt. Sol.	9	0		32	51	
10,	Alt. Sol.	10	0		32	48	
11,	Alt. Sol.	10	0	AB	32	43	Sol Ellipticus.
				CD	32	37	
12,	Alt. Sol.	13	0		32	42	
15,	Alt. Sol.	9	0	AB	32	42	Sol Ellipticus.
				CD	32	32	
Feb. 1,	Alt. Sol.	19	30		32	37	

All these diameters were observed in my tube of 164½ inches. At none of these times could I find any spot under the sun, but he has been constantly clear. These observations, I think, may serve very well to communicate to the French, your correspondents, and de Mouton; to whom if you think it convenient I should now write, pray let me know. I will prepare a letter either in English, or my homely Latin, as you shall direct, and put it into your hands to be conveyed. My friend, if he have time to see you, will pay you for de Mouton. I am now excerpting something from Mr. Gascoigne's letters to Mr. Crabtree, and his answers, but find some schemes and papers wanting, of which Mr. Crabtree could not be careless, but they are supposed to be fallen into Mr. Jonas Moore his hands. Pray let him know what I am doing when you see him. They can be of no concern by themselves; wherefore he may do well to put them into Mr. Townley's hand, who has the

[n] The letters refer to a rough drawing in the margin, which it was unnecessary to have copied. AB refers to the horizontal and CD to the vertical diameter.

letters referring to them. I desire to hear from you at your leisure, and in haste acknowledging your constant courtesies, subscribe

yours to command,

JOHN FLAMSTEED.

This letter is printed in the Gen. Dict. vol. V. p. 254.

CLXXII.

FLAMSTEED TO COLLINS.

Derby, April 1, 1672.

Mr. Collins.—Sir,

The included paper contains the last observation of Jupiter's transit by a fixed star, and another since made, with my deductions therefrom. These, (being busy at this time of the year,) to save a labour of transcription, I send through your hands to Mr. Oldenburg, that so you may have the perusal of them. I hope you had my last, informing you that I have received Mr. Horrox's papers, and satisfying you concerning the contents of my last letter. I have perused Horrox his Venus sub sole, and find he needs no comment save in this. He has taken the sun's semidiameter less by 20″, or more, than really it was, so that his calculations will need some little correction; which, if you think fit, I shall give them by way of comment. He has assumed the diameters of the fixed stars too large, which Hevelius has only well corrected; and his diameters of the planets are much amiss, but not having observed them all myself, as I intend, (if God afford me opportunity,) I shall not meddle with them. Yet only to you I may say that those settled by Hevelius, except in Venus and Mercury, are as erroneous as those of Mr. Horrox. Hevelius is much mistaken in the parallactical angle in the Venus sub sole, but it is so easily proved that I cannot think but, if ever he again read over that piece and his notes, he could not but see his mistake.

I find, by my frequent observations, that the remotest satellite of Jupiter goes not twenty-three semidiameters only from him, as Cassini has it, but rather twenty-four and a half, or three fourths, as Mr. Townley gave it me in a loose note. And this you may prove by an observation made March 19, amongst the rest, if you try it. I think those observations the exactest that ever we had of this planet; and, if we had but a few more such in several places of his orbit, I should not doubt but to restore his motions more accurately than we have yet done, or perhaps can expect. I cannot think the satellites of Jove subject to any great inequalities in their motions, since I find their distances from Jupiter, on each side [of] him, equal in their greatest elongations. If I had but the opportunity of observing one transit of Mars, in his achronical appearance, from a fixed star, I should not doubt but to derive the sun's parallax and distance a terra, as well, if not better, therefrom, than from any observations as yet we possess. This I expect, and have forecasted for; but by an accident the course of my observations are, and will be, a little interrupted: for being, last Thursday night, preparing to observe the diameter of Jupiter, I took my object glass of my long tube to smear it, as I had often done before, at my candle. As I was holding it over the light, it gave a crack, and a little piece splintered from it in the middle, and the glass cleft clear through. I durst not remove it out of the box, because I was afraid I could not so well make it close again. I keep it by me, but do not adventure to make any observations by it, though I have viewed the moon with it, and find it does not perform much worse, if any, than it did. Desiring to hear from you at your leisure, I rest

your affectionate and obliged friend,

JOHN FLAMSTEED.

When Mr. Sargeant comes to London I shall send money, and directions for procuring me a new object glass, and beg your care or Mr. Hooke's in it.

This letter is printed in the Gen. Dict. vol. V. p. 254.

CLXXIII.

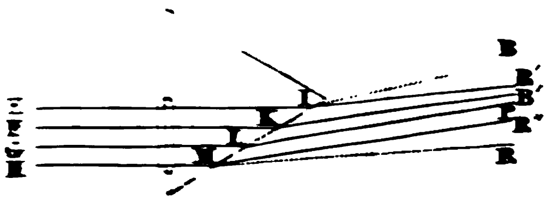
FLAMSTEED TO COLLINS.

Derby, April 17, 1672.

Mr. Collins,

I have yours of the 12th instant, whereby you intimate your desires to have me say something further concerning the lunar theory of Mr. Horrox, for the satisfaction of Dr. Wallis. I am content; and therefore shall ere long send you the whole calculation of the moon's place at her last transit over the Pleiades, after his method, with my further thoughts upon it. I am very desirous to be framing lunar numbers, but want observations of her apogæon diameters, which the inconveniency of my dwelling permits me not to make, because the apogæon moon in Sagittarius goes so very low this year; and further, the refractions in her low meridional heights would greatly diminish her vertical diameters, which, upon continual experience, I find can only be taken to the requisite exactness; so that I fear I must wait till the year 1674, or 1675, ere I can well observe them. I cannot but much approve of the [illegible] Mr. [illegible] theory [illegible] considered it [illegible] we might almost as easily [illegible] place of [illegible] were [illegible] to his method; [illegible] I intend to [illegible] so much [illegible] may afford it me.

I have perused Mr. Newton's letter concerning colours, but cannot think them to proceed from different rays, capable of different refractions. For then no particular colour, but a mixture of all sorts, should be produced by the prism. For let four rays of light fall parallel to each other into the prism perpendicular to the first face, so that they may not be refracted till they pass forth from the second: of these we suppose the two outermost (E and H*) to be of that sort, which



are least refracted and make the red, they, therefore, passing through the prism, in their emersion move parallel to each other, and will paint the red at R′ and R″. Let F be a ray, yet more refracted, framing the blue. G yet more, causing the purple violet, these are refracted betwixt the other two to B′ and P, where they paint their proper colours. But you see that it will follow they mix with the other and will cause no determinate, but a confused colour, mixed of all sorts, and if I should urge what Mr. Newton somewhere

* The liberty has been taken of altering the letters of reference on the diagram, in a manner which was thought to make them more clear when printed.

supposes) indeed a white, which he says is the mixture of all sorts of rays. I rather think, therefore:

1. That the rays of light move not so swiftly through the prism, as in the free air, and that by several opaque particles of its substance many of them are reflected, or reverted, and at last lost in the glass:

2. That those rays, which move through a less portion of the glass prism, have their liberal motion less retarded, and consequently in their emersion move quicker, and less refracted from the way they took in the free air: so the ray HM moves swifter from the glass after its emersion at M, than either GL, FK, or EI, and retaining a greater propensity to the way it made in the free air is less refracted, and painted nearer the perpendicular than they. Whereas the ray at I, and those inceding near it, moving through the thicker parts of the prism, are many of them destroyed in their passage, and never come to emerge on that face, but those that do, having their motion much retarded by their longer journey within the glass, and a greater inclination to the refracted way impressed on them, move wider from the perpendicular, and, remotest from it, paint the blue and purple violet upon the recipient wall or scene. Hence I concluded that the red is painted by rays which move swiftly, but are refracted in no great quantities, for it is painted (in the experiment by the prism) by those rays, which proceed from the outward remotest edges of the sun; yellow is painted by rays moving not so swiftly as red, but in greater quantities; the orange is but a composition of the red and yellow on one side, as green is of yellow and blue on the other; blue is made by fewer rays than red by far, and much slower moved; the purple violet by fewer yet, and slower moved than they.

These are my first thoughts concerning colours, of which I wrote a longer letter to Mr. Wroe, an ingenuous fellow of Jesus College, Cambridge. Pray let Mr. Oldenburg see this, for I have not time to write to him, and tell him I desire his private opinion of these thoughts.

I am glad to hear that Mr. Newton's telescopes are made so well, as you intimate, by Mr. Cox. I must beg of you further to inform me, by your next, at what rates they are valued, and with what effect they perform. I mean, how I may buy one of about 2½ feet, and whether it will do as well as a long one of thirty feet. As soon as I can send you money to buy me a new object glass I shall; the length of my tube is fourteen feet betwixt the glasses. Mr. Hooke procured my last, which was not very excellent. I hope he will not, at your request, refuse to try one when you speak for it, that I may not have a bad one. The two which he procured me cost thirty shillings; I shall send you forty shillings. What shall be to spare you may retain to pay for those books I owe you for, or such as I shall want hereafter. The heavens, never since I broke my glass, afforded me any opportunity for observation till yesterday, when at noon, (having covered the broken part of my glass,) with the bigger piece I took the sun's diameter 5325 = 31′ 58″, which still conspires for the exactly bisected excentricity, and the perigæon semidiameter 16′ 25″. I shall have occasion to write to you again ere long; wherefore, hoping only to hear again from you in the mean, at present I rest

your affectionate friend,

JOHN FLAMSTEED.

Pray impart to me what you know or hear of Mr.

Hooke's new tube. I have wrote to Mr. Nuns, but hear nothing from him. Pray inquire of his friend which way I may direct my letters, and I will write to him again.

This letter is printed in the Gen. Dict. vol. V. p. 255.

CLXXIV.

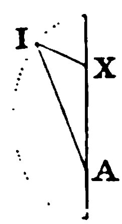
FLAMSTEED TO COLLINS.

☿ Derby, May 6, 1672.

Mr. Collins,

Last Saturday, by our carrier, I received my father's lease; for your pains about it I shall ever acknowledge myself obliged to you. I have likewise received two letters from you, conveying the most current news, for which I am also bound to give you hearty thanks, requesting that you would continue to give me what you shall hear of certain for the future in your letters. The gazettes we have by the post, so you need not trouble yourself to send me any.

In your first letter you had something proposed by Mr. Strode for facilitating the method of finding the prosthaphæreses of the ellipsis without severally collecting the excentric and optical equations, which is done by Streete, but rather to be attributed to Dr. Ward, from whose Astronomy I believe he derived it, and the way is so facile that I cannot expect an easier. You suggest that you can find XI, XA and AXI being given; but I cannot think that can be found without some trouble, and afterwards XA, XI, and AXI being given, it will be more difficult to find AIX than by the method of



Streete. However, you will very much pleasure me to let me know how he (Mr. Strode) operates, or how you can find XI.

I hope you have by this time received a letter from me by Mr. Sargeant, with twenty shillings, [the] fourth of which you must pay yourself for Mouton, Horrox, and Fabri, the remainder may lie in your hands till further occasion. I intimated by that letter that I would send the lunar tables by Monday next; I have a book to send back to Mr. Oldenburg, they shall be within it; but I fear I cannot send what I intend to add till the week following, when I shall send it by the post; for an uncle of mine, mayor of our town, died yesterday, and I am bound to attend with my friends at his funeral, and in the mean time their company scarce affords me liberty for this. You will, therefore, for once pardon this abruptness of

your affectionate and obliged servant,

JOHN FLAMSTEED.

Pray let me know whether any Transactions have been printed of late. Our bookseller's chapman has sent none this three months. I have returned your receipt as you desired.

Part of this letter is printed in the Gen. Dict. vol. V. p. 255.

CLXXV.

FLAMSTEED TO COLLINS.

♃ Derby, May 13, 1672.

Mr. Collins,

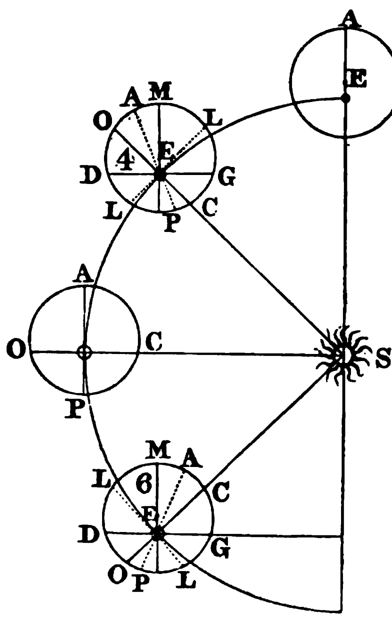
With this you will receive the promised lunar tables, to which I have added an Epilogus directed to yourself,[p] but my desire of brevity, and not to meddle where Mr. Horrox had done any thing himself, restrained my thoughts in that place. Here, with your leave, I shall enlarge.

How the apogæon of the moon is librated Mr. Horrox has shewn us very well, but why it should be so librated methinks he shews no good reason: what he says of the sun's attracting that end of the axis of the system next him, is framed from Kepler's groundless notions, and may, in my opinion, be more naturally solved thus.

The moon's system, being disposed about our earth, seems to be most likely poised upon its centre, and seeing that the part contained betwixt the apogæon and a line drawn perpendicular to the transverse diameter of the orbit, through the earth's centre, is more large and capacious than the part contained betwixt the said line and the perigæon, it follows that the apogæon part of the system may contain more of matter and celestial substances than the perigæon region, and consequently (as the end of a false balance, which is longest from the centre) may supragravitate and incline the axis towards the synodical line, where, by reason of the sun's directest action on the earth's

[p] This address to Collins is omitted in the Epilogus as printed in Horrox's Opera Posthuma, p. 489.

sphere, or rather the moon's, the action of the earth upon the lunar system, whereby the apogæon and the ascending node are carried about, may be most broken, and the system may most easily receive and admit this libration. To exemplify this in the 4th and 6th figures[q] of this theory, let MP be the line of the mean apogæon making an acute angle with the syzigiacal line SCO. Now by reason the apogæon part of the system MDGM is more capacious than the perigæal part DPGD, it follows that, containing the greater quantity of celestial bodies, it may overweigh the other, and incline the system towards the syzigiacal line, where by reason of the sun's direct action upon the æther, it is attenuated and can least resist this libration.



The physical equation of time I admit, not as if the heavens required it in the other planets, but because there is a true physical cause for it, as you find hinted in my papers; for if the planets, which are remotest from the sun and in their aphelions, move slower than planets nearer and in their perigæons, &c. [perihelions], I think it is most rational to think that their parasitical attendants move with their masters more slow in their orbs, they being in apheliis, more swift about them when they are in periheliis. This I shall for my own

[q] See Horroccii Opera Posth. p. 471.

use turn into parts of a circle, for it ought not to be applied like the demonstrative equation of time, since it is an inequality in the moon's motion, with which no other planet is concerned, nor can it cause any alteration in any celestial appearances except in the satellites of Jupiter, in which it can be but small; yet shall I have occasion to inquire what it may effect and to inform you ere long.

You have sometimes desired to know what I found amiss in the Hevelian edition of Horrox his Venus sub sole. I am not willing to let all be known I have against that great man, but in his comments on that treatise the main cause of his error there is, that he hath misstated the parallactical angles, calculated by Mr. Horrox.

	h	m		°	
At	3	15		70	56
	3	35	supputat	68	53
	3	45		67	55

and used instead of them

°		
46	34	which were the angles made by a line passing
48	37	
49	35	

through the centres of the sun and Venus with the ecliptic, or that he hath used the angle ECV instead of the angle NCL: whence have proceeded all those faults, and faulty corrections of Mr. Horrox his work, which are found everywhere in the succeeding comments.

He finds fault with Mr. Horrox for having framed the diameters of the planets amiss, whilst his own, if well considered, though something alike in proportion to each other, are not less erroneous in quantity. His greatest diameter of Jupiter is but 24½″, yet I find that Mr. Gascoigne once observed it 51″, he says accurately.

I have seen it above 46″, and Mr. Townley no less; and that it is wholly so much, the observed elongations of the satellites from Jupiter's centre, taken in minutes and seconds, compared with the estimated diameters, doth plainly evince.

But Mr. Horrox hath erred himself in the sun's diameter, which he states only 31′ 30″, which my observations warrant 32′ 42″. Hevelius has stated it 32′ 30″; he has likewise, in his comment on the sixth chapter, p. 124, delivered the parallaxes of longitude and latitude in his own hypothesis, which supposed the sun's distance from the earth 5064 semid. ter. and its horizontal parallax 41″. I know not what warrant he may have for this, since he says nothing of it in his works, that I have seen; but I hope at the next achronical apparition of Mars, or at the latter end of September next, to make those observations of him, which may shew his parallax, and by consequence the sun's. I hoped to have observed him this morning, but the skies are clouded; yet, if they clear before Tuesday, I doubt not to gain another accurate observation of him, and to measure his diameter, which yet I could never do conveniently. I have observed the return of Jupiter to the star in ♍ 9° 58′; if you desire it, I shall send you both it and what I observe of Mars, when I have heard from you.

On Monday I shall deliver Mr. Oldenburg's book to the carrier. Pray deliver him the enclosed, that he may be informed of it. I have never heard from him since February last, nor seen any Transactions since that month's; none either having come forth, or our stationer's chapman having failed him more than usually. Pray let me know how he does in your next, and desire him to send me an answer by the post.

I have often requested you to let me know what you hear of Mr. Newton's telescope, how it performs, and at what rates they may be purchased. You will gratify me much if you can satisfy me concerning them, and I much desire you would; for, if the rates be not extreme, I would not want one long.

I must beg your excuse for my blots and interlineations in this letter: I have written it hastily, and was called often off whilst I was doing the first side. However I hope it will be intelligible. I have not the time to write it over again, else I might have made it more easily understood; but to you my meaning will not be difficult to find. I thank you for your constant civilities, and rest

your very affectionate friend and servant,

JOHN FLAMSTEED.

Since I wrote this I have received yours of the 13th instant. I have, therefore, pasted new radixes to the meridian of Derby, that so they may comply the better with my solar numbers. I have so pasted them, that, if you turn them up, you may find those for London under; but, if you approve the radixes to Derby, which I had rather have, the title may be written thus[r]:

Lunares numeri
ad novum lunæ systema excogitatum
ab Astronomo peritissimo
Jeremia Horroxio
ad meridianum Derbiæ notissimi
Coritanorum oppidi, in ipso fere
totius Angliæ umbilico siti,
accommodati
ab
J. F.

Something should be a little altered in a paragraph

[r] See Horroccii Opera Posthuma, p. 473.

at the foot of the second page of the Epilogus, which is so little I refer it to you to perform, being I have very little leisure, and it would too much soil my blotted copy, which needs no alteration, if you admit the London radixes. J. F.

Pray let me know, as soon as you have received this. I will send you a catalogue of the differences of longitudes and latitudes to be printed at the end of the piece, if you think fit, from London or Derby, as you shall judge most convenient.

Pray procure the six single copies of my exercise, and those tables, if they be printed, so as they may be severed from the rest, that so I may have them to bestow upon my friends, and I will pay for them. J. F.

This letter is printed in the Gen. Dict. vol. V. p. 255.

CLXXVI.

FLAMSTEED TO COLLINS.

Derby, May 20, 1672.

Mr. Collins,

The many good offices you have done me, since your first acquaintance and correspondence on the occasion of my studies, persuade me to confide in you, for one, on the account of my other affairs, and to be so bold as to request your assistance in it, which yet I would not have troubled you with, if I had had any friend in the city, or relation, by whom I might have performed it. The business is this. My father has taken a lease of some lead mines in Staffordshire of the duke of Albemarle his bailiff. One Mr. Edmond Leneve, of Clifford's Inn, his solicitor, has drawn the leases, which he certifies us my lord has sealed. Last week he sent us down the counterpart, which my father has sealed, and I return up to you by the carrier, with five pounds to be paid to Mr. Leneve for the writing. I have added *2s. 6d.* over, which will pay the expenses, and serve to drink with him. I would desire you to do me the kindness to receive my lord's lease, examine it with the counterpart, and return it to me with all speed your occasions permit. If, as you go about your occasions, you pass near Clifford's Inn, I would desire you to call on him, and to appoint some time on Saturday for the delivery of our part and payment, and the receipt of my lord's, that we may have it down by the return of the carrier, if possible. One thing I have to inform you: together with the counterpart Mr. Leneve sent a bond of two hundred pounds penalty for performance of covenants, which we suppose he intended for my father to seal, only if he was willing, for he said nothing of it in his letter; but my father thinks it not fit to be sealed, because there is a clause for reentry in the lease, and the miners, to whom we allow shares, and employ in the work, are the basest sort of people, and may, do we what we can, commit some small faults, for which it is not just that my lord should exact both the forfeiture of the lease, and two hundred pounds bond, of my father only and his heirs. My father has written to him a letter besides the inclosed, but made no mention of the bond it[self], of which you need not say any thing to him; but if he mention it, you may deliver the inclosed, which I suppose will satisfy him, otherwise you may keep it to yourself, if he say nothing of it. I am ashamed to put you to so much trouble, but the urgency of this affair enforces me. If you expend more than the half crown, I will make it good after Whitsuntide, when I shall send you up some money by Mr. Sargeant to pay for such books as I have had of you, and such as I shall want: and if you can command me any service in the country, that may concern you, wherein I may serve you as much, you shall command my best services and endeavours; and I will be glad of any occasion to shew how much you have obliged me, and how grateful I would be.

As for my last performances, they are these: I have lately observed the distance of Mars from a fixed star very accurately; last week I made equations for Mr. Horrox's greatest excentricity of the moon—for the least I have them made ready to my hands. This week I will more accurately calculate the equations of the apogæon and excentricity, and reduce the mean motions to current time; so that you may, within a fortnight, or three weeks at furthest, expect the lunar tables from me, if God spare me health. I shall now have more time than formerly. I intend likewise to note the erroneous places in Hevelius his notes on the Venus sub sole, and send them as soon as I have finished the new lunar tables. My observation of Mars I intend not to send you till I have made some more, which I expect about the middle of the next month. Pray save one of Honorato Fabri's Synopsis Geometriæ for me, against Mr. Sargeant comes up. I mean that you call his pocket companion.

Pray direct your next letters to myself; and let me hear from you next week. If Mr. Leneve inquire about the witnesses, you may inform him that Mr. Spateman, my uncle, is an alderman of Derby, Mr. Rode is his brother-in-law. No more, but craving your assistance in this business, and pardon for the trouble of it, I rest, your very affectionate and obliged friend and servant,

JOHN FLAMSTEED.

If Mr. Leneve will not deliver the lease without the bond, pray keep the money in your hand till our further directions. I have paid the carrier for carriage of the money. Pray call it.

The latter part of this letter is printed in the Gen. Dict. vol. V. p. 255.

CLXXVII.

FLAMSTEED TO COLLINS.

Mr. Collins, Derby, May 30, 1672.

By yours of May 25 I understand that you have done my father's business, for which he returns you hearty thanks. Our carrier comes not in yet; therefore I cannot in this include the receipt, but by my next I hope to send it you. I have sent you twenty shillings by the bearer to pay for Mouton, Fabri, and such books as I shall want hereafter, of which pray let the remainder lie in your hands till I let you know what I want. I am very bold with you to put [you] to so many troubles; but you may and shall in lieu of them command my greater services upon any occasion.

I have this morning completed my numbers for Horrox's theory, which I shall transcribe and send you, with some little account of the method of calculation, as my leisure and occasions will permit. These I hope to send you on Monday come sevennight. I have lately observed the return of Jupiter to the star in ♍ 9° 58′, but have not yet had time to commit it to my book, and calculate the times from the observed altitudes; but with the lunar tables you may expect them from

Your very much obliged friend and servant,

JOHN FLAMSTEED.

I have forgot to what meridian I made the radixes of the solar tables I sent you. Pray let me know, or transcribe one, and the time, and send it me, that I may frame your lunar radixes to the same. I think I made them for London, but am not confident. My solar numbers are very near the same with Mr. Horrox's. So that except you think it necessary, I shall not calculate new equations and numbers for the sun, since mine satisfy all the sun's places better than his will do; and though our equations of time be different, yet I think I can satisfy you that his physical equation needs only to be applied in the moon.

J. F.

The latter part of this letter is printed in the Gen. Dict. vol. V. p. 256.

CLXXVIII.

FLAMSTEED TO COLLINS.

Mr. Collins, Derby, June 12, 1672.

I return you thanks for your two last letters, which brought me the news; but desire that hereafter you would please to direct your letters as formerly to my friend Mr. Litchford.

I had delivered my lunar tables after Mr. Horrox's system to our carrier last Monday, but that some employments, cast upon me since the death of my uncle, put me so behindhand, that I have finished but this morning. However I hope they will be no whit the later in your hands, for I shall procure one of our attorneys to deliver them to you, either the end of this week, or the beginning of the next. I have wrote in Latin an epistle to you, containing what I have altered in their form, and what I think concerning the equation of time, the alteration of the excentricity, and the variation. I suppose you print Mr. Crabtree's letter[s],

[s] In the edition of Horrox's Opera posthuma of 1673, p. 465, the Lunar Theory is introduced under the following title: "Novæ Theoriæ Lunaris a Jerem. "Horroccio primum adinventæ, "et postea in emendatiorem formam redactæ; ex epistola socii ipsius Gulielmi Crabtrii ad "eruditissimum Guil. Gascoi"gnum scripta, explicatio. Ac"cesserunt Johannis Flamstedii, "Derbiensis, numeri lunares et "calculus eidem Theoriæ innixus. "Londini, typis Gulielmi God"bid, Anno Dom. 1673." The title to the Theory is also precisely the same in the edition of 1678. The following notice is taken from one of Thos. Hearne's pocketbooks, (No. 102,) preserved in the Bodleian library: "Feb. 8, 1728. Saturday. Hor"rox's - - - - posthumous works "were printed by Dr. Wallis. "They are now scarce. Mr. "Whiteside, of the Museum, "bought them several years "agoe, but gave 7*s*. 6*d*. for "them." The following particulars may not be without interest. The edition of 1678 is in substance the same as that of 1673. It has the same errata uncorrected at the end; and in each, pp. 127 and 134 are numbered 227 and 334. Three tracts of Wallis's are added to the one of 1678, and bear that date in the title. In the original, p. 247 was printed p. 311: this leaf appears to have been reprinted, but 347 has been inserted on it for 247. This occurs in some copies of the edition of 1673, and does not prove that the reprint or correction was made for the later edition: there is no other variation in the page.

Lalande, Bibliographie Astronomique, p. 278, mentions an edition of Horrox in 1672, with a different titlepage. He says that it is the same as the Opera Posthuma, "excepté que les "pages 465–470 contenaient en "1672 l'ancienne théorie de la "lune d'Horroccius faite en 1638, "et que l'on changea en 1678." [1673.] See above, pp. 150 and 156. In a copy belonging to Mr. Baily, the Lunar Theory is thus entitled, p. 465: "Jere"miæ Horroccii Lunæ Theoria "Nova: ex ipsius epistola ad "Gulielmum Crabtræum scripta "Dec. 20, 1628." [1638.] At p. 468 is the following note: "Quosdam ex his numeris ipse "vivus mutavit. Atque Flam"stedius, jam illam emendatio"nem prosecutus, ad tantam ac"curationem reduxit, ut vix scru"pulis 2′ peccetur, cum tabula"rum aliarum optimæ peccent "scrup. 15′ circiter." It concludes thus: "Adjungam tamen bre"viter calculum meum (ad hanc "formam) sex observationum "Tychonis, ut rectius intelli"gas quid velim." Then follow, at p. 469, the observations, and, p. 470, a remark on the fourth observation, and a correction of the deduced place of the moon. There are also three errata for pp. 468, 469, of which it is remarkable, that they refer, not to the pages of the sheet on which they are printed, but to those pages in the sheet which Flamsteed substituted for it.

which contains this theory, with Mr. Horrox's works; for the theory contained in the letter of Dec. 10, [20] 1638, as I find in some loose papers and the exercises, is not that which Horrox resolved upon; and further, in it the optical part of the equation varies not, but only the focus of the mean motion and the physical equation, so that the distance of the moon a terra varies not above five semidiameters of the earth, which in his

They remain, and are unnoticed, in the common editions of 1673.

In this copy, pp. 465–470 are marked O o o 2, O o o 3, in letters the same as the signatures of the former part of the book, in which there is no O o o. Flamsteed begins O o o, &c. but in different letters. The title is: "Jeremiæ Horroccii Liverpoli-"ensis, Angli, Opuscula Astro-"nomica, viz. Astronomia Kep-"leriana," &c. The remainder of the title is the same as that of 1673, and it closes, "Lon-"dini, typis W. G." [William Godbid.] "Prostant apud Rob. "Scott, ad insignia Principis in "vico vulgo vocato Little Bri-"tain. 1673."

Mr. Baily concludes that the two publications of 1673 are one and the same edition, as, on a comparison of the letter-press in several places, *broken* and *misplaced* letters are found alike in both copies. He conjectures that the duplicate leaves were distributed by the publisher together with the amended, or altered, titlepage.

To this may be added the following notice of the book, from the Philosophical Transactions:

"Jeremiæ Horroccii Angli "*Opera Posthuma*, una cum "Guil. Crabtræi Observationi-"bus Cælestibus; nec non Joh. "Flamstedii de Temporis Æqua-"tione Diatriba, Numerisque "Lunaribus ad novum Lunæ "Systema Horroccii. Londini, "impensis Joh. Martyn, Regiæ "Societatis Typographi, A. 1672, "in 4to."

Contents. 1. Keplerian astronomy. 2. Extracts of letters to Crabtree. 3. Catalogue of astronomical observations. 4. His new theory of the moon, together with the lunar numbers of Mr. Flamsteed upon it.

"To these are annexed first the "celestial observations of Wil-"liam Crabtree concerning Sa-"turn, Jupiter, Mars, and Ve-"nus, and then Mr. Flamsteed's "dissertation of the inequality "of the solar year, wherein are "demonstrated the Prosthaphæ-"reses of the time necessary to "make an equation, and pro-"ceeding from the unequal mo-"tion of the earth from the "aphelion to the perihelion, and "the inclination from the equi-"noxes to the solstices, and vice "versa." *Phil. Trans.* vol. VII. p. 5078. No. 87. for Sept. and Oct. 1672.

The book is mentioned as having been presented to the Society Oct. 30, 1672. *Birch's Hist.* vol. III. p. 58.

later papers he says all eclipses require to alter above seven. You have done well therefore to omit that letter, but in the room of it I suppose you will give Mr. Crabtree's, which will lead to mine. I have written to you with what brevity I could, and without compliment, which with you is needless, and to me to make it more difficult than to dispatch a tedious calculation.

Pray let Mr. Oldenburg know that I shall return his book by the carrier next week, and a letter to him with it. I have never heard from him, nor seen any Transactions these three months; for either our bookseller's chapman has forgot him, or none come forth. Pray let me know in your next. I shall not be long ere you hear further from me, who in haste am forced to conclude,

your very affectionate friend and servant,

JOHN FLAMSTEED.

This letter is printed in the Gen. Dict. vol. V. p. 256.

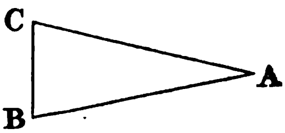
CLXXIX.

FLAMSTEED TO COLLINS.

Mr. Collins, Derby, July 10, 1672.

Included I send you the Catalogue I promised. I have chosen those towns where celestial observations have been made, and where I have found none I have ordered them by comparing their positions to one another in our maps, which yet I dare not trust; for by celestial observations of the pole's height, and eclipses, made in several places at once, compared together, I find they place Italy too near us, and all the towns of Poland, Germany, and France over remote. The maps I used were Speed's, where I wanted, but I find them not good: if you can procure me four sheet maps of upper Germany, Italy, France, and Spain, each one, you will do me a kindness to send them when you send me the remainder of Horrox. I would have the latest, and such as are printed and sold in their respective countries. I have one of the seventeen Provinces already, which was lately set forth; it is the best I ever saw, for almost all the towns, where the pole's height has been observed, stand in the latitudes assigned by the observers. I have made much use of it, and I would desire no larger.

I would gladly make some further inquiry into the nature of light and colours, but want a prism for experiments, which may have two faces of an equal breadth, the third not half so broad as either of them, that so the angle included by the broader faces AB, AC, may be less than 30°. It need not be large, so it be but clear glass and well wrought. If



you can procure me such an one, provide it me against the latter end of the next week. Mr. Sargeant will be in town, and I will get him [to] call for it: the money in your hands will, I hope, do more than pay for it.

I have received one letter from Mr. Oldenburg lately, to which Mr. Sargeant shall bring him an answer. He mentions nothing of your solution of Mr. Strode's problem, which yours had caused me to expect. Perhaps his next may give it. I have all the Transactions I wanted by our bookseller, whose chapman had only forgotten them, so you need not trouble yourself about them. I would gladly know what forwardness you hear Tycho's works are in at Amsterdam, or

Hevelius' at Dantzic, and what is said further of Mr. Newton's tube, or Mr. Hooke's. These, if you can inform me, will much pleasure, Sir,

your affectionate friend and servant,

JOHN FLAMSTEED.

This letter is printed in the Gen. Dict. vol. V. p. 256.

CLXXX.

FLAMSTEED TO COLLINS.

Derby, July 23, 1672.

Mr. Collins,

Sir, I have received Dr. Wallis's letter[t] to you of the 18th of this month, which finding me at an unusual leisure, I thought fit not to delay an answer that might give you satisfaction, though I hope you are satisfied already. The difference of the least and greatest excentricities in my tables is as the Doctor makes it, 2323.5 qualium rad. 100000.0; that I wrote 22235 was a mistake in the first transcript of my numbers, and ought to be made everywhere 2323.5; for that I have used in all my calculations, and the table of equations is framed upon that difference, else, the greatest equation in the least distance being 5°, the like in the greatest distance could not be 7° 40′, as I have calculated it. The other faults of my copy pray correct after the Doctor's intimations. You need alter nothing in the example of the calculation, for the fault exceeds not one second, which, since we cannot observe to such

[t] This will be found among Dr. Wallis's letters in the latter part of the present publication.

exactness, I willingly yield in any calculation; and few expect that severity.

I hope you have my note from Mr. Sargeant, desiring you to procure me an object glass for a three feet tube: it is to fix upon the index of my brass quadrant, whose radius is three feet. I am now dividing it, and hope to get it raised this month against the next. Pray send me the glass when you have occasion to send me any thing next: if the money in your hands suffice not for payment, Mr. Sargeant will make it out.

One thing perhaps is needful to remember. I mention in my Epilogus the letter, in which you have an account of Mr. Horrox's theory. I mean that you sent me the copy of the theory from, which, if I remember, you wrote me word that Dr. Wallis had long since translated. This I suppose you intend to print, and it is necessary to be placed before my tables: and, supposing that you intend it, I have said nothing of any thing contained in that letter, but only of such things as Horrox has said nothing of.

I have given Mr. Oldenburg an account of Jupiter's return to the fixed star May last; but could not, for Mr. Sargeant's haste, gain time to delineate a scheme of my observations. Wherefore pray deliver him the included, with my services, and thanks for his last letter. I am much indebted to Mr. Jonas Moore for that micrometer, wherewith I have made those observations, which, in your opinion I believe, never had their equals for exactness; at least that we have yet seen. I ought to impart my observations to him, and should do it, but that I suppose him not unacquainted with those I publish by Mr. Oldenburg's means, and satisfied with my open acknowledgments of his obliging courtesy. I am informed by Mr. Townley, that the person who made the screws for his micrometer, and has the screw-boxes, is with Mr. Moore. I pray inquire of Mr. Moore when you see him, and let me know if he be, that, if the workman I now employ cannot make me screws to my mind, I may procure them of him, for the making my new micrometer, which Mr. Townley thinks will be more convenient and useful than his own.

I have late procured some Lough-water, (so miners call it,) for one Webster, the author of the Metallographia. 'Tis found in the midst of a firm stone in the lead mine: this I have is very transparent, but looks a little whitish, and smells of sulphur. I am promised some stones, which being made of water that congeals as it drops, are yet all of them hollow in the middle. These are rarities seldom met with or heard of, therefore I inform you of them; which, if you affect, I can sometimes procure you, and shall be glad if I can have any occasion of serving you, or gratifying you for the singular favours you have often done to

your most affectionate friend and servant,

JOHN FLAMSTEED.

This letter is printed in the Gen. Dict. vol. V. p. 256.

CLXXXI.

FLAMSTEED TO COLLINS.

Derby, August 5, 1672.

Mr. Collins,

Your last I received on Wednesday, and, having then a letter new-written to Mr. Townley, I sent the included papers of yours to him, according to your desire. When he returns them, I shall impart them to Mr. Halton, to whom I believe they will be as welcome as to myself. I thank you for them, and shall be glad sometimes to hear what is performed betwixt your friend and you.

But the business of this letter is to let you know, that you mistook my meaning concerning the letter I mentioned in my Epilogus, which was not that of Dec. 20, 1638, but one of Crabtree to Mr. Gascoigne, dated Junii 21, 1642, in which he describes Mr. Horrox's system upon the author's second thoughts, which he might find in his exercises, and his calculations on some loose papers. This differs much from that in the Dec. epistle; for in that he makes [1.] the greatest equation of the apogæon less than in his later papers; 2. the greatest physical equation 5° 10′, the greatest optical 2° 30′; whereas afterwards he made either of them 3° 50′, equal to each other. 3. In that epistle he directs to take $\frac{5}{6}$ of the Tychonic variation, but on afterthoughts he assumes $\frac{9}{10}$. Lastly, that calculation is compiled by aid of Lansberg's and Kepler's tables, which in this epistle is done more artificially by trigonometry. Add, that he directs no correction of the mean motions, which yet I find in his exercises and in Crabtree's letter. These considered, the difference betwixt this first system of Mr. Horrox in the letter of Dec. 20, 1638, and that collected from his later papers by Mr. Crabtree is so wide, that I believe you would not think that fit to be placed before tables, from which they differ so much. I have therefore translated so much from that epistle of Crabtree as concerns the system, and trigonometrical method of calculation, which you may cause to be printed before the tables, which are framed from them and agree punctually with them.

I am, without further compliment,

your affectionate friend to serve you,

JOHN FLAMSTEED.

P.S. I have only translated the Epistle; for a title-page the Doctor or you may write what you think convenient. Keep the glass till you have occasion to send to me by the carrier. I shall try to get a prism at our glasshouses, since they are not easy to be had with you, as I supposed them.

J. F.

This letter is printed in the Gen. Dict. vol. V. p. 256.

CLXXXII.

FLAMSTEED TO COLLINS.

Derby, August 13, 1672.

Mr. Collins,

I have yours of the 10th instant, with the glass, from the carrier. I could wish you had not paid for it, for the carrier will be paid here; and, Mr. Litchford making much use of him, he is kindlier used than either you or I can be.

I am glad you are satisfied of Horrox's system by the letter I sent you. The precepts I found translated by the ingenuous Mr. Shakerly, which I transcribed from him, because I thought them clearer expressed than the English ones in Crabtree's letter, though they are in substance the very same. I remember not that he bids, any where, double the cosine of an arch, or that he uses natural numbers at all. I doubt you mistake, and that the third precept may have caused your error. It is, 3dly, Dupliceter argumentum annuum, and *duplicati cosinui*, &c. The last two words I fear you read *to the doubled cosine*, by which nothing can be understood, but *to the cosine of that argument doubled*, which is clear another sense and sound enough.

For his method of calculating the prosthaphæresis of the orb, it is so framed as that it may include that little arch, which Streete applies at the focus of mean motion, and calls the variation; otherwise the difference betwixt Anomalia Media and Anom. excentrica doubled, as Crabtree finds it, will give Bp. Ward's elliptic equation, or Streete's without the variation.

As for that inequality in the Moon's motion, which Horrox calls the variation, and, in his letter of Dec. 20, confesses he knew not how to apply, and Crabtree alike in that of June 21, 1642, I esteem it purely physical, and have not forgotten to say something of it in the Epilogus to the tables, as you will find if you peruse it again. Nor can Horrox be thought to have judged it otherwise on his afterthoughts, since he makes no use of it in computing the Moon's distance from the earth, which, if it were not only physical, it must necessarily vary.

I thought Crabtree's precepts sufficiently clear for calculation; if they shall not be so to some, our tabular method and directions will explain them, and render them easy: though we need not fear that any will give themselves the trouble to calculate by them trigonometrically, when the tabular calculation is so easy, accurate, and expeditious.

I think 'tis not anyways evident from Crabtree's letter, that his friend Mr. Horrox died not the 3d of Jan. 1640, except I have amiss translated something: pray peruse it again, and let me know. For besides the note on the back of the letters, I find that, in a letter of Crabtree's to Gascoigne, dated March 18, 1640–1, he much laments the death of Horrox. Mr. Gascoigne was slain in our wars, I believe, in the year

1642, for I find no letters either of his, or Crabtree's to him, after that of Junii 21, 1642, if I have not miswritten. Crabtree lived much longer, I believe till 1652, if his neighbour Mr. Wroe informed me truly. I shall to-morrow write to Mr. Townley, and will make one part of my desires to be ascertained of the time of his death.

Present my services to Mr. Oldenburg, and tell him I shall be careful to make inquiries of the old people, and how they have lived, as he desired, but, having no great acquaintance, I fear I cannot procure any long list. I waited last night[u] for the sun's eclipse, but it was such cloudy and rainy weather I could never see him plainly after five o'clock. I am, Sir,

ever yours,

JOHN FLAMSTEED.

P.S. When I came to compare Horrox's own system in the letter of Dec. 20, 1638, with his in Crabtree's letter, June 21, 1642, I found that Crabtree had demonstrated it in Horrox's own words from that letter in Dec. —38. I mean the libration of the apogæon, concerning which what I had written to you formerly I intended once to have composed into the Epilogus; but, upon second thoughts, judged it better to take your opinion of it. I consent you should insert it, since I perceive you esteem of it not amiss, and I doubt not but hereafter to have something that may further illustrate and confirm it, if occasion be given.

J. F.

This letter is printed in the Gen. Dict. vol. V. p. 257.

[u] Sic in MS.

CLXXXIII.

FLAMSTEED TO COLLINS.

Derby, Feb. 20, 1672–3.

Mr. Collins,

Some affairs of my friend Mr. Litchford, to whom you direct my letters, drawing him up to London, not having written [to] or heard from you of late, I thought this a good opportunity, and not to be slipt, that I might inquire what is done among you. With us Mr. Halton is translating Kinkhuysen's Moone-Wiser into English, that I may have a view of it; and at Mr. Townley's request I have lately wrote and sent him the History of Malting. I have begun that tract concerning the distances and diameters of all the planets, which I promised in my letter to Mr. Oldenburg, but shall go but very slowly forward, by reason that sometimes my infirmities, oftener affairs, interrupt me; but this I have certainly learned from my observations, that the sun's parallax is not above 10″, yea, probably but 7″, and his distance a terra 26000 semidiameters, which is a distance to which none ever durst remove him yet, and thrice as far off as I supposed him formerly in my solar tables. You will let me know what you hear of Tycho's volumes, and when they may be expected.

I am ever, Sir,

your affectionate friend and servant,

JOHN FLAMSTEED.

This letter is printed in the Gen. Dict. vol. V. p. 254, where it is erroneously placed among those of 1671–2.

CLXXXIV.

FLAMSTEED TO COLLINS.

Derby, March 19, 1672-3.

Mr. Collins,

I have received from you, by Mr. Litchford, the translation, and learned comment on Manilius, I suppose the work of Mr. Sherburne, but the beginning and appendix are wanting. I desire to know whether they are yet wrought out of the press, and what it was you informed Mr. Litchford concerning them, for he, being cumbered with much other business, has forgotten that discourse.

I desired him either to buy me a rule of proportion, or procure you to buy me one. He returns me answer that you would inform me of a device of yours, by three rulers to find the azimuths and hours, or other proportions, more exactly than by that ruler; which I can easily conceive to be true. But my desire is to have one only for finding proportional parts, and making other gross calculations, for which I conceive the three graduated rulers will not be so convenient as that instrument, by reason they are accommodated to some particular latitudes, and for finding the azimuths and hours only, which I can find otherways by some of your or Mr. Halton's quadrants. Pray, therefore, procure me one of those double rulers of proportion, about two feet long, according to those directions. You have five shillings of mine in your hands, if you lay down what more it costs you, next time Mr. Sargeant comes to town you shall be paid, or perhaps sooner.

I understand that my letter to Mr. Oldenburg was left in your hands by Mr. Litchford. I never had any answer either to it or a preceding one. I should be very glad to hear from him, and what news you have.

I am ever, Sir,
your much obliged friend and servant,
JOHN FLAMSTEED.

I desire to know whether you have still a correspondent in France; as also whether Fermat's works may be, any of them, had in London.

CLXXXV.

FLAMSTEED TO COLLINS.

Derby May 5, 1673.

Mr. Collins,

I have received the box ruler I wrote for, which I like very well, and give you thanks for your pains in procuring it: the price you write is ten shillings, of which you have five in your hands, so that I must be five shillings your debtor till the back of Whitsuntide, when Mr. Sargeant comes to London, who will pay you it. Pray let me know the price of the other ruler you sent me formerly; if I like the rate I shall keep it, otherwise I can return it by Mr. Sargeant.

With the ruler I received the sheet omitted in Mr. Horrox his book, without which I think it may do as well as with it; for the corrections in Crabtree's letter, I find by Mr. Horrox's exercises, are the very same he intended, only by this addition we shall see the progress of Horrox's endeavours, and how he framed that in the rough, which he after mended, but left to his successors to polish and perfect.

I have spent my spare hours of late in correcting Kepler's numbers in the planet Mars, so as they may represent my observations, which I think they will do very accurately; I mean those of September last, which I made for finding his parallax and the sun's. To make them fit all others is impossible, by reason that the places of the fixed stars are erroneously sometimes stated by Tycho; but if we should once be so happy as to have them restored by telescope-observations, on large instruments as Tycho's, I should not doubt, in a short time, to frame numbers that should represent all the celestial appearances, at least of our age, and perhaps the next, very exactly. What I have done I think will fit all observations as well as any; but my great desire is to have them represent my own well, which, being made with larger and more accurate instruments than were ever used before Mr. Townley's time, must needs be more exact than any before his. I hoped to have had a many accurate observations this spring of all the planets, but the clouded heavens have almost constantly deprived me of my foreseen opportunities in all but Jupiter, of whom what I have observed Mr. Oldenburg can now inform you. No more but my hearty respects till Mr. Sargeant's journey.

I am, Sir,
you obliged debtor and servitor,
JOHN FLAMSTEED.

This letter is printed (without the beginning) in the Gen. Dict. vol. V. p. 257.

CLXXXVI.

FLAMSTEED TO COLLINS.

Derby, July 7, 1673.

Mr. Collins,

I wrote to you about a week agone to bespeak me such glasses as I described to you in that letter. I have now sent you forty shillings to pay for them, if they be ready; it will do somewhat more than pay, if Mr. Cocks use me kindly. What there shall remain in your hands I may, perhaps, have occasion to use ere long.

I might have informed you in my last, that after I had discovered the sun's distance in September last to be near 21,000 semidiameters, I place it just so much in perigæo; then will his parallax be just 9.82″, which is precise the hundredth part of the sun's semidiameter there, i.e. 16′ 22″. So that I find the earth is but the millionth part of the sun. This caused me to think of putting it in a problem, which I did to puzzle some boasting pretenders to skill, thus: The sun's distance from the earth is 21,000 of the earth's semidiameters, whose body is but the $\frac{1}{1000000}$ part of the sun; quære, what is the sun's semidiameter? Pray let him hear from you, on the receipt of this, who is

your affectionate and obliged friend,

JOHN FLAMSTEED.

This letter is printed in the Gen. Dict. vol. V. p. 257.

CLXXXVII.

FLAMSTEED TO COLLINS.

Derby, July 26, 1673.

Mr. Collins,

Thursday last I received a letter from Mr. Gregory about the Scotch observatory and instruments. I had written a good way of an answer, but when I came to describe my new micrometer, I find my papers, in which I had described it, removed. I have not time to search for it at present, and am on Monday next to go some thirty miles from home, but shall return within three days, God willing. After, I shall search for my papers, and return him an answer; in the mean time, yesterday, I wrote to Mr. Townley to send you his advice, and think it convenient you would visit Sir Jonas Moore, who has one of Mr. Townley's micrometers, which Mr. Gregory may there see fitted to the tube—better, if I mistake not, than by Mr. Hooke's. Sir Jonas can also inform him, where he may procure one of Mr. Townley's instruments made, for he knows the workman. If you visit him, present my most humble service, and tell him I shall ever gratefully acknowledge the kindness of that gift, and his good will to astronomers and the science of astronomy.

Present my services to Mr. Oldenburg when you see him. He informed me, that he had a piece came late from one Mr. Scivers, of Hamburgh, which if I was minding to peruse, he would send me. Pray desire him to deliver it to you, that you may send it with the glasses when they are ready: I shall return it safe. I desire to know whether my money will hold out to pay for them, and what I owe you. My humble services to Mr. Gregory and yourself is all at present from

your very affectionate servant,

JOHN FLAMSTEED.

This letter is printed in the Gen. Dict. vol. V. p. 257. Upon it Collins has written, "Mr. Dary hath occasion to use this "proposition in gauging;" in a given ellipse to find the semi-diameter drawn to that point on the circumference, from which the chords will be equal, which are drawn to the vertices of the major and minor axis. He gives a diagram with specific references to it; but no solution of the problem.

CLXXXVIII.

FLAMSTEED TO COLLINS.

Derby, August 20, 1673.

Mr. Collins,

I forbore to give you an account of my receipt of the glasses, because I was minding together to inform you how they would perform. I tried my object glass last night, but the air being very bad and hazy, I could not make any good estimate; but I hope it will prove well. The other are as good as can be desired, or expected of that length, but I am sorry Mr. Cocks used me so unkindly as that my money would not extend to discharge that small debt I owe you, which, I think, is five shillings. He took but seventeen shillings formerly for as good glasses for the short tube of seven feet, and now has taken twenty shillings, and as much for the object glass. If he use me thus he will lose a customer. However, I am very much obliged to you for your pains, and shall take care to pay you my small debt next term.

If Vlacq's Canon would be had for a crown or a noble, I should be glad of it, but I cannot go beyond that rate, for my father takes notice of my expenses, and I am now at the outside of my allowance. I have more than half finished my Hecker, for so I call my Ephemeris, and, after the next week, hope to get time to conclude him, so that you may have him before Michaelmas. My service to Mr. Gregory; if he be still in the city, I shall be glad to know what instruments and books he takes into Scotland with him, and what other news you have, whereof you may inform,

Sir,

your much obliged friend and servant,

JOHN FLAMSTEED.

The latter part of this letter is printed in the Gen. Dict. vol. V. p. 258.

CLXXXIX.

FLAMSTEED TO COLLINS.

Derby, Sept. 29, 1673.

Sir,

This day I have received one from you, by which I understand you have found those tables of Vlacq's I desired, which I am glad of, since the person who lent me them has called them home, and will no longer spare them to me: but I perceive I shall be then something indebted to you, which I shall procure to be paid you at the term. I am very well pleased with the news of Hugenius his treatise being come over, of which, had

I money in your hands, I would have one; but more to hear of Cassini's book of refractions being expected. I am of his opinion, that they ought to be continued to the zenith, because the refractive air reaches some height above our heads; but from Tycho's observations, such as I find in Ricciolus his Almagest, and some late experiments of mine own, (which I intend to repeat as soon as I can borrow a convenient room,) I have good reason to think that he makes them too large a little in the horizon, and their decrease over slow, or themselves much too big in altitudes above thirty degrees. I shall shortly write to Mr. Townley, to engage him to make some such experiments as I have thought of, and perhaps, if mine own succeed, I shall, in the mean time, let you know the event of them. This day was a sevennight, in the morning, I took my baroscope from home to Allhallows steeple, at the bottom of which the quicksilver in the tube stood then 29.24 inches above the superficies of the stagnant Mercury, but at the top, 142 feet higher, I found it stand only 29.10, or 29.09. Bringing it to the bottom again, it returned to 29.23, so in 142 feet height it varied 0.14 of an inch, and shewed the air $\frac{1}{209}$ part lighter[b] at the bottom than at the top. I have heard of some such experiments made at London on high steeples: if you can inform me of any, you will do me a kindness.

I wish I could have anyways assisted Mr. Sherburne's Catalogue with particulars of Mr. Gascoigne's life, who, I believe, was the first that applied screws to telescopes. He made with them several curious observations (ab anno 1638 to 1642). Such as could be found in his letters I have digested into a small book, with notes of my own to them, where they were needful, for my own use; but I fear we have lost many

[b] [Heavier.]

more, and that past hopes of recovery. I shall be very desirous to see both the Apollonius and Archimedes of Dr. Barrow, and to receive a line or two, with my book, by the carrier.

I am, ever,

your much obliged and affectionate servant,

JOHN FLAMSTEED.

This letter is printed in the Gen. Dict. vol. V. p. 258.

CXC.

FLAMSTEED TO COLLINS.

Sir,

I have received Vlacq's tables, and Hugenius De Horologio, &c. by our carrier, for which I find I shall be in your debt twelve shillings and sixpence, which I shall get a friend to pay you at the term. By the same person I intend to send you my Hecker, which I hope to finish either this day or to-morrow. If that my father's affairs had not pressed me extraordinarily, I had long since finished it, but hope now will be soon enough. I find a table of the equation of natural days in Hugenius his piece, seemingly much different from mine, from which it keeps a constant distance of about fourteen minutes of time, which that ingenuous person has so contrived, that the equation may always be of the same species. I doubt not but that though it be seemingly different, I shall find it on further examination to agree exactly with ours, but not fit to be applied to astronomical calculations, or any experiments, but those about his watches: but of these more hereafter. In the mean time, when you find an opportunity to write to me again, I shall be glad to know what Mr. Gregory has done in reference to the instruments he was about procuring, and what is the opinion about my alteration of Mr. Townley's micrometer, which is all I have at present, but to subscribe myself

your much obliged servant,

JOHN FLAMSTEED. Derb.

This letter is printed in the Gen. Dict. vol. V. p. 258.

CXCI.

FLAMSTEED TO COLLINS.

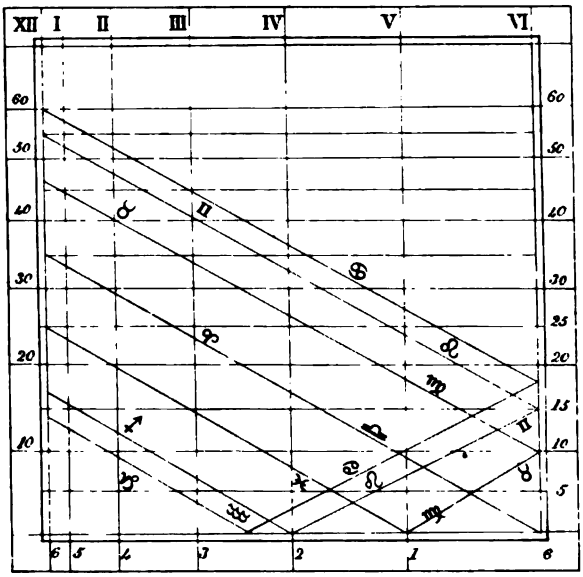
Derby, Dec. 27, 1673.

Sir,

Yours informs me of the reasons why my Ephemeris is not printed, with which I am very well satisfied. But, if you have not already done it, I would not have you propose the printing it at the charge of the Royal Society, lest they lose by it, to whom I am so much already obliged for their respects, that I desire not to be made their further debtor. As for Mr. Gregory, after I wrote the letter to him, with the description of the micrometer in it, I never heard anything from him, and therefore forbore to write, supposing he would, if desirous to have any correspondence with me, have given me an occasion to continue it by some answer to mine. I desire to know from *you* what instruments he took into Scotland. If he desire any intercourse of letters, if he please to give me a good occasion, he shall ever find me ready to satisfy and comply with his desires. —— I thank you for your

Plate 5.

to face p. 171.



other informations, and shall request you to continue them as you have occasion.

Lately, in discourse with Mr. Halton, he was pleased to shew me a straight-lined projection for finding the hour by inspection, the sun's declination and height being given; but concealing the proportion from which it was derived, gave me occasion to vary it into this form (plate 5), of which I need say no more to you than that it is derived from this known proportion, demonstrable from the analemma:

As the difference of the sines of the sun's meridional altitude, and his altitude at 6 : is to the radius or 6 hours : : so the difference of the sines of the said altitude at 6, and the altitude given : to the sine of the hour from six.

By this sinical projection, which I have drawn with a radius of ten inches, I can find the hour of the day to a minute, except when the sun or star is near the meridian; and therefore I find it of great use to me, when I have not time to make use of my pen, to calculate the hour. It were easy to add the ecliptic to it, and parallels of latitude for five or six degrees on each side, which I intend to draw in some other-coloured ink in a large projection of this sort, of twenty inches radius, when the holidays are over. My success in this has caused me to set my proportions for finding the parallaxes in altitude, longitude, and latitude on such scales as they would permit, by which, if the planets be in the ecliptic, I can find them to half-a-minute's exactness by bare inspection. If they have latitude it is more troublesome, but I hope to facilitate it much, of which at better leisure you may hear more from, Sir,

your affectionate friend and servant,

JOHN FLAMSTEED.

The early part of this letter is in the Gen. Dict. vol. V. p. 258.

CXCII.

FLAMSTEED TO COLLINS.

The Observatory in Greenwich, May 25, 1677.

Sir,

I here send you the account of the experiments I promised you, which were made with the steel bow. It requires more time than I have to spare to delineate all the parts of the machine for discharging the bullet from it, and I suppose would not be very much desired by your friend. I shall run them over, in short, and I hope to your satisfaction. The place on which the shots were made was a level before the observatory, twelve score yards long or better, which has at each end a descent; on the western end of which a frame was fixed in the earth, so as the bow, being laid parallel to the horizon, might be also even with the plane of the earth: and on this frame was a piece of wood hung on a centre. To this a toothed arch was fastened, by the help of which, and a worm screw, the piece of wood which was to receive the bow, might be raised or depressed easily and conveniently, and thereby itself set to any inclination. The bow was of three good steel laths; the string, two thirds of an inch thick, had a small wooden plug fastened by one end to the middle of it; this was about a foot long or more, and had the other end somewhat bigger, almost as wide as the hollow of the cylinder, from which it cast the shot. This cylinder was about eighteen inches long, and so placed as that, when the string was drawn up, the end of the plug which served to discharge the shot might fall within it about one inch. The string was forced up by the help of a long worm screw; and to take the inclinations of each shot there was provided a cylinder of wood, some seven or eight inches longer than the hollow brass one, which it was so turned as to fill, when it was put into it. To this a quadrant of seven inches was fastened, with a thread and plummet for taking the inclinations. At the first trials the wind was very little, or rather a perfect calm; at the latter it blew sometimes briskly, which was the reason, as was thought by gunners, why all the upper ranges fell short of the like made but four days before; and I cannot but allow of their opinion, for all the under ranges were defended from the wind by the hill, the upper, flying above it, were not.

The length of every shot was taken by a chain of twenty-two yards, or sixty-six feet, divided into an hundred links; the bullets were of brass, nearly all of the same bigness, and eleven ounces weight; but if any was heavier than another, that shot at the same range always fell nearer the bow.

Elevation.	Range. Ch.	Links.	Elevation.	Range. Ch.	Links.
°			°		
5	1	89	85	0	62
10	3	2	80	1	78
15	4	5	75	2	87
20	4	88	70	3	87
25	5	59	65	4	74
30	6	13	60	5	46
35	6	50	55	6	16
40	6	76	50	6	44
45	6	70			

Again, after four days,

Elevation.	Range. Ch.	Links.	Elevation.	Range. Ch.	Links.
°			°		
5	1	85	85	0	53
10	2	98	80	1	67
15	4	8	75	2	73
20	4	91	70	3	65
25	5	61	65	4	52
30	6	17	60	5	23
35	6	32	55	5	84
40	6	39	50	6	25

They conclude the longest shot to be made at 42 degrees, and what they find by the shot from the bow, they find also in the arrow shot from it, and in the bullet discharged from the mortar-piece. The longest range in all being about 42 degrees, and the rest of the ranges in one proportional to the like at the same elevations in the other.

I hope I have discharged my promise: if in any thing I may further inform you, any note shall command the services of

your friend and servant,
JOHN FLAMSTEED.

I doubt not you will remember your promise not to let any one know from whom you had these informations. J. F.

CORRESPONDENCE WITH JAMES GREGORY.

CXCIII.

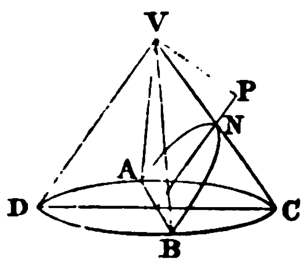
COLLINS TO GREGORY.

Worthy sir,

Mr. Thompson lent me to peruse one of your books, De quadratura circuli et hyperbolæ, and hath sent the other to Dr. Wallis. The book doubtless deserves much applause, and consequently the press. It is the first I have seen of that argument, illustrated in numbers. At the end of a French grammar, I am informed there is a catalogue of mathematical books, amongst which it is said there is one of this title, L'Egalité de la parabole avec un ligne droit par M. Fermat: and if so, as Van Heurat at the end of second edition of Descartes, the hyperbola is squared by necessary consequence. The quadrature of the hyperbola was promised by the Lord Brounker, as in Dr. Wallis his first epistle to L. B., also by Dr. Wallis himself, page 51 Commercii Epistolici, but about it I have formerly received what follows from Mr. Barrow of Cambridge.

* * * * * * *

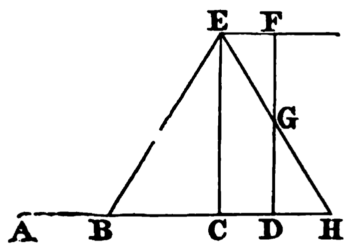
and I have some papers of Mr. Warner deceased, wherein he proves if parallels be drawn to one asymptote, so as to divide the other into equal parts, the spaces between them, the hyperbola, and asymptote, are in musical progression, the which, if desired, I may communicate. The quadrature of the hyperbola is a proposition very necessary in gauging, and consequently of great use in relation to the king's revenue; for many brewers' tuns are like silver tankards, trunci conici circulares, divided into two partitions with a plane erect to the base to hold liquors of different strengths, and also stand stooping, and the quadrature of the hyperbola doth capacitate us to cube any segment of a cone. Let us take such a one as the plane passing through the base doth cut an hyperbola, through the same chord line in the base let another plane pass, so as likewise to pass through the vertex of the cone, then by this latter plane is the cone divided in such ratio as the segments of its base; and the wedge between both planes is $\frac{1}{3}$ of the conic figure whose base is the hyperbola, whose height is a perpendicular falling from the vertex of the cone on the hyperbolic plane: and so in any case where the plane passeth through the base, the proposition being of frequent use. One end of my writing is to excite you to render it easy and practicable; to this purpose I send you some approaches



to examine used by some gaugers to supply the quadrature of an hyperbola, but without demonstration; as,

1. Suppose the triangle through the axis of a cone equal to the area of the semicircle in the base, then it may be queried, if an hyperbola in that cone parallel to the axis be equal, or near equal, to the segment of circle in the base, whose chord is cut off by the same plane: one Mr. Dary asserts a connection between the quadrature of an hyperbola and that of a segment of a circle; if so, then a table of areas of segments, the best of which is in Sybrant Hantz, will be of greater use than imagined.

2. Mr. Dary, being a gauger at Bristol, was necessitated to use this approach: Let BEH be the triangle through the axis of a cone, which cone is supposed circumscribed with a cylinder, and cut by two planes erect to the triangle through the axis, the one passing through the axis, the other parallel to it; and let AB be a radius. Then AD : AB : : segment of the cylinder FECD : the segment of the cone, between those planes, GECD.



I herewith send you a paper of interest I printed for my own use: thereby you see I desire a book of Mengolus that is not here to be had, and that in order to the attaining the sum of a musical progression.

These uses I mention as hoping you will have the honour to solve the problem and add it as an appendix to your book, to which it seems to have a necessary connection.

About irrational quantities there is extant a French tract, De quantités incommensurables, et sur la dixième livre d'Euclide, by the Sieur de Taneur, à Paris, 1650.

Il Antanalisi di Salvator Grisio (or Grisius) against Maghet, printed at Rome, 1644, and Dibuadius.

At the end of your book you speak of curves for effecting of equations; in order thereto I think fit to acquaint you what hath been lately done about it.

Slusius in Mesolabio.

In a letter to Mr. Henry Oldenburg, Secretary of the Royal Society, (who desires your correspondency,) he thus writes:—

One of the late P. Transactions gives notice of a book newly published in France, which is not yet come over.

F. Dulaurens.

One John Henry Rhonius published an Algebra in high Dutch. He was Dr. John Pell's scholar; the book is translated into English, refined by the Doctor, and almost out of the press; I hope to send you one of them, but therein are not contained some of his chiefest inventions in Algebra, which are:

* * * * * *

About my paper of interest, the third proposition of simple interest; the intendment of it is to find in what time a sum of ready money, at simple interest, shall amount to any sum proposed, though it be proposed   Equation of Payments, whereas there can be no such thing at simple interest.

Prop. 9. About compound interest Prop. 9 may be more easily performed. What I mean by logarithm curve I may take from Mersennus, and conceive the nature of logarithms is more naturally explained thereby than by the hyperbola. In præfatione ———

Sir, it was once my good hap to meet with you in an alehouse, or in Sion College, and though I have not been educated at universities, and so my attainments are mean, yet I have an ardent love to these studies, and endeavouring to raise a catalogue of mathematical books, and to procure scarce ones for the use of the Royal Society and my own delight, I crave your assistance in procuring what I mention in this letter, or the enclosed paper. As for Borellius de vi Percussionis, Ricciolus, Renaldini Algebra, Direttione di fiumi, a bookseller hath them coming. One Mr. John Collins, my namesake and intimate friend, one of the yeomen of the chaundry to the king, informs me that one Mr. Joseph May, a merchant in Venice, is his brother-in-law, by whom I can remit to you money for this purpose, and your aid herein will be very acceptable to Sir Robert Murray, who is much my friend, and to whom I am much obliged. Mr. Anderson of Aberdeen, in his mathematical exercises, printed at Paris, 1619, mentions his Stereometria, which Guldinus bewails and excites others to inquire after. Sir Alexander Hume informed [me] he had it, but is sure 'tis lost: however he still hath his Stereometria Triang. Sphæricorum, whereof I have taken a copy and hope to get it printed, but could wish you would furnish me with his sheet against Kepler's Stereometria to reprint therewith, it being nowhere else to be had. I can give you an account of many books, printed lately at Paris, and some of them for private persons, which I wish I could have for my money, and perchance you will desire to meet with some of them in your return. I am now in a troublesome employment that prevents my studies, to wit, chief accountant to the commissioners for examining the accounts of the late war, and manage an accountantship in the Excise office by a substitute; so that, if you vouchsafe an answer, direct it to me as an accountant at the Excise-office in Bloomsbury, to be left with Mr. Bourne at the posthouse. I remain ——

CXCIV.

JAMES GREGORY TO COLLINS.

Padua, 26 March, 1668.

Mr. Collins—Sir,

I am sorry that the short time I have to stay here should render me incapable to serve you in relation to the books you writ of. I have seen several of them, and imagine they will hardly answer your expectation. For any thing I can understand, none hath writ against Viviani de Maximis et Minimis, but his book is generally little esteemed. As for myself, I never did read any thing of it upon that account. Antimus Farbius (as I hear) hath published none of these things he promised. I believe none knoweth what papers of Soverus there are in the library here; for those, that can understand them, will not go to see them, fearing that, if they should publish any thing of their own, it should be thought stolen from Soverus writes.[k] However, I hear for certainty he hath papers in this library. As for these seven books of Diophantus I do not hear any thing, nevertheless that I have asked several. Michael Angelo Riccio hath writ De Maximis et Minimis, only in two sheets of paper, but to extraordinary good purpose. Cassini hath observed

[k] Soverus' writings?

the motion of Jupiter circa axem in ten hours, and of Mars in twenty-three hours, and hath writ concerning them: he hath also observed the motion of Venus circa axem, but knoweth not yet the period precisely. He hath lately published astronomical tables for the Stellæ Medicææ, with an Ephemeris of the same for this year, all which are much applauded.

I did write to Mr. Thompson in September or October last, and admired much I had not heard from him. In the mean time several stationers had writ here to Padua for books, after I had scattered these hundred and fifty I had printed through the world; and I, seeing the stationers here minded to reprint it, adjoined this treatise, which I do here send to you enclosed in four packets, two unto you, and two unto Mr. Thompson, not having any occasion but the post, and being shortly to go from hence. The stationer hath printed five hundred with a privilege, for the state of Venice. I have done so to satisfy your curiosity, (for there are several things in it relating to what ye writ of,) as also to give Mr. Thompson occasion to regulate his affairs. However, if any of you take it ill, I shall pay you back your charges at meeting. These methods ye propose to measure an hyperbola cannot serve without sensible failing. Mr. Warner his demonstration (viz. if parallels be drawn to one asymptote, so as to divide the other into equal parts, the spaces between them, the hyperbola, and asymptote, are in musical progression) is false, for the parallels are in musical progression, but not the spaces. I do not understand that problem of Mr. Barrow. As to that equality betwixt a right line and a parabolic, without supposing quadratura hyperbolæ, I know nothing, for I never did see it. I never did see any thing of Anderson save in print, but I am confident I did read something of him in print de Stereometria Kepleri, of which I remember nothing else, it being a long time ago. I do exceedingly esteem these problems concerning equations, which ye say Dr. Pell and others have discovered. All the propositions of my book De circuli et hyperbolæ quadratura, after the eleventh, are for facilitating the practice, which I apprehend I bring no small length, seeing I teach in the 53rd page to make any logarithm by one multiplication, two divisions and one extraction of the quadrate root, neither have I any hopes to make the practice any shorter. The real and true way to measure an hyperbola is (as Schotenius says) by a table of the segments of an hyperbola, whose asymptotes make a right angle, for this hyperbola hath the same relation to others, which the circle hath to ellipses. The logarithm curve ye speak of (as I apprehend) is not so fit for the calculation of the logarithms as the hyperbola, but more fit for the mechanical practice, as I take notice in my proœmium. No more at present, but entreating you to pardon my brevity, being in haste, I rest,

your most humble servitor,

James Gregory.

CXCV.

JAMES GREGORY TO COLLINS.

Sir,

Be pleased to write to Dr. Wallis and Mr. Barrow, that I was not satisfied with that which Dr. Wallis did write[1] unto you, viz. that I have demonstrated only the quadrature of the circle to be impossible

[1] See Dr. Wallis's letter to Collins, dated Sept. 8, 1668.

analytically by my method. For I apprehended that in my 10th Prop. I had given a sufficient base for an absolute demonstration of the following consectary, only from the general proprieties of all converging series; but seeing Mons. Hugenius misunderstood me, I have set down the same demonstration in the Trans. fol. 734., which I pretend to be geometrical and absolute, not relating to my method or any else; if otherways, I desire of them to be informed of it, as also of any thing else they think false, or not fully demonstrated in that treatise.

CXCVI.

J. GREGORY TO COLLINS.

St. Andrew's, 20 Jan. 1669.

Sir,

I have lately received yours of the 7th of this instant. I cannot but acknowledge myself infinitely obliged unto you for sending unto me this extract of Hugenius his letter, but especially for the pains ye have been at in drawing up the state of the controversy betwixt Hugenius and me. I confess it the best way imaginable to publish my late answer, neither can I imagine how that can be reasonably opposed. I should be glad to see Mercator his new series for the circle, and these new additions to Diophantus. I must have my controversy ended before I publish my Optics and Astronomy; for I have several things in my head, as yet only committed to my memory, neither can I dispose of myself to write them in order and method till I have my mind free from other cares. I thank you and Jonas Moore very heartily for your proffer, but if my answer be not published in the Transactions, I am fully resolved (upon several accounts too tedious to be related here) to publish it in Edinburgh. No more at present, but that I am, Sir,

Your very humble,

and most obliged servitor,

J. GREGORY.

Some days before Christmas I sent to Mr. Oldenburg an addition to my former answer, which (because ye have not mentioned) I am afraid is lost, by the post; and therefore (it being the most considerable and substantial part of my answer) I do here repeat it: it follows the words " nam talis compositio qualis resolutio," interposing a period and beginning a new line.

Etiamsi prædicta, meo quidem judicio, abunde sufficiant; ut tamen nullus relinquatur cavillationi locus, undecimam nostram propositionem etiam in quantitatibus definitis hic demonstrabimus. Sit ergo B polygonum intra circuli sectorem, 2B polygonum circumscriptum, et priori simile, sufficit enim polygonorum proportionem definire. Continuetur series convergens, ut sit ejus terminatio, seu circuli sector, Z. Dico Z non posse componi analytice ex polygonis definitis B, 2B. Si fieri potest, componatur Z analytice ex polygonis definitis B, 2B; sintque duæ quantitates indefinitæ a et x, e quibus componatur m eodem modo quo Z componitur a B, 2B; item eodem modo componatur n ex quantitatibus $\sqrt{ax}$, $\frac{2ax}{a+\sqrt{ax}}$. Quantitates m, n non sunt indefinite æquales ex propositione undecima. Si igitur inter m et n fingatur æquatio; a manente quantitate indefinita, æquatio inter m et n tot habebit radices seu quantitates ipsi x æquales, quot quantitatum

B	2B
C	D
E	F
G	H
Z	

(inter se diversas rationes habentium) binarii sunt in

$$a \quad x$$
$$\sqrt{ax} \quad \frac{2ax}{a+\sqrt{ax}}$$
$$m$$
$$n$$

rerum natura, quæ vices quantitum a x subire possunt, hoc est, quæ quandam quantitatem analytice ex se ipsis componunt eodem modo, quo eadem quantitas componitur ex ipsarum media geometrica $\sqrt{ax}$, et ex media harmonica inter dictam mediam geometricam et x, nempe $\frac{2ax}{a+\sqrt{ax}}$, ita ut compositio sit eodem modo quo Z componitur ex B et 2B : atque, ex const. decimæ, omnes quantitatum binarii B 2B, C D, E F, G H, &c. in infinitum, possunt supplere vices quantitatum a x, quoniam Z eodem modo componitur ex B 2B, quo ex C D, E F vel G H, &c.; et proinde æquatio inter m et n infinitas numero habet radices. Sed omnis æquatio habet ad summam tot radices quot habet dimensiones, et proinde æquatio inter m et n infinitas numero habet dimensiones, quod est absurdum ; et igitur Z, seu circuli sector, non potest analytice componi ex polygonis definitis B et 2B, quod demonstrandum erat. Hinc manifestum est terminationem cujuslibet seriei convergentis, si non possit componi analytice ex terminis convergentibus indefinitis, nec posse componi ex eisdem definitis analytice; atque ita evanescit, simul cum nostra distinctione, prima Hugenii objectio.

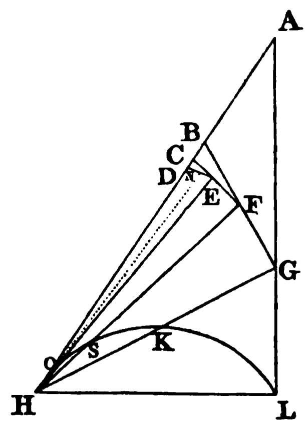
CXCVII.

J. GREGORY TO COLLINS.

St. Andrew's, Feb. 15, 1669.

Sir,

I perceive by Dr. Wallis his last to me that he is not satisfied with my answer to Hugenius, if so be he hath seen it. However, if any man object against it without denying some vulgar truth either in geometry or analytics, I have nothing to reply. As for my exercitations, I am not curious when they be mentioned in the Transactions, but my approaches to perimeters, pag. 8 and 5, are somewhat illustrated in my late answer to Hugenius: however, for your further satisfaction I explicate them by another method, so:



Sit arcus quilibet circularis semicirculo minor HKL,[m] cujus chorda HL; ducatur recta HA tangens arcum in puncto H, sitque angulus ALH rectus; deinde recta HG dividat arcum HL bifariam in K, sitque angulus HGB rectus; et recta HF dividat arcum HK bifariam in S, sitque angulus HFC rectus; item recta HE, dividat arcum HS bifariam in O, sitque angulus HED rectus, et ita de cæteris in infinitum: arcus HKL erit major quam HL et minor quam HA, item major quam HG et minor quam HB, item major quam HF et minor quam HC, item major quam HE, et minor quam HD, &c. [in] infinitum: erit quoque arcus minor quam $\frac{96HG-22HL+HA}{75}$, item minor quam

[m] The diagram is on a separate paper, and Collins has written on it, "Schooten Prop. Geom. "20. If an angle be bisected, "the rectangle of the legs is "equal to the rectangle of the "segments of the base + the "square of the bisecting line. "Prop. If the said line bisect "the base, then the squares of "the legs are double to the "square of the bisecting line "and of half the base. Prop. "18."

$$\frac{16HG - 3HL + 2HB,}{15}, \text{ et major quam}$$

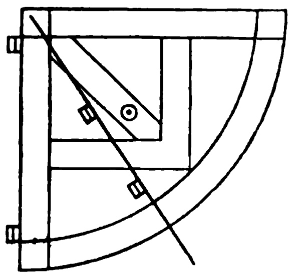
$$\frac{320HG + 52HB - 56HL - AH}{315}, \text{ et major quam}$$

$$\frac{64HF - 20HG + HL}{45}, \text{ et major quam}$$

$$\frac{4096HE - 1344HF + 84HG - HL}{2835}, \text{ et major quam}$$

$$\frac{1048576HN - 348160^{n}HE + 22848HF - 340HG + HL}{722925}, \text{ \&c.}$$

I would have a quadrant after the enclosed fashion, made of joined wood, with a brazen limb, and a ball and socket (or something else to turn upon) in the centre of gravity, three feet in the radius (1.08 ball and three-footed staff[o]) exactly, and as minutely divided as the quantity can suffer. I do not desire any other scales and divisions on it, save only a needle of a considerable length, for observing the declination of the same upon one of the radiuses or middle, as ye think most convenient. I would have also a chain, two good compasses, and a brazen sector. I desire to know the price of every one of these by themselves.



This [p]opinion of Tacquet is old and easily refuted,

[n] This is written 34160 in the text, with a caret between 1 and 6, but 348160 is written in the margin, and is therefore adopted here. See also p. 189.

[o] 4 is here written in the margin, possibly for the length of the staff. There are several other numbers added in the same way; but they are omitted because it is impossible to conjecture what they refer to. Over the word "needle," a little below, "a foot and box" is interlined.

[p] So in the MS.—Nothing is here omitted.

because the congregation of the rays by refraction cannot be so large as the body of the comet, but the tail of the same is almost always larger than the body. No more at present, but I am,

Sir,

your humble servant,

J. GREGORY.

CXCVIII.

J. GREGORY TO COLLINS.

St. Andrew's, 6 Jan. 1670.

Worthy Sir,

I received some weeks ago two letters from you, to which I had answered before now, had I not been expecting these books, concerning which ye writ, that I so might have given you a full answer. These books ye writ of together with that of Slusius, which ye purposed also to have sent unto me, are the books, in the world, I desire most to see; but as yet I have received none of them. However, I am as much engaged to you, and render you as hearty thanks as if I had received them. I do indeed acknowledge that I never deserved any such favour at your hand. I cannot but desire your pardon for intermitting that correspondence, which was so advantageous to me, and so troublesome and expensive to you. I did it indeed with a great deal of regret and reluctancy, considering that I was not in a capacity here to render you service worth half your pains. In April last I had an answer to Mr. Oldenburg his queries, from one Mr. Bruce of Earlshall here in Fifeshire, but being accidentally in Edinburgh at that time, and seeing the Transactions of February last, I was altogether discouraged, by the lines prefixed to my answer to Huygens, from entertaining any such correspondence. I have since received an answer to the same queries from one Mr. Gordon, a preacher in Aberdeenshire, but am not yet so much a Christian as to help these who hurt me. I do not know (neither do I desire to know) who calleth, in that preface, Hugenius his animadversions of Nov. 12, 1668, judicious, but I would earnestly desire that he would particularize (if he be not an ignorant) in what my answer, which is contradictory to Hugenius his animadversions, is faulty: for in geometrical matters, if any thing be judicious, its contradictory must be nonsense. I do not know what need there was of any apology for inserting my answer, but to compliment Hugenius, and violently (if it be possible) to bear down the truth. I imagined such actions below the meanest member of the R. Society: however, I hope I may have permission to call to an account in print the penners of that preface. I hope ye will excuse my freedom in this particular, which concerneth me so near[ly]. I thank you kindly for your civil proffer of publishing any of my lucubrations: I am now following out, in my public lectures, a full course of mathematics, which if I publish, either altogether or in part, I will add to it what else of my own I think worthy of public view, and print it in Edinburgh. I entreat you to advertise me upon what account my books are suppressed in Italy, as also if Hugenius hath answered, or be to answer any thing. I long earnestly to see the approaches, which (as ye did write unto me the last year) Mr. Mercator hath invented for the circle. I shall examine, according to my power, the spiral ye have described unto me, and when I shall receive Anderson his book, I shall strive to give you a satisfactory examination of it. No more at present, but desiring earnestly an occasion to testify my respects to you, I rest,

your most humble and obliged servant,

J. GREGORY.

There are some press faults escaped in the printing of my Answer to M. Hugenius, which I could wish were mended: viz.

Pag. 882. lin. 5. *pro* hyperbola *lege* hyperbolæ

Item, pag. 884. lin. 3. *pro* polygonis definitis 2B *lege* polygonis definitis B 2B

Item, pag. 884. lin. 8. *pro* $\sqrt{ax}\ \frac{2ax}{a+\sqrt{ax}}$ *lege* $\sqrt{ax},\ \frac{2ax}{a+\sqrt{ax}}$

Item, pag. 884. lin. 11, 12. *pro* quot quantitatum, inter se diversas rationes habentium, binarii *lege* quot quantitatum binarii

Item, pag. 884. lin. 18, 19. *pro* quantitatum binarii, rationes quoque diversas inter se habentium, B 2B *lege* quantitatum binarii, B 2B

I admired much of these two last errors, but the truth is, when I looked [at] my manuscripts, I found them both in my first copy, so that I question not the mistake to be my own.

Item, pag. 884. lin. 38. *pro* componitur *lege* componuntur.

In yours [of] the 15th March 1668–9, ye advertise me that the approximations I sent you in my letter are the same with these in my book, saving one of them, viz. 34160. It is true they are the same with these in my book, and that 34160 should also be as it is in my book 348160, for I intended in my letter no new approximations, but only a new illustration or explication of these in my book, as ye had formerly desired in yours to me, Feb. 2, 1668–9. I imagine that Fermat doth very much, if he illustrate all these places in Diophantus, which were left obscure by Bachet.

CXCIX.

J. GREGORY TO COLLINS.

St. Andrew's, 29 Jan. 1670.

Worthy Sir,

Shortly after I did write my last to you I received the books ye sent unto me, for which I render you very hearty thanks. Mr. Barrow, in his Optics, sheweth himself a most subtle geometer, so that I think him superior to any that ever I looked upon: I long exceedingly to see his geometrical Lectures, especially because I have some notions upon that same subject by me. I entreat you to send them to me presently as they come from the press, for I esteem the author more than ye can easily imagine. If ye mind to write against Mr. Anderson[q], ye have indeed matter enough. He saith in his epistle to the reader, pag. 8, "That the areas of ellipses are in proportion, one to another, as the rectangled figures of their transverse and conjugate diameters," which is false; nevertheless that he cites Archimedes for it. For the areas of ellipses are in proportion of the parallelograms of their transverse and conjugate diameters, of which two opposite angles are equal to the angle betwixt the transverse and conjugate diameter. In the immediately next period he faileth also; for the circle is to the ellipse, as the

[q] Stereometrical Propositions variously applicable, but particularly intended for gauging, by Robert Anderson.—8vo London, 1668.

square of the diameter of the circle is to the foresaid parallelogram. In his Prop. 9, 10, 11, he calleth ZC, NC, HG, the axis of the section, and nevertheless it may be the diameter only, and no axis; and if it be only the diameter, there is no conoid made by turning the semi-section round it. His 19th Prop. is the same with No. 4, in the pag. 66. In his Prop. 20, he saith, "The solid whose base is KGOCA, and altitude APK;" and nevertheless, it is apparent by his discourse that he meaneth the solid generated by all the rectangles of PR in RO; but his impertinences in this kind are innumerable. His 23rd Prop. is all perfect nonsense, for in the beginning he saith its transverse diameter FH, its intercepted axis FC, and the same also he repeats in many other places; but if FH be the transverse diameter, wherefore must not FC also be the intercepted diameter? He does not once mention that the hyperbolas should have the same conjugate diameter, and without that he concludeth nothing. In a word I understand nothing of his No. 1, and No. 3, of his Prop. 23, and see neither reason, sense, nor geometry in them: and therefore I send ye the real demonstration of them both, as they should be, here enclosed. In the 2nd and 4th he doth pretty well, supposing tacitly the tranverse diameters to be the same; and again, in his consectaries, he neglecteth this condition, nevertheless that it be essential. The sum of his 23rd Prop. should be, Portiones ellipsium inter se, et hyperbolarum inter se, habentes basin eandem, et diametrum conjugatam eandem, et diametros transversas in eadem recta linea, sunt sicut diametri interceptæ, seu altitudines: item portiones ellipsium inter se, et hyperbolarum inter se, habentes diametrum transversam eandem, et diametrum interceptam eandem, et bases in eadem recta linea, sunt sicut bases. In the pag. 69, his third condition of like portions of ellipses and hyperbolas, (viz. " if the conjugate diameter of A is to the conjugate diameter of B, as the ordinate of A is to the ordinate of B,") is altogether redundant, for the two former are sufficient, and always bring the third along with them, if the ordinate make the same angle with the diameters, and without this the third condition signifieth nothing. I find nothing he hath concerning cylindric hoofs and spindles, which is not in Gregory of St. Vincent, " in libro de ductibus." His 25th Prop. is nothing but the 21st of Geom. pars univers., which being written by me only in general, he hath applied to a great many several cases. In pag. 96, No. 13, the two first periods are false, and the last, together with the last of the 14th, [and] the third of the 15th, are ridiculous; for who knoweth not that all like solids are in triplicate ratio of their corresponding terms? Pag. 99, No. 17, all is false. In the errata, he hath corrected the proportion between the parabola and circumscribed parallelogram, yet in the end of pag. 72, (upon that same account,) he saith $\frac{3}{4}$ in place of $\frac{2}{3}$. It were no great matter for any, that had leisure and patience, to find many more mistakes; but I think these are sufficient to make Mr. Anderson sensible of his too great forwardness for the press.

I entreat you to thank Sir Robert Murray, in my name, for his sending of these books to me.

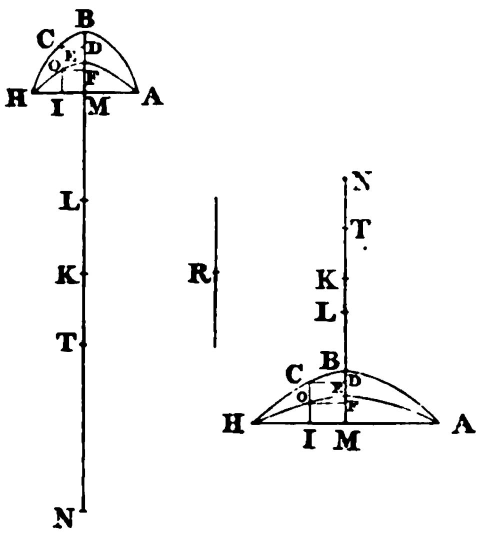
No more at present, but assuring you of my willingness to serve you according to my mean abilities, and desiring earnestly to hear from you, I rest

Your obliged servant,

J. Gregory.

Sint ABH, AEH, duæ portiones ellipsium vel hyperbolarum super eadem basi HA, cui utrique sit eadem ordinatim applicata HM, et eadem semidiameter conjugata R; dico portionem ABH esse ad portionem AEH, ut altitudo seu portio diametri BM ad altitudinem seu portionem diametri EM.

Sit portionis ABH diameter transversa BN, semidiameter BK, item portionis AEH diameter transversa ET, semidiameter EL. Ducatur diametris parallela recta ad libitum COI, et a punctis C, O, demittantur ordinatim applicatæ ad diametrum CD, OF.



Ex natura hyperbolæ et ellipseos

$$KB^2 : R^2 :: NMB : MH^2$$
$$LE^2 : R^2 :: TME : MH^2$$

et permut. $KB^2 : NMB :: R^2 : MH^2$
$$LE^2 : TME :: R^2 : MH^2$$

(11. 5.) $KB^2 : NMB :: LE^2 : TME$

(5. 2.) in ellipsi dividendo

$$KB^2 : KB^2 - NMB (= MK^2) :: LE^2 : LE^2 - TME$$
$$(= LM^2)$$

(6. 2.) in hyp. convert. componendo et rursus convertendo

$KB^2 : KB^2 + NMB (= MK^2) :: LE^2 : LE^2 + TME (= LM^2)$

(22. 6.) $KB : MK :: LE : LM$

et perm. $KB : LE :: MK : LM$,

(19. 5.) $KB : LE :: MB : ME$.

Ex natura hyp. et ellipseos

$KB^2 : R^2 :: NDB : DC^2$

$LE^2 : R^2 :: TFE : OF^2$

et perm. $KB^2 : NDB :: R^2 : DC^2$

$LE^2 : TFE :: R^2 : OF^2 (= DC^2)$

(11. 5.) $KB^2 : NDB :: LE^2 : TFE$.

(5. 2.) in ellips. dividendo

$KB^2 : KB^2 - NDB (= DK^2) :: LE^2 : LE^2 - TFE (= LF^2)$

(6. 2.) in hyp. conv. comp. et rursus convertendo

$KB^2 : KB^2 + NDB (= DK^2) :: LE^2 : LE^2 + TFE (= LF^2)$

(22. 6.) $KB : DK :: LE : LF$

(et perm.) $KB : LE :: DK : LF$

(19. 5.) $KB : LE :: DB : FE$

(11. 5.) $MB : ME :: DB : FE$

(19. 5.) $MB : ME :: MD (= CI) : MF (= OI)$.

Cum igitur recta COI parallela ipsi BM sit arbitraria, ex doctrina indivisibilium, erit portio ABHM ad portionem AEHM, ut BM ad EM; quod demonstrandum erat.

CC.

COLLINS TO J. GREGORY.

Mr. Gregory,
Sir,

By yours dated the I perceive you are inclined to take the business of Algebra, and especially the finding the roots of equations, into serious consideration: a worthy undertaking, and that which will be very acceptable to the learned, the rather seeing all those, that have made great promises in this kind, fail in the performance, and are like so to do. Hudden, twenty-two years since, promised a treatise, ab ovo ad montem, but being made eminent in state employments, intends not to perform. Riccio (as Borellius writes) hath a considerable employment in rebus ecclesiasticis, ideoque mathematicas otiari sinit. Erasmus Bartholinus fails of his promises made in 1663, being intent upon medicine, and the publication of a better edition of Tycho's twenty years' observations. We have been fed with vain hopes from Dr. Pell——— about twenty or thirty years; but now at last, forasmuch as his scholar Rhonius (as you will afterwards see) intends a specious Diophantus, Mr. Kersey the like, and Billy is lately come out with two volumes of Diophantus Redivivus[r] (not yet come over), that his promises may not seem to be altogether illusory, he is a preparing something, and I relate the occasion.

The notes of Fermat upon Diophantus, it seems, were no other than marginal notes he wrote upon his own Bachetus, and do (if printed by themselves) scarce

[r] 12mo. Lugduni 1670, which marks the time at which this letter was written. It is not dated.

amount to three sheets of paper, and so the book is no other but Bachet reprintedwith those notes, the prefaces omitted; and when he comes to knotty problems he is frequently complaining he wanted room, and therefore hath left the matter untouched. I give instances.

A square number may be divided into two square numbers: the like, he saith, cannot be done in any of the higher powers, but he wanted room to demonstrate it.

The sum of two cube numbers may be divided into two other cubes, but he wanted room to explain the rule and the reason.

Every whole number is either a triangular number, or composed of two or three triangular numbers: it is likewise a square number, or composed of two, three, or four squares; it is likewise a pentagonal number, or composed of two, three, four, or five pentagonal numbers: and so ad infinitum. This he wanted room to demonstrate.

At the beginning of the book, Billy, out of Fermat's letters, hath added about eight sheets, which he calls inventum novum, about such problems as require a series of numbers, so and so qualified, for answers; as if an equation were given, and it were required to find such roots that the homogenea thereto should be a series of square or cube numbers, &c., or such unlimited problems, where more numbers are sought than there are equations given, which he calls problems of a duplicate, triplicate, quadruplicate, &c. equality, concerning which kind of problems Dr. Pell saith he finds the method very deficient; to wit, that after there is found one answer, there may [be] another derived out of that, and a third out of the second, which will presently run into vast numbers, and no ways discovers a multitude of intermediate answers. To supply these defects he is a writing about twenty sheets, with which the three sheets of Fermat's notes are to be reprinted, and when this is done [I] shall send it to you, and forbear to send you Fermat. Wallisii Mechanica, Bartholini Dioristice, and a little tract, De Motu, I delivered above a month since to Sir Robt. Murray, directed to you, and since wrote by the post.

I really believe that your own knowledge, considering what you meet with in Hudden's Annexa Geometriæ Cartesianæ, Dulaurens, Bartholinus, and Ferguson's Labyrinthus Algebræ Belgice, which I hope hereafter to procure and send you, will prompt you to write a treatise about finding the roots of equations by direct methods, much more easily than hitherto hath been done, and what I can add to my former imperfect narratives about this matter I proceed to trouble you withal. And first, in all these kinds of problems about Algebra, the second segments of the hyperbolical spindle, &c., if you can think that tables may facilitate the same, and prescribe a method for making them, we shall find those here, that will take the pains. We have good tables for the areas of the segments of a circle, prepared for the press by Dr. John Newton and Mr. John Smith, and as to equations, Pell's assertions and the tables I mentioned in Dr. Thorndyke's hands keep me in mind of thoughts that tables will resolve equations: and what Schooten saith in his Commentaries on Descartes, page 295, seems to intimate that we may be as well satisfied by answers found by tables, as otherwise ——— thus he saith:

Ubi porro notandum quod, postquam æquatio quælibet a fractionibus aut surdis liberata est, atque in faciliorem transmutata, fieri non potest ut ulla ex hujus radicibus, sive falsis sive veris, sit numerus aliquis fractus, quemadmodum ex septimo Elementorum libro demonstrari potest. Adeo ut si illa deinde, sicut pag. 77 est ostensum, dividi nequeat, concedendum sit nullam ex radicibus, sive falsis sive veris, numero explicari posset, sed omnes esse irrationales.

So that in this case, if the equation hath none of its roots integers, they are all irrational.

Being in discourse with Dr. Pell, I told him that Dulaurens had made such equations of the third, fifth, seventh, ninth degree, as might be solved by finding

$$\left.\begin{matrix}2\\4\\6\\8\end{matrix}\right\}\text{means, or}\left\{\begin{matrix}\text{trisection}\\\text{quinquisection}\\\text{septisection}\\\text{nonisection}\end{matrix}\right\}\text{\&c.,}$$

and asked whether all equations of odd degrees might not be solved by the like methods: he said they might, and that he was in these equations always sure to find a root possible; that this argument would be the subject of a treatise, he long intended, to bear the title of Canon Mathematicus; that herein he should have to deal with the chords of divers figures besides the circle, ellipsis, &c.

Our good friend Mr. Michael Dary, failing in his trade, I have procured him an employment as gauger under the farmers of the excise of Newcastle, Northumberland, Cumberland, Westmoreland; and he is gone to Newcastle, and a letter directed to him at the Excise Office will be safe.

Concerning Cardan's rules, De Beaune, in his commentaries on Des Cartes, p. 142, saith, Multo facilius investigari queant juxta methodum a Vieta in tractatu de numerosarum potestatum resolutione traditam, quam per regulas Cardani.

But Cardan's method hath above half the trouble of it now removed by Dulaurens.

To proceed: I asked Dr. Pell how he could solve equations of even powers by the said methods, and he confesseth they must be so advanced, as in cubics, that their roots will be the squares of the roots sought, or else he requires the limits in store, as I formerly wrote, in order to the solving of them by logarithms; now that something may be performed by having a preparation in store, is the assertion of Hudden, pag. 503.[s]

Diversas adhuc alias regulas in paratu habeo, quas hic simul adjungerem, si non aliquid in futurum reservare animus esset: nimirum inter cæteras una est, per quam omnes irrationales radices tam numeralium quam literalium æquationum invenio: una, per quam omnes æquationes numerales, quæ ex duabus rationalibus produci possunt, ad easdem reduco, non cognitis divisoribus ultimis: item alia, per quam sæpe literales æquationes reduco, quæque in eo consistit, quod unam aut alteram literam pono$=0$, vel alii alicui quantitati quam libuerit, et quod hanc æquationem inde resultantem prius reducere coner, et postea etiam propositam per hanc.

In requiring the limits Pell prevents the searching after that, which in these cases may perchance never be found, to wit, all the roots may be impossible, and when they are possible, methinks he hath reduced it to this: a series arithmetical is assigned to belong to a series of homogenea, and giving a number in the series of homogenea it is required to find the correspondent number in the arithmetical series, whereof there can be but two cases, either both series increase, or the one increaseth and the other decreaseth.

Furthermore, as Dr. Wallis, at the end of the first tome of his former Opera Mathematica, sheweth that a pure unaffected biquadratic parabolaster is described, by leaning ordinates, out of an affected cubic equation, so it is worth inquiry, whether the portions of the curves that are described for other affected equations

[s] Geometria a Renato Des Cartes. 4to. Amst. 1659. Vol. I.

are not portions of pure parabolasters of some higher degree, and whether a line in them that bisects all the ordinates, however inclined, be a right or curved line.

If in logarithms or sines we admit the last differences (where the intervals are not very wide) to be equal, and give a sine or logarithm, and find the arch or number correspondent, thereby the root of such an equation is found, that hath but one true root. Now the equation is found by aid of a table of figurate numbers, of which kind you have instances in Dary's Miscellanies, page [ ], and of which more hereafter, whereby you will perceive that the figurate numbers stand almost in a posture fit for resolving equations. Now forasmuch as every equation may have its roots so much increased or diminished, that the equation shall have but one true root, doth it not seem possible that, by considerations of this kind, the business of a multitude of particular tables may at last be reduced to some few general tables, that shall much facilitate, if not perform the matter sought?

Ferguson pretends to find a root of any cubic or biquadratic equation by aid of the roots of binomials, and promiseth to extend the method to equations of higher degrees, but his method finds but one root at a time.

I was in hopes that Fermat had shewed us how to assume certain whole numbers, partly negative and partly affirmative, so that the sums of the affirmative products should have been equal to the sums of the negative products, and this when the products are the products of binaries, ternaries, quaternaries of the numbers assumed, &c.; as likewise to have assumed them so that some of the affirmative and negative products, as aforesaid, should have been equal, but others not; and this is the constitution of incomplete equations of known roots; but of this he saith nothing.

These things I only mention as probabilities that there is a plus ultra in these affairs, that may deserve the pains of the learned to consider, and hope you will not be displeased for so doing. For when your doctrine of Tangents, of the Infinite Series, and of these matters are explained, certainly every one will think the most invincible difficulties, and greatest toil, in pure mathematics are conquered and removed.

As to mathematical intelligence, I send you a transcript [t] of a part of a letter from Dr. Pell's scholar that wrote the high Dutch Algebra, (translated into English and enlarged by Dr. Pell,) to Mr. Haak, an ancient gentleman of the R. S., that translated the notes of the Dutch divines on the Bible into English.

CCI.

J. GREGORY TO COLLINS.

St. Andrew's, March 7, 1670.

Worthy Sir,

I have lately received yours of the 12th Febr., of which I have answered the greatest part in my last to you. I have spoke to several here concerning Sir * * * * [u], but I can find none who know him: some only have heard of him, that he is poor and base, in so far that he doth no way regard his credit. However, I have written with this post to one Mr. David Thoires, an advocate in Edinburgh, who is my cousin-german, concerning your business. I have desired him also to give me notice what may be expected of this Sir * * * * , and in what condition he is. Ye may write to Mr. David Thoires yourself,

[t] This extract has not been found.

[u] The name is suppressed, being that of a respectable family in Scotland.

directing your letter thus: "For Mr. David Thoires, Advocate in Edinburgh." I have signified to him that ye will do so. Any thing that ye send to me, direct it unto him.

I see Mr. Anderson hath dealt very basely with you: what I have to say against his book I sent you in my last. The truth is, I cannot constrain myself to examine his book accurately, for I find almost in every line some nonsense: as for example, pag. 105, speaking of numbers, "the sum shall be a square and its root commensurable;" first, the last words are superfluous, for the root of a square in numbers is always a number, and then they are nonsense, for there is no quantity, which is not commensurable to infinite other quantities: and immediately after, "the product may be AZ;" nay, the product must be AZ: and immediately after, "the sum of their squares and product may be," for must be: and immediately after, "equal to a square," for, which may be equal to a square: and then he, speaking in his book De quantitate continua, or of Magnitudes, (as in Prop. 23, Prop. 24,) he citeth the demonstrations of the 7th [book] of Euclid, which, albeit they be true in quantitate continua, can never be demonstrated from the principia of the 7th book of Euclid: and then, he giveth the title of his 22nd Prop. in numbers, (and afterwards citeth it, speaking de quantitate continua,) and demonstrateth it in indefinite quantities; and then he citeth the 7th book, which is only proper to numbers, and the 6th, which is only proper to rectilineal figures. In his preface to the reader he saith, that seven years since he resolved these following propositions, and in the mean time, saving two or three, they were all resolved seven hundred years ago. It were easier to find an hundred mistakes of this nature than to read over his book.

I have received all those books, concerning which ye [wrote], save only Slusius. I shall be very willing ye writ to Dr. Caddenhead in Padua, for some of my books. In the mean time, I desire you to present my service to him, and to inquire of him if my books be suppressed, and the reason thereof. No more at present, but rest

yours to serve you,

J. GREGORY.

Extract[x] *of a letter from J. Gregory to Collins.*

Analytically there can be no touch line given to the rhumb spiral. I know not if from this, and the fourth problem in the 124th page of Dr. Barrow's lectures, it follow that there is no analytic proportion betwixt assignable parts in a circle, and an hyperbola, which is of considerable consequence, and in the inquisition of which I have very often concerned myself.

Extract[y] *of a letter from J. Gregory to Collins.*

I suppose these series I send you here enclosed, may have some affinity with those inventions you advertise me that Mr. Newton had discovered. It was upon this account I so often desired you to communicate the same unto me. I shall also give here an approximation for the sines[z].

[x] From a paper in Collins's handwriting, on which he has written "in his letter of the 5th of Sept. 1670."

[y] In Collins's handwriting, headed, "Mr. Gregory in his letter of 23 Nov. 1670."

[z] These extracts seem to have been made by Collins for his own convenience, and unfortunately the original letters do not appear. It will be found that the working of some portions is inaccurate, and the detected errors leave a suspicion of the existence of others; but to introduce corrections with their proofs would needlessly lengthen the foot-notes of the page, and they will be detected by any one, who takes sufficient interest in the subject to examine it throughout. To correct them in the text would not have given a fair view of the letters as they stand. Two have been noticed, but the numerical labour of examining the work throughout is far beyond what could be introduced.

Sit arcus aliquis minor quadrante, sitque defectus $= c$
Item alius quilibet quem superat quadrans excessu $= a$
Sinus totus .... $= b$
Et ejus excessus super sinum prioris arcus .... $= e$
Fiant duæ series continue proportionalium, prima nimirum

$$b,\ 2e,\ \frac{4e^2}{b},\ \frac{8e^3}{b^2},\ \frac{16e^4}{b^3},\ \&c.,$$

secunda $-e, -\frac{2e^2}{b}, -\frac{4e^3}{b^2}, -\frac{8e^4}{b^3}, -\frac{16e^5}{b^4}$, &c.

Sintque aliæ duæ series, quarum prima $\frac{a^2-ac}{1\times2\times c^2}$,

$$\frac{a^2-ac-1\times2\times c^2}{3\times4\times c^2},\ \frac{a^2-ac-2\times3\times c^2}{5\times6\times c^2}.$$

$$\frac{a^2-ac-3\times4\times c^2}{7\times8\times c^2},\ \&c.$$

Et vocetur primus terminus $\frac{o}{c}$, et productum ex duobus primis $\frac{p}{c}$, ex tribus primis $\frac{q}{c}$, ex quatuor $\frac{z}{c}$, ex quinque $\frac{o'}{c}$, &c.

Secunda vero series,

$$\frac{a}{c},\ \frac{a^2-c^2}{2\times3\times c^2},\ \frac{a^2-4c^2}{4\times5\times c^2},\ \frac{a^2-9c^2}{6\times7\times c^2},\ \&c.$$

Sitque productum ex duobus primis $\frac{r}{c}$, ex tribus primis $\frac{s}{c}$, ex quatuor primis $\frac{t}{c}$, &c. Arcus ille, quem superat quadrans excessu a, habebit sinum $=$

$$b - \frac{2oe+ae}{c} + \frac{4pe^2+2re^2}{bc} - \frac{8qe^3+4se^3}{b^2c} +$$

$$\frac{16ze^4+8te^4}{b^3c} - \frac{32o'e^5+16ve^5}{b^4c} + \&c.:$$

hinc, posito $a : e :: 1 : 7$, erit sinus arcus, quem superat quadrans excessu a, =

$$b - \frac{e}{49} - \frac{8e^2}{2401b} - \frac{104e^3}{117649b^2} - \frac{11440e^4}{40353607b^3} - \&c.$$

I have been more large in the approximations to the sines, and numbers of logarithms, than in these to the arches and logarithms, because I suppose the former are more unknown. However these approximations to the arches I hint at are the same that I mentioned in my last answer to Hugenius.

Nam positis, radio .... $= r$,

Semisse lateris quadrati circulo inscripti ... $= d$,

Et differentia inter radium et quadrati latus $= e$,

esset semicircumferentia =

$$\frac{4r^2}{2d - \frac{e}{3} - \frac{e^2}{90d} - \frac{e^3}{756d^2} - \frac{23e^4}{113400d^3} - \frac{263e^5}{7484400d^4} - \&c.},$$

quæ series facile ita producitur, ut ab ipsa semicircumferentia minori differat intervallo quam quævis ejus pars assignata, imo nullo negotio infinitæ tales exhibentur.

It were no hard matter to bring from these several approximations for the segments of a circle, but it were to no purpose, seeing I cannot take away the alternate powers, as Mr. Newton doth in his series, (if it be one, for the truth is I cannot reduce it to any of mine,) yet I imagine that my series may be as shortly performed as his, seeing that my continued proportion (cæteris paribus) is much greater than his, et proinde seriei termini multo citius evanescunt. I could also apply many of the former approaches to the hyperbola, but by that means I should gain nothing more ready than what is already known in it.

I must also give you a series for an arch of a circle;

which I apprehend to be one of the most succinct, especially in the semicircumference.

Reliquis stantibus ut prius, sit s sinus versus arcus, qui ad arcum c rationem habeat 3 ad 2; erit $c=$

$$\frac{rs}{\frac{9d}{8}-\frac{3e}{128}+\frac{3e^2}{5120d}+\frac{1279e^3}{3440640d^2}+\frac{4819e^4}{45875200d^3}+\frac{159857e^5}{1614807040d^4}+\&c.}$$

Si igitur ponatur radius $=r$, et $r-\sqrt{\frac{3r^2}{4}}=e$, erit circuli semicircumferentia, seu $c=$

$$\frac{6r^2-\sqrt{18r^4}}{\frac{9r}{16}-\frac{3e}{128}+\frac{3e^2}{2560r}+\frac{1279e^3}{860160r^2}+\frac{4819e^4}{5734400r^3}+\frac{159857e^5}{100925440r^4}+\&c.}$$

Furthermore:

Sit datus arcus, cujus sinus $=d$, sinus arcus dupli $=2d-e$, oportet invenire alium arcum, qui ad arcum cujus sinus d, rationem habeat a ad c.

Sint continue proportionales in infinitum d, e, $\frac{e^2}{d}$, $\frac{e^3}{d^2}$, &c. fiatque series infinita $\frac{a}{c}$, $\frac{a^2-c^2}{2\times 3\times c^2}$, $\frac{a^2-4c^2}{4\times 5\times c^2}$, $\frac{a^2-9c^2}{6\times 7\times c^2}$, $\frac{a^2-16c^2}{8\times 9\times c^2}$, $\frac{a^2-25c^2}{10\times 11\times c^2}$, &c.

Sitque productum ex duobus primis seriei terminis $\frac{b}{c}$, ex tribus primis $\frac{k}{c}$, ex quatuor primis $\frac{l}{c}$, ex quinque primis $\frac{m}{c}$, &c. Erit

$\frac{ad}{c}-\frac{be}{c}+\frac{ke^2}{cd}-\frac{le^3}{cd^2}+\frac{me^4}{cd^3}-\frac{ne^5}{cd^4}+\&c.$ = sinui arcus quæsiti. Hinc si $c=225$, et $a=1$, erit sinus $\frac{1}{225}$ arcus, cujus sinus d, $= \frac{d}{225} + \frac{25312e}{34181875} + \frac{1281413547e^2}{8652287109875d} + \frac{41703058777952e^3}{1314066104736328125 6b^2}$[*] $+$ &c.

Hinc quoque non difficulter colligitur positis, radio $= r$,

arcu $= c$,

ejus sinu $= d$,

sinu arcus dupli $= 2d - e$,

sinu verso $= v$,

sinu verso arcus dupli $= t$,

erit $c = \frac{rv}{\frac{d}{2} + \frac{e}{24} + \frac{11e^2}{1440d} + \frac{191e^3}{120960d^2} + \&c.} = \frac{rt}{2d - \frac{e}{3} - \frac{e^2}{90d} - \frac{e^3}{756d^2} - \&c.}$

Ex prædictis etiam infinitæ aliæ methodi ad mensurandos arcus aptissimæ deduci possunt.

Sint tres arcus æquidifferentes, quorum communis differentia c; sitque primi sinus $b - e - d$, secundi b, tertii $b + d$, sitque alius arcus ad libitum a.

Fiant duæ series continue proportionalium, prima nempe, b, e, $\frac{e^2}{b}$, $\frac{e^3}{b^2}$, &c., et secunda d, $\frac{ed}{b}$, $\frac{e^2d}{b^2}$, $\frac{e^3d}{b^3}$, &c.; sintque aliæ duæ series quarum prima, $\frac{a^2 - ac}{1 \times 2 \times c^2}$, $\frac{a^2 - ac - 1 \times 2 \times c^2}{3 \times 4 \times c^2}$, $\frac{a^2 - ac - 2 \times 3 \times c^2}{5 \times 6 \times c^2}$, $\frac{a^2 - ac - 3 \times 4 \times c^2}{7 \times 8 \times c^2}$, &c. voceturque primus $\frac{o}{c}$, et ex duobus primis productum $\frac{p}{c}$, ex tribus primis $\frac{q}{c}$, ex quatuor primis $\frac{s}{c}$, &c.: et secunda series $\frac{a}{c}$, $\frac{a^2 - c^2}{2 \times 3 \times c^2}$, $\frac{a^2 - 4c^2}{4 \times 5 \times c^2}$.

[*] On another paper he gives the denom. 1314066104736328125b^2: it should be 1313681671142578125d^2.

$\frac{a^2-9c^2}{6\times7\times c^2}$, &c.; sitque productum ex duobus primis $\frac{r}{c}$, ex tribus primis $\frac{s}{c}$, ex quatuor primis $\frac{t}{c}$, &c. Erit

$$\text{sinus arcus } a = \begin{cases} b - \frac{eo}{c} + \frac{e^2p}{bc} - \frac{e^3q}{b^2c} + \frac{e^4x}{b^3c} - \&c. \\ + \frac{da}{c} - \frac{edr}{bc} + \frac{e^2ds}{b^2c} - \frac{e^3dt}{b^3c} + \&c. \end{cases}$$

hinc etiam infinitæ aliæ, a prioribus diversæ, deduci possunt methodi ad arcus circulares mensurandos.

Addatur adhuc una egregia series sed alterius generis. Sit itaque arcus c, radius r, sinus rectus t, sinus complementi $r-e$, erit arcus seu

$$c = \frac{rt}{r - \frac{e}{3} - \frac{e^2}{45r} - \frac{e^3}{168r^2} - \frac{113e^4}{64800r^3} - \&c.}$$

Hujus etiam generis dantur infinitæ numero series eundem arcum designantes.

About a method of interpolations out of the same letter.

In the end of my Geometrical Exercitations I fail exceedingly, for where I speak of trilinea, quadratica, cubica, quadrato quadratica, &c. I should say, trilinea æquationibus quadraticis, cubicis, biquadraticis inservientia; and hence, in place of any thing I have described there,

$$\text{ponendo AP} = \text{PO} = c$$
$$\text{PB} = d$$

$$\text{primam ex differentiis} \begin{cases} \text{primis} & = f \\ \text{secundis} & = h \\ \text{tertiis} & = i \\ \text{quartis} & = k \\ \text{quintis} & = l \end{cases}$$

et omnes differentias affici signo +, erit ABP =

$\frac{dc}{2} - \frac{fc}{12} + \frac{hc}{24} - \frac{19ic}{720} + \frac{3kc}{164} - \frac{863\,lc}{60480} +$ &c. in infinitum.

However the differences be affected, I can easily square the figure, and by this means all figures imaginable.

I cannot also but advertise you, that Mr. Mercator's quadrature of the hyperbola is a consectary of this.

I remember you did once desire of me my method of finding the proportional part in tables, which is this.

In figura octava mearum Exercitationum in recta AI imaginetur quælibet Aα, cui sit perpendicularis $\alpha\gamma$ sitque γ in curva ABH. Reliquis stantibus ut prius, sit series infinita $\frac{a}{c}$, $\frac{a-c}{2c}$, $\frac{a-2c}{3c}$, $\frac{a-3c}{4c}$, &c.; fiatque productum ex duobus primis seriei terminis $\frac{b}{c}$, ex tribus primis $\frac{k}{c}$, ex quatuor primis $\frac{l}{c}$, ex quinque primis $\frac{m}{c}$, &c. in infinitum; erit recta $\alpha\gamma = \frac{ad}{c} + \frac{bf}{c} + \frac{kh}{c} + \frac{li}{c} +$ &c. ad infinitum.

This method, as I apprehend, is both more easy and universal than either that of Briggs, &c., and also performed without tables.

Out of the same.

To find the number of a logarithm.

Sint duo numeri, primus b, secundus $b+d$, logarithmus numeri b sit e, et logarithmus numeri $b+d$ sit $e+c$, quæritur numerus cujus logarithmus $e+a$.

Sit series continue proportionalium,

$$b,\ d,\ \frac{d^2}{b},\ \frac{d^3}{b^2},\ \&c.;$$

et alia series $\frac{a}{c}$, $\frac{a-c}{2c}$, $\frac{a-2c}{3c}$, $\frac{a-3c}{4c}$, &c.;

fiatque productum ex duobus primis hujus seriei terminis $\frac{f}{c}$, ex tribus primis $\frac{g}{c}$, ex quatuor primis $\frac{h}{c}$, ex quinque $\frac{i}{c}$, &c.; erit numerus logarithmi $e+a=$

$$b+\frac{ad}{c}+\frac{fd^2}{cb}+\frac{gd^3}{cb^2}+\frac{hd^4}{cb^3}+\frac{id^5}{cb^4}+\frac{kd^6}{cb^5}+\&c.$$

Hinc adhibita quadam industria nullo negotio resolvitur quævis æquatio pura.

An example was desired about finding the first of 364 means between 100 and 106.

Put $b=100$
$d=6$
$e=0$
$a=1$
$c=365$

Prima series 100,6, $\frac{36}{100}$, $\frac{216}{10000}$, $\frac{1296}{1000000}$,

and these four terms are the first, second, third, and fourth differences.

Secunda $\frac{1}{364}-\frac{364}{730}\left(\frac{182}{365}\right)-\frac{729}{1095}-\frac{1094}{1460}\left(\frac{547}{730}\right)-\&c.$

Hinc fit $\frac{f}{c}=-\frac{182}{133225}$, $\frac{g}{c}=+\frac{132678}{145881375}$; patet ergo ex prædictis

$$b = 100 \qquad = 100$$

$$\frac{ad}{c} = +\frac{6}{365} \qquad = +0.01647123$$

$$\frac{fd^2}{cb} = -\frac{6552}{13322500} \qquad = -0.00049199$$

$$\frac{gd^3}{cb^2} = +\frac{28658448}{1458813750000} = +0.00001964$$

Quæ omnes simul additæ efficiunt mediam proportionalem quæsitam $\Big\}$ = 100.0159919

(he makes it[a] 100,0160101).

An example of Interpolations.

Quæritur cubus numeri 23.

Num.	Cubi	Diff. 1ª	2ª	3ª	
10	1000	2375			$c = 5$
15	3375	4625	2250	750	$a = 23 - 10 = 13$
20	8000	7625	3000		$d = 2375$
25	15625			750	$f = 2250$
30	27000	11375	3750		$h = 750$

The infinite series $\frac{a}{c}, \frac{a-c}{2c}$, &c., is $\frac{13}{5}, \frac{4}{5}, \frac{1}{5}, \frac{1}{10}$,

[a] The real sum of the figures here given is 100.0159989 (the divisions also are not free from error); but several alterations and erasures have been made in writing them, some of which may have occurred after the addition and subtraction were completed. Collins, on another part of the paper sets down the numbers as follows with two additional terms:

+100.	
+ .01647123	−.00049199
+ .00001964	−.00008266
+ .00000423	57465
.01649510	
57465	
100.01592045	

The products are $\frac{13}{5}, \frac{52}{25}, \frac{52}{125}, \frac{26}{625}$,

$$
\begin{aligned}
\text{And} \qquad & +1000 = 1000. \\
& +\tfrac{13}{5} \times 2375 = 6175 \\
& +\tfrac{52}{25} \times 2250 = 4680 \\
& +\tfrac{52}{125} \times 750 = 312 \\
& = \text{Cub. } 23 = 12167.
\end{aligned}
$$

Extracts of a letter from J. Gregory to Collins[a].

Out of his letter of the 19th of December, 1670.

Sir,

In my last to you I had not taken notice, that Mr. Newton's series for the zones of a circle (which you sent me a long time ago), together with an infinite number of series of the like nature, may be a consectary to that which I sent you concerning logarithms, viz. dato logarithmo invenire ejus numerum, vel radicem potestatis cujuscunque puræ in infinitam seriem permutare.

I admire much my own dulness, that in such a considerable time I had not taken notice of this; nevertheless that I had taken much pains to find out that series. But the truth is, I thought always (if so be it were a series) that I might fall upon it by some combination of my series for the circle, seeing I had such infinite numbers of them, not so much as once desiring any other method.

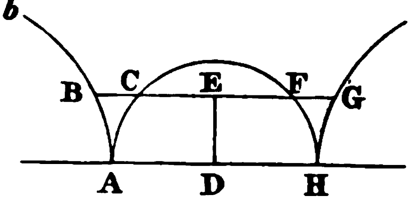
[a] From a paper in the handwriting of Collins.

Putting R for the radius, and B for the breadth of the zone, the area thereof, producing the series a little further, is viz. $= 2RB - \frac{B^3}{3R} - \frac{B^5}{20R^3} - \frac{B^7}{56R^5} - \frac{5B^9}{576R^7} - \frac{7B^{11}}{1408R^9} - \frac{21B^{13}}{6656R^{11}} - \frac{11B^{15}}{5120R^{13}} - \&c.$

Eisdem etiam positis, erit arcus, cujus sinus $B, = B + \frac{B^3}{6R^2} + \frac{3B^5}{40R^4} + \frac{5B^7}{112R^6} + \frac{35B^9}{1152R^8} + \&c.$

I could give you several other series of this nature, but perchance you know more of them than myself.

It may be worth noticing, that in sectionibus oppositis AB, HG, quarum axes conjugati sunt æquales, positis AD $= r$, DE $= b$ fit zona ABGH $= 2rb + \frac{b^3}{3r} - \frac{b^5}{20r^3} + \frac{b^7}{56r^5} - \frac{5b^9}{576r^7} + \&c.$



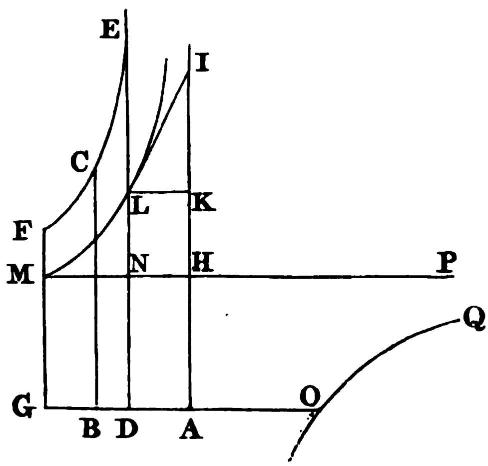
Erit igitur figura ABC, vel HFG, $= \frac{b^3}{3r} + \frac{b^7}{56r^5} + \frac{7b^{11}}{1408r^9} + \frac{11b^{15}}{5120r^{13}} + \&c.$

Et zonarum ABGH, ACFH $= 4rb - \frac{b^5}{10r^3} - \frac{5b^9}{288r^7} - \frac{21b^{13}}{3328r^{11}} - \&c.$

Hæ series nullo negotio applicantur omni tam hyperbolæ quam ellipsi, modo ellipsis (quæ hyperbolæ jungitur) eandem habeat axium rationem, quam habet hyperbola.

About straightening the Logarithm Curve. Out of his letter of the 19th of December, 1670.

I promised once to give you the proportion betwixt a right line and a logarithm curve, which is this:



Sit LM curva logarithmica, cujus asymptoton AI; curvam tangat in quolibet puncto L recta LI asymptoto occurrens in I, sintque LK, MH, perpendiculares rectæ in asymptoton. Fiat HA ipsi KI æqualis, et compleatur rectangulum MHAG, sitque LND parallela asymptoti.

$$\text{Sit KI} = r$$
$$\text{AD} = b$$
$$\text{AG} = c$$

fiatque curva FE talis naturæ, ut (posita indeterminata $AB = a$) fiat semper $BC = \sqrt{r^2 + \frac{r^4}{a^2}}$

Ex hactenus vulgatis satis patet rectam MN esse ad curvam ML, sicut rectangulum MD ad mixtilineum EDGF, quod mixtilineum ita metimur.

$$\text{Sit } d=\sqrt{r^2+\frac{r^4}{b^2}}-\frac{r^2}{b}$$

$$\text{et } f=\sqrt{r^2+\frac{r^4}{c^2}}-\frac{r^2}{c}$$

Producatur GA in O, ut sit AO = IK, fiatque hyperbola OQ, cujus asymptoti HP, HA; in qua hyperbola supponantur duo spatia, unum comprehensum ab uno asymptoto et rectis $\sqrt{r^2-d^2}$, $\sqrt{r^2-f^2}$, alteri asymptoto parallelis, quod vocetur g^2, alterum vero ab uno itidem asymptoto et rectis b, c, alteri etiam asymptoto parallelis, quod appelletur h^2, erit figura mixtilinea DEFG $=fc-2g^2+h^2$. Datur ergo mensura curvæ quam quærebamus.

The demonstration is more prolix than difficult, and therefore I trouble you not with it; but here impart the series for the logarithmic curve.

Reliquis manentibus ut prius, sit KH $=m$; erit curva ML $=m+\frac{c^2-b^2}{4r}-\frac{c^4-b^4}{32r^3}+\frac{c^6-b^6}{96r^5}-\frac{5c^8-5b^8}{1024r^7}+\frac{7c^{10}-7b^{10}}{2560r^9}-$&c. Ex dictis non difficile est invenire non solum centrum gravitatis ipsius curvæ, sed etiam centra gravitatis superficierum ex conversione curvæ genitarum, et ipsarum superficierum dimensionem.

Extract[a] *of a letter from J. Gregory to Collins.*

In his letter of the 15th of Feb. 1671.

Sit Radius $= r$

Arcus $= a$

Tangens $= t$

Secans $= s$

Erit $a = t - \frac{t^3}{3r^2} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \frac{t^9}{9r^8} - \&c.$

Eritque $t = a + \frac{a^3}{3r^2} + \frac{2a^5}{15r^4} + \frac{17a^7}{315r^6} + \frac{3233a^9}{181440r^8} + \&c.$

Et $s = r + \frac{a^2}{2r} + \frac{5a^4}{24r^3} + \frac{61a^6}{720r^5} + \frac{277a^8}{8064r^7} + \&c.$

Sit nunc tangens artificialis $= t$

et secans artificialis $= s$

et integer quadrans $= q$

Erit $s = \frac{a^2}{2r} + \frac{a^4}{12r^3} + \frac{a^6}{45r^5} + \frac{17a^8}{2520r^7} + \frac{3233a^{10}}{1814400r^9} + \&c.$

Sit $2a - q = e$

Erit $t = e + \frac{e^3}{6r^2} + \frac{5e^5}{24r^4} + \frac{61e^7}{5040r^6} + \frac{277e^9}{72576r^8} + \&c.$

Sit nunc secans artificialis $45^o = s$.

Sitque $s + L$ secans artificialis ad libitum.

Erit ejus arcus =

$$\frac{q}{2} + L - \frac{L^2}{r} + \frac{L^3}{3r^2} - \frac{7L^4}{3r^3} + \frac{14L^5}{3r^4} - \frac{452L^6}{45r^5} + \&c.$$

Eritque

$$2a - q = t - \frac{t^3}{6r^2} + \frac{t^5}{24r^4} - \frac{61t^7}{5040r^6} + \frac{277t^9}{72576r^8} - \&c.$$

You shall here take notice that the radius artificialis $= 0$: and that when you find $q > 2a$, or the artificial secant of 45^o to be greater than the given secant, to alter the signs, and go on in the work according to the ordinary precepts of Algebra.

[a] In Collins's handwriting.

CCII.

COLLINS TO J. GREGORY.

March 25, 1671.

Mr. Gregory,—Sir,

I have yours of the 15th of Feb., for the which I render you hearty thanks; what you write in relation to Dr. Barrow I sent him, but have not heard from him since. I must confess my unwilling neglect in not answering you sooner, but the case is this: His Majesty is pleased to increase the number of the Council of Plantations by adding divers persons of great dignity thereto; to wit, His Highness the Duke of York, Prince Rupert, the Duke of Buckingham, the Duke of Ormond, the Lord Lauderdale, the Lord Culpepper, Mr. Evelyn of the Royal Society; and it fell to my lot to transcribe copies of the Commissions and Instructions for their use. I think the Lord Lauderdale's sister's son is to be his heir, he is now at Oxford in St. John's College, and one Mr. Bernard reads the mathematics to him, likewise the astronomy lecture for Dr. Wren, who is now surveyor of His Majesty's buildings. The said Mr. Bernard is a good mathematician, and understands the Arabic tongue well; he hath found in the libraries there two entire copies of the first seven books of Apollonius his Conics, (and some other tracts of that author,) the one of Ben Musa, the other of Abdelmelech, and one of them hath Eutocius his notes. The three latter books, when translated and put into Dr. Barrow's method, may probably be printed with Dr. Barrow's comment on the first four, and be sold together.

As for foreign mathematical intelligence, I have to add, since the last I sent you, that there is lately come out in Italy,

Borellius de Liquidis, a physico-math. treatise;

Mengolus his body of Music, long expected;

Honorati Fabri commentaria in Archimedem; the Comment of Borellius on Archimedes is to be printed at Lyons;

Gottigni's Dioptrics; he is accounted a good geometer, and I think so truly by a sight of his small Euclid.

In France,

A Capuchin hath published Dioptrics, speculative and practical, in folio, of which Berthet the Jesuit writes he hears a good character.

Mons. Picart de mensura perimetri terræ.

None of these books are yet come over, nor Fermat's Diophantus, which are not to be bought at Paris, (there being none save presents sent from Toulouse,) one was sent from thence, by the Lord Aylesbury's man, intended for Mr. Oldenburg, but the fellow sold the book by the way, and spent the money. I have remitted money to our friend Mr. Vernon, at Paris, from whom I may expect some of those Commentaries on Diophantus, as soon as they are to be had, and, God willing, I shall not fail to send you one. Young Fermat writes there are notable algebraical inventions in it, which I should greedily covet to see; and as for my narrative about finding the roots of equations by tables, I now render you a better account of it.

One Mr. Warner deceased, whose Optics you find mentioned in Mersennus, did, about thirty-two years since, spend above an hundred pounds for aid, and took great pains himself, with some assistance from Dr. Pell, to calculate a table, to twelve places of figures, of 100,000 continual proportionals, to wit, to find 99999 mean proportionals between an unit and 100,000. Such a large table, elegantly writ, remains in the hands of Dr. Thorndyke, a prebendary of Westminster; the construction and uses of it, with the tactions of circles rendered analytical, were lent to one Gibson, deceased in anno 1650, author of a book entitled Syntaxis Math., after whose death all his papers were consumed to light tobacco. But this was the Canon Mathematicus intended purposely for the solving of equations; and indeed Vieta shewing the constitution of many equations from continual proportionals, some of the terms whereof shall be the roots of equations, and the sums or differences of other terms the coefficients, renders it probable to me that such a table might be very expedite for tentative work. But Dr. Pell asserts that in any equation, after he hath the limits, (viz. where the serpentine curves for equations cross the base line, when it so happens,) and the greatest ordinate or homogeneum in the said portion of the serpentine curve given, that then he finds the logarithms of the roots so precisely by logarithmical operations, that the logarithm found shall not err an unit in the last figure, and this without tentative work, but admitting trials or the rule of false position in logarithms. I see it may be done, and that the homogenea to a series of roots of an equation may be made up by multiplication, without adding or subtracting of biquadrates, cubes, squares, &c. according to Dr. Barrow's method.

Dr. Pell in discourse affirms that in a complete equation of the eighth power, between certain limits, the six intermediate terms may be all taken away; between other limits there can be but four of them taken away; and again between other limits but two of them taken away, Mediis sese mutuo perimentibus, but always any one of them taken away, (as Dulaurens hath shewed,) and the like in all equations, and this he explained long since by instances; yea, our Harriot, printed forty years ago, shews how in a complete biquadratic equation to take away any two of the inferior powers. Dr. Pell communicates nothing: he once refused me a proposition, and I am resolved never to move him more. He doth assert that Frenicle made a large table of figurate numbers, and by help of them solveth equations. I see thereby that to any series of numbers, whose last differences are equal, there may be a common equation found, and hereof I give you an instance, but I cannot perform the converse.

As for the musical progression, I now send you my last thoughts about it, which indeed are better prioribus curis. By the same method you will easily obtain the reciprocals of the squares or cubes of an arithmetical progression. From Slusius we have lately received his geometrical method for determining of all equations, of the which, together with other papers of good worth, do not doubt to receive a copy as soon as my leisure can permit. I proposed unto these booksellers, viz. Pitts, Scot, Martin, and Kettleby, the quondam servant of Mr. Thompson deceased, the printing of your intended treatise, but they all refused it, and the like is the fate of Mr. Kersey's Algebra; but at length conferring with one Hickman, (the late servant of Mr. Allestry deceased,) newly set up for himself, he is willing to undertake the same, and promiseth it shall be well done: and I promise you my care and assistance in drawing the schemes and correcting [the press], and would not have you withhold any thing of your former thoughts of enlarging it. Book-

[illegible]

[illegible]

Mr. Gregory,—Sir.

I wrote to you the 25th of March last, and would have done since had I obtained leisure. Mr. Vernon is come over from Paris, and informs me that, upon the sickness of Hugenius, the Royal Academy had a great esteem for you, and had it in their thoughts to have recommended you to His Majesty of France for a pension. yea, Hugenius himself approved of it, though he said he had reason to think himself disobliged by you. He hath recovered his health in Holland, and the States were desirous to have kept him there, offering him the same pension the French King allowed him, which was 6000 livres per annum: the said King hearing this, to induce him to return hath increased his pension to 12000 livres per annum: yea, some of the Royal Academy wrote over to Mr. Oldenburg, who was desired to impart the same to the council of the Royal Society, that the French King was willing to allow pensions to one or two learned Englishmen, but they never made any answer to such a proposal. Had not our Pell been a second Roberval, and no ways obliging to his acquaintance or to the Royal Society, they might, and probably would, have recommended him. The case being thus, I acquainted Sir Robert Murray therewith, who promised to confer with Huygens the father, (now here about the affairs of the Prince of Orange, to whom he is secretary,) and to procure the friendship of him and his son in your behalf, and Sir Robert also promised to make an address to the French ambassador, and if so, I wish a good success, and do not think it very improbable. Mr. Vernon hath furnished me with one of Fermat's Diophantus, which is nothing but the former of Bachet reprinted with animadvers[ions] therein, and about seven sheets at the beginning collected by Billy out of Fermat's papers about such unlimited problems as have a series of whole numbers, squares, or cubes, &c. for their answer. The book is in value 20*s.*; and, as soon as I have another, [I] shall perform my promise in sending you the same, with Dr. Wallis his third part on Mechanics, near finished. In Italy, Mengolus his long expected Body of Music is come forth, also Borellius de Motu Animalium, de Liquidis, &c. the Dioptrics of Gottigni, who was the most learned of Gregory of St. Vincent's scholars, and somewhat of Honorati Fabri on Archimedes; Borellius his Archimedes is said to be at the press at Lyons. Mr. Vernon coming from Paris on a sudden, made the Jesuit Berthet fail of sending me Billy's two vol. of Diophantus Redivivus (whom I now reckon to be exceeding subtile in such problems), and the Dioptrics of P. Cherubin, of which Regnault of Lyons, one of the best mathematicians of France, gives their character in a private letter to the aforesaid Jesuit, "J'admire la belle impression de la "Dioptrique du P. Cherubin, sa doctrine est fort bonne. "Je n'avois pas une si grande idée, que je n'aie; ce "livre est excellent, le public lui en est grandement "obligé." Ere long we may expect Mons. Picart's neat treatise De Mensura Perimetri terræ. You have obliged me with many good notions, and are pleased to promise more about the roots of equations. I should be glad to be informed whether the method of infinite series hath a good success in finding the second segments of a sphere, spheroid, &c.

I have now sent you some notions out of Slusius his papers, and as soon as I have leisure hope to send you more.

5 May, 1671, with Slusius his method of determinations.

CCIV.

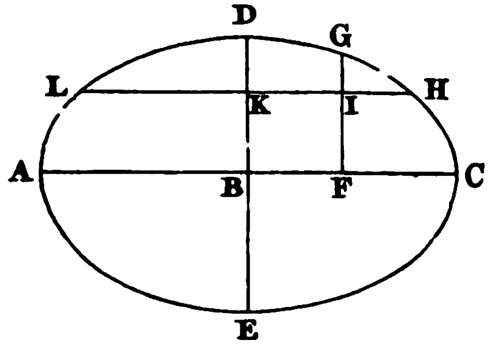
J. GREGORY TO COLLINS.

St. Andrew's, 17 May, 1671.

Sir,

I thank you very kindly for the mathematical intelligence ye sent me in yours of the 25th of March last. I never had any thoughts of such a table as ye mention, (viz. of continual proportionals,) for resolving of equations; but the thing is probable, and I would be curious to see it. I imagined formerly that ye had thought all equations resolvable by the tables of sines and logarithms, which I exceedingly doubt of, for I have had some thoughts to that purpose. As to the matter of equations, either in taking away what terms may be taken away, or reducing all equations, when it is possible, to pure equations, or finding their limits, or reducing all of them to infinite serieses; I may entertain you more at length with these things afterward. For your intelligence, shewing me that so many persons of vast learning were upon that subject, hath engaged me to look over my notes to these purposes, where I find great improvements can be made. I am now much taken up, and have been so all this winter by-past, both with my public lectures, which I have twice a week, and resolving doubts, which some gentlemen and scholars propose to me. This I must comply with, nevertheless that I am often troubled with great impertinencies; all persons here being ignorant of these things to admiration. These things do so hinder me, that I have but little time to spend on these studies my genius leads me to; so that I am necessitate to delay your answers much against my own humour. I resolve, when the college riseth, which will be within six weeks, to apply myself seriously to the doctrine of equations, especially to facilitate the continuation of a series, which is yet to me horridly prolix. I have brought the continuation of other serieses to a much greater facility than I expected, which encourageth me as to these also. When I have come such a length as to satisfy myself, ye may assure yourself that I will make no secret of it to you. This inclination I find in myself to prosecute these things I had once laid up, hindereth my design at present to publish any thing. Yet I cannot express how much I am sensible of your kindness, in exposing yourself freely to so much trouble upon the account of any notions my dulness can produce. This method of infinite series hath no good success in the second segments of round solids, at least so far as I can improve them, yet such as it is ye shall have it.

Sit, igitur, ellipsis ADCE cujus axis transversus AC,



conjugatus DE, centrum B. Supponatur nunc sphærois generari ex revolutione semiellipseos ADC circa axem AC, seceturque sphærois a duobus planis parallelis per puncta K, B, axi DB normalibus, et a duobus planis parallelis per puncta B, F, axi BC normalibus. Sit $BF=a$, $BK=b$, eritque pars sphæroidis KIFB a quatuor dictis planis comprehensa =

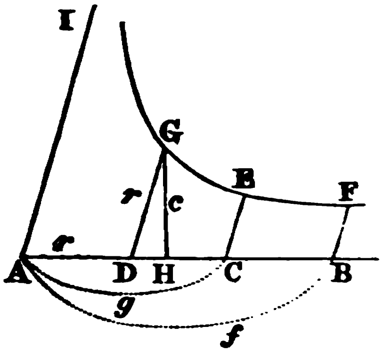
$$\frac{1680\, r^6 ba - 280\, r^4 b^3 a - 42\, r^2 b^5 a - 15\, b^7 a - \&c.}{840\, r^5} - \frac{1680\, r^6 ba^3 + 280\, r^4 b^3 a^3 + 126\, r^2 b^5 a^3 + 75\, b^7 a^3 + \&c.}{5040\, r^5 c^2} - \frac{16\, r^6 ba^5 + 8\, r^4 b^3 a^5 + 6\, r^2 b^5 a^5 + 5\, b^7 a^5 + \&c.}{320\, r^5 c^4} - \frac{48\, r^6 ba^7 + 40 r^4 b^3 a^7 + 42 r^2 b^5 a^7 + 45 b^7 a^7 + \&c.}{2688\, r^5 c^6} - \&c. \text{ in}$$

infinitum.

This series is nothing but a congeries of other serieses, all of them being infinite, yet is the best I can have to this purpose; if Mr. Newton know any other, I hope ye will inform me. If ye would have it agree to the sphere, ye shall only put c in the place of r, which will render it more simple. I can give such a series as this for the second segment of any round solid; and if ye like this, I shall give you with the next a series for the second segments of an hyperbolic spindle, which I imagine is of greater consequence than anything else for gauging.

Having now room and leisure, I resolve to perfect some things I formerly sent you. Ye know the series I gave you for the hyperbolic curve serveth only near to the vertex; and therefore for the complete measure of that curve,

Sit hyperbola GEF cujus asymptoti AI, AB; ducantur asymptoto AI parallelæ DG, CE, BF, quarum DG ipsi AD sit æqualis, reliquæ vero CE, BF, ad libitum; fiatque GH asymptoto AB perpendicularis. Sint $AD = r$, $GH = c$,



$AC = g$, $AB = f$, $\sqrt{4r^2 - 4c^2} = b$. Si angulus IAB fuerit acutus, erit curva $EF = f - g + \frac{br}{2f} - \frac{br}{2g} +$

$$\frac{4r^4 - b^2r^2}{24g^3} + \frac{b^2r^2 - 4r^4}{24f^3} + \frac{4br^5 - b^3r^3}{80g^5} + \frac{b^3r^3 - 4br^5}{80f^5} +$$

$$\frac{24b^2r^6 - 16r^8 - 5b^4r^4}{896g^7} + \frac{16r^8 + 5b^4r^4 - 24b^2r^6}{896f^7} +$$

$$\frac{40b^3r^7 - 48br^9 - 7b^5r^5}{2304g^9} + \frac{48br^9 + 7b^5r^5 - 40b^3r^7}{2304f^9} +$$

$$\frac{64r^{12} - 240b^2r^{10} + 140b^4r^8 - 21b^6r^6}{11264g^{11}} +$$

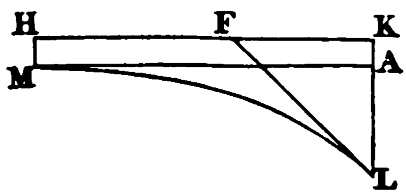
$$\frac{240b^2r^{10} - 64r^{12} + 21b^6r^6 - 140b^4r^8}{11264f^{11}} + \&c.$$

Si vero (reliquis manentibus) angulus IAB fuerit

obtusus, erit curva $EF = f - g + \frac{br}{2g} - \frac{br}{2f} + \frac{4r^4 - b^2r^2}{24g^3} +$
$\frac{b^2r^2 - 4r^4}{24f^3} + \frac{b^3r^3 - 4br^5}{80g^5} + \frac{4br^5 - b^3r^3}{80f^5} +$
$\frac{24b^2r^6 - 16r^8 - 5b^4r^4}{896g^7} + \frac{16r^8 + 5b^4r^4 - 24b^2r^6}{896f^7} +$
$\frac{48br^9 + 7b^5r^5 - 40b^3r^7}{2304g^9} + \frac{40b^3r^7 - 48br^9 - 7b^5r^5}{2304f^9} +$
$\frac{64r^{12} - 240b^2r^{10} + 140b^4r^8 - 21b^6r^6}{11264g^{11}} +$
$\frac{240b^2r^{10} - 64r^{12} + 21b^6r^6 - 140b^4r^8}{11264f^{11}} + \&c.$ Si denique angulus IAB fuerit rectus, foret $b = 0$, et proinde evanescerent omnes quantitates in quibus reperitur b.

This series is the more exact the further CE be taken from the vertex, contrary to that other I gave [you], which was also applicable, by a little alteration, unto the ellipsis. I gave you also only one series for the measure of the logarithmic curve, which I do not remember; and therefore, in case I give you the same over again, I shall give you two to complete the measure of it.

Sit igitur ML curva logarithmica, cujus recta asymptota HK, in quam sint ordinatæ MH, LK; sitque MA ipsi HK parallela, et LF recta curvam tangens in puncto L. Sint $FK = r$,



$HK = m$, $LK = b$, $AK = c$. Erit curva $ML = m + \frac{b^2 - c^2}{4r} + \frac{c^4 - b^4}{32r^3} + \frac{b^6 - c^6}{96r^5} + \frac{5c^8 - 5b^8}{1024r^7} + \frac{7b^{10} - 7c^{10}}{2560r^9} + \&c.$

Erit itidem eadem curva $ML = b - c + \frac{r^2}{2c} - \frac{r^2}{2b} - \frac{r^4}{24c^3} + \frac{r^4}{24b^3} + \frac{r^6}{80c^5} - \frac{r^6}{80b^5} - \frac{5r^8}{896c^7} + \frac{5r^8}{896b^7} + \&c.$

The second series helpeth the defect of the first; so that either the one or the other is abundantly sufficient for the measure of the curve. I know that the segments of circles are of great use in practice, but if the segment be little, Mr. Newton's series, which ye sent me, is not of ready use, and therefore ye may make use of this:

Sit radius $=r$, sagitta segmenti circularis $=a$, et $2ra=b^2$, erit segmentum circulare $=\frac{4ba}{3}-\frac{2a^3}{5b}-\frac{a^5}{14b^3}-\frac{a^7}{36b^5}-\frac{5a^9}{352b^7}-\frac{7a^{11}}{832b^9}-$&c., ejusque arcus integer $=2b+\frac{a^2}{3b}+\frac{3a^4}{20b^3}+\frac{5a^6}{56b^5}+\frac{35a^8}{576b^7}+\frac{63a^{10}}{1408b^9}+$&c.

I have received lately another of yours dated 6th May, in which ye continue still engaging me with excellent extracts and information of the best mathematical books. I am heartily sorry that Doctor Wallis is so troubled with a quartan ague, yet it may prove healthful to him. It is a disease that I am very happily acquaint[ed] with, for thirteen years ago I had it a whole year and a half, and since that time I never had the least indisposition; nevertheless that I was of a very tender and sickly constitution formerly. As to the rest of your letter, I have not so much vanity as to persuade myself that ye are serious, I having never heard any thing relating to that formerly. I have had sufficient experience of the uncertainty of things of that nature before now, which maketh me, since I came to Scotland, how mean and despicable soever my condition be, to rest contented, and satisfy myself with that, that I am at home in a settled condition, by which I can live. I have known many learned men, far above me upon every account, with whom I would not change my condition. No more at present, but rest

your most humble and obliged servant,

J. GREGORY.

CCV.

J. GREGORY TO COLLINS.

St. Andrew's, 17 Jan. 1672.

Sir,

A considerable time ago I received one from you by Mr. Sinclair his hand, for which I acknowledge myself much obliged to you and him. I think [it] strange that so many eminent men have failed of their promises. I am confident Huddenius is able to perform what he hath promised, for his two epistles, in my opinion, go beyond all who ever did write in Algebra, yea, [Des] Cartes himself not being excepted: only in Epist. 1ma, pag. 492, he seemeth to fail in his assertion "neque quod sedulo observo," &c.; for by his two equations any of these unknown quantities x or z can be taken away. It is true ye may perchance have ground enough at present to think as much of me, yet, albeit I know myself not to be comparable to the least of these ye mention, God willing, with the first occasion, I shall perform the utmost of what I ever promised. As for Bartholini Dioristice, if I be not mistaken, it is infinitely outdone, even in the highest equations, by what may be easily deduced from Huddenii Epist. 2da. As for the second segments of round solids, I have no other but what I sent to you; I expected that ye would have advertised me, if Newton had done any thing more. I am confident that the tables of logarithms and sines cannot resolve all equations, albeit they can very many, yet not without great preparation, so as a sursolid equation, which can be reduced to a pure one, must first ascend to the twentieth potestas, not without extraordinary work. The only universal method I know is the infinite serieses. There can be given one which will serve for all cubic equations, another for all biquadratics, another for all sursolids, &c.: and I suppose that tables of these serieses were the best of any. I should be glad to see how Ferguson gives the roots of all cubic equations in intelligible surds. I suppose he doth it only in some particular examples, (and not indefinitely,) which is no great matter. I believe ye will find a great abbreviation of Cardan's method in Vieta himself, (I know not if it be that ye ascribe to Dulaurens,) as also in De Beaune de Natura Æquationum, pag. 113; and in Schotenius' Appendix de Cubic. Æquat. resolutione, pag. 367; as also in Huddenii Epist. 1ma, pag. 499. There is no great mystery in what ye mention from Dr. Pell and Mons. Dulaurens; for ye may know by this that there is no great affinity betwixt equations and sines or logarithms, the curve of sines and that of logarithms have no contrary turnings, but these of equations have many. It is true, ye can take them all away from the side where the true roots are, but when this is done, nothing can be applicable to the side of the true roots, which is not also applicable to the other, suo modo. I wonder how ye speak yet of interpolation by the help of figurate numbers, seeing I sent you, a long time ago, a method much shorter and readier by a series. To let you see that there is a plus ultra (as ye tell me) in these things:—in omni æquatione indefinita, si desit secundus terminus, in ejus radice nulla erit quantitas rationalis; si desint secundus et tertius, nulla erit quantitas rationalis in radicis quadrato; si desint secundus, tertius et quartus, nulla erit quantitas rationalis in radicis cubo; et sic in infinitum. Si vero quantitas cognita secundi termini fuerit $= -p$, tertii $= +q^2$, quarti $= -r^3$, quinti $= +s^4$; erit $+p$, divisa per numerum dimensionum æquationis, sola quantitas rationalis in ejus radice; eritque $p^2 - 2q^2$, divisa per eundem numerum, sola quantitas rationalis in radicis quadrato; et $p^3 - 3pq^2 + 3r^3$, divisa per eundem numerum, sola quantitas rationalis in radicis cubo; et $p^4 - 4p^2q^2 + 4pr^3 + 2q^4 - 4s^4$, divisa per eundem numerum, sola quantitas rationalis in radicis biquadrato; et sic in infinitum, continuata semper hac serie. Atque hæc, consideratis considerandis, in omni æquatione vera sunt.

I could send you several general notions of all equations, which, for what I know, are yet untouched by any; but I am afraid they should hardly be so pleasing to you, as it were troublesome to me, to seek them out and transcribe them; I being now upon another study. I thank you for your mathematical intelligence. I am afraid ye will find these Cogitationes Physico-mechanicæ Ant. Joan. Ott. Scaphusa Helvetio to be but ranting. No more at present, but rest

your humble servant,

J. GREGORY.

I would gladly see that numerical problem which Dr. Pell proposed to Dr. Wallis several years ago. I remember ye did once shew it me, but I have forgot it.

CCVI.

J. GREGORY TO COLLINS.

St. Andrew's, 14 Feb. 1672.

Sir,

In my last to you, by Mr. Sinclair, I did mistake these two equations of Huddenius, pag. 492; for the truth is, both the unknown quantities x and z cannot be taken away there, nevertheless that I affirmed otherwise from my inconsiderate calculation. However, my assertion, of which indeed upon good ground I was confident, is yet true, viz. that every equation taketh out an unknown quantity. For these two equations of Huddenius are really one, to wit $x^2 - 2zx \left.\begin{matrix} +bz \\ +b^2 \end{matrix}\right\} = 0$, multiplied in two arbitrary quantities $x-z$, $x-2z$, which is presently manifested by searching the maxima communis mensura betwixt them.

My method de maximis et minimis, (which is evident from Huddenius his 2[d] Epistle, of which also I sent you a specimen long ago,) is this:

Sit æquatio quævis

$$\frac{x^7 - ax^6 + b^2x^5 + c^3x^4 + d^4x^3 - e^5x^2 + f^6x - g^7}{r^6} = 0,$$

sitque propositum inquirere quot habeat radices hæc æquatio, et inter quos terminos consistant earum valores. Multiplico singulos æquationis terminos in proprios suos exponentes, atque ita provenit æquatio

$$\frac{7x^6 - 6ax^5 + 5b^2x^4 + 4c^3x^3 + 3d^4x^2 - 2e^5x + f^6}{r^6} = 0.$$

Hujus æquationis omnes inveniantur ordines, (nam uno saltem gradu depressior est æquatione proposita,) $+h$, $+i$, $+k$, $+l$, $-m$, $-n$. Fiant nunc

$$\frac{h^7 - ah^6 + b^2h^5 + c^3h^4 + d^4h^3 - e^5h^2 + f^6h}{r^6} = -p,$$

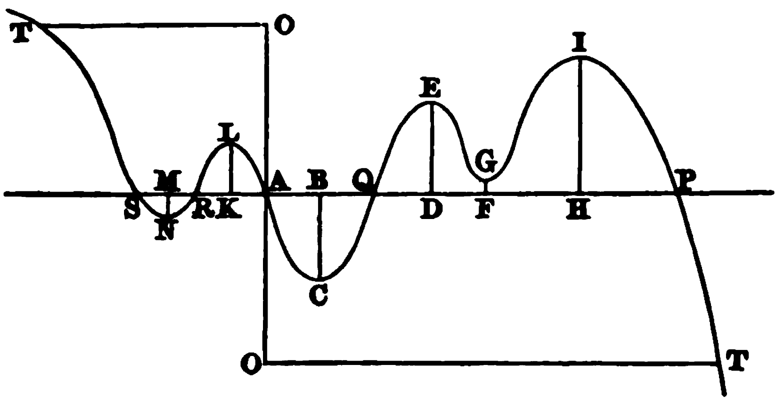
$$\frac{i^7 - ai^6 + b^2i^5 + c^3i^4 + d^4i^3 - e^5i^2 + f^6i}{r^6} = +q,$$

$$\frac{k^7 - ak^6 + b^2k^5 + c^3k^4 + d^4k^3 - e^5k^2 + f^6k}{r^6} = +s,$$

$$\frac{l^7 - al^6 + b^2l^5 + c^3l^4 + d^4l^3 - e^5l^2 + f^6l}{r^6} = +t,$$

$$\frac{-m^7 - am^6 - b^2m^5 + c^3m^4 - d^4m^3 - e^5m^2 - f^6m}{r^6} = +v,$$

$$\frac{-n^7 - an^6 - b^2n^5 + c^3n^4 - d^4n^3 - e^5n^2 - f^6n}{r^6} = -y.$$



Sumantur nunc in linea recta, (phantasiæ juvandæ gratia,) dextrorsum radices affirmativæ AB $= h$, AD $= i$, AF $= k$, AH $= l$; et sinistrorsum radices negativæ AM $= m$, AK $= n$; sumantur etiam perpendiculares rectæ ad respectivas radices, affirmativæ nimirum perpendiculares sursum, negativæ vero deorsum, BC $= p$, DE $= q$, FG $= s$, HI $= t$, MN $= y$, KL $= v$. Manifestum est ex Huddenii Methodo de maximis et minimis puncta N, L, C, E, G, I, esse maximi et minimi puncta, in curva æquationem propositam designante.

sit igitur dicta curva TSNRLACQEGIPT. Fiant denuo ex perpendicularibus affirmativis HI maxima, ED sequens, LK tertia, GF minima; et e negativis BC maxima, MN minima. Si in æquatione g^7 sit negativa, sumatur sursum perpendicularis $AO = \frac{g^7}{r^6}$. Hisce stantibus, si $AO > IH$, habet æquatio proposita unicam tantum radicem, eamque negativam et $> AS$. At si $AO > ED$ et $< IH$, æquatio tres tantum recipit radices, unam nempe negativam $> AS$, unam affirmativam $> AH$ et $< AP$, et alteram affirmativam $> AF$ et $< AH$. Si vero $AO > LK$ et $< ED$, æquatio quinque tantum admittit radices, unam puta negativam et duas affirmativas sicut ante, et duas veras, quarum una $> AD$ et $< AF$, et altera $> AQ$ et $< AD$. Et si $AO > GF$ et $< LK$, æquatio septem accipiet radices, quinque nimirum sicut ante, et duas negativas, quarum una $> AK$ et $< AR$, et altera $< AK$. Denique si $AO < FG$, æquatio quinque tantum admittit radices, duas nempe veras, quarum una $> AH$ et $< AP$, et altera $> AQ$ et $< AD$, et tres falsas, quarum prima $> AS$, secunda $> AK$ et $< AR$, tertia $< AK$. Si in æquatione g^7 sit affirmativa, sumenda est AO perpendicularis deorsum; et in hac hypothesi, si $AO > BC$, æquatio unicam tantum recipiet radicem $> AP$ et affirmativam. Si vero $AO > MN$ et $< BC$, æquatio tres tantum recipit radices easque veras, unam scilicet $> AP$, unam $> AB$ et $< AQ$, et tertiam $< AB$. At si $AO < MN$, æquatio quinque tantum admittit radices, tres nempe veras, quales ante, et duas falsas, unam $> AM$ et $< AS$, alteram vero $> AR$ et $< AM$. Atque hæ sunt omnes mutationes, quas recipere potest hæc æquatio, g^7 manente indefinita. Positis aliis quantitatibus in æquatione indefinitis indies adhuc variaretur æquatio, at prædicta nostro proposito abunde sufficiunt.

Rectæ AP, AQ, AR, AS, (quas hic cognitas supponimus,) sunt omnes radices æquationis $\frac{x^6 - ax^5 + b^2x^4 + c^3x^3 + d^4x^2 - e^5x + f^6}{r^6} = 0$; nam duas veras radices admittit, cum curvatura EGI rectam AP non secet, suntque AP, AQ, reliquæ duæ radices veræ, et AR, AS falsæ. Omnium horum demonstrationes ex ipsa figuræ inspectione sunt manifestæ. Proclive esset multos alios exactiores radicum limites exhibere, si modo otium suppeteret. Si æquatio proposita esset parium dimensionum, impar esset numerus punctorum maximi et minimi, et proinde curvæ continuationes PT, ST, ad easdem partes vergerent, ad quas si ducenda esset AO, duæ semper forent ad minimum radices. Si vero ad contrarias partes duceretur, AO existente majore quam ulla ex maximis ad eas partes, omnes radices æquationis propositæ essent impossibiles, seu imaginariæ tantum. Si in æquatione ante ultimum terminum desint termini numero impares, curva hæc æquationi inserviens rectam SP solummodo tangit in A. Si vero desint termini numero pares, curva rectam tangit, et secat, in A, hoc est in A existit punctum flexus contrarii. Sed quoniam æquationes, quarum ope puncta maximi et minimi et radicum terminos invenimus, uno tantum gradu propositam deprimunt, operæ pretium duco aliam methodum universalem exhibere, qua hæ æquationes duobus semper ad minimum gradibus proposita sint simpliciores.

Primo igitur ex æquatione proposita auferatur secundus terminus, sitque resultans $\frac{z^7 + a^2z^5 - b^3z^4 - c^4z^3 + d^5z^2 + e^6z + f^7}{r^6} = 0$, sitque g quantitas arbitraria, et $z = \frac{g^2}{y}$: si igitur loco z substituatur semper $\frac{g^2}{y}$; oritur æquatio

$$\frac{f^7y^7+e^6g^2y^6+d^5g^4y^5-c^4g^6y^4-b^3g^8y^3+a^2g^{10}y^2+g^{14}}{f^7r^6}=0.$$

Duæ igitur æquationes, quibus utimur ad invenienda puncta maximi et minimi et radicum terminos, sunt

$$\frac{7f^7y^5+6e^6g^2y^4+5d^5g^4y^3-4c^4g^6y^2-3b^3g^8y+2a^2g^{10}}{f^7r^6}=0,$$

$$\frac{f^7y^5+e^6g^2y^4+d^5g^4y^3-c^4g^6y^2-b^3g^8y+a^2g^{10}}{f^7r^6}=0,$$

quæ duobus gradibus proposita sunt simpliciores. Cognita autem y, aut ejus limitibus vel valorum numero, non ignorantur eadem de z, cum $z=\frac{g^2}{y}$, et g sit quantitas assumpta ad libitum, nec de x, cum $x=z+\frac{a}{7}$.

This is the most ready general method, which I know, for determinating all equations. I know many readier in particular cases, but seeing they follow easily (this being understood) from Hudden his second epistle, I need not insist; and therefore I rest

your humble servant,

J. GREGORY.

Ut inveniantur radicum limites propiores, dividatur æquatio $x^7-ax^6+b^2x^5+c^3x^4+d^4x^3-e^5x^2+f^6x+r^6p=0$, per $x-h$. Eadem quoque æquatio, mutata tantum $+r^6p$ in $-r^6q$, dividatur per $x-i$; et, mutata $+r^6p$ in $-r^6s$, dividatur per $x-k$; item, mutata $+r^6p$ in $-r^6t$, dividatur per $x-l$; item, mutata $+r^6p$ in $-r^6v$, dividatur per $x+m$; denique mutata $+r^6p$ in $+r^6y$, dividatur per $x+n$. Omnes hæ divisiones absque residuo fieri possunt, et sex æquationes resultantes uno gradu proposita simpliciores exhibent radices, quæ omnes sunt limites æquationis propositæ radicum, singulæ suis casibus inservientes. Hæc facile, ope figuræ propriæ intelliguntur, at paucis verbis non explicantur.

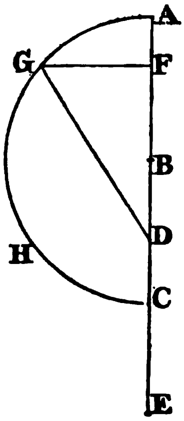
CCVII.

J. GREGORY TO COLLINS.

St. Andrew's, 9 April 1672.

Sir,

I have received, since my last to you, two of yours; one dated the 23d of February and the other the 14th of March. I am much obliged to you for these excellent improvements of learning ye advertise me of. I was exceedingly surprised with these experiments of Mr. Newton; they will cause great changes throughout all the body of natural philosophy, by all appearance, if the matter of fact be true, which I have no ground to question. I would gladly see what Mr. Hooke can say against the doctrine raised upon them, and am most willing to be at all the charges for the Transactions containing that debate, if ye will be pleased to send them to me. I am about to settle a correspondence, whereby I may send you money for what I have been chargeable to you: as also to put you to some trouble, if ye be not averse to it, in furnishing me with books I desire to see. I have considered that problem which ye sent me, viz. "The distance in the rhumb on the earth's surface being given," &c.: I cannot resolve it without a very tedious series, which I shall send to you, if ye can resolve it by no easier method. As to Kepler his problem, ye shall receive this solution:



Sit semicirculus AHC, cujus centrum B, dividendus e puncto D in ratione p ad q. Sint BD, BC, BE, continue proportionales; sitque BD ad BC, sicut semiperipheria AHC ad m; fiat $\frac{pm}{p+q}=a$, $AB=r$, $AE=b$; et sumatur $AF=\frac{ra^2}{2b^2}+\left(\frac{r^2a^4}{6b^5}-\frac{ra^4}{24b^4}\right)+\left(\frac{ra^6}{720b^6}-\frac{13r^2a^6}{360b^7}+\frac{7r^3a^6}{72b^8}\right)+\left(\frac{19r^4a^8}{630b^{11}}+\frac{173r^2a^8}{107520b^9}-\frac{199r^3a^8}{13440b^{10}}-\frac{113ra^8}{1290240b^8}\right)+$ &c.; denique ex F erigatur diametro AC perpendicularis FG, peripheriæ occurrens in G, et ducatur recta DG. Dico GDA : GHCD : : $p:q$. Hujus seriei prolixitas provenit duntaxat a puncto D indefinite sumpto; nam posita recta DB determinata, viz. $\frac{r}{3}=DB$, series hæc evanescit in simplicissimam; erit namque $AF=\frac{a^2}{200\,r}-\frac{a^4}{300000\,r^3}-\frac{a^6}{800000000\,r^5}-\frac{799a^8}{1702000000000000\,r^7}-$ &c. I suppose this series, in all astronomical uses, may be much exacter than any table of sines. Yet when the point D falleth near C, and the ratio p to q is majoris inæqualitatis, the following series may be more exact:

Reliquis manentibus ut supra, $\frac{m}{2}+r-a=e$, et $BE=d$; erit $BF=\frac{re}{d}-\frac{r^2e^2}{2d^3}+\left(\frac{r^3e^3}{2d^5}-\frac{re^3}{6d^3}\right)+\left(\frac{7r^2e^4}{24d^5}-\frac{5r^4e^4}{8d^7}\right)+\left(\frac{7r^5e^5}{8d^9}-\frac{r^3e^5}{2d^7}+\frac{re^5}{120d^5}\right)+$ &c.

If e come to be noted with $-$, BF will also have $-$, and in that case F is taken betwixt B and C. These infinite serieses have the same success in the roots of equations, which they have in other problems; only this, because in equations there are many indeterminate quantities, the serieses are exceedingly tedious; but when these are determinate, the serieses are abundantly simple.

I thank you kindly for these papers ye delivered to Mr. Sinclair to be transcribed upon my account. Be pleased to deliver him the other half of this letter, directed to him. If ye desire these serieses for Kepler's problem to be prolonged, or others in their place, or any thing else within my reach, ye may command,

Sir,

your most humble servant,

J. GREGORY.

CCVIII.

J. GREGORY TO DARY.[a]

April 9, 1672.

Let the radius of a circle be $=r$

The versed sine or arrow $=a$

And make $b^2=2ra$

The segment of the circle is $=\frac{4ba}{3}-\frac{2a^3}{5b}-\frac{a^4}{28rb}-\frac{a^5}{144r^2b}-\frac{5a^6}{2816r^3b}-\frac{7a^7}{13312r^4b}-$&c.

[a] From an extract in the handwriting of Collins.

And the whole arch $= 2b + \frac{a^2}{3b} + \frac{3a^3}{40rb} + \frac{5a^4}{224r^2b} + \frac{35a^3}{4608r^3b} + \frac{63a^6}{22528r^4b} +$ &c.

An approach for the sum of the infinite series, whereby the logarithms are made.

Let the penultimate term be $= a$,
The last term - - $= b$.
The sum of all the terms before the penultimate, together with $\frac{a^2}{a-b}$, is = to the aggregate of the whole infinite series almost, as for example, the aggregate of the whole series $\frac{n}{c} + \frac{n^3}{3c^3} + \frac{n^5}{5c^5} + \frac{n^7}{7c^7} + \frac{n^9}{9c^9} +$ &c. in infinitum is more than $\frac{n}{c} + \frac{n^3}{3c^3} + \frac{n^5}{5c^5} + \frac{9n^7}{63c^7 - 49c^5n^2}$.

This infinite series changeth exceedingly, according to the proportion of n to c. This approximation is of no great value in the beginning of the series, albeit it be also true there, but about the 9th, 11th, or 13th potestas it is of considerable use.

CCIX.

J. GREGORY TO COLLINS.

St. Andrew's, 2 July, 1672.

Sir,

Since my last to you I have received two of yours, one the 28th of May, and another the 13th of June. I have received also these books ye sent to me, for which I thank you kindly; I am sorry that I am not capable to recompense you for them some way else, seeing ye will not take money for them. Any fancies I could send to you were no great matter, and deserved no more than your correspondence, if they did so much. I am much obliged to Mengoli for that superlative compliment he giveth me. I have heard from several in Italy that he had some esteem for me, but I never spoke with him myself. I think there is no great matter in that which ye say Dr. Pell promiseth. I despair of any general construction for the series; neither think I that it can be expected, because of the great variety of them. That is indeed a pretty construction of Alhazen's problem, which Huygens hath discovered, but all is reducible to Slusius his excellent general method. I understand not how ye resolve that rhumb problem; seeing ye have in your analogy three unknown quantities, to wit, the meridional parts, the sine of the rhumb, and the difference of latitudes. It is true that one of them is easily taken away, leaving yet another equation with two unknown quantities; but I desire earnestly a method for taking them all away without a series, and am in hopes that ye will communicate it. I desire only the title of Doctor Pell's problem, because I remember it seemed pretty to me, so that ye need not trouble yourself with any transcription. There is one Master Sinclair, who did write the Ars Magna et Nova, a pitiful ignorant fellow, who hath lately written horrid nonsense in the hydrostatics, and hath abused a master in the university, one Mr. Sanders, in print. This Mr. Sanders is very knowing in the mathematics, and is resolved to cause the Bedel of the university [to] write against him; and upon this account hath desired me to write to you for Stevin's Mathematics, which I intreat you to send, suppose ye should leave some thing I did write for unanswered. He hath also written against Mr. Boyle for weighing water in water. We resolve to make excellent sport with him. No more at present, but rest,

your humble servant,

J. GREGORY.

CCX.

J. GREGORY TO COLLINS.

Sir, Aberdeen, 6 August, 1672.

I am glad that ye have informed me that the parallelogram is not yet bought; for since my last to you, several gentlemen have fallen upon a method to gather contributions for mathematical instruments to the university of St. Andrew's, which probably may take effect. It is like upon this account I may see you within a twelvemonth. I desire, therefore, that for this time ye let alone the parallelogram, but I should be glad that ye would answer the rest of my commission, together with that Diophantus Redivivus, (if ye can have it,) as soon as ye can. It is like indeed that Mr. Newton his telescope may have an advantage above that which I mentioned in my Optica Promota, because the eyeglass is so near to the plane mirror; yet the obliquity of the mirror hindereth somewhat. Nevertheless my telescope hath one advantage also very considerable, for the same concave mirror, together with the same plano-convex eyeglass, may give the same object-mirror any desired charge. I thank you for the Breretonian problem, which ye have sent me. No more at present, but rest,

your humble and obliged servant,

J. GREGORY.

An extract from this letter is printed in Gregory's Optics, 1732, p. 261.

CCXI.

J. GREGORY TO COLLINS.

Sir, St. Andrew's, 23 Sept. 1672.

Yours of the 3rd of August I received a considerable time ago, which I resolved not to answer till I had received these books ye have sent me. The box being so small, it was almost neglected, for it lay above a month in a private house by the way, so that I was necessitate to cause the carriers inquire after it: upon this account they came to my hands but very lately. Afterward, when ye send any thing to me, see to get it into some parcel of goods for any merchant or bookseller in Edinburgh. I have all the books in very good order, only I want the Transactions for Feb. 1671–2, in number 80, and for that I have Jan., in number 79, twice. I am exceedingly engaged to Mr. Vernon for Fermat on Diophantus. I wish I were able to serve him in any thing here. He is a person I always admired for his great knowledge in many sciences and languages: I should think it my honour to contribute any thing for such a worthy person his satisfaction.

I have cast an eye on Mr. Cassegrain his telescope, which seemeth to be the same with that in my Optica Promota, pag. 94, only he hath a convex speculum F[e] in place of my concave, which is no great alteration. I think myself therefore obliged to answer to these disadvantages Mr. Newton finds in it[f]. I make therefore F a plane speculum, and then almost the whole

[e] There is no diagram drawn in the letter.

[f] Phil. Trans. for May 20, 1672; vol. VII. p. 4059. See also p. 247.

disadvantages evanish, except only the third; and for that there is an advantage as considerable, if not more, viz. that the distance EF groweth almost the one half less, and therefore the errors of the concave CD are also diminished upon the plane F by one half. There is yet another advantage of this telescope: that it will be little more than half the length of Mr. Newton's, and do the same effect. Nevertheless of these disadvantages, which Mr. Newton mentions, even with a concave or convex speculum this telescope may be worth the trying, seeing the eyeglass and speculum F being moveable, the speculum CD can have, by their help, any desirable charge, which I think a great advantage. What I either did or said needeth not discourage any; for I speak there only of the hyperbolic and elliptic glasses and speculums, which were attempted in vain, as it is clear from the sense of the words. As for my experiment with Mr. Rives, he could not polish the large concave upon the tool, and I (not knowing any advantage of the catoptric telescope above the dioptric, save only the shortness, and similitude betwixt the circle and parabola, which is greater than that betwixt the circle and hyperbola,) imagined that this great defect in the figure would easily counterbalance these two petty advantages. Upon this account, and being about to go abroad, I thought it not worth the pains to trouble myself any further with it, so that the tube was never made; yet I made some trials both with a little concave and convex speculum, which were but rude, seeing I had but transient views of the object, being so possessed with the fancy of the defective figure, that I would not be at the pains to fix every thing in its due distance. There is no such exactness required in the speculum F as in the speculum CD, but indeed more than in the eyeglass. I suppose there is no question that direct rays have the advantage of oblique, seeing a ball thrown directly on a rough wall hath a more regular reflection than when it is thrown obliquely. However this is not derogate from Mr. Newton, whose excellent discoveries have made the catoptric telescopes preferable to the dioptric.

When ye have occasion, I pray you, send to me the lantern for projecting the species, together with the rest of the Transactions since July last, and that of February last. No more at present, but rest, thanking you for the care ye have had in sending forward these books,

your humble servant,

J. GREGORY.

This telescope with the plane speculum will indeed lose many of the best rays, (but these nevertheless are always less than one quarter of the whole, the eye-glass being advantageously situate,) which defect some perchance may think recompensed by the shortness of the telescope.

I suppose there is no great hazard of overcharging the telescope by the concave or convex speculum, for the charge can be changed at pleasure: neither is it probable to me that the errors of the object speculum are made more sensible (the magnification being always the same) by a concave or convex speculum and an eyeglass, than by a plane speculum and an eyeglass, save only upon the account of greater distances, which I think the only defect of this telescope.

Printed in Gregory's Optics, p. 261, but there wrongly dated Sept. 26. There are also other variations and omissions.

CCXII.

J. GREGORY TO COLLINS.

Sir, St. Andrew's, 7 March, 1673.

I have received yours dated Febr. 20, together with Mr. I. Newton's answer, with which I am exceedingly satisfied. I am much engaged to you both, for the pains ye have been at. I am almost convinced that oblique reflection causeth more light than the direct, but I am not fully persuaded that it is more regular. I conceive that the rudely polished plate of metal in an oblique position causeth the image [to] appear more distinct, because the obliquity hideth the concavities, so that no rays come to the eye but from the tops of the little tuberculæ, which are certainly best polished, the other rays, which confused the image, being kept away. But if the plate be exactly polished, (I speak here as to sense,) the position must be so oblique, before the insensible concavities can be hid, that the plane shall almost turn to the sight in a line. I grant that I have been mistaken in that first advantage which I mentioned; for the plane speculum F, having certainly (as all human artifice hath) some errors in it, causeth greater prejudice by their being remote from the focus, than being near to it, and in it there is none at all caused, where if it could be placed, and a near and direct aspect had of it, this were certainly the best telescope of this sort.

It is true, indeed, that in telescopes with convex or concave specula, to double the charge the length must be almost doubled; but to double is a great alteration, and hardly sufferable (as I suppose) in very good glasses, if the least charge be considerable. But I understand not how the charge can be altered at all, with the same glasses, in Mr. Newton's telescope; for I know nothing of that which was described to Mr. Oldenburg. It is true that eyeglasses can be changed in all telescopes, if they be at hand of the required depth. I think there is no great hazard in these telescopes of overcharging, seeing the charge of the eyeglass can be diminished at pleasure; neither upon this account needs the angle of vision be so small, seeing it is equal to the angle of the eyeglass from its focus, its other focus being the little speculum; nor the darkness at all augmented, if the apertures of the speculums be proportional to the diameters of the spheres. But above all things I desire to know this; that seeing the image made by the great speculum may be esteemed a small visibile, and seeing Mr. Newton, in the Transactions, pag. 3080,[g] thinketh it fitter to make a microscope or tube, to behold a small visibile, of one concave speculum and one eyeglass, rather than with one single eyeglass, and much rather than with one plane speculum and with one eyeglass; wherefore also to look to this small visibile the first also should not be preferred to the last. This image, indeed, is not capable of such magnification as a visibile is; yet I am hardly sensible how this should cast the balance, taking in the defects of a plane speculum together with other inconveniences in taking up the object. I said, indeed, that hyperbolic or elliptic glasses were tried in vain, but I spoke not so of spheric speculums, (as Mr. Newton's words seem to imply, Transactions, pag. 4059,) for any thing I did deserves not the name of a trial, seeing Mr. Rive and Mr. Cox both know that the great speculum was polished only with a cloth and putty. Neither, the truth is, thought I it worth the pains, at that time, to be serious about

[g] Phil. Trans. vol. VI. for Feb. 19, 1671–2.

farther inquiry in that business; for they undertook, indeed, to polish a less speculum to me upon the tool. I am not yet fully convinced which of these two ways has the advantage, albeit I incline more to Mr. Newton's, especially because of the small distance betwixt the plane speculum, focus, and the eye. However, experience must determine all: neither am I concerned how it happen. I had no intention that my thoughts of these telescopes should be printed: my design was only before, as now, that (if ye thought fit, otherwise not) ye might send them to Mr. Newton.

I received these letters ye mention, as also that box, together with the things contained, and particularly Horroccii Posthuma, for which I must acknowledge myself exceedingly engaged to you. I have perused him, and am satisfied with him beyond measure. It was a great loss that he died so young, many naughty fellows live till eighty.

I am well pleased both with the parallelogram and lantern, and thank you kindly for your care in satisfying my humour. I have not yet received the box with the figures.

About three months ago our Arch-Bedel's book against Mr. Sinclair came out, at which time I wrote to the stationer, who caused print it, desiring him to send you one of them, with Mr. Sinclair's Hydrostatics, upon my account, which he promised. I sent him a letter also, directed for you, with these two books; he sent the letter back to me lately, shewing me that he could not find an occasion to send any thing of his own to London, (for, as I understood from him, he had books and several other things of his own to send,) having accidentally missed the vessel, in which my Lord Kincardin returned home. I am at a great loss, being here where I can direct nothing myself. If ye can understand from your stationer in London that any of ours keep exact correspondence with them, advertise me of the person, and I shall write to him to send you these books. Mr. Sinclair is printing again. I hope he shall turn our Hobbes.

Mr. Newton's discourse of reflection puts me in mind of a notion I had of burning-glasses several years ago, which appears to me more useful than subtil. If there be a concave speculum of glass, the leaded convex surface having the same centre with the concave, (or, to speak precisely, albeit perchance to little more purpose, let the radius of the concavity be c, the thickness of the glass in axis transitu f, the radius of the convexity equal to $\frac{9c^2 + 18cf - 5f^2}{9c + 5f}$, this speculum shall have the foci of both the surfaces in the same point, and not only that, but all the rays, which are reflected betwixt the two surfaces, shall in their egress come, quam proxime, to the common focus. The making of such a speculum requireth not much more art than an ordinary plane glass, seeing great exactness is not necessary here; so that I believe they who make the plane mirror glasses would make one of these, three feet in diameter, for four or five pounds sterling, or little more; for I have seen plane glasses almost of that bigness sold even here for less money. Now seeing (as Mr. Newton observeth) that all reflecting metals lose more than one third of the rays, this concave glass, even cæteris paribus, would have a great advantage of a metal one, for certainly an exactly plane piece of mirror glass of good transparent matter, after a few reflections, doth not lose one fourth of the rays; and upon other accounts this hath many [illegible] advantages, seeing it is more portable, free from tarnishing, and, above all, hardly one twentieth of the value.

The great usefulness of burning concaves, this being so obvious, and as yet (for what I know) untouched by any, makes me jealous that there may be in the practice some fallacy. Ye may communicate this to intelligent persons, and especially to Mr. Newton, assuring him that none hath a greater veneration for him, admiring more his great and subtil inventions, than his and

your humble servant,

J. GREGORY.

If I be not disappointed, I hope to see you within two months, or little more.

If ye please, let me hear with the first convenience what may be judged the result of this burning concave; for I am as much concerned to be undeceived, if there be any insuperable difficulty, as to be informed of a most surprising success. I have spoke of it to several here, but all were as ignorant of it as myself. Several months by past I have been so much busied in some private studies that I have forgot to pay my respects to you, which otherwise my inclination leads me to, upon which account I am more tedious now than at other times.

I desire yet to be more particular in the matter of telescopes. I suppose a four feet telescope [to] have the aperture six inches: [and] the little concave, having the aperture three quarters of an inch, may magnify eight times, the radius being one foot. In this case the hole in the middle of the great concave is only three quarters of an inch, which, being filled with an eyeglass equally convex on both sides, amplifying the charge of the little concave twenty-four times, doth make a telescope magnifying the object 192 times, (which is no extraordinary charge, seeing Mr. Newton's table giveth 171, and might be much less without inconvenience,) taking in an angle of vision of above 20 degrees, and with this there is not lost $\frac{1}{60}$ of the rays. With the loss of $\frac{1}{16}$ of the rays it might magnify not above 144 times, and take in an angle of vision of above 28 degrees. With all this the middle of the object is illustrate with all the rays, which the aperture of the great concave doth reflect. By these means I think that I keep off from these two inconveniences mentioned by Mr. Newton in the seventh particular of his considerations. The event of these other considerations, as I suppose, can only be determined by experience.

Printed in Birch's Hist. of R. S. vol. iii. p. 79, and with omissions, in Gregory's Optics, p. 268.

CCXIII.

J. GREGORY TO COLLINS.

Sir, St. Andrew's, 13 May, 1673.

I thought to have seen you before now, but some affairs of the university have delayed my journey for some weeks. I received lately yours dated the 19th April, together with Mr. Newton's to you, (for whose fair correspondence I give you both hearty thanks,) to which I have only these few things to say.

As to his first, I understand not well his meaning. An oblique position seemeth to expose all its inequalities more fully to the rays, and either altogether to hide the lowest of the regular surfaces, or otherwise to reflect the rays coming from them on the adjacent tubercula.

His way of varying the charge is indeed exceedingly ingenious; but I think three surfaces too liable to the errors of the artificer's hand. The opacity of the glass prism, together also with the irregularities, which he hath discovered in refraction, may help to darken and confuse the sight.

As for the next, I know not if it be worthy of the pains to look with excellent telescopes on terrestrial bodies. For, as the object is magnified, so is the grossness of our atmosphere to our sense increased; so that the one hindereth as the other helpeth. In celestial observations any little thing applied to one or more sides of the little speculum may stop the rays of the moon, or any other of the brighter planets, if these be also thought worthy noticing. I suppose that all these adventitious rays may be hindered, even in daylight, by putting in the focus of the eyeglass towards the eye a thin plate of some metal with a little round hole in the middle in diameter $\frac{1}{18}$, $\frac{1}{20}$, or $\frac{1}{30}$ of an inch, which is calculated so;—as the distance of the eyeglass from the little concave is to the distance of the eyeglass from its focus, so is the aperture of the little concave to the diameter of this hole. It is true, at some times this may hinder some of the rays, but they are always the worst; and by increasing the aperture of the little concave not much above what my method requires, it will hinder none at all. I could not have judged that Mr. Newton had thought on this inconvenience in Mr. Cassegrain his telescope, seeing it seemeth to me, even in his own microscope, Trans. pag. 3080. For not only the direct rays of the object O, (nevertheless that it be looked to only with daylight,) but also these proceeding from the object before the concave, are always scattered through the whole image: neither do I see how it can be exactly helped.

That we may see what effect this scattered light may cause in the sight, let us suppose the telescope to magnify 160 times, and the aperture of the great concave to contain eight times the aperture of the eyeglass, or little concave, and the object to be a planet in apparent diameter one half of a minute, in whose image there pass the rays of another planet of the same apparent bigness and brightness, the angle of vision is about 16 degrees, the planet appears in an angle of 1¼ deg., that is to say, it illustrates so much of the retina. Now the other planet illustrates, (I take no notice of the little concave, which is to my disadvantage, seeing it keepeth off many of these rays,) 16 deg. of the retina. Now because of the apertures there are 64 times as many rays in 1¼ of a deg. as in 16; that is to say, these adventitious rays have but $\frac{1}{256}$ of the splendor of the image, which I think hardly sensible. The brightness, indeed, of the moon were near one half, and not sufferable, which therefore is to be helped by some of the foresaid means. In the twilight the inconvenience may be, for the most part, very inconsiderable, and perchance sometimes (as also other adventitious rays) advantageous, by making insensible the circumradiancy of celestial bodies. All this is supposing the eyeglass convex, for if it be concave the effect is otherwise.

As to his last, I imagine that all images do require (cæteris paribus) to be magnified as much as may be. Neither doth his other reason appear to me; for pencils of the same angles are more truly reflected by a concave than refracted by a lens; and, albeit in telescopes the said angle transcend not the limit of a lens, commonly assigned, yet surely the more it is exceeded by this limit, it is so much the better: and all this is observed in my design, yea, there are three times as much stress of magnifying, also, laid upon the eyeglass as on the little concave. It may also be noticed that here there are no very small sizes of spheres to be polished, which can hardly be done (as I suppose) to preciseness. It is possible that, even in telescopes, there may be more stress laid on the eyeglass than it can carry, especially in the extreme pencils, where the incidence is oblique and refraction perhaps so great that $\frac{1}{35}$ of it may be sensible. Also an ordinary microscope suffers no aperture above the limit of a lens, and nevertheless it doth much more than one simple lens, or else the world hath been exceedingly deceived. I dare not confidently affirm that ordinary microscopes may outdo any improvement of one lens; but if they do, I think it more than a probable argument that my project shall exceed Mr. Newton's, seeing beside the only disadvantage which I see in mine, (to wit, the distance of the glass,) it hath the great irregularities of refraction.

I think nothing can be inferred concerning the trial of my telescope from my assertions, seeing the trial was after that assertion; but Mr. Newton could not be supposed to know this.

If ye think fit, ye may signify to Mr. Newton a small experiment, which (if he know it not already) may be worthy of his consideration. Let in the sun's light by a small hole to a darkened house, and at the hole place a feather, (the more delicate and white the better for this purpose,) and it shall direct to a white wall or paper opposite to it a number of small circles and ovals, (if I mistake them not,) whereof one is somewhat white, (to wit, the middle, which is opposite to the sun,) and all the rest severally coloured. I would gladly hear his thoughts of it.

I received, shortly after my last to you, that box of glasses, for which I am exceedingly obliged to you. Ye will do me a favour to present my service to

Sir R. Murray, and render him thanks in my name for that and his many other kindnesses to me. No further at present, but rest, Sir,

your most humble servant,

J. GREGORY.

Printed, with omissions, in Gregory's Optics, p. 280.

CCXIV.

J. GREGORY TO COLLINS.

Sir, St. Andrew's, 2 April, 1674.

I lately received yours of the 5th of the last month, which is the second I have had from you since my return. I sent not long ago some papers to Mr. Boyle: I would gladly know if he hath received them, and how he is pleased with them. Be pleased to advertise me if our Scotch vessels be yet arrived, for it was with one of them I caused a stationer, Gideon Shats, to send up some of our Bedel's books to you by Mr. Pitt's hands, with whom I desired Gideon to enter a correspondence. I thank you kindly for the care ye have of my affairs. When ye shall be pleased to entrust me with Sir Andrew Dick his business, I shall strive to repay you. I suppose the problem ye desire coincides with this.

Sit progressio geometrica, cujus primus terminus $=a$, ratio primi ad secundum b ad a, numerus terminorum $=n$, eorundem summa $=c$.

In your terms I think b [is] the annuity, c the present worth, n the time of continuance, and $\frac{b^2}{a}-b$ a year's interest of the annuity.

E datis, igitur, reliquis quæratur a.

Sit $n+1=p$, $n-1=m$, $b+c=f$.

Si rite examinetur problema, resultat sequens æquatio $b^n c = b^m fa - a^p$. Hæc æquatio duas semper admittit radices veras, modo quæstio sit possibilis, quarum major ad problema non pertinet, cum sit semper $=b$, et in puncto maximi, puta cum usura nulla sit, utraque æqualis est ipsi b. At in omni casu radix minima nostro proposito inserviens per hanc seriem (modo ita nominari possit) invenitur; $a > \frac{bc}{f} = d$, item $a > \frac{bc}{f} + \frac{d^p}{b^m f} = g$, item $a > \frac{bc}{f} + \frac{g^p}{b^m f} = h$, item $a > \frac{bc}{f} + \frac{h^p}{b^m f}$, atque ita de cæteris in infinitum.

The longer the continuance is, and the greater the interest is, this approximation is the nearer, and cometh the sooner to a period; but if the approximation continue any considerable time, ye may observe that the first differences are quam proxime in progressione geometrica majoris inæqualitatis, and therefore the whole sum of them in infinitum easily to be gathered, which, subtracted from the respective approximation, giveth the true root. This method is, of all which I know, the best, especially because of the easy continuation of it in infinitum. I could give some more compendious, but the method of the invention of its terms were infinitely more tedious. If in any thing else I can serve you, ye may command, Sir,

your humble servant,

J. Gregory.

If ye think not this clear enough, send me any question ye please, and I shall resolve it according to this method. I suppose I need not advertise you that if the continuance be considerable, ye must use logarithms.

CCXV.

J. GREGORY TO COLLINS.

Edinburgh, 8 October, 1674.

Sir,

I received yours just now at my return from the north of Scotland, where some business called me. I thank you for the pains ye have been at in buying the tube and prisms for me, and for these books ye are pleased to bestow on me.

I am exceedingly surprised with these objections, which are moved against the Tentamina de motu project., &c. They that affirm the curve VTP, figure vi[f], to be no parabola must be grossly ignorant of the doctrine De locis solidis[g], and therefore no competent judges. Others, who affirm that I suppose an uniform motion, are impudent to admiration, seeing in the viii and ix de motu pend. &c., there is expressly supposed a motus æqualiter retardatus, yea all along that uniform motion supposed by Galilæus and all others hitherto (who have treated of that subject) is declared contrary to the mind of that author, as in 7^{ma}, and manifestly declared to be the principal, if not the only reason of these Tentamina. As for Mr. Anderson, I would have him to prove his first assertion, viz. $x = \sqrt{s^2 - \frac{s^2 b^2}{r^2}} + \sqrt{b^2 - \frac{s^2 b^2}{r^2}}$ to be the same with

[f] Here again he refers to the diagram in the original.

[g] Note in Gregory's handwriting. "For by the first rule "of the Loca solida, which Des "Cartes sets down, and demon-"strates also, in his 2nd book, "(and Van Schooten analyseth "in his Commentaries,) the curve "VTP is a parabola. If I had "Des Cartes by me at present, "I would quote the rule in his "own words and apply it to this "case, but this I reserve to the "next occasion, if this satisfy "not the objector; for the truth "is, few understand the Loca to "purpose.

$x^2 r^2 = s^2 r^2 - s^2 b^2 + b^2 r^2 - s^2 b^2$, and then I shall answer the rest of his learned discourse. If he understood either multiplication of surds or the fourth of the second of Euclid, he had not been so ridiculous. I believe, (until I be otherwise taught by his new inventions,) that if $x = \sqrt{s^2 - \frac{s^2 b^2}{r^2}} + \sqrt{b^2 - \frac{s^2 b^2}{r^2}}$, then $x^2 r^2 = s^2 r^2 - s^2 b^2 + b^2 r^2 - s^2 b^2 + \sqrt{4r^4 s^2 b^2 - 4r^2 s^4 b^2 - 4r^2 s^2 b^4 + 4s^4 b^4}$. He hath manifestly here left out the one half of the equation. If ye would have me examine any one part of his Gunnery, I shall obey you in examining it accurately, but I can hardly spare time for so much pitiful stuff. I satisfy myself with this, that it in general can be to no purpose, seeing he supposeth the motion, abstracting from gravity, to be uniform, which is palpably absurd, seeing the air resists. This (albeit I be thought by some to maintain the same) satisfieth me as much as a million of experiments on Blackheath manifesting its falsehood. That question of Mr. Kersey, to which Dr. Pell finds so many answers, is nothing strange, seeing it ascends to such a high equation; for equations may suffer as many roots as dimensions. No further, but rest,

your most humble servant,

J. GREGORY.

I mind not to renew any correspondence with Mr. Kerre, for I never got any thing by the carrier, which was not in great hazard of miscarriage, by the change of the carriers and opening the packs on the border, for by this small things are neglected. Being here in Edinburgh, I can have things securely that are directed to me by sea, if I be advertised by the post.

I shall go about Sir A. Dick's business as effectually as I can, which I am obliged to do. I wish I could do you more considerable service.

I am daily expecting from my scholar an account of these [sp]ecula[h] ye desire, which I shall immediately transmit to you.

CCXVI.

J. GREGORY TO COLLINS.

Edinburgh, May 26, 1675.

Sir,

I received lately two of yours, one dated April 20th, and the other May 1st. I admire that ye fancy any difficulty in the cubic equation of three roots, seeing Des Cartes long since hath reduced it to the trisection of an angle, and the trisection of an angle can be turned infinite[i] several ways into an infinite series; some of which methods I sent you long ago, not only of trisecting an angle, but also dividing it in ratione data.

That which ye intimate of the sum of the squares, cubes, biquadrates of the roots in a biquadratic equation, is pretty obvious in any equation, as for example, let $x^7 - px^6 + q^2x^5 - r^3x^4 + s^4x^3 - t^5x^2 + k^6x - l^7 = 0$, the sum of all the roots $= p$, all their squares $= p^2 - 2q^2$, their cubes $= p^3 - 3pq^2 + 3r^3$, their biquadrates $= p^4 - 4p^2q^2 + 4pr^3 + 2q^4 - 4s^4$, their surdesolids $= p^5 - 5p^3q^2 + 5pq^4 + 5p^2r^3 - 5q^2r^3 - 5ps^4 + 5t^5$, their

[h] This word is very doubtful.

[i] This word ought probably to be omitted.

sixth powers[k] $=p^6-6p^4q^2+9p^2q^4+6p^3r^3-12pq^2r^3-6p^2s^4+6pt^5-4q^6+6q^2s^4+3r^6-6k^6$, the seventh powers $=p^7-7p^5q^2+14p^3q^4+7p^4r^3-21p^2q^2r^3-7p^3s^4+7p^2t^5-7pq^6+14pq^2s^4+7pr^6-7r^3s^4-7q^2t^5+7q^4r^3-7pk^6+7l^7$. It is no hard matter to give the rule, whereby to continue this in infinitum; for it is so in all equations, as (I believe) I did intimate to you some time ago, albeit under another dress, viz. to give the rational quantities contained in the potestates of the surd roots of all equations.

But this signifieth nothing to Dr. Davenant's problem, neither (if I be not much mistaken) doth that lemma ye mention, for it presently discovers itself in the natural progress of the solution. I have had some thoughts upon that problem; if any man resolve it by an equation under thirty dimensions, erit mihi magnus Apollonius. It is true the equations I brought it to were so tedious, that my patience would not permit me to apply rules of reduction, but I tried so many several equations, that if they had been capable of reduction, sure some of the reductions had been obvious. It is easy to constitute equations so that either two, three, &c., or all the intermediate terms, may easily go off: but to take off even two intermediate terms in an arbitrary equation, without elevating it, is absolutely impossible. By elevating it I can take away all the intermediate terms myself, which (so far as I know) the world is yet ignorant of.

That equation about the rate of interest is composed of a simple lateral equation with a rational root, (which hath nothing to do with the question,) and a long adfected equation with one only root. Always, I

[k] This is expressed in a symbol, which could not be given in print; the same, in some measure, applies to the following expression for the seventh power.

assure you,[1] [the] breaking of it in two such equations as ye susp[ect is] impossible.

I have now abundantly satisfied myself in these thing[s I] was searching after in the analytics, which are a[ll about] reduction and solution of equations. It is possible that [I flatter] myself too much, when I think them of some value, [and] therefore am sufficiently inclined to know others' thoughts, both (as ye say) as to the quid and quomodo of them; but that I have no ground to expect, till time and leisure suffer me to publish them.

I desire that ye would do me the kindness to let me know if Mr. Oldenburg hath received a packet of letters I sent to him some time ago in the black box. They were from a gentleman living in the Highlands, concerning some of the most remarkable rarities there. I would gladly know if Mr. Anderson hath any thing to say against that paper I sent in my vindication, or if he dare undertake to make out what formerly he asserted in the examination of my equation.

I suppose there is yet some mistake among us, as to our accounts, for I desired Mr. Nicolson to give you in sixteen shillings, the value of which I delivered him here. It was from a gentleman, to buy to him a three feet tube and two prisms, which I received. This was beside the 10*l.* and the 4*l.* 3*s.* 6*d.* (for it was not, as ye intimate, 4*l.* 4*s.* 6*d.*) It is true, I find not these 16*s.* mentioned in any of your letters, so that I know not if the mistake be yours or Mr. Nicolson's. As to all your other particulars, they agree with your letters. Mr. Nicolson is pleased here to give a note

[1] The words, and letters, between brackets have been torn off the original letter, and have been supplied by Collins.

under his hand,[m] that he gave you that money. I am sorry I put you to so much trouble, and therefore crave your pardon. I rest,

your much obliged servant,

J. GREGORY.

CCXVII.

J. GREGORY TO OLDENBURG.[a]

8th of June, 1675.

I have seen Mr. Hooke's excellent treatise to prove the motion of the earth, and have had some thoughts thereon, which perchance (if not too obvious and already known to you) may be of some consequence.

Let C, D, be two fixed stars, S the sun, CDBA a plane going through the three points C, D, S, and cutting the orb of the earth in A and B. Let a circle pass through the points A, C, D, cutting CB in E.

The sine of the angle CAD is to the sine of the angle CBD as DB to DE, which proportion may be pretty sensible if the star D be much nearer than C, yea, sometimes perchance so sensible that D may, from B, seem on the one side of C, and from A on the other. The points A, B in the orb of the earth, may be with ease found out more precisely than is required for this business. My thoughts briefly are these: if from A

[m] This refers to the following memorandum which is annexed to the letter,

Edinburgh, 31st May, 1675.

Mr. Collins,—Sir,

In Septemb. 1674, I delivered you upon Mr. Gregory's acct., in London, sixteen shillings sterl., which ye have forgotten to charge yourself with in your account to him. Per me,

JA. NICOLSON.

[a] From a copy in the handwriting of Collins.

and B, the angles CAD, CBD be observed, and found unequal, from thence two things may be inferred hitherto questioned, viz. the motion of the earth, and the unequal distance of the fixed stars; secondly, these angles (as to their inequality, if such a thing be) may be observed easily, because any two stars in the firmament, if they fall within one view of the telescope, may be chosen for this effect; one of which may be a large star of the first magnitude, consequently, by all probability, near to us, and the other of the sixth, yea, perchance of the sixtieth magnitude, and far from us. And, which is most of all, this without any considerable preparation may be easily and exactly observed by any sort of micrometer, or (if D be seen on both sides of C, which may sometimes fall out) with a simple telescope. If it is objected that from this only can be gathered that the fixed stars have parallax, and not how much it is in this or that star, albeit the main business be to prove they have parallax, yet from a third observation, as suppose at F, may be gathered geometrically the parallax of both C and D, if that be esteemed operæ pretium——

See Birch's Hist. of R. S. vol. iii. p. 225.

CCXVIII.

COLLINS TO J. GREGORY.

8th July, 1675.

Mr. Gregory,—Sir,

By mine of the 3d instant I gave you some account of a new method for finding the roots of equations, &c., invented by Mr. Tschirnhaus, a gentleman of Saxony, who I told you was just upon departing for Paris; and, presuming you have that letter, I proceed. Upon the parting visit I received from him[o] he, in answer to the doubt I mentioned about that series, said it was only fitted to the condition there proposed. I further objected that it seemed to serve only to equations that had two pairs of equal roots, but affirming (though in haste) that it served for other cases (according to the example taken out of Des Cartes) wherein all the roots were unequal, and gave another rule for another easy case as follows, shewing the variety thereof, affirming that he imparted only some of the rules for easy cases, reserving the universal rule to himself, but might possibly impart that when at Paris, and that the reducing all to pure high powers (the method of shunning asymmetries mentioned in Des Cartes, 3d vol. of Epistles, p. 473, being understood) is but a corollary of his doctrine. That which he further imparted is viz.

1. Let there be a biquadratic equation, viz. $x^4 - px^3 + qxx - rx + s = 0$, which, if it have this condition, $\frac{pp}{4} + \frac{2r}{p} = q$, then the roots of this equation are

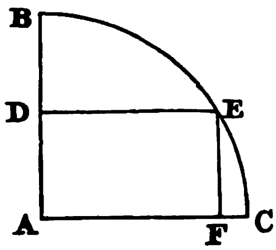
$$x = \frac{p}{4} \pm \sqrt{\frac{pp}{16} - \frac{r}{p} \pm \sqrt{\frac{rr}{pp} - s}}.$$

2. If the equation be such that the four roots are in arithmetical progression, the said roots will also be found by the rule above.

3. If you assume any two quadratic equations, in each of which the second term is the same, and multiply them together, the resulting biquadratic will also be solved by the rule above.

[o] "I received further from him he informed" has been here originally written by Collins, but has been entirely struck out, it not having occurred to him that by these means he left his sentence incomplete.

4. For example, a problem solved by the said rule. Let there be a quadrant ABC, in which is to be inscribed a rectangle DEFA = the rectangle *ab*.



Let AB or $AC = c$,

$BD = x$,

Hence $DA = c - x$.

Now from the nature of the circle;

multiply $DE = \sqrt{2cx - xx}$,

by $DA = \sqrt{cc - 2cx + xx}$.

The fact is $\sqrt{2c^3x - 5ccxx + 4cx^3 - x^4} = ab$. Squaring both sides it is $x^4 - 4cx^3 + 5ccxx - 2c^3x + aabb = 0$.

$p, \quad q, \quad r,$

Now because this equation hath the conditions prescribed, namely that $\frac{pp}{4} + \frac{2r}{p} = q$, all the four roots thereof are true, and may be found by the former rule.

And in the plano-quadratic, $x^4 - pxx + q = 0$, the roots are

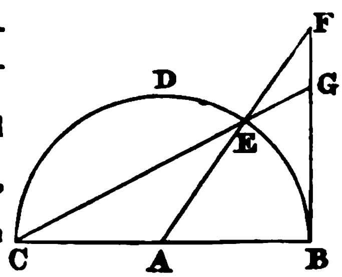
$$x = \pm\sqrt{\frac{p}{4} - \sqrt{\frac{q}{4}}} \pm \sqrt{\frac{p}{4} + \sqrt{\frac{q}{4}}}.$$

A construction for a cubic equation.

Let $x^6 - px^4 + qxx - r = 0$.

In this equation if $p = q = r$, then the construction is plane, and performed as followeth.

Having described the semicircle ABCD with a radius $= \sqrt{\frac{p}{8} + \frac{1}{16}\sqrt{5pp}}$, and drawn the tangent BGF, make $BF = \frac{p}{4}$, and then



drawing the secant AF, and the line CG, intersecting each other at the point E, the line GB will be the quadratic root of this equation, and being reduced to a cubic (according to Des Cartes's method) will become a cubic of the same condition with the former, namely, that the terms p, q and r shall be equal, and alike signed. This he mentioned only as a slight instance of his knowledge in being able to distinguish all cases in any equation that afford any ease, from the rest that are more difficult. And this, with his many and laborious calculations, (and constructions,) that I cast my [eye] upon, render me persuaded that his method is new and universal; but how far it doth quadrare[p] with your last discourses will not be unwelcome to ——

CCXIX.

J. GREGORY TO COLLINS.

Edinburgh, 23 July, 1675.

Sir,

I received lately these books ye did write of, for which I thank you kindly; they seem indeed to be well and neatly done. Some time ago I had also a letter from you dated 29th June, in which ye intimate that De Beaune hath that method of removing the penult term, as also Kinkhuysen, without fractions. I deny it not, for I never called that method mine: yea, in that long letter I did write to you I did shew you that it was all expressly in Hudden's second epistle, being well understood. Yet I question if any

[p] Possibly "quadrate."

man (yea, even Bartholinus, who hath made the limits and determinations of equations his study for a considerable time) hath made that use of it. I think it indeed hard to do it without fractions indefinitely as I do it, that is to say, that the known quantities of any term may be what quantity ye please; and if it be done otherwise I think it argues the ignorance of the analyst. As for Kinkhuysen or De Beaune's breaking of equations of the sixth potestas into two cubics or three quadratics, Hudden doth the latter in pag. 488 by an equation of fifteen dimensions, and sheweth how to do the former, pag. 489, by an equation of twenty dimensions wanting the alternate terms; yea, I shall be glad to see it done more easily, if the equation be arbitrary, otherwise it is not worth speaking of, seeing equations may be made infinite several ways of twenty dimensions, (yea, of as many as ye please,) with all the terms, and yet resolvable in what equations ye please. Equations also may be made up, whose roots may be found out by the tables of sines, secants, and tangents, or by what tables ye please to fancy; for particular methods are infinite, and hardly worth any man's pains.

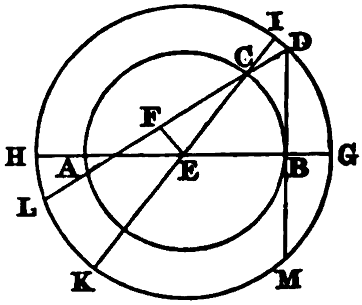
I should be glad that Cardan's rule were made general; I deny not that its speculation might be pleasant, yet I fancy we have methods more easy than it could be, and I think the impossibility of its generality might be demonstrated. However, I think it not made general, suppose it reach all these roots, as I think it sufficiently doth, which admit the extraction of the cubic root out of the binomium, which ye only seem to mention; for by that means the cubic equation can always be brought to a quadratic. I entreat you to do me the favour (when Slusius sends the solution of Dr. Davenant's problem) to send me a copy of it, for if he bring it to a biquadratic equation, I will judge it an extraordinary mystery.

I am glad that Dr. Pell is engaged to publish his papers. I question not that they will enrich the learned world. I wish heartily that others, viz. Newton, Frenicle, Huygens, &c., were obliged likewise to do so. As for making the logarithms from a chiliad, yea, from a very few given, especially if they be of considerable large numbers in the natural order, it is no hard matter to make all the rest, by the combination of these given, from my method of differences, which I sent you long ago. The canons are these (which are also general for all other tables, sines, tangents, secants, &c.):

Let the logarithms of numbers in the natural order be $a, b, c, d, e, f, g, h, i$, &c.; $h = 2g - f = 3g - 3f + e = 4g - 6f + 4e - d = 5g - 10f + 10e - 5d + c = 6g - 15f + 20e - 15d + 6c - b = 7g - 21f + 35e - 35d + 21c - 7b + a$, &c. It is easy to perceive how these canons may be continued in infinitum, as these six are; the third of which is sufficient to make up a table of logarithms, from the first chiliad, true to more places, (if I be not mistaken,) than any yet attempted, (this third is $h = 4g - 6f + 4e - d$,) for by having of d, e, f, g, h, any four, the first may be found by this canon: the like, consideratis considerandis, may be said of the rest, only further they are in order, they are the more exact and the more laborious.

I am heartily sorry, if any thing I have writ to you hath given you occasion to call your hints to me impertinencies; I have no reason to call them so, and must acknowledge that I owe much to them.

If Barrow's Euclid be reprinted, it might perchance be worth the pains to resolve the 36.3. (which is none of the easiest) into the 35 of the same, so: Reliquis manentibus, sicut apud D. Barrow exstant, centro E, intervallo ED, fiat circulus DGL, et ducantur HEBG, ICEK rectæ, et EF in LD perpendicularis, producatur quoque DB in M; (3. 3) LF = FD, et AF = FC, est ergo LA = CD et ADC = LCD; est autem angulus DBE rectus, et propterea DB = BM (3.3), quare HBG = DB^2 (35.3); est autem HBG = KCI = LCD (35. 3) = ADC, et proinde ADC = DB^2, quod, &c. No further; but, craving your pardon for this tedious scribble, I rest,



your most humble and obliged servant,

J. GREGORY.

CCXX.

J. GREGORY TO COLLINS.

Sir, Edinburgh, 20 August, 1675.

I received lately two of your letters, whereby I perceive ye have fallen in acquaintance with a very learned gentleman and a great admirer of Des Cartes, whom I also admire so much that I expect not that he or any other shall help him as to his solution of biquadratic and cubic equations. Des Cartes his method is general, and doth its effect always sicut natura rei patitur. Where it fails, being fitly applied, I have not seen, neither do expect to see any thing used with better success. If a man will trifle his time in particular methods, he may have enough to do perchance in the very quadratic equation, concerning which ye know all the ancients busied themselves. It is true, particular methods may be, and commonly are, much easier than the general, yet are not to be preferred, since they require much time in learning, and much memory in retaining. His first method, viz. cum $\frac{r^2}{p^2}=s$, holds only when the biquadratic equation is produced of two quadratics of such a nature: let the equations be $x^2-ax+b=0$, $x^2-cx+d=0$, if $\frac{cb+da}{c+a}$ be not equal to $\sqrt{bd}$, then $\frac{r^2}{p^2}$ is not equal to s, and therefore no place for the rules. Now it is one to ten thousand if $\frac{cb+da}{c+a}=\sqrt{bd}$, as ye may easily see. His second method, viz. cum $\frac{p^2}{4}+\frac{2r}{p}=q$, is only the common way of throwing off the second term; for when this determination is, the second term being put off, the fourth goes with it, and so the biquadratic comes to want the alternate terms, and consequently falls to be a quadratic equation with a plane root.

As to the roots of equations while they are in arithmetical progression, we may perchance be in some difficulty to know when they are so; which, without question, this gentleman hath determined, seeing it is easily done by comparing of equations, which I take by your letter to be his method. Sit enim $x^3-px^2+qx-r=0$, si $r=\frac{9pq-2p^3}{27}$, the equation hath three roots in arithmetical progression.

I am hardly of this gentleman's mind to think that Hudden understands not thoroughly his method de maximis et minimis: yea, I think two or three arithmetical progressions much more proper for Hudden's design than a trigonal or pyramidal progression, seeing by the former ye may take away any two or three terms ye please, which I see not performable by the latter. I have as little charity for his overvaluing Des Cartes his method so as to think that all since discovered are but its consectaries. Yet this hinders nothing the esteem I have for the gentleman, who (if I may judge ex ungue leonem) surely is a great algebraist, and, albeit probably he may be inferior to Mr. Newton, is without question far beyond me, whom ye are pleased too much to overvalue.

I see no connexion betwixt my general method of giving the surd roots of all equations and these particular rules invented by that gentleman. Mine in the biquadratic and cubic coincides with Des Cartes; and in higher equations, as in Des Cartes his rules, there occur frequently impossible cases. As for the asymmetry of equations, to turn it off, I use only De Beaune's method, pag. 114, which I dare say is not inferior (if rightly applied) to any possible, and is nothing but a consectary to the general method of turning off unknown quantities, which (albeit extremely easy) I find not clearly handled by any, yea, not touched by any but Hudden and that place of De Beaune.

It is probable that this gentleman's universal method, when it comes to public, may be more compendious than mine, for the truth is, mine, in the invention of the particular canons, (for one canon serves always for all equations of the same number of dimensions,) is very laborious, yea, beyond any thing I have yet seen practised. If his be not more compendious than mine, I question if a twelvemonth shall serve for to calculate the canons for the equations of the first ten dimensions. And yet the method of my procedure comes near to persuade me that there is none more compendious; yea, in the cubic and biquadratic it is more compendious than any I have yet seen, but indeed the labour increases at a strange rate as the dimensions augment. If any would undertake to calculate the canons, I would willingly communicate the method with its demonstration; for the truth is, I have not patience for such tedious work, and I suppose ye may have as little to look over this pitiful scribble of

your most humble servant,

J. GREGORY.

CCXXI.

J. GREGORY TO COLLINS.

Edinburgh, 11 Sept. 1675.

Sir,

I have yours of the 4th of this instant with Mr. Dary's paper enclosed, where indeed he hath the algebra more fully than others hitherto have treated of it. I know not if his chap. ii. x. xi. be the same with what I did communicate to you or better. Yet there are in this paper many necessary and easy things wanting: viz. in any equation having such a relation betwixt its roots, that one being given all the rest may be found by it, how to bring the said equation to a simple lateral one; or if two roots being given all the rest may be found by them, how to bring it to a quadratic equation; or if three roots being given all the rest may be found by them, how to bring it to a cubic equation, et sic de cæteris in infinitum. This contains his rules 3, 4, and some more. Also having two or more equations, to find a new equation, whose root shall be the sum of the roots of the given equations, or their difference, or their product, or (in a word) any thing [which] can be made up of the roots, or by the roots of the former equations. This contains his rules 6, 7, and considerably more.

I thank you kindly for your proposal to calculate my canons; yet, if ye will suffer me to use freedom with you, I cannot communicate my method upon an uncertainty. For ye tell me that ye only expect encouragement to Mr. Dary; yea, not only that, but I am extremely apprehensive that Mr. Dary hath not patience enough for such a tedious work. And if ye will yet allow me a little more freedom, I have no more for this than I had for the publication of Kepler's problem resolved by me; yea, not so much, seeing ye promised that absolutely, and had the book (viz. Horroccii Posthuma) in your own power, and for this ye only expect encouragement to another, and despair not to get it published in the Transactions. I am glad that the Transactions are filled with such fine things that it is hard to get place among them for analytical improvements. I only desire that either one or more persons, known to be sufficiently expert in such calculations, may engage themselves to the world in the Philosophical Transactions for perfecting of the first ten canons and sending them to me within such a time, and I shall immediately thereafter send to you the method with its demonstration. This, I hope, will not require much room, and consequently not much divert the candid reader from what else he may justly expect in these philosophical occurrences.

In one of yours lately to me ye say ye are owing me 44*s*., but ye are not owing so much. You have to rebate the price of the Archimedes, &c., which I desire to know with the first occasion. I entreat you earnestly to send me, with the first post, the sheet of additions to Dr. Barrow's Geometry, enclosed in a letter. No further, but, desiring your pardon for this and many other troubles I have given you, I rest,

your most humble and obliged servant,

J. GREGORY.

CCXXII.

J. GREGORY TO COLLINS.

Edinburgh, 2 October, 1675.

Sir,

By yours of the 21st of the last month I am sufficiently satisfied as to Kepler's problem, neither did I mention it upon the account of any jealousy I had of you, or that I had any particular esteem for it beyond other things I sent you, yea, it is but a consectary of some of the rest: yet I thought it strange, (seeing ye did write for it upon the account to publish it, shewing me also one that Splessius in Germany was about to publish it there,) that after you received it, I heard no more of it. This, I assure you, surprised me a little, and made me much less communicative than otherwise my inclination had prompted me.

As for the person who should calculate these canons, I am little concerned who do it, if it be done, and I see no hazard of the miscarriage of the calculation. I am not much afraid of these, who promise to take away all the intermediate potestates, for if ever that be done, my method will not be worth sixpence. It is much more laborious, and, if I be not much mistaken, I can demonstrate the impossibility of the other. By varying the signs of these quantities that compose one root, (for each respective dimension,) all the other roots are composed. This variation of signs, by making quantities negative, and consequently incapable of a square root, biquadrate, quadratocubic, &c. doth make many equations incapable of surd roots, expressed by my method, and if not by all others mine is worth nothing. The method is by depressing the equation from a superior degree always to one inferior. I understand not what ye mean when ye ask if these surdities can be taken away by advancing the same to pure potestates; for that cannot be done in the surd root of any adfected equation, no not in the quadratic; for all the potestates of the surd root of an adfected quadratic equation are surd in infinitum. And on the contrary, the potestates of the roots of any equation being multiplied by some quantities, (if this be your meaning,) viz. their coefficients, and added together, or sometimes subtracted, can always make up a commensurable quantity. I desire only canons for the first ten, because I am afraid hardly any man's patience will reach further: I wish some may come that length. I dare not affirm that this shall be the best practical way for finding the roots of equations quam proxime, even when logarithms are applied, which may easily be done; yea, I think I have methods of serieses going very far beyond it, and, to be ingenuous, I think one of its greatest uses is by drawing learned men from this contemplation, which busied so many fine wits. They may employ themselves in other things of no less concernment to the advancement of learning and perchance much more obvious. I am much obliged to you and the stationers for these books, which I shall do my best to cause some of [my] scholars to examine; but since I came here, I have none yet capable but one, whom I keep busied in drawing up some things to

T 2

myself. I desire you will do me the favour to send me one of these pieces of Pere Malbranche, and also that piece of Mercator, and any else ye hear much famed by learned mathematicians; if it be a peculiar treatise as for course and system, I fancy them but little.

I desire also ye would find me the most compendious and most pertinent writer about the rules, grounds, and reasons of keeping merchants' accounts. I have your late piece, which, as I fancy, supposeth a man sufficiently known in the grounds of it before. I learned it in Zealand a considerable time ago after Stevinus his method, where the capital (which I suppose is your stock) balanceth the whole book. This I understand abundantly; but I cannot perceive how the balance cometh to balance the book, and what things are brought debitor and creditor to it, and what to the stock.

I know what the additions are to Barrow's Geometry and desire them, because I am to bind all his works together. Send me the prices of these things I desire, and when my money shall be expended I shall send you more. In the mean time, if I can serve you in any thing here, ye may freely command

your most humble servant,

J. GREGORY.

I would gladly hear news of Dr. Davenant's problem, yea, I entreat you to acquaint me with what Slusius hath done or is like to do with it. I am no less curious to know if Mr. Anderson doth yet push at these Tentamina, and what he can reply to my answer.

CCXXIII.

COLLINS TO J. GREGORY.

Mr. Gregory,—Sir, 19 Oct. 1675.

I have yours of the 11th Sept. and 2d Octob., and both of them seem to me like a recantation of the offer made in yours of the 20th of Aug., of imparting your last general method with its demonstration, if a fit person could be found to calculate the canons for the first ten dimensions. Now unless it be to serve you, and consequently the public, I am not in the least covetous to know it, and I am in some doubts about it, viz.

1. In yours of the 26th of May, 1675, you say you can, by elevating the equation to higher dimensions, take away all the intermediate terms: and in yours of the 2d Octob. you say you are not afraid of those that promise to take away all the intermediate potestates, for if ever that be done, your method will not be worth sixpence, and that you can demonstrate the contrary. These to me are contradictories, unless in the latter clause be understood, without advancing the equation, which is readily granted, and I myself very well know what habitudes the coefficients must have to each other to take away two, three, more, or all the inferior terms; and though this cannot be absolutely done, yet whether asymptotically approached, and the difference from a pure power rendered inconsiderable, and consequently with safety neglected, may be worth consideration. The French pretend to take away all intermediate potestates, but have said nothing as to the modus. Malbranche's book is not yet come over, but fresh commendations of it are arrived both from Justel and others.

2. This removal of terms you say is done by advancing, whereas your new method is done by depressing the equation from a superior degree always to a degree inferior. If this could always be done, all cubic equations might be solved by aid of quadratics, and all solid problems would become plane; but perchance your words have a particular meaning, and that any equation being proposed, you can so alter it as to cause it to have a pair of equal roots, that both the last terms, upon new forming, may fall off together; whereas Hudden's method of limits only makes the penultimate term drop off. [I] doubt whether this can be done by an equation a degree lower, but I am much mistaken if to force an equality between a negative and affirmative root be not a mere useless matæotechnia.

3. In yours of the 20th August you say you see no connexion between your general method of giving the surd roots of all equations and those particular rules invented by the gent. (lately here). And yet in yours of [the] 2d Octob. 1675, you say many equations are incapable of surd roots, viz. all such as have in the roots negative squares, biquadratics, &c. I nothing doubt but the roots of such negative squares, &c., denote an impossibility; as for instance Dr. Wallis, in his first tome, assumes the two legs of a triangle, 2 and 1, to be less than the base 4, and that to shew that algebra might be fallacious to a tiro, and really finds the segments of the base well; but had he proceeded further he would have found the perpendicular to have been $\sqrt{-105}$, which had manifested the impossibility; but George Morh the Dane, when he was lately here, denied that you need be encumbered with any such negative squares in the amendment of Cardan's rules, he adding the quantities that beget it instead of subtracting

them, and asserted that Cardan's rules might be as well rendered general for finding all the three roots in a cubic equation when the resolvend is a prime number and no root an unit, as by the same one root might always be as well found as the surd root of any prime number, when the equation had but one root possible. He is gone to Paris after M. Tschirnhaus, from whence he will impart the same, if I do not meet with it in a little treatise of that argument and of Fortifications that he left in Holland to be printed in low Dutch, or if I do not apprehend it out of a paper he left about extracting the cube roots of binomials, about which the learned have hitherto but fumbled, giving but one root, whereas if the binomial be capable of one root, it is always capable of two more. And whereas you say you think you can demonstrate the impossibility of rendering Cardan's rules general, I could wish you would begin the attempt to try if an adversary or correspondent cannot be found.

4. In some of your letters you thought your new methods of some value, (unless you flattered yourself too much, as you say,) and yet in your last letter you think, concerning your last method, that one of the greatest uses of it will be to draw learned men from that contemplation, and that the methods of series go (I suppose you mean only in some cases) very far beyond it. To which I answer, What need then of such tedious calculations for the ten first canons?

When I accidentally mentioned unto Dr. Pell the method of series for finding the roots of adfected equations in numbers, those series being made by extracting the adfected roots in species, he said, it was no new thing to him, and that he used methods more speedy and easy, and, as I remember, Mr. Newton is of the same opinion, who communicated some such series to me, with a method of extracting them, much different from that of Vieta in numbers, were it applied to species. And I have some reason to incline to their opinion. But as to other cases, where infinite series are applied to quadratures, Mr. Newton (whom I have not writ to or seen these eleven or twelve months, not troubling him as being intent upon chemical studies and practices, and both he and Dr. Barrow beginning to think mathematical speculations to grow at least dry, if not somewhat barren) is not of the same mind; but Dr. Pell is positive that whatsoever the same shall be applied to, even out of the same series a better method may be derived from the performance.

5. To clear up somewhat you do not understand when I talk of taking away surdities; I answer, I should have worded my meaning better, viz. by taking away the radical signs. But to clear up my meaning the better, suppose in your or M. Tschirnhaus's method, you have a root composed of diverse quantities, whereof some are surds, I presume when in the same equation you take away all the intermediate potestates, you do at last find some high potestate, by aid of the root whereof you attain a root of the equation first proposed.

But enough of this: you will say, quorsum hæc perditio? I shall tell you. If it be your real desire that somebody should be employed to calculate your first ten canons, I think you ought to make a more clear proposal of their benefit, (in answer to the queries I put concerning them, or others that you yourself doubtless could have proposed more proper,) and I accordingly shall make an address to the Royal Society to employ and encourage an undertaker, and the end of this long harangue is but to plead my apology for what else I thought I should have seemed too rude to have said barely in a magisterial way——

CORRESPONDENCE OF NEWTON.

CCXXIV.

NEWTON TO COLLINS.

Trin. Coll. Cambridge,
Sir, Jan. 19,[q] 1669.

I received Dr. Wallis his Mechanics, which you sent to Mr. Barrow for me. I must needs acknowledge you more than ordinarily obliging, and myself puzzled how I shall quit courtesies.

The problems you proposed to me I have considered, and sent you here the best solutions of one of them that I can contrive; namely, how to find the aggregate of a series of fractions, whose numerators are the same, and their denominators in arithmetical progression. To do this I shall propound two ways. The first by reduction to one common denominator as followeth:

If $\frac{a}{b}, \frac{a}{b+c}, \frac{a}{b+2c}, \frac{a}{b+3c}$, &c. be the series: multiply all their denominators together, and the product will be $b^4+6b^3c+11bbcc+6bc^3$; each term of which multiplied by its dimensions of b $^{4\,3\,2\,1}$, and the product again multiplied by $\frac{a}{b}$, the result shall be the numerator of the desired aggregate

$$\frac{4ab^3+18abbc+22abcc+6ac^3}{b^4+6b^3c+11bbcc+6bc^3}.$$

[q] The day of the month is omitted in the letter and is restored from the postmark.

If $\frac{a}{b-2c}, \frac{a}{b-c}, \frac{a}{b}, \frac{a}{b+c}, \frac{a}{b+2c}$, &c. be the series: the factus of their denominators is $b \times \overline{bb-cc} \times \overline{bb-4cc}$, or $b^5 - 5b^3cc + 4bc^4$, the denominator; which multiplied as before, gives $5ab^4 - 15abbcc + 4ac^4$, the numerator of the aggregate. If $\frac{a}{b-c}, \frac{a}{b+c}, \frac{a}{b+3c}$, &c., is the series, then $\overline{bb-cc} \times \overline{b+3c}$ or $b^3 + 3bbc - bcc - 3c^3$ is the denominator, and $3abb + 6abc - acc$ the numerator of the aggregate.

In numbers: if $\frac{1}{2}, \frac{1}{3}, \frac{1}{4}, \frac{1}{5}, \frac{1}{6}$, is the series, then putting $b = 2$, that the series may be $\frac{1}{b}, \frac{1}{b+1}, \frac{1}{b+2}, \frac{1}{b+3}, \frac{1}{b+4}$; the factus of their denominators will be $b^5 + 10b^4 + 35b^3 + 50bb + 24b$, the denominator, and consequently $5b^4 + 40b^3 + 105bb + 100b + 24$ the numerator of the aggregate.

But it is better to put $b = 4$, that the series may be $\frac{1}{b-2}, \frac{1}{b-1}, \frac{1}{b}, \frac{1}{b+1}, \frac{1}{b+2}$. And so shall the aggregate be $\frac{5b^4 - 15bb + 4}{b^5 - 5b^3 + 4b}$.

The annexed table will much facilitate the multiplication of denominators together.

				$+1$	-1	for 3 terms.
			$+1$	-5	$+4$	for 5 terms.
		$+1$	-14	$+49$	-36	for 7 terms.
	$+1$	-30	$+273$	-820	$+576$	for 9 terms.
$+1$	-55	$+1023$	-7645	$+51276$	-14400	for 11 terms.
b^{11}	b^9cc	b^7c^4	b^5c^6	b^3c^8	bc^{10}	

This rule holds good, though the differences of the denominators be not equal; as if $\frac{a}{b+c}, \frac{a}{b+d}, \frac{a}{b-e}$, are to be added, the factus of their denominators is $b^3+bbc+bbd-bbe+bcd-bce-bde-cde$, the denominator, which multiplied by the dimensions of b, and again by $\frac{a}{b}$, produces $3abb+2abc+2abd-2abe+acd-ace-bde$, the numerator of the desired sum.

The other way of resolving this problem is by approximation. Suppose the number of terms in the propounded series be p; and make $\frac{pp-p}{2}=q$, $\frac{2pq-q}{3}=r, qq=s, \frac{6qr-r}{5}=t, \frac{4qs-s}{3}=v, \frac{12rs-5t}{7}=x$, $2ss-v=y$, $\frac{rv-rs}{3}+t=z$, &c. Now if the propounded series be $\frac{a}{b}, \frac{a}{b+c}, \frac{a}{b+2c}, \frac{a}{b+3c}$, &c. their aggregate shall be $\frac{a}{b}$ in $p-q\frac{c}{b}+r\frac{cc}{bb}-s\frac{c^3}{b^3}+t\frac{c^4}{b^4}$, &c. in which progression, the further you proceed the nearer you approach to truth.

But it is better to put b for the denominator of the middle term of the propounded series thus, $\frac{a}{b-2c}$, $\frac{a}{b-c}, \frac{a}{b}, \frac{a}{b+c}, \frac{a}{b+2c}$; and making n the number of terms from the said middle term either way, as also

$$nn+n=m,\ 2n+1=p,\ \frac{mp}{3}=r,\ \frac{3mr-r}{5}=t,$$

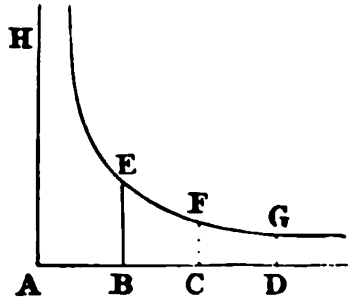
$\frac{3mmr-5t}{7}=x, \frac{m^3r-2mmr}{3}+t=z$, &c. the desired aggregate shall be $\frac{a}{b}$ in $p+r\frac{cc}{bb}+t\frac{c^4}{b^4}+x\frac{c^6}{b^6}+z\frac{c^8}{b^8}$, &c. a progression wanting each other term, and also converging much more towards the truth than the former.

Now a series of fractions being propounded; first, consider how exact you would have their aggregate; suppose not erring from truth above $\frac{\iota}{\epsilon}$ part of an unit. Then make a rude guess how many times $\frac{b}{nc}$ multiplied into itself will be about the bigness of $\frac{5a\epsilon}{2b}$, more or less, and omit all those terms of the progression where b is of more than so many dimensions. For example, if the aggregate of $\frac{10000}{100}+\frac{10000}{106}+\frac{10000}{112}+\frac{10000}{118}+\frac{10000}{124}+\frac{10000}{130}+\frac{10000}{136}$ be desired to the exactness of $\frac{1}{8}$ part of an unit; then is $a=10000$, $b=118$, $c=6$, $n=3$, $\epsilon=8$, $\frac{b}{nc}=\frac{118}{18}$, or about $6\frac{1}{2}$, $\frac{5a\epsilon}{2b}=\frac{200000}{118}$ or about 1700, to which $6\frac{1}{2}$ square-squared, or multiplied three times into itself, is about equal. Therefore I take only the two first terms of the rule, $\frac{a}{b}$ in $p+r\frac{cc}{bb}$, b in the rest being of above three dimensions, and so making $2n+1\ (=7)=p$, $\frac{nn+n}{3}\times p\ (=28)=r$, the desired aggregate will be $\frac{a}{b}\times\overline{7+28\frac{cc}{bb}}$ or $\frac{70000}{118}$ in $\frac{14068}{13924}$, wanting about an eighth part of an unit. But if an exacter aggregate be desired, take another term of the rule, and the error will not be above $\frac{1}{350}$ of an unit. Thus if the said series were continued to 21 terms, $\frac{10000}{100}$ being the first, $\frac{10000}{160}$ the middle, and

$\frac{10000}{220}$ the last term, three terms of the rule would give an aggregate too little by about $\frac{1}{2}$ of an unit, four times by about $\frac{1}{12}$ or $\frac{1}{15}$ part, and five terms by about $\frac{1}{100}$ part, or less. But perhaps it might be more convenient to resolve this at twice, first finding the aggregate of the last eleven terms, and then of the next nine, and lastly adding the first term to the other two aggregates. And this may be done to about the 60th part of an unit by using only the three first terms of the rule.

From these instances may be guessed what is to be done in other cases. But it may be further noted that it will expedite the work to subduct the logarithm of b from that of c, and multiply the remainder by 2, 4, 6, 8, &c. which products shall be the logarithms of $\frac{cc}{bb}$, $\frac{c^4}{b^4}$, $\frac{c^6}{b^6}$, $\frac{c^8}{b^8}$, &c. whose computation in proper numbers would be troublesome.

This problem much resembles the squaring of the hyperbola; that being only to find the aggregate of a series of fractions, infinite in number and littleness, with one common numerator to denominators whose differences are equal and infinitely little. And as I referred all the series to the middle term, the like may be done conveniently in the hyperbola. If AC, AH, are its rectangled asymptotes and the area BDGE is desired: bisect BD in G, make $AC = a$, $CF = b$, and CD or $CB = x$, so that



$\frac{ab}{a+x}=$ DG, and $\frac{ab}{a-x}=$ BE. Then, according to Mercator, the area GDCF is $bx-\frac{bxx}{2a}+\frac{bx^3}{3aa}-\frac{bx^4}{4a^3}+\frac{bx^5}{5a^4}$, &c., and the area BCFE is $bx+\frac{bxx}{2a}+\frac{bx^3}{3aa}+\frac{bx^4}{4a^3}+\frac{bx^5}{5a^4}$, &c. And the sum of these two makes the whole area BDGE $=2bx+\frac{2bx^3}{3aa}+\frac{2bx^5}{5a^4}$, &c. where each other term is wanting, and x is less by half than it would otherwise have been, which makes the series more converging toward the truth.

As to your other problem about the resolution of equations by tables; there may be such tables made for cubic equations, and consequently which shall serve for those of four dimensions too; but scarcely for any others. Indeed, could all equations be reduced to three terms only, tables might be made for all; but that is beyond my skill to do it, and belief that it can be done. For those of three dimensions there needs but one column of figures be added to the ordinary tables of logarithms, and the construction of it is pretty easy and obvious enough. If you please, I will some time send you a specimen of its composition and use, but I cannot persuade myself to undertake the drudgery of making it.

Your Kinkhuysen's Algebra I have made some notes upon, I suppose you are not much in haste of it, which makes me do that only at my leisure.

Your obliged friend and servant,

Is. NEWTON.

CCXXV.

NEWTON TO COLLINS.

Trin. Coll. Feb. 6, 1669.
Cambridge.

Sir,

Mr. Barrow shewed me some of your papers, in which I was much pleased at M. Cassini's invention for finding the apogæa and eccentricities of the planets.

For your annuity problem, I have sent you the following solution, to consider of what use it may be.

To know at what rate (N per cent) an annuity of B is purchased for thirty-one years at the price A.

The rule is, $$\frac{6 \text{ log. of } \frac{31\,B}{A}}{100 - 50 \text{ log. of } \frac{31\,B}{A}} = \text{log. of } \frac{100+N}{100}.$$

As, for example, if 1200*l.* be paid at present for 100*l.* yearly for thirty-one years, then is the logarithm of $\frac{31\,B}{A} = 0.41218$, and consequently by the rule $\frac{2.47308}{79.39100}$, or 0.03111, is the logarithm of $\frac{100+N}{100}$; whence $\frac{100+N}{100} = 1.0743$, and $N = 7.43$, or 7*l.* 8*s.* 7*d.* So that the annuity was bought at the rate of 7*l.* 8*s.* 7*d.* per cent.

This rule is not exact, but yet so exact as never to fail above 2*d.* or 3*d.* at most, when the rate is not above 16*l.* per cent. And if the rate be above 16*l.* or 18*l.* per cent, or, which is all one, if $A < 6\,B$, then this rule $\frac{A+B}{A}=\frac{100+N}{100}$ will not err above 2*s*. You may try the truth of these rules by the equation $X^{32}=\frac{A+B}{A}X^{31}-\frac{B}{A}$, putting $\frac{100+N}{100}=X$, and working in logarithms.

You seem to apprehend as if I was about writing elaborate notes upon Kinkhuysen. I understood from Mr. Barrow that your desire was only to have the book reviewed, that, if any thing were defective or amiss, it might be amended, and to that purpose, about two months since, I reviewed it, and made some such observations upon it. But, though the book be a good introduction, I think it not worth the pains of a formal comment, there being nothing new or notable in it which is not to be found in other authors of better esteem.

You make mention of another book of the same author, translated badly into Latin by a German gunner, which you would have me correct. I understand not Dutch, and would not willingly do the author so much wrong as to undertake to correct a translation, where I understand not the original. I suppose there want not mathematicians in London that understand Dutch.

In finding the aggregate of the terms of a musical progression there is one way by logarithms very obvious, (viz. by subducting the logarithms of each denominator from that of the numerator, &c.) which I supposed to be the ordinary way in practice, and therefore mentioned it not in my letter. If you mean another way, I would be glad to have it communicated. Thus, sir, I am

your troublesome friend and servant,

ISAAC NEWTON.

CCXXVI.

NEWTON TO ——[a].

Trin. Coll. Cambridge,
Feb. 23, 1668-9.

Sir,

I promised in a letter to Mr. Ent to give you an account of my success in a small attempt I had then in hand: and it is this. Being persuaded of a certain way whereby the practical part of optics might be promoted, I thought it best to proceed by degrees, and make a small perspective first, to try whether my conjecture would hold good or not. The instrument that I made is but six inches in length, it bears something more than an inch aperture, and a plano-convex eyeglass, whose depth is $\frac{1}{6}$th or $\frac{1}{7}$th part of an inch; so that it magnifies about forty times in diameter, which is more than any six feet tube can do, I believe, with distinctness. But, by reason of bad materials, and for want of good polish, it represents not things so distinct as a six feet tube will do; yet I think it will discover as much as any three or four feet tube, especially if the objects be luminous. I have seen with it Jupiter distinctly round and his satellites, and Venus horned. Thus, sir, I have given you a short account of this small instrument, which, though in itself contemptible, may yet be looked upon as an epitome of what may be done according to this way, for I doubt not but in time a six feet tube may be made after this method, which will perform as much as any sixty or hundred feet tube made after the common way; whereas I am

[a] Printed from a paper in the handwriting of Collins.

persuaded, that were a tube made after the common way of the purest glass, exquisitely polished, with the best figure that any geometrician (Des Cartes, &c.) hath or can design, (which I believe is all that men have hitherto attempted or wished for,) yet such a tube would scarce perform as much more as an ordinary good tube of the same length. And this, however it may seem a paradoxical assertion, yet it is the necessary consequence of some experiments, which I have made concerning the nature of light.

The paper is continued in Collins's handwriting.

The above is [a] copy of a letter written by Mr. Newton to a friend of his.

The telescope therein mentioned hath been lately sent up to the Royal Society, who gave Mr. Cox order to make one after the same manner of contrivance four feet long, the which hath been done; one end of the tube is open, at the other end is placed a concave metalline mirror, the diameter whereof is betwixt four and five inches; it was ground on a sphere of fourteen feet diameter, and about its focus, which is about four feet off, is placed a reflecting plate as big as a twopence, inclined at an angle of forty-five degrees to the axis, so that the reflected rays falling thereon, are again reflected upright to the side of the telescope, where the eye, through a small hole, wherein is placed a small plano-convex glass, beholds the object on the reflecting plate, as much magnified as it could have been done by an ordinary telescope of forty feet long or more, and void of colours. The mirror and reflecting plate are made to be taken out and wiped at pleasure; they are not yet pleased with the metal or polish of the reflecting plate, but are trying Lapis

Osmandinus, a black stone that comes from mount Hecla in Iceland, and other materials, whereof you may afterwards hear the success.

Mr. Hooke, seeing this telescope to obtain esteem, about a month since put in a proposal in writing to the Royal Society in words to this effect:

The perfection of telescopes, microscopes, scotoscopes, and burning glasses, by figures as easily made as those that are plane or spherical, whereby the light and magnitude of objects is prodigiously increased, and whatsoever hath hitherto been attempted or almost desired in dioptrics accomplished——with a cipher containing the mystery, the which he disclosed to the Lord Brouncker and Dr. Wren, who report plausibly of it, and what is done in this way is performed by glass refraction.

Mr. Hooke moreover affirmed, coram multis, that in the year 1664 he made a little tube of about an inch long to put in his fob, which performs more than any telescope of fifty feet long, made after the common manner; but the plague happening which caused his absence, and the fire, whence redounded profitable employments about the city, he neglected to prosecute the same, being unwilling the glass grinders should know any thing of the secret.

Gottigni, the scholar of Gregory of St. Vincent, whose remains he hath, is said to have made wonderful (but in what respect I know not) telescopes at Rome, and to have published a treatise of dioptrics there.

Another useful instrument lately invented here is Sir Samuel Moreland's loud speaking trumpet, of which he hath written a book or history, with the title of Tuba Stentorophonica, value one shilling, by which persons may discourse at about a mile and a half distance, if not more.

CCXXVII.

NEWTON TO ASTON.

Fr. Trin. Coll. Cambr. May 18, 1669.

Since in your letter you give me so much liberty of spending my judgment about what may be to your advantage in travelling, I shall do it more freely than perhaps would otherwise have been decent. First, therefore, I will lay down some general rules, most of which I believe you have considered already; but if any of them be new to you, they may excuse the rest: if none at all, yet it is my punishment more in writing them than yours in reading them.

When you come into any fresh company,
1. Observe their humours. 2. Suit your own carriage thereto, by which insinuation you will make their converse more free and open.
3. Let your discourse be more in queries and doubtings than peremptory assertions or disputings, it being the design of travellers to learn, not teach; besides, it will persuade your acquaintance that you have the greater esteem of them, and so make them more ready to communicate what they know to you; whereas nothing sooner occasions disrespect and quarrels than peremptoriness. You will find little or no advantage in seeming wiser or much more ignorant than your company.
4. Seldom discommend any thing, though never so bad, or do it but moderately, lest you be unexpectedly forced to an unhandsome retraction. 'Tis safer to commend any thing more than it deserves, than to discommend any thing so much as it deserves. For commendations meet not so often with oppositions, or at least are not usually so ill resented by men that think otherwise, as discommendations. And you will

insinuate into men's favour by nothing sooner than seeming to approve and commend what they like: but beware of doing it by a comparison. 5. If you be affronted, 'tis better in a foreign country to pass it by in silence, or with a jest, though with some dishonour, than to endeavour revenge: for in the first case, your credit is ne'er the worse when you return into England, or come into other company that have not heard of the quarrel; but in the second case, you may bear the marks of your quarrel while you live, if you outlive it at all. But if you find yourself unavoidably engaged, 'tis best, I think, if you can command your passion and language, to keep them pretty evenly at some certain moderate pitch, not much heightening them, to exasperate your adversary or provoke his friends, nor letting them grow overmuch dejected to make him insult. In a word, if you can keep reason above passion, that and watchfulness will be your best defendants. To which purpose you may consider, that though such excuses as this, He provoked me so much I could not forbear, may pass amongst friends, yet amongst strangers they are insignificant, and only argue a traveller's weakness.

To these I may add some general heads for inquiries or observations, such as at present I can think on. As, 1. To observe the policies, wealth, and state affairs of nations, so far as a solitary traveller may conveniently do: 2. Their impositions upon all sorts of people, trades, or commodities, that are remarkable: 3. Their laws and customs, how far they differ from ours: 4. Their trades and arts, wherein they excel or come short of us in England: 5. Such fortifications as you shall meet with, their fashion, strength, and advantages for defence, and other such military affairs as are con-

siderable: 6. The power and respect belonging to their degrees of nobility or magistracy.
7. It will not be time misspent to make a catalogue of the names and excellencies of those men that are most wise, learned, and esteemed in any nation.
8. Observe the mechanism and manner of guiding ships. 9. Observe the products of nature in several places, especially in mines, with the circumstances of mining and of extracting metals or minerals out of their ore and refining them; and, if you meet with any transmutations out of one species into another, (as out of iron into copper, out of any metal into quicksilver, out of one salt into another, or into an insipid body, &c.), those above all others will be worth your noting, being the most luciferous, and many times lucriferous experiments too in philosophy: 10. The prices of diet and other things: 11. And the staple commodities of places.

These generals, (such as at present I could think of,) if they will serve for nothing else, yet they may assist you in drawing up a model to regulate your travels by.

As for particulars, these that follow are all that I can now think of, viz. whether at Chemnitium in Hungary (where there are mines of gold, copper, iron, vitriol, antimony, &c.) they change iron into copper by dissolving it in a vitriolate water, which they find in cavities of rocks in the mines, and then melting the slimy solution in a strong fire, which, in the cooling, proves copper. The like is said to be done in other places which I cannot now remember. Perhaps too it may be done in Italy; for about twenty or thirty years ago there was a certain vitriol came from thence, (called Roman vitriol, but of a nobler virtue than that which is now called by that name,) which vitriol is not now to be gotten, because perhaps they make a greater gain by some such trick as turning iron into copper with it, than by selling it. 2. Whether in Hungary, Sclavonia, Bohemia, near the town Eila, or at the mountains of Bohemia near Silesia, there be rivers whose waters are impregnated with gold; perhaps the gold being dissolved by some corrosive waters like aqua regis, and the solution carried along with the stream that runs through the mines. And whether the practice of laying mercury in the rivers till it be tinged with gold, and then straining the mercury through leather, that the gold may stay behind, be a secret yet, or openly practised. 3. There is newly contrived in Holland a mill to grind glasses plane withal, and I think polishing them too; perhaps it will be worth your while to see it. 4. There is in Holland one —— Bory, who some years since was imprisoned by the Pope, to have extorted from him some secrets (as I am told) of great worth, both as to medicine and profit, but he escaped into Holland, where they have granted him a guard. I think he usually goes clothed in green: pray inquire what you can of him, and whether his ingenuity be any profit to the Dutch. 5. You may inform yourself whether the Dutch have any tricks to keep their ships from being all worm-eaten in their voyages to the Indies—whether pendulum clocks be of any service in finding out the longitude, &c. I am very weary, and shall not stay to part with a long compliment, only I wish you a good journey, and God be with you.

Is. Newton.

P.S. Pray let us hear from you in your travels. I have given your two books to Dr. Arrowsmith.

Printed in the Gen. Dict. vol. VII. p. 478.

CCXXVIII.

NEWTON TO COLLINS.

Feb. 18, 1669-70.

Sir,

Two days since, I received yours and Mr. Dary's letter with a book, for which I thank Mr. Dary, and have, here inclosed, sent him my thoughts of what he desired. That solution of the annuity problem, if it will be of any use, you have my leave to insert it into the Philosophical Transactions, so it be without my name to it. For I see not what there is desirable in public esteem, were I able to acquire and maintain it. It would perhaps increase my acquaintance, the thing which I chiefly study to decline. Of that problem I could give exacter solutions, but that I have no leisure at present for computations. I now see a way, too, how the aggregate of the terms of musical progressions may be found, (much after the same manner,) by logarithms, but the calculation for finding out those rules would be still more troublesome, and I shall rather stay till you have leisure to do me the favour of communicating what you have already composed on that subject.

Your much obliged servant,

I. Newton.

CCXXIX.

NEWTON TO COLLINS.

Trin. Coll., July 11, 1670.

Sir,

I have here sent your Kinkhuysen's Algebra with those notes, which I have intermixed with the author's discourse. I know not whether I have hit your meaning or no, but I have added and altered those things, which I thought convenient to be added or altered, and I guess that was your desire I should do. All and every part of what I have written I leave wholly to your choice, whether it shall be printed together with the translation or not. If you think fit to print any of it, the directions I have writ in English will shew you where it is to be inserted. But if you have a mind not to change the author so much, I would not have you recede from your intentions upon the account of what I have done. For I assure you I writ what I send you, not so much with a design that they should be printed, as that your desires should be satisfied to have me revise the book. And so soon as you have read the papers, I have my end of writing them. In a letter you hinted something to be supplied out of Ferguson's Labyrinthus about the extraction of cubic roots; if you meant pure roots, I have done that in as brief, plain, and full a manner as I can. But if you meant adfected roots, 'tis already done by Kinkhuysen, pag. 91, as well as by Ferguson. Indeed Ferguson seems to have done more, insomuch as to comprehend all cases of cubic equations, within the same rules; but that more is inartificial, because it supposes the extraction of cubic roots out of imaginary binomiums, which how to do he hath not taught us, his rule, taught in pag. 4, not extending to it. Thus his second example, $x^3=6x+4$, supposeth the cubic root of $2+\sqrt{-4}$ to be extracted, which indeed is $-1+\sqrt{-1}$, but I would know by what direct method he teacheth to find it. Not but that it may be done, and I know how to do it, but I think it not worth the inserting into Kinkhuysen, yet if you think it convenient, (and indeed it may be congruently enough inserted into him at pag. 91,) I will send you it done in my next letter. There remains but one thing more, and that's about the title-page, if you print these alterations, which I have made in the author. For it may be esteemed unhandsome and injurious to Kinkhuysen to father a book wholly upon him, which is so much altered from what he had made it. But I think all will be safe, if after the words, Nunc e Belgico Latine versa, be added, Et ab alio authore locupletata, or some other such note.

Something I have yet to say, and that's about your paper concerning the aggregate of the terms of a musical progression. Namely, your way, deduced from Mercator's squaring of the hyperbola, is the same with the last of those two I had sent you together before. Only I had taken a great deal of pains to bring it to such a form as might be most convenient for practice, and so had made it so intricate, as to other respects, that it is no wonder if you did not discern its fountain, or by what method I had composed it. I beg your pardon, therefore, for that obscurity, but I have since committed a greater fault than that; and that's a neglect of writing to you.

Yet I doubt not but that you have goodness enough to pardon all. In confidence of which I rest

Your most humble servant,

Is. Newton.

I had sent your book immediately upon the receipt of your letter, but that I staid two or three days expecting to see Mr. Pitts. As for the copies of Kinkhuysen you mentioned to send me, I know 'tis usually not without some unwillingness that mathematical books are printed: and I would not so far discourage the printing of it, as to have any copies reserved for me. I had rather purchase your friendship than books; yet, if you please to send me one copy, I shall acknowledge myself your debtor for that, together with Dr. Wallis his Mechanics and the rest.

I. N.

CCXXX.

COLLINS TO NEWTON.

July 13, 1670.

Mr. Newton—Sir,

I received yours with Kinkhuysen's Introduction, and perceive you have taken great pains, which, God willing, shall be inserted into the translation and printed with it. Hereby you have much obliged the young students of Algebra, and the bookseller, who was at the Commencement on Monday and Tuesday, but was so taken up, and conceived you were so too, that he did not see you, but remains your much obliged debtor. I formerly intimated that I thought Kinkhuysen had too lightly handled the doctrine of surd numbers, and that the same might be transcribed from others. I find you in the same opinion, for in one of your marginal notes you say thus, The author having slipped over the Addition, Subtraction, Multiplication and Division of all but quadratic surds, &c., (and he acknowledgeth as much by consequence, himself referring the reader to Wassenare Onwissen Wistconstenaer, however he chiefly thereby intended the roots of binomials, which you have supplied,) being unwilling the young student should be referred to other books, which are scarce, for the doctrine of surds, I will a little further presume, and therefore crave your judgment of what you think necessary to be taken either out of Scheubelius, Von Ceulen, or Hume's, which books I herewith send; and having another Scheubelius here, you need not return that sent, wherewith be pleased to accept of another Libellus de Machina Aquatica. There are two other authors have excellently handled surds; those are Frans Vander Huyps, in his low Dutch Algebra, in 1654, and the Sieur de Taneur in French, in his Tract of irrational quantities and Commentaries on 10 Euclid at Paris in 1640.

I am glad to know you have a better way for the musical progression than the latter you sent up, (being the same I mentioned, not having seriously considered your letter, reserving it to better leisure, having hinted so much in an intermediate letter,) but sorry it put you to so much trouble, being very loth to intrude upon your patience. Mengolus, an excellent mathematician and musician, hath a treatise of Music in the press, and there is newly come over by the post (sent to the society) a little treatise of his in Italian of the sun's parallax and refraction, which I have not yet seen. I was apt to believe that Ferguson had done more than Kinkhuysen in these three particulars:

1. In applying one general rule to both kinds of cubic equations; to wit, as well those that are solved by mean proportional proportionals as those that require trisection:

2. In rendering the roots of cubic and biquadratic equations properly; that is to say, in giving the roots, when they are explicated by fractions or surds exactly, and not by a quam proxime:

3. In improving the general method.

But having failed in the first, I conceive it opportune to shew at least wherein he hath failed, and if you please (which is by you offered and seems desirable) supply his defect.

Lastly, why you should desire to have your name unmentioned I see not; but if it be your will and command so to have it, it shall be observed by——

CCXXXI.

NEWTON TO COLLINS.

July 16, 1670.

Worthy Sir,

I sometimes thought to have altered and enlarged Kinkhuysen his discourse upon surds, but judging those examples I added would in some measure supply his defects, I contented myself with doing that only. But since you would have it more fully done, if the book go not immediately into the press, I desire you'll send it back, with those notes I have made (since you are resolved to print them also) and I will do something more to it. Or if you please to send all but the first sheet or two, while that is printing, I'll review the rest, and not only supply the wants about surds, but that about equations soluble by trisections, and something more I would say in the chapter, Quomodo quæstio aliqua ad æquationem redigatur; that being the most requisite and desirable doctrine to a tiro, and scarce touched upon by any writer, unless in general circumstances, bidding them only Nota ab ignotis usu discernere et adhibere debitum ratiocinium.

As to Ferguson's rendering the roots of equations soluble by trisection, his defect will appear by example. Let us take his second, $x^3 = 6x + 4$, in pag. 12. In order to solve this he bids extract the cubic root of these binomiums $2 + \sqrt{-4}$ and $2 - \sqrt{-4}$. To do this his rule, pag. 4, is, Multiply the binomium by 1000, put it in pure numbers, &c. Now $2 + \sqrt{-4}$ in 1000 makes $2000 + \sqrt{-4000000}$, but to put this in pure numbers is impossible, for $\sqrt{-4000000}$ is an impossible quantity, and hath no pure number answering to it. His rule therefore fails; and the like difficulty is in his third example, and in all other such cases. In general I see not what he hath done more than in Cardan's rules. For in this instance Cardan's rules will give you $x = \sqrt[3]{2 + \sqrt{-4}} + \sqrt[3]{2 - \sqrt{-4}}$ in which the only difficulty, as before, is to extract the roots of the binomiums $2 + \sqrt{-4}$ and $2 - \sqrt{-4}$; which roots indeed are $-1 + \sqrt{-1}$, and $-1 - \sqrt{-1}$, as he assigns them, but tells not how to extract them. Nor do I see what he hath done more than Des Cartes in his solution of biquadratic equations; for both go the same way to work in reducing them first to cubic, and then to quadratic equations. Lastly, I see not in what case his rules will render the roots of cubic or biquadratic equations in proprio genere where those of Cardan or Des Cartes will not. But in haste I must take my leave, remaining

your most obliged servant,

I. NEWTON.

I thank you for your two last books.

CCXXXII.

COLLINS TO NEWTON.

July 19, 1670.

Worthy Sir,

Perceiving by your last that you are willing to take some more pains at present with Kinkhuysen, I remand the same, but do not press yourself in time. Your pains herein will be acceptable to some very eminent grandees of the Royal Society, who must be made acquainted therewith; and, forasmuch as Algebra may receive a further advancement from your future endeavours, and that you are more likely than any man I know herein to oblige the republic of learning, give me leave to remind you of some discourse I had when I first had the happiness of your acquaintance. I intimated that D[r]. P[ell] affirmed that he could most exactly limit any equation, shewing what the homogeneum must be to make any pair or pairs of roots gain or lose their possibility; and secondly, that out of that doctrine of limits he could fill up (with no great toil) columns containing all those ranks of roots both negative and affirmative. I send you a specimen hereof in cubic equations that my meaning may be the better understood. His way of doing it was not by depression as here, but scandendo: the like in his limits, limiting precisely, first quadratic equations, then cubics, then biquadratics, then by aid of these columns making the roots ordinates applied either to the respective homogenea, or to the roots of those homogenea, according to the degree of the first term of the equation, find the genus of curves proper to equations of each kind passing through the tops of those ordinates. This I mention not to put you to any trouble to undertake it, or out of a desire to do it on my account, or when done that you should impart it, but as a thing that will advance the science of algebra, and clear up that which other authors have pretermitted.

Moreover you have happily found out a method of turning any equation of two terms besides the homogeneum into an infinite series, and the total of some of the terms of that series may be obtained by tables. As a help suppose we then that to such an equation as this, $a^5 \pm Ba = N$, I assume a rank of roots to be in arithmetical progression, and make up a series of N, or homogenea, whose fifth differences will be equal. First then I think it will be hereafter proved, that if barely that rank of homogenea were proposed, to find what equations were common to that rank, there might be found five several equations common thereto, and that each of these equations hath, or at least may have, as the equation may be put, one rank of roots in arithmetical progression, and consequently that the differences of the roots of each equation are proportional one to another.

But to return into the way; when I had Ferguson's papers, I only viewed his examples, and that cursorily. It seems he soared but Icari fine to accomplish what Hudden promised, p. 503, in annexis Geometriæ

Cartesianæ. I scrupled his roots of negative quadratic quantities, and imagined that they expunged one another, being affected with contrary signs, but conceited there might be more done in cubics than authors yet insist on, because Hugenius in libro de Magnitudine Circuli divides a sphere in a given reason by trisection, and Lalovera in Elementis Tetragonismicis divides a parabola throughout proportional to a sphere by finding of two means. Are not, both ways, the solution[s] of the cubic equation of the same kind? And for finding the roots mere trisection is used. See especially Dulaurens (which book Dr. Barrow can shew you), page 205—207. That author, as also Lalovera and Leotaud are deceased.

Bartholinus in 1657 wrote a small book de Arte Analytica inveniendi omnia Problemata Proportionalium, maxime Harmonicorum, wherein (detecting some of Vieta's errors) he treats of cubic equations and of framing of rules for binomial roots: I believe there is little new, but possibly his mode of expressing himself may be pleasing. I have not the book of my own, but hope ere long to send you those chapters transcribed.

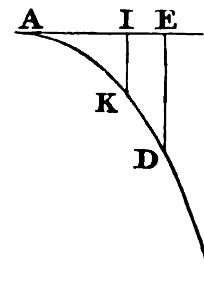
CCXXXIII.

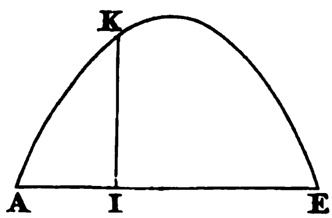
NEWTON TO COLLINS.

Sept. 27, 1670.

Sir,

The receipt of your last letter staying me from sending back your Kinkhuysen's Introduction, I have hitherto deferred writing to you, waiting for Dr. Barrow's return from London, that I might consult his library about what you propounded, in your last letter but one to me, concerning the solutions of cubic equations, before I sent you my thoughts upon it. And yesterday viewing Hugenius de Quadratura circuli, and Lalovera de Elementis Tetragonismicis, I cannot, by that instance you gave me out of those two compared together, be convinced that two mean proportionals may be found by trisecting an arch, or contrarily. Hugenius indeed divides a sphere in a given ratio by trisection, and Lalovera divides a certain parabolic segment in a given ratio by two mean proportionals, Prop. 8. Lib. 4.; but then the divisions of that segment and the sphere are not analogous. For suppose AKD the parabola, touched at its vertex by AE, that IK, ED are parallel to its diameter, then are AIK, AED such segments as he divides by two means; but these segments are as the cubes of the lines AI, AE, whereas the segments of a sphere are not as the cubes of their respective axes. If indeed AKE be a parabola insisting on the base AE and cut by IK parallelly to its axis, the segments of this will be analogous to those of a sphere, but this Lalovera divides otherwise than by two means, Prop. 9. Lib. 4.





I cannot therefore yet be convinced that any one problem can be solved both those ways, which if it could, it would be no hard matter to take away both the two middle terms of any cubic equation, which whoever performs I shall esteem as a great Apollo, and admire as much as if he had squared the circle; because I judge both impossible. And my reason is this, that equations, to what terms soever they are reduced, their real roots never become imaginary nor their imaginary roots real, though indeed their true roots may become false and false ones true. But could a cubic equation, which hath three real roots, (and consequently is solvible by trisection,) have its two middle terms taken away, (and consequently become soluble by three means,) two of its real roots must be transformed into imaginary ones; for all simple cubic equations can have but one root real and two imaginary.

I thank you for your intimation about the limits of equations and differencing their homogeneal terms, but, though the speculation be pretty, I much suspect it will never become useful for the solving of equations. If I chance to meet with any thing that may improve it, you shall have notice thereof.

Upon the receipt of your last letter, I sometimes thought to have set upon writing a complete introduction to Algebra, being chiefly moved to it by this, that some things I had inserted into Kinkhuysen were not so congruous as I could have wished to his manner of writing. Thus having composed something pretty largely about reducing problems to an equation, when I came to consider his examples, (which make the fourth part of his book,) I found most of them solved, not by any general analytical method, but by particular and contingent inventions, which, though many times more concise than a general method would allow, yet, in my judgment, are less proper to instruct a learner, as acrostics, and such kind of artificial poetry, though never so excellent, would be but improper examples to instruct one that aims at Ovidian poetry. But considering that by reason of several divertisements I should be so long in doing it as to tire your patience with expectation, and also that, there being several introductions to Algebra already published, I might thereby gain the esteem of one ambitious among the crowd to have my scribbles printed, I have chosen rather to let it pass, without much altering what I sent you before. Yet, because you seem to be most solicitous about the doctrine of surds delivered in it, I desire that, when your leisure will permit you to write, you would intimate the particulars, in which you think it most defective. For at my revising the papers, I judged it not so imperfect as I thought it had been, when I sent for them back again, and so have hitherto added two or three examples only more than was done before.

I have sent back your Hume, Van Ceulen, Ferguson's Labyrinthus Algebræ, both parts of it, and Kinkhuysen on the Conic Sections; but his Algebra I presume to keep by me till you have occasion for it. So thanking you for the said books, with other favours, and desiring to be excused for troubling you thus amongst the midst of your business, I rest,

your humble servitor,

Is. Newton.

CCXXXIV.

NEWTON TO COLLINS.

Sir, July 20, 1671.

I purposed to have given you a visit at the late solemnity of our Chancellor's creation; but I was prevented in that journey by the sudden surprisal of a fit of sickness, which not long after, (God be thanked,) I again recovered of. And since I am prevented from a verbal acknowledgment of your undeserved favours, I must be yet contented to do it in writing. In which respect I find, by your last letter, that I am still become more your debtor, both for the care you take about my concerns, and for Borellius de Motionibus. But for Borellius I beg that I may be accountable to you at our next meeting, and that you would not, for the future, put yourself to the like trouble in sending any more books. I shall take it for a great favour, if in your letters you will only inform me of the names of the best of those books which newly come forth.

The last winter I reviewed the Introduction, and made some few additions to it. And, partly upon Dr. Barrow's instigation, I began to new methodize the discourse of infinite series, designing to illustrate it with such problems as may (some of them perhaps) be more acceptable than the invention itself of working by such series. But being suddenly diverted by some business in the country, I have not yet had leisure to return to those thoughts, and I fear I shall not before winter. But since you inform me there needs no haste, I hope I may get into the humour of completing them before the impression of the Introduction, because, if I must help to fill up its titlepage, I had rather annex something, which I may call my own, and which may be acceptable to artists, as well as the other to tiros.

There having some things past between us concerning musical progressions, and as I remember you desiring me to communicate something, which I had hinted to you about it, which I then had not (nor have yet) adjusted to practice; I shall in its stead offer you something else, which I think more to the purpose.

Any musical progression, $\frac{a}{b}, \frac{a}{b+c}, \frac{a}{b+2c}, \frac{a}{b+3c}, \frac{a}{b+4c}$, &c., being propounded, whose last term is $\frac{a}{d}$; for the following operation choose any convenient number ϵ, (whether whole, broken, or surd,) which intercedes these limits $\frac{2mn}{b+d}$ and $\sqrt{mn}$, supposing $b-\frac{1}{2}c$ to be m, and $d+\frac{1}{2}c$ to be n. And this proportion will give you the aggregate of the terms very near the truth;

As the logarithm of $\frac{\epsilon+\frac{1}{2}c}{\epsilon-\frac{1}{2}c}$ to the logarithm of $\frac{n}{m}$, so is $\frac{a}{\epsilon}$ to the desired sum.

Example. Suppose the progression be $\frac{100}{5}, \frac{100}{6}, \frac{100}{7}, \frac{100}{8}, \frac{100}{9}, \frac{100}{10}$; that is, $a=100$, $b=5$, $c=1$, $d=10$, $m = 4.5$, $n = 10.5$, $\frac{2mn}{b+d} = 6.3$, $\sqrt{mn} = 6.9$, and $\epsilon = 6.6$, the number equally interceding those limits, 6.3, and 6.9. And the operation will be as follows,

$\frac{\epsilon+\frac{1}{2}c}{\epsilon-\frac{1}{2}c}=\frac{7.1}{6.1}$; its logarithm is 0.065929, and the logarithm of that logarithm is 4.819076;

$\frac{n}{m}=\frac{10.5}{4.5}$, its logarithm is 0.367976,

and the logarithm of that logarithm is 5.565819

$\frac{a}{\epsilon}=\frac{100}{6.6}$, its logarithm is 1.180456

and hence the fourth proportional its logarithm is 1.927199

which indicates 84.566 to be the desired aggregate. The same by adding the several terms together will be found more justly to be 84.5636. But note that, if there were more terms inserted into the progression, (as suppose it was $\frac{100}{5}$, $\frac{100}{5\frac{1}{2}}$, $\frac{100}{6}$, $\frac{100}{6\frac{1}{2}}$, $\frac{100}{7}$, &c.) the rule would still more approach to truth. And so it will in the examples of usury, $\frac{100}{106}$, $\frac{100}{112}$, $\frac{100}{118}$, $\frac{100}{124}$, &c., or $\frac{100}{108}$, $\frac{100}{116}$, $\frac{100}{124}$, $\frac{100}{132}$, &c.; or in any other, where the difference of the denominators bears a less proportion to the denominator of the first term. The ground of this rule I believe you will easily apprehend by contemplating the hyperbola, what relation its area bears to musical progressions. Farewell,

your most obliged servitor,

I. Newton.

CCXXXV.

NEWTON TO OLDENBURG.

Cambridge, Jan. 6, 1671-2.

Sir,

At the reading of your letter I was surprised to see so much care taken about securing an invention to me, of which I have hitherto had so little value. And therefore since the Royal Society is pleased to think it worth the patronising, I must acknowledge it deserves much more of them for that, than of me, who, had not the communication of it been desired, might have let it still remain in private as it hath already done some years.

The description of the instrument you sent me is very well, only the radius of the concave metal, which you put 14 inches, is more justly $12\frac{2}{3}$ or 13 inches; and the radius of the eyeglass, which you put half an inch, is the twelfth part of it, if not less. For the metal collects the sun's rays at $6\frac{1}{3}$ inches distance, and the eyeglass at less than one sixth part of an inch distance from its vertex. By the tools also, to which they were ground, I know their dimensions, and particularly measuring the diameter of the hemispherical concave, in which the eyeglass was ground, I find it the sixth part of an inch.

Perhaps it may give some satisfaction to Monsieur Huygens to understand in what degree it represents things distinct and free from colours, and to know the aperture by which it admits light. And after the words (—versus focum E reflectatur) it may not be amiss to add this note:

Conferendo distantias foci istius a verticibus lentis et speculi concavi, hoc est EF $\frac{1}{6}$ dig., et ETV $6\frac{1}{3}$ dig., prodit ratio 1 ad 38, qua indicatur objecta 38 vicibus circiter ampliari.

And to this proportion is very consentaneous the observation of the crowns on the weathercock. For the scheme represents it bigger by $2\frac{1}{2}$ times when seen through this, than when through an ordinary perspective. And so supposing that to magnify 13 or 14 times, as by the description it should, this by the experiment proportionally must magnify almost as much as I have assigned it.

To the objection, that with it objects are difficultly found, I may answer, that that's the inconvenience of all tubes that magnify much, and that after a little use the inconvenience will grow less. For I could readily enough find any day objects, by knowing which way they were posited from other objects, that I accidentally saw in it; but in the night to find stars I confess is troublesome enough. Yet this may be easily remedied by two sights affixed to the iron rod by which the tube is sustained. And such I once intended should have been made before I sent it away from me, but that I thought the defect would not be judged material. If such sights be not found a sufficient remedy, there may be an ordinary perspective glass fastened to the same frame with the tube, and directed towards the same object, as Des Cartes in his Dioptrics hath described for remedying the same inconvenience of his best telescopes.

The plane side of the eyeglass is apt to be soiled with dust falling upon it; and therefore the little leaden ring put into the orifice of the bigger leaden barrel to moderate its aperture must be sometimes taken out, and the glass wiped with leather done upon the small end of a stick, or other such like contrivance; but care must be taken that the said ring be not lost, for without it objects appear very confused at the edges of the apparent space. So if the concave metal contract any dulness by moisture or otherwise, it ought to be taken out and rubbed with gentle leather, but not with putty or any thing that may wear the metal.

I am very sensible of the honour done me by the Bishop of Sarum in proposing me candidate, and which I hope will be further conferred upon me by my election into the society. And if so, I shall endeavour to testify my gratitude by communicating what my poor and solitary endeavours can effect towards the promoting your philosophical designs.

Sir, I am

your very humble servant,

I. Newton.

This letter is printed, without the beginning, in Birch's

Hist. of the Royal Society, vol. iii. p. 2. It is there mentioned as having been written to Oldenburg: the address is wanting in the original.

CXXXVI.

NEWTON TO OLDENBURG.

Cambridge, Jan. 18. 1671–2.

Sir,

Understanding by your last that some of the fellows of your honourable society, in order to a bigger reflective telescope, are devising a fit metalline matter, let me presume to give them this caution, that whilst they seek for a white, hard, and durable metalline composition, they resolve not upon such an one as is full of small pores, only discoverable by a microscope. For though such an one may to appearance take a good polish, yet the edges of those small pores will wear away faster in the polishing than the other parts of the metal, and so however the metal seem polite, yet it shall not reflect with such an accurate regularity as it ought to do. Thus tin-glass mixed with ordinary bell-metal makes it more white and apt to reflect a greater quantity of light, but withal its fumes raised in the fusion, like so many aereal bubbles, fill the metal full of those microscopical pores. But white arsenic both blanches the metal and leaves it solid without any such pores, especially if the fusion hath not been too violent. What the stellate regulus of Mars, (which I have sometimes used,) or other such-like substance, will do, deserves particular examination. Let me add this further intimation, that putty, or other such-like powder with which 'tis polished, by the sharp angles of its particles fretteth the metal, if it be not very fine, and filleth it full of such small holes as I speak of: and therefore care must be taken of that, before judgment be given whether the metal be, throughout the body of it, porous or not.

I desire that in your next letter you would inform me for what time the society continue their weekly meetings; because, if they continue them for any time, I am purposing them, to be considered of and examined, an account of a philosophical discovery, which induced me to the making of the said telescope, and which I doubt not but will prove much more grateful than the communication of that instrument, being in my judgment the oddest, if not the most considerable detection, which hath hitherto been made in the operations of nature.

I desire also that since I am elected fellow of your honourable society, you would, in a word or two, inform me what duties I am thereupon subject to, and you will further oblige me, who already am

your much obliged friend and servant,

I. NEWTON.

On the back of this letter Oldenburg has written "Rec. "Jan. 19, 1671–2. Answ. Jan. 20. Desired to have his con- "sent of printing his invention. Let him know the duty of "a fellow, and the uninterruptedness of the meetings of the "Society except long vacation."

"Writ again Jan. 27,–71: repeated what I said before, and "desired the proportions of arsenic and metal."

This letter is printed in Birch's History of the Royal Society, vol. iii. p. 5.

See also Phil. Trans. No. 81, vol. vii. p. 4006. This reference is inserted on the original by Oldenburg.

CCXXXVII.

NEWTON TO OLDENBURG.

Sir, Jan. 29, Cambridge, 1671–2.

Not having tried many proportions of the arsenic and metal, I am not assured which is absolutely best, but there may conveniently be used any quantity of arsenic equalling in weight between a sixth and eighth part of the copper. A greater proportion makes the metal brittle.

The way which I used it is this. I first melted the copper alone, then put in the arsenic, which being melted I stirred them a little together, bewaring, in the mean time, that I drew not in breath near the pernicious fumes. After that I put in the tin, and again, so soon as that was melted, which was very suddenly, I stirred them well together, and immediately poured them off.

I know not whether by letting them stand longer on the fire, after the tin was melted, a higher degree of fusion would have made the metal porous, but I thought that way I proceeded to be safest. In that metal which I sent to London there was no arsenic, but a small proportion of silver: as I remember 1*s.* in 3 oz. of metal. But I thought the silver did as much harm in making the metal soft, and so less fit to be polished, as good in rendering it white and luminous. At another time I mixed arsenic 1oz., copper 6oz., and tin 2oz.; and this an acquaintance of mine hath polished better than I did the other.

The publishing a description of the telescope in the Transactions I wholly leave to your pleasure, being willing to submit my private considerations in any thing that may be thought of public concernment. I have sent you, by the bearer, John Stiles, 40*s*. for admission money: and I hope I shall get some spare hours to send you also suddenly that account, which I promised in my last letter. In the mean time I rest

your very faithful servant,

I. NEWTON.

See Phil. Trans. No. 81, vol. vii. p. 4006–7. This is also noted on the original by Oldenburg.

CCXXXVIII.

NEWTON TO OLDENBURG.

Sir, T. C. Cambridge, Feb. 10, 1671-2.

'Twas an esteem of the Royal Society for most candid and able judges in philosophical matters, which encouraged me to present them with that discourse of light and colours, which since it has been so favourably accepted of, I do earnestly desire you to return them my cordial thanks. I before thought it a great favour to have been made a member of that honourable body; but I am now more sensible of the advantage. For believe me, Sir, I do not only esteem it a duty to concur with them in the promotion of real knowledge, but a great privilege, that instead of exposing discourses to a prejudiced and censorious multitude, (by which means many truths have been baffled and lost,) I may with freedom apply myself to so judicious and impartial an assembly.

As to the printing of that letter, I am satisfied in their judgment, or else I should have thought it too strait and narrow for public view. I designed it only to those, that know how to improve upon hints of things, and therefore, to shun tediousness, omitted many such remarks and experiments as might be collected by considering the assigned laws of refractions, some of which I believe, with the generality of men, would yet be almost as taking as any of those I described. But yet since the R. S. have thought it fit to appear publicly, I leave it to their pleasure. And perhaps to supply the aforesaid defects, I may send you some more of the experiments to second it, (if it be so thought fit,) in the ensuing Transactions.

I have no more, but to offer my acknowledgments of your kindness in particular, and my thanks for the pains you are pleased to undertake in printing that letter. Sir, I am

your faithful servant,

I. NEWTON.

This letter is printed in the Gen. Dict. vol. vii. p. 780.

CCXXXIX.

NEWTON TO OLDENBURG.

Sir, Cambridge, Feb. 20, 1671–2.

I received your's [of] Feb. 19th. And having considered Mr. Hooke's observations on my discourse, am glad that so acute an objector hath said nothing that can enervate any part of it. For I am still of the same judgment, and doubt not but that upon severer examinations, it will be found as certain a truth as I have asserted it. You shall very suddenly have my answer.

In Mons[r]. Hugenius's letter there are several handsome and ingenious remarks. And what he saith concerning the grinding parabolical conoids by geometrical rules, I do with him despair of; but I doubt not but that the thing may be in some measure accomplished by mechanical devices. This is all at present from

your faithful servant,

I. NEWTON.

This letter is printed in the Gen. Dict. vol. vii. p. 780, and in Birch's Hist. of R. S. vol. iii. p. 15.

CCXL.

COLLINS TO NEWTON.

30th April, 1672.

Worthy Sir,

A little before Christmas the Reverend Dr. Barrow informed me you were busy in enlarging your general method of infinite series or quadratures, and in preparing twenty Dioptric lectures for the press; and lately meeting with Mr. Jonas Moore, he informed me that he heard you had something at the press in Cambridge, possibly about the same argument: if so I am very glad, partly in regard I now live at Westminster, remote from the printing house, and partly because our Latin booksellers here are averse to the printing of mathematical books, there being scarce any of them that have a foreign correspondence for vent, and so when such a copy is offered, instead of rewarding the author, they rather expect a dowry with the treatise. I am employed as a clerk in the Council of Plantations, and the prospect of the present war hath not a little added to the increase of my pains, yet in the interim have taken some care about the printing the Astronomical Remains of Horrox[y], whereof about three fourths are done, which I now send you, and I hope within two months the rest shall follow. The Royal Society gave five pounds with the copy to encourage a bookseller (whereas scarce any were willing) to undertake it. With it also be pleased to accept two small tracts of Honorato Fabri, whereof I had many sent over, which though of no use to yourself, you may bestow on some of your pupils. Now, Sir, as soon as this book is done, if yours be not undertaken at Cambridge, I shall most willingly afford my endeavour to have it well done here: and, if so, what you have written might be sent up the sooner, in order to the preparing of schemes. I live at the house of Mr. William Austin, in Petty France, Westminster, against the Adam and Eve, whither you may vouchsafe to direct your letters to me. Dr. Barrow's late books are now common and in the hands of other booksellers, besides that insolvent one that undertook them. I could not but observe with much content, that you were pleased to become a member of the Royal Society, though withal sorry that it should redound to your charge, especially seeing it was your design thereby to enrich learning with your excellent contemplations about the same.

As to mathematical news take this brief account. Some English booksellers here, if they find suitable encouragement, have undertaken the printing of Mr. Kersey's pains in Algebra, (whereof you have a synopsis,) to which your general method of analytical quadratures, when extant, might be translated and annexed.

y This again will indicate the date of the first edition of the Opera Posthuma.

Dr. Pell hath lately published a table of squares. At Dantzic Hevelius labours about publishing his Machina Cælestis, with the remains and life of Kepler.

In Italy, Mengolus hath published three volumes of Music and Quadratures, with a tract de Quadratura Circuli; none of which have yet arrived. At Liège, Slusius is about publishing his Method de Maximis et Minimis et Tangentibus Curvarum. In France, Claudius Millet de Chales is printing his general Cursus Mathematicus at Lyons. And M. Huygens, at Paris, is about to publish his tract de Cycloide et Pendulis.

From thence are newly come over three octavo tracts, with elegant schemes, entitled Le[s] Travaux de Mars, by Allain Manesson Mallet, treating of Gunnery and Fortification, wherein the methods of all modern writers are examined and modestly censured.

Let this suffice at present, but not without the addition of the name of

your much obliged, thank[ful]

Servitor.

CCXLI.

NEWTON TO COLLINS.

Sir, Cambridge, May 25, 1672.

This day fortnight I received your letter accompanied with part of the Remains of Mr. Horrox[1], two tracts of Honorato Fabri, and four or five copies of a Synopsis of Mr. Kersey's Algebra. For these and Dr. Wallis's Mechanics, together with many other civilities, I must acknowledge your obligingness and

[1] We may connect this with the preceding letter, to which it is an answer, on the date of this publication.

affection to me, and shall be ever ready to testify as much. Nor is your mathematical intelligence less grateful; for I am very glad that Dr. Barrow's book is abroad, and that the world will enjoy the writings of the excellent astronomers Mr. Horrox, and Hevelius, and those complete mathematicians M. Huygens and Slusius.

Your kindness to me also in proffering to promote the edition of my lectures, which Dr. Barrow told you of, I reckon amongst the greatest, considering the multitude of business, in which you are involved; but I have now determined otherwise of them, finding already, by that little use I have made of the press, that I shall not enjoy my former serene liberty till I have done with it, which I hope will be so soon as I have made good what is already extant on my account. Yet I may possibly complete the discourse of resolving problems by infinite series, of which I wrote the better half the last Christmas with intention that it should accompany my Lectures, but it proves larger than I expected, and is not yet finished.

The book here in the press is Varenius his Geography[a], for which I have described schemes, and I suppose it will be finished about six weeks hence. The additions to Kinkhuysen's Algebra I have long since augmented with what I intended, and particularly with a discourse concerning invention, or the way of bringing problems to an equation; and those are at your command. If you have not determined any thing about them, I may possibly hereafter review them, and print them with the discourse concerning infinite series.

I take much satisfaction in being a member of that honourable body the Royal Society; and could be glad of doing any thing which might deserve it;

[a] Published in 1672. See Phil. Trans. vol. vii. p. 5172. See also letter ccxlvii.

which makes me a little troubled to find myself cut short of that freedom of communication, which I hoped to enjoy, but cannot any longer without giving offence to some persons whom I have ever respected. But it is no matter, since it was not for my own sake or advantage that I should have used that freedom.

The copies of the Synopsis of Mr. Kersey's Algebra I have communicated to our mathematicians, but meet not with any subscriptions. However to encourage the undertaking I shall subscribe for one, and hope, ere long, to send you another or two.

For my tardiness in returning you this answer I have no excuse, but that I staid four or five days, in hopes to send you some of those subscriptions, and being intent upon the duty of this term, the time slipped on faster than I was aware of. But I promise myself, by your so much testified friendship, that you will pardon it, and believe that I think myself really

your most obliged debtor,

I. NEWTON.

This letter is printed in the Gen. Dict. vol. vii. p. 780.

CCXLII.

NEWTON TO OLDENBURG.

Sir, Woolstrope, June 19, 1672.

Having been in Bedfordshire since my last to you, at my return to Cambridge I received your letter, containing the Philosophical Transactions for the last month inclosed, for which I return you many thanks. At present I am removed into Lincolnshire, and, if you have occasion to write to me before the 28th day of this month, I pray direct your letter to me at Mrs. Smith's house, at Woolstrope, in Coulsterworth Parish, near Grantham, in Lincolnshire. And for greater security you may further assign it to be left at Post-Witham, to be sent according to direction. In the answer to Sir R. Moray's experiments, there was 'reflexion' printed twice for 'refraction,'[a] which in the next Transactions may be noted amongst the errata. The particular places I know not how to direct you to, not having the book by me, but by the sense you will easily find them.

I desire you would not yet print any thing more concerning the theory of light, before it hath been more fully weighed. And, begging pardon for the haste in which I write, I subscribe myself

your humble servant,

NEWTON.

Upon the back of this letter Oldenburg has written as follows:—

"Rec. June 24, -72.

"Answ. June 25, repeating what I had said June 20, and "adding that P. Pard.[b] objections and his answers were "already in the press, and would please, espec. if the sequel "of the same P., with his answer, should follow.

"That to Mr. Hooke to be deferred till further order.

"July 2, -72. I wrote again, commun. to him the contents "of M. Huygens' letter of July 1, -72. and intimating withal "that what he had to return to him I would carefully con-"vey, as I hoped also to do Mr. Hooke's return to his answer.

"Desired to send the next carrier to my house."

[a] The error occurs Phil. Trans. Vol. vii. p. 4060, No. 83, for May 20, 1672. The name of the author of the experiments is not mentioned in the Transactions.

[b] P. Pardies—his first letter, with Newton's reply, is printed in the Phil. Trans. Vol. vii. pp, 4087–4093, No. 84, for June 17, 1672. Pardies' second letter also, with the reply, is to be found in the same Vol. pp. 5012–5018, No. 85, for July 15, 1672.

CCXLIII.

NEWTON TO OLDENBURG.

Stoke, July 6th, 1672.

Sir,

In the inquiry, which, in yours of June the 25th, you propound in these words, 'whether a physical 'point in a glass may not, by the diversity of the pores 'and angles in it, cause in the rays, falling thereon, 'such really different, though seemingly equal refrac-'tions, that thence may proceed those several distinct 'colours, which in my doctrine are esteemed to pro-'ceed from the aggregate of the rays of light?' I know not what to understand by really different though seemingly equal refractions. For if you mean those different refractions from whence I denominate light unequally refrangible, their differences are so great that they are far from being seemingly equal. And I apprehend not what other differences you should mean, (if there be any other,) since there is so constant and strict an analogy between these and the several species of colours. However since you suppose those unequal refractions to proceed from the diversity of pores and angles in the glass, they must be comprehended under the contingent irregularities, which I have already disproved in my answer to Mr. Hooke and P. Pardies. And further, if colours were originated from refractions, as is supposed in your inquiry, then all colours would be changeable by refractions, contrary to what I find by experience. From either of these two heads, your inquiry is determined negatively, which, if you think requisite, I shall further explain hereafter.

In[c] the mean while give me leave to insinuate that I cannot think it effectual for determining truth to examine the several ways, by which phænomena may be explained, unless where there can be a perfect enumeration of all those ways. You know the proper method for inquiring after the properties of things is to deduce them from experiments: and I told you that the theory, which I propounded, was evinced to me, not by inferring 'tis thus because not otherwise, that is not by deducing it only from a confutation of contrary suppositions, but by deriving it from experiments concluding positively and directly. The way therefore to examine it is by considering whether the experiments, which I propound, do prove those parts of the theory, to which they are applied, or by prosecuting other experiments, which the theory may suggest for its examination. And this I would have done in a due method; the laws of refraction being thoroughly inquired into and determined before the nature of colours be taken into consideration. It may not be amiss to proceed according to the series of these queries, the decision of which I could wish to be stated, and the events declared, by those that may have the curiosity to examine them.

1. Whether rays, that are alike incident on the same medium, have unequal refractions, and how great are the inequalities of their refractions at any incidence?

2. What is the law, according to which each ray is more or less refracted, whether it be that the same ray is ever refracted according to the same ratio of

[c] An extract from this letter, beginning with this paragraph, to the words "they must render all "objections invalid," is printed in the Phil. Trans. Vol. vii. p. 5004, No. 85, for July 15, 1672. The page is wrongly numbered 4004. It is there spoken of as of July 8 instead of July 6.

the sines of incidence and refraction, and divers rays according to divers ratios; or that the refraction of each ray is greater or less without any certain rule? That is, whether each ray have a certain degree of refrangibility, according to which its refraction is performed, or is refracted without that regularity?

3. Whether rays which are endued with particular degrees of refrangibility, when they are by any means separated, have particular colours constantly belonging to them; viz. the least refrangible, scarlet; the most refrangible, deep violet; the middle, sea-green; and others other colours? and the contrary?

4. Whether the colour of any sort of rays apart may be changed by refraction?

5. Whether colours, by coalescing do really change one another to produce a new colour, or produce it by mixing only?

6. Whether a due mixture of rays, endued with all variety of colours, produces light perfectly like that of the sun, and which hath all the same properties and exhibits the same phenomena?

7. Whether the component colours of any mixture be really changed, or only separated, when out of that mixture various colours are again produced by refraction?

8. Whether there be any other colours produced by refractions, than such as ought to result from the colours belonging to the diversely refrangible rays, by their being separated or mixed by that refraction?

To determine by experiments these and such like queries, which involve the propounded theory, seems the most proper and direct way to a conclusion. And, therefore, I could wish all objections were suspended, taken from hypotheses, or any other heads than these two; of shewing the insufficiency of experiments to determine these queries, or prove any other parts of my theory, by assigning the flaws and defects in my conclusions drawn from them; or of producing other experiments, which directly contradict me, if any such may seem to occur. For if the experiments which I urge be defective, it cannot be difficult to shew the defects; but if valid, then, by proving the theory, they must render all other objections invalid.

In the margin of my answer to Mr. Hooke[d] I noted the contents of it in twelve particulars, which, when I came to number them in the copy, I found thirteen, so that there is either a marginal note omitted, or else slipped over without its number prefixed. If the last hath happened, you may prefix its number and alter the numbers of those that follow. But if the first, I will supply the note when I return to Cambridge, where my papers are, because there may possibly be occasion of referring to that discourse hereafter. Sir, I am

your humble servant,

I. Newton.

Yours dated June the 20th I doubt I shall not receive till my return to Cambridge. I desire you would suspend the impression of P. Pardies'[e] second letter. If you write to me before July 14, pray direct your letter to me at Mrs. Arundell's house in Stoke Park in Northamptonshire, and assign it to be left with the postmaster of Towcester to be sent thither.

Newton.

If you see Mr. Collins, pray acquaint him that there are three more books of Mr. Kersey's Algebra desired

[d] Probably the paper entitled "Mr. Isaac Newton's answer to "some considerations upon his "doctrine of light and colours, "&c." Phil. Trans. Vol. vii. p. 5084, No. 88, for November 18, 1672.

[e] For the date of the publication of this letter, see above, p. 324, note [b].

in Cambridge, for which he may at present subscribe my name.

Since the writing of this I received your two letters dated June the 20th and July the 2nd. I understand that John Stiles is ordered to call upon you for what you are pleased to promise me, otherwise I should have ordered another carrier to have brought it hither. For the transmission of it from Cambridge hither will not be so sudden. I am much obliged to M. Huygens for what he hath wrote to you, which I should have answered now, but for want of time and room.

Oldenburg has written on the back of this letter, "Answ. "July 9, acquiesce in his answer to H., intend to print his "set of inquiries, and to recommend them at the R. S. "Desired to take off the suspension of printing the second "letter of Pard[ies], and to send me his answer to Huygens."

This letter is printed in the Gen. Dict. Vol. vii. p. 781.

CCXLIV.

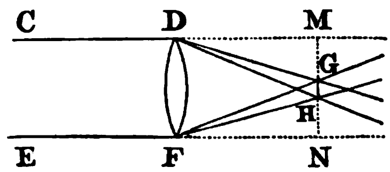
NEWTON TO OLDENBURG.

Stoke, July 8, 1672.

Sir,

I am glad to find by your abstract of M. Hugenius' letter, which you transmitted to me, that he, who hath done so much in Dioptrics, hath been pleased to undertake the improvement of telescopes by reflection also, though without the desired success. For I hope the event of his next essay, if he shall think fit to attempt any thing further, will prove more happy by a little altering the manner of his proceeding. I should be glad to hear whether Mr. Cox hath finished the four feet telescope, and what its effects are, as also what are the best effects of those of six or eight inches in length which he hath made at any time. But I know not whether I shall make any further trials myself, being desirous to prosecute some other studies.

Touching the theory of colours, I am apt to believe that some of the experiments may seem obscure, by reason of the brevity wherewith I writ them, which should have been described more largely and explained with schemes, if they had been then intended for the public. But I see not why the aberration of the rays from the transverse of the object-glass of a telescope should be more than about $\frac{1}{50}$ of the glass's aperture. For suppose DF be the lens, CD and EF two lines parallel to its axis, or indefinitely near, to which all variety of difform rays are successively incident on two opposite parts of its perimeter. And of those rays let DH and FG be the most refracted, and DG and FH the least refracted, intersecting the former in G and H. Draw GH and produce it both ways till, at M and N, it occur with CD and EF also produced.



Now since by my principles the difference of refraction of the most difform rays is about the 24th or 25th part of their whole refraction, the angle GDH will be about a 25th part of the angle MDH, and consequently the subtense GH, (which is the diameter of the least space into which the refracted rays converge,) will be about a 25th part of the subtense MH, and therefore about a 49th part of the whole line MN, the diameter of the lens; or in round numbers about a fiftieth part as I asserted.

This in haste, Sir, from your servant,

I. NEWTON.

CCXLV.

NEWTON TO COLLINS.

Stoke, July 13, 1672.

Sir,

I think I told you that I had altered my resolution of printing my Dioptric Lectures. And for the exercise about infinite series I am not yet resolved, not knowing when I shall proceed to finish it. I will inquire of some of our booksellers whether they will purchase Mr. Pitts his copy of Kinkhuysen, and if not I will send it you. In the mean while I would know whether Mr. Pitts thinks it will be more advantageous to print the author without alteration, or to insert those notes which you formerly saw, that I may according[ly] send them with the copy or detain them. Mr. Gregory's problem of finding the solidity of the second segments of a sphere, and yours of finding the surfaces of inclined round solids may be solved divers ways by infinite series, as I find by considering them in general, but I foresee the calculations are intricate, and unpleasant, which has made me neglect them, not thinking them worth transmitting to you. If I ever applied Gunter's sector to the resolving of adfected equations, it hath now slipped out of my memory. Possibly it might be Gunter's line, which, being set upon three or four several rulers, is of ready use for finding the two or three first figures of any adfected equation ; but there is no difficulty in the invention. And if it be the same which you mean, you may command it. The way of resolving equations of five or six dimensions by a locus linearis was, I believe, by the intersection of that and a conic section, something after the manner that Des Cartes hath done it, but more conveniently in my opinion, because the same locus linearis once described will serve for the resolving of all equations of those dimensions. And, as I remember, the calculations to that intent are shorter and less intricate.

I am at present in Northamptonshire, whither your letter was sent to me from Cambridge; but hope within eight or nine days to be at Cambridge to receive what you may send thither, if you shall have occasion to write to

your humble and much obliged servant,

NEWTON.

There are three more of Mr. Kersey's books of Algebra desired in Cambridge, for which at present you may subscribe my name.

This letter is printed in the Gen. Dict. vol. vii. p. 782.

CCXLVI.

NEWTON TO OLDENBURG.

Stoke, July 13, 1672.

Sir,

I am glad you are pleased to accept my answer to your inquiry, together with the following discourse about the properest method of examining the truth of my proposals, which you may print when you think fit. And so you may P. Pardies' second letter with my answer thereto, since you desire it, and have intimated to him that you would do so in this month of

July. I intended to suspend it for a while, thinking it would be more convenient to print together what shall be said of this subject, especially since there are some other papers at Cambridge to be added to them. But if what hath passed be inserted into the Transactions to entertain them at present, that are in expectation of further information about these matters, they may be hereafter reprinted by themselves, if it shall be thought fit.

I hope you have before this time received my thoughts upon M. Huygens's two particulars, which therefore I shall not repeat. You will gratify me much by acquainting me with the particular dimensions, fashion, and success of the four feet tube, which I presume Mr. Cox, by this time, hath finished. And to inform myself of the advantages of the steely matter, which is made use of, you will much oblige me if you can procure me a fragment of it. I suppose it is made by melting steel with a little antimony, perhaps without separating the sulphureous from the metalline part of that mixture. And so though it may be very hard, and capable of a good polish, yet I suspect whether it be so strongly reflective as a mixture of other metals. I make this inquiry, because if I should attempt any thing further in the fabric of the telescope, I would first inform myself of most advantageous materials. On which account, also, you will further oblige me if you can inquire whether Mr. Cox, or any other artificer, will undertake to prepare the metals, glass, tube, and frame of a four feet telescope, and at what rates he will do it, so that there may remain nothing for me to do but to polish the metals. A gross account of this will at present suffice, until I send you a particular design of the fabric of the instrument, if I resolve upon it.

I presume John Stiles hath called at your house for your promise, which, how slender soever you may esteem it, will be very acceptable to

your humble servant,

I. NEWTON.

Sir,

Being at a place where the quick arrival of news is a rarity, if there be any thing considerable lately come to your knowledge about the events of this war, or proposals in order to peace, &c., I beg a word or two of it in your next letter to gratify my friends with here, who are very desirous of such intelligence. I think I shall stay here till the 20th or 22d of this month, after which time I hope I shall return immediately to Cambridge.

Oldenburg's memoranda on this letter are as follow:

"July 15, -72. Ans. July 16—see copy.

"Written again Sept. 17, -72, to inquire whether he received my last of July 16, and of Boyle and Glisson."

This letter is printed in the Gen. Dict. vol. vii. p. 782.

CCXLVII.

NEWTON TO COLLINS.

Cambridge, July 30, 1672.

Sir,

To your last I sent you an answer out of Northamptonshire, which I hope you received, and therefore shall not repeat the particulars. Only I add, that if Mr. Gregory yet expects my answer of his problem, I will work it and send it to you, though to yourself I believe it will not be very grateful. Yesterday I spoke with a bookseller here about the translation of Kinkhuysen, who upon my motion was willing to take it off of Mr. Pitts his hands at 3*l.*; but he has not yet seen the book. Varenius is newly out of the press, a copy of which I send you by this bearer, John Stiles. And this, Sir, is all at present from

your humble servant,

I. NEWTON.

The bookseller desired me to acquaint you that Varenius will [be] sold by Mr. Martin, if any of your friends desire it.

CCXLVIII.

COLLINS TO NEWTON.

1st of August, 1672.

Sir,

Since I writ the former I received yours of the [30th of July] with an exemplar of your new edition of Varenius, for which I heartily thank you, as also for your kind proffer about sending up a calculation for the second segments of a sphere or spheroid, which indeed may be of great use to gaugers, (though tedious,) were it but to compare with such false approaches as they are driven to use, to find how much they are erroneous. What Mr. Gregory hath sent me about it I send you a copy of. I rather wish his several series had been distinct than compounded, and that, as a parabola is throughout proportional to a sphere, he had found the genius of that curve that is throughout proportional to a segment of a sphere, but about these things I am unwilling to put either you or him to trouble. Dr. Barrow is come up. Horrox's Posthuma are not yet finished, though I hope they will be ere long. Mr. Pitts is willing to take 3*l.* for his interest in K[inkhuysen's] Introduction, if the bookseller will moreover give him ten copies in quires, when printed, and if you cannot procure so many I shall, if he insists on it, compensate what falls short: the rather in regard I am willing to shun the trouble at the press, which I promised to undertake, but chiefly because I think that Introduction proper to accomp[any] your doctrine of infinite series.

Sir, I remain——

CCXLIX.

NEWTON TO OLDENBURG.

Cambridge, Sept. 21, 1672.

Sir,

That letter which you directed to Stoke, in answer to mine from thence, I received not, as I told you formerly; but your last, wherein you repeated the contents of that, I received, and am troubled that I have answered it no sooner; especially since I was obliged to thank you for the Transactions of July, and more particularly for your elegant translation of my letter published in them,[f] and for the trouble you was pleased to take upon you in inquiring of Mr. Cox about the telescope.

[f] "Excerptum ex Isaaci Newtoni Epistola, nuper ad Editorem scripta, qua ipse genuinam suggerit methodum doctrinam suam de luce et coloribus, antehac propositam, evincendi, subjecta certorum quæsitorum, debitis experimentis solvendorum, serie." Phil. Trans. vol. vii. p. 5006.

To comply with your intimation of communicating experiments proper for determining the queries, which that letter contained, I drew up a series of such experiments, on design to reduce the theory of colours to propositions, and prove each proposition from one or more of those experiments, by the assistance of common notions set down in the form of Definitions and Axioms, in imitation of the method, by which mathematicians are wont to prove their doctrines. And that occasioned my suspension of an answer, in hopes my next should have contained the said design. But, before it was finished, falling upon some other business, of which I have my hands full, I was obliged to lay it aside, and now know not when I shall take it again into consideration. However if the answer to Mr. Hooke's considerations will conduce to the determination of any of those queries, (as in some particulars I think it will,) you may, if you think fit, publish it. To which end I desire you to mitigate any expressions that seem harsh, that its publication, as you intimated, may be done to common satisfaction. And though I intend at present nothing further for the public, yet if to any of your private acquaintance, that endeavour to satisfy themselves by an experimental determination of those queries, experiments sufficient to determine them all occur not; upon your intimation of the particulars, which they stick at, I shall for your sake do my endeavour, as much as I can in short, to supply what they desire.

I have not yet perused those two books you mention, but by your description of the first in the Transactions, it seems to contain a doctrine most highly probable, and in the latter I expect to meet with many things as improbable. And than that particular, which you mention, I know not what can be more difficult.

I am sorry for the miscarriage of your aforesaid letter, and blame myself for my postscript, suspecting that may have occasioned in yours what you would not have fall into other hands than those of,

Sir,
your humble servant,
NEWTON.

Oldenburg's memoranda on the back of this letter.

"Rec. Sept. 23, 1672.
"Answ. Sept. 24—Of Salvetti's making his telescope: to "move him to prosecute it, as well as to put out of doubt his "doctrine of colours; of Th. Haak; of Huygens his sense "about his doct. of colours."

This letter is printed in the Gen. Dict. Vol. vii. p. 782.

CCL.

NEWTON TO COLLINS.

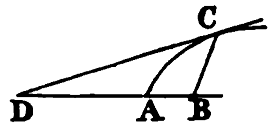
Cambridge, Dec. 10, 1672.

Sir,

My unwillingness to trouble you in the midst of your late business made me suspend writing, though I stood obliged to thank you for the rest of Mr. Horrox's works, which I received some weeks since.[k] 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 rev. friend Dr. Barrow's Lectures find with foreign mathematicians, and it pleased me not a little to understand, that they are fallen into the same method of drawing tangents

[k] This will fix this year as the date of their first, and complete publication.

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 expressed by any equation, as $x^3-2xxy+bxx-bbx+byy-y^3=0$, whereby this curve is determined.



To draw the tangent CD, the rule is this. Multiply the terms of the equation by any arithmetical progression according to the dimensions of y; suppose thus, $\underset{0}{x^3}-\underset{1}{2xxy}+\underset{0}{bxx}-\underset{0}{bbx}+\underset{2}{byy}-\underset{3}{y^3}$, also according to the dimensions of x, suppose thus, $\underset{3}{x^3}-\underset{2}{2xxy}+\underset{2}{bxx}-\underset{1}{bbx}+\underset{0}{byy}-\underset{0}{y^3}$. The first product shall be the numerator, and 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 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 straight lines or to other curve lines, but also to the resolving other abstruser kinds of problems about the crookedness, areas, lengths, centres of gravity of curves, &c. Nor is it (as Hudden's method de maximis et minimis, and consequently Slusius his new method of tangents as I presume) limited to equations which are free from surd quantities. This method I have interwoven with that other of working in equations by reducing them to infinite series. I remember I once occasionally told Dr. Barrow, when he was about to publish his Lectures, that I had such a method of drawing tangents, but some divertisement or other hindered me from describing it to him.

Of resolving, by Cardan's rules, equations that have three possible roots, there may be examples framed at pleasure; but unless Brasser shew a direct method of performing it, which Ferguson doth not, it will not be allowed scientific. How it is to be done directly I may possibly shew upon occasion.

Mr. Gregory is pleased to consider further the most advantageous construction of catadioptrical telescopes. And as his design in his Optica Promota excels that of M. Cassegrain, (though they differ so slightly that I thought it not worth 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 proposal excels them both, of making that speculum plane.[g] And this I conjecture is the way, which Sign^r. Salvetti, one of the great Duke's musicians, 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, 3rd, and last of those difficulties, which I urged against M. Cassegrain, and in my judgment not wholly capable of the advantages, which Mr. Gregory propounds. The first disadvantage was, that more light is lost in direct than oblique reflexion. 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

[g] See J. Gregory's Letter, No. ccxi. p. 243.

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, until 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 metal, 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 metal 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 Mr. 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 polished plate of metal may persuade 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 sphere, (as it is in Mr. Gregory's design, a plane being equivalent to a sphere whose centre is infinitely distant,) that would cause too many of the best rays to be intercepted. And though in his design scarce a fourth 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 Mr. Gregory's tube may perhaps be thought less advantageous; as that unless 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 eye-glass, if placed at the bottom, will scarcely be well defended from the unuseful glaring light, which in the day-time comes from objects on all sides [of] 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 metal near that hole will be apt to wear away too fast as it doth near the exterior limb. And though the hole may be made after it is polished, yet if the figure happen to be less true, or if afterwards the metal chance to tarnish, it must be polished again.

As for the advantages propounded by Mr. Gregory, I see not why the first should be reckoned for one, viz. that the distance EF, groweth almost the one half less, and therefore the errors of the concave CD are also diminished upon the plane F by one half.

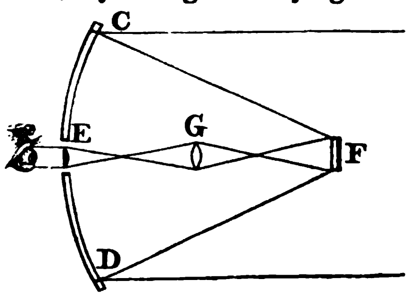
For how much those errors of the concave CD are increased or diminished, is to be estimated by the prevarication of the rays, not at the plane F, but at the focus of that concave CD. And there the errors in both cases will be alike, provided the speculum F be accurately plane; but if there be any irregularities in the figure of that speculum F, they will cause errors so much greater in one case than in the other, as that speculum is remoter from the eye-glass, which in large telescopes may be more than fifteen or twenty 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 equal art in the mechanism it would be made to do the same effect. The greatest difficulty is in forming the 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 and concave speculums, in that they may have any desirable charge by altering the distance of the eye-glass and specula, agrees more conveniently to my design of the instrument, if that speculum be made use of, which I described in a letter to Mr. Oldenburg in answer to M. Auzout's 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 than a quarter of the tube's diameter. The charge may be also conveniently varied by having two or three eye-glasses, of several 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 fastened.

That telescopes by convex and 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. Cassegrain's tube,[h] 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 eye-glasses. Suppose CD represent the great concave, F the little speculum, E the eye-glass, and G another double convex glass between E and 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 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 rendered less distinct, as is manifest in dioptric telescopes: where two or three eye-glasses are applied to erect the object.

[h] See Phil. Trans. Vol. vii. p. 4058, No. 83, May 20, 1672.

As to the attempt, in which Mr. Rieve was employed, I presumed it had been done with more accurateness than Mr. Gregory now signifies, because Mr. Hooke, who you know is a curious and accurate experimenter, affirms in his considerations on my letter to Mr. Oldenburg, concerning refractions and colours, published in the Transactions, Numb. 80, that he made several experiments with that instrument. And though he lays the blame on Mr. 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 trials both for telescopes "and microscopes by reflexion, which I have mentioned in my Micrographia, but deserted it as to telescopes, when I considered that the focus of a spherical "concave is not a point, but a line, and that the rays "are less true reflected to a point by a concave than "refracted by a convex, which made me seek that "by refraction, which I found could not be expected from reflexion. Nor indeed could I find any "effect of it by one of six feet radius, which about "seven or eight years since Mr. Rieve made for Mr. "Gregory, with which I made several trials; 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 Mr. Hooke was discouraged from attempting further that way only by these two or three considerations; that a convex (as he presumes) refracts more truly than a concave reflects; that he found no effect by one of six feet radius, which till now he attributed to some other cause than the want of a good encheiria, namely to the supposedly less true reflexion of a spherical concave; and 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 will 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 Mr. Hooke's 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 several trials with it.

And that you may not think I strained Mr. Gregory's sense, where he spake of hyperbolic or elliptic glasses and speculums attempted in vain, I would ask to what end these speculums were attempted, if not to compose optic 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 trial of any practicable way, or to contend with Mr. 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 then thought it worth his pains. Because M. Cassegrain propounded his supposed invention pompously, as if the main business 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 Mr. Gregory's answer, only to let you see that I chose the most easy and practicable way to make the first trials. Others may try other ways. Nor do I think it material 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 Mr. Gregory's discourse looks as if intended for the press. We are here very glad that we shall enjoy Dr. Barrow again, especially in the circumstances of Master, nor doth any rejoice at it more than, Sir,

your obliged humble servant,

NEWTON.

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

On the back of this letter there is the following note in the handwriting of Collins, which possibly may have been intended for Oldenburg.

"Scripsi ad te jam sæpius de regula ducendarum tangen-
"tium 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 problematum, tum in pluribus aliis ostendere
"volebam quorum aliqua, ni fallor, tibi antehac indicavi. Ve-
"rum cum nec mihi nunc otium sit, decrevi, si ita videatur,
"regulam absque demonstratione et corollariis (ne moles nimia
"sit) ad te mittere, Transactionibus Philosophicis inserendam,
"qua virorum doctorum censuram subire possit. Fac igitur
"me certiorem quid ea de re censeas. Ego enim judicio tuo
"parebo libenter, eamque cum tibi placere indicaveris, statim
"transmittam."

CCLI.

NEWTON TO OLDENBURG.

Sir, Cambridge, March 8, 1672–3.

I received both your letters, and thank you for Hecker's Mercurius in Sole. As for M. Huygens' observations, I conceive they are but the abstract of a private letter sent to you, and therefore concern not me to take notice of them. But yet if he expect an answer, and intends that this should be made public, I will return you my thoughts upon them, if you please to send me the original letter, and procure from M. Huygens that I may have liberty to publish what passeth between us, if occasion be.

Sir, I desire that you will procure that I may be put out from being any longer Fellow of the Royal Society: for though I honour that body, yet since I see I shall neither profit them, nor (by reason of this distance) can partake of the advantage of their assemblies, I desire to withdraw. If you please to do me this favour you will oblige

your humble servant,

I. Newton.

I have presumed to put you once more to the trouble of receiving my quarterly duty as Fellow of the Royal Society. At next Lady-day I am behindhand for half a year, and have therefore sent you 1*l.* 6*s.* by John Stiles. I hope you will excuse this trouble, it being the last. I shall be henceforth absent from Cambridge for about a month.

Oldenburg's memoranda on the back of this letter.

"Answ. March 13, 1672. Sent the original of M. Huygens's

"letter of Jan. 14, 1672-3, and the Transactions of Feb. 1672;
"and withal represented to him my being surprised at his re-
"signing for no other cause than his distance, which he knew
"as well at the time of his election. Offering withal my
"endeavour to take from him the trouble of sending hither
"his quarterly payments—without any reflection."

CCLII.

NEWTON TO OLDENBURG.[s]

Cambridge, April 3, 1673.

It seems to me that M. Huygens takes an improper way of examining the nature of colours, whilst he proceeds upon compounding those that are already compounded, as he doth in the former part of his letter. Perhaps he would sooner satisfy himself by resolving light into colours, as far as may be done by art, and then by examining the properties of those colours apart, and afterwards by trying the effects of reconjoining two or more, or all of those, and lastly, by separating them again to examine what changes reconjunction had wrought in them. This will prove a tedious and difficult task, to do it as it ought to be done, but I could not be satisfied till I had gone through it. However, I only propound it, and leave every man to his own method.

As to the contents of his letter, I conceive my former answer to the query about the number of colours is sufficient, which was to this effect; that all colours cannot practically be derived out of yellow and

[s] There is no address to this letter, but Oldenburg has written on it, "Mr. Newton's answer to "Monsr. Hugenius's letter of "Jan. 14, 1672–3." The handwriting is clearly Newton's.

blue, and consequently that those hypotheses are false, which imply they may. If you ask what colours cannot be derived out of yellow and blue, I answer none of those, which I defined to be original; and if he can shew by experiment how they may, I will acknowledge myself in an error. Nor is it easier to frame an hypothesis by assuming only two original colours rather than an indefinite variety, unless it be easier to suppose that there are but two figures, sizes, and degrees of velocity or force, of the ethereal corpuscles or pulses, rather than an indefinite variety; which certainly would be a very harsh supposition. No man wonders at the indefinite variety of waves of the sea, or of sands on the shore, but were they all of but two sizes it would be a very puzzling phænomenon. And I should think it as unaccountable if the several parts or corpuscles of which a shining body consists, which must be supposed of various figures, sizes and motions, should impress but two sorts of motion on the adjacent ethereal medium, or any other way beget but two sorts of rays. But to examine how colours may be thus explained hypothetically is beside my purpose. I never intended to shew wherein consists the nature and difference of colours, but only to shew that de facto they are original and immutable qualities of the rays, which exhibit them, and to leave it to others to explicate by mechanical hypotheses the nature and difference of those qualities, which I take to be no very difficult matter. But I would not be understood, as if their difference consisted in the different refrangibility of those rays. For that different refrangibility conduces to their production no otherwise than by separating the rays, whose qualities they are. Whence it is that the same rays exhibit the same colours, when separated by any other means; as by their different reflexibility; a quality not yet discoursed of.

In the next particular, where M. Huygens would shew that it is not necessary to mix all colours for the production of white; the mixture of yellow, green, and blue, without red and violet, which he propounds for that end, will not produce white but green, and the brightest part of the yellow will afford no other colour but yellow, if the experiment be made in a room well darkened, as it ought, because the coloured light is much weakened by the reflexion, and so apt to be diluted by the mixing of any other scattering light. But yet there is an experiment or two mentioned in my letter in the Transactions, No. 88, by which I have produced white out of two colours alone, and that variously, as out of orange and a full blue, and out of red and pale blue, and out of yellow and violet; as also out of other pairs of intermediate colours. The most convenient experiment for performing this was that of casting the colours of one prism upon those of another, after a due manner. But what M. Huygens can deduce from hence I see not. For the two colours were compounded of all others, and so the resulting white, to speak properly, was compounded of them all, and only decompounded of those two. For instance, the orange was compounded of red, orange, yellow, and some green; and the blue of violet, full blue, light blue, and some green, with all their intermediate degrees; and consequently the orange and blue together made an aggregate of all colours to constitute the white. Thus if one mixes red, orange, and yellow powders to make an orange; and green, blue, and violet powders to make a blue; and lastly, the two mixtures to make a grey; that grey, though decompounded of no more than two mixtures, is yet compounded of all the six powders, as truly as if the powders had been all mixed at once. This is so plain that I conceive there can be no further scruple, especially to them who know how to examine whether a colour be simple or compound, and of what colours it is compounded: which, having explained in another place, I need not now repeat. If therefore M. Huygens would conclude any thing, he must shew how white may be produced out of two uncompounded colours; which when he hath done, I will further tell him why he can conclude nothing from that. But I believe there cannot be found an experiment of that kind, because, as I remember, I once tried by gradual succession the mixture of all pairs of uncompounded colours, and though some of them were paler and nearer to white than others, yet none could be truly called white. But it being some years since this trial was made, I remember not well the circumstances, and therefore recommend it to others to be tried again.

In the last place, had I thought the distinctness of the picture, which (for instance) a twelve feet object-glass casts into a darkened room, to be so contrary to me as M. Huygens is pleased to affirm, I should have mended my theory in that point before I propounded it. For that I had thought on that difficulty you may easily guess by an expression somewhere in my first letter, to this purpose, that I wondered how telescopes could be brought to so great perfection by refractions, which were so irregular.[h] But to take away the difficulty, I must acquaint you first, that though I put the greatest lateral errors of the rays from one another to be about $\frac{1}{50}$ of the glass's diameter, yet their greatest error from the points on which they ought to fall will be but $\frac{1}{100}$ of the diameter; and

h Phil. Trans. vol. vi. p. 3079: No. 80, for Feb. 19, 1671–2.

then that the rays, whose error is so great, are but very few in comparison to those which are refracted more justly: for the rays, which fall upon the middle parts of the glass, are refracted with sufficient exactness, as also are those that fall near the perimeter, and have a mean degree of refrangibility. So that there remain only the rays, which fall near the perimeter, and are most or least refrangible, to cause any sensible confusion in the picture. And these are yet so much further weakened by the greater space, through which they are scattered, that the light, which falls on the due point, is infinitely more dense than that which falls on any other point round about it. Which, though it may seem a paradox, yet is easily demonstrable. Yea, although the light, which passeth through the middle parts of the glass, were wholly intercepted, yet would the remaining light convene infinitely more dense at the due points than at other places. And by this excess of density, the light, which falls in or insensibly near the just point, may, I conceive, strike the sensorium so vigorously that the impress of the weak light, which errs round about it, shall in comparison not be strong enough to be animadverted, or to cause any more sensible confusion in the picture than is found by experience. This, I conceive, is enough to shew why the picture appears so distinct, notwithstanding the irregular refraction. But if this satisfy not, M. Huygens may try, if he please, how distinct the picture will appear when all the lens is covered, excepting a little hole next its edge on one side only. And if, in this case, he please to measure the breadth of the colours thus made at the edge of the sun's picture, he will perhaps find it approach nearer to my proportion than he expects.

This letter is printed in the Phil. Trans. vol. viii. p. 6108, No. 97, Oct. 6, 1673. Huygens' name is however suppressed.

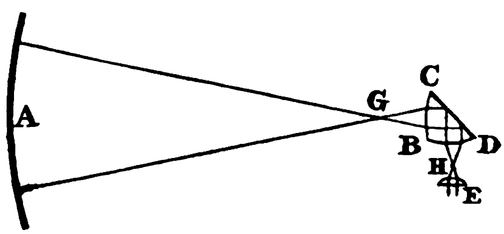
CCLIII.

NEWTON TO COLLINS.

Sir, Cambridge, April 9, 1673.

Having perused Mr. Gregory's candid reply[r], I have thought good to send you these further considerations upon the differences that still are between us. And first, that a well polished plate reflects [rays] at the obliquity of 45 degrees more truly than direct ones, seems to me very certain. For the flat tuberculæ or shallow valleys, such as may be the remains of scratches almost worn out, will cause the least errors in the obliquest rays, which fall on all sides the hill, excepting on the middle of the foreside and backside of it; that is, where the hill inclines directly towards, or directly from the ray. For if the ray fall on that section of the hill, its error is in all obliquities just double to the hill's declivity: but if it fall on any other part of the hill, its error is less than double, if it be an oblique ray, and that so much the less by how much the ray is obliquer; but if it be a direct ray, its error is just double to the declivity, and therefore greater in that case. I presume Mr. Gregory, if you think it convenient to transmit this to him, will easily apprehend me.

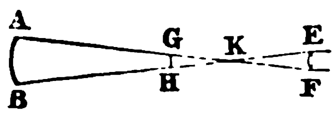
How the charge may be varied at pleasure in my telescope will appear by this figure, where A represents the great concave, E the eyeglass, and BCD a prism



[r] See Gregory's letter of March 7, 1673, No. ccxii. p. 246.

of glass or crystal, whose sides, BC and BD, are not flat but spherically convex, so that the rays which come from G, the focus of the great concave A, may by the refraction of the first side BC be reduced into parallelism, and after reflexion from the base CD be made by the refraction of the next side BD to converge to the focus of the eyeglass H. The telescope being thus formed, it appears how the charge may be altered by varying the distances of the glasses and speculum.

As for the objection that Mr. Gregory's telescope will be either overcharged, or have too small an angle of vision, &c., I apprehend that the difference between us lies in limiting the aperture of the eyeglass. Mr. Gregory puts it equal to that of the little concave, but I should rather determine it by this proportion: that if a middle point be taken between the eyeglass and its focus, the apertures of the eyeglass and concave be proportional to their distances from that point. That is, suppose AB the little concave, EF the eyeglass, GH their common focus or image, and K the mean distance between GH and EF, from the extremities of AB draw AK and BK butting on the eyeglass at F and E, and EF shall be its aperture. The reason of this limitation is, that the superfluous light which comes on all sides of the speculum AB to the space GH, in which the picture of the object is made, may fall beside the eyeglass. For if it should pass through it to the eye, it would exceedingly blend those parts of the picture with which 'tis mixed, and such are those parts of it, which extend themselves beyond the lines AK, BK. As I remember I said in my former letter, that the scattering light, which falls on the eyeglass, will



disturb the vision, and this is to be understood of any straggling light, which comes not from the picture, but if it come from the picture to the eyeglass, the disturbance will be much greater, so as not to be allowed of. Against the first I see no very convenient remedy, and against the last none but assigning a small aperture to the eyeglass; supposing the telescope is used in the daytime, or in twilight, or to view the moon, or any star very near her, or near the brighter planets. And if for this reason the aperture be limited by my rule, the angle of vision will become very small, as I affirmed. For instance, in that case where Mr. Gregory in his postscript puts it above 20 degrees, it will be reduced to less than half a degree. Yet, I confess, there is a way, by which the angle of vision may be something enlarged, but it will not be very considerably unless the eyeglass be also deeper charged.

Why I assign a concave with an eyeglass to magnify small objects, (in Transact. p. 3080,) and yet an eyeglass without such a concave to magnify the image of the great concave, which is equivalent to a small object, is because that image doth not require to be magnified so much as an object by a microscope; and further, because the angle of the pencil of rays, which flow from any point of the small object, that the object may appear sufficiently luminous, ought to be as great as possible, and a concave will, with equal distinctness, reflect the rays at a greater angle of the pencil than a lens; but in the telescope the angles of those pencils are not so great as to transcend the limits, at which an eyeglass may with sufficient distinctness refract them, and therefore in these instruments I chose to lay all the stress of magnifying upon the eyeglasses. In microscopes also I would lay as much stress of magnifying upon the eyeglass as it is well capable of, and the excess only upon the concave.

Concerning my citation of Mr. Gregory against M. Cassegrain, the force of it lies only in the inference, that optic instruments most probably, according to M. Cassegrain's design, have been tried by reflexion; which I think I might well infer without having regard to the specific figure of the speculum, which Mr. Gregory there spake of. And therefore I think it cannot be said, that I made him speak of spheric figures where his meaning was of hyperbolic and elliptic ones. But if I should be so understood, because I put the figure of the great concave to be spherical wherever I specify it, I know not why I might not, by way of consequence, make that interpretation. For it is not probable that any man would attempt hyperbolic and elliptic figures of speculums, until the event of spherical ones had been first tried.

And accordingly the trial of Mr. Gregory with Mr. Rieve was by a spherical figure. Which trial, although I am now satisfied that it was made very rudely, yet, by the information which I had of it when I wrote the latter about M. Cassegrain's design, I apprehended it to have been made with very great diligence and curiosity, as I signified in my former letter at large. And this I hope may excuse me for speaking of it in the Transactions, as if it had been tried with more accuracy than really it was. And thus much concerning the telescope.

The design of the burning speculum appears to me very plausible, and worthy of being put in practice. What artists may think of it I know not, but the greatest difficulty in the practice, that occurs to me, is to proportion the two surfaces, so that the force of both may be in the same point according to the theory. But perhaps it is not necessary to be so curious, for it seems to me that the effect would scarce be sensibly less, if both sides should be ground to the concave and gauge of the same tool. I suppose you have received a letter from me, sent last week to signify my receipt of the books you sent in quires, &c. It comes now into my mind that when I sent Mr. Pitts four pounds for Kinkhuysen, he further urged a promise of some copies. When you have opportunity you will oblige me to remember him, that his proposal was either four pounds absolutely, or three pounds with some copies. I must join with Mr. Gregory in admiring Mr. Horrox. And this is all at present from, Sir,

your humble servant,

I. NEWTON.

Sir,

A friend here desires to have your judgment in the price of Francis Niceron his Thaumaturgus Opticus, printed in Latin at Paris, A.D. 1646.

This is the letter referred to by Gregory, in letter No. ccxiii. p. 251.

CCLIV.

NEWTON TO COLLINS.

Cambridge, May 20th.

Sir,

I received your two last letters, with Heuret's Optics, which, (not being so ready in the French tongue myself, as to read it without the continual use of a dictionary,) I committed to the perusal of another, who gives me this account of it. That he is not so plain and methodical as M. Boss; that he takes too much pains in demonstrating many things, which of themselves are sufficiently obvious, especially to one a little versed in Euclid; that his reprehensions of M. Boss are usually groundless and frivolous, as, for instance, being sometimes for his omission of some lines in his draughts, as if done out of ignorance, which yet a candid reader would rather think omitted lest his schemes should be cumbered with too great a multiplicity of lines, especially since the drawing of them might be deduced from his precepts; that his way of designing without regard to the point of distance is not preferable to the other ways, in which the point of distance is considered; and that it is as convenient to make use of a scale as of those other ways, which he would substitute instead thereof. So that, although this author hath enriched perspective with many new considerations, yet these, in practice, will have little or no advantage above those which are already in use.

This, Sir, in short, is the account of him, which I received from my friend, who esteems him a very good author, and one that thoroughly understands this science, yet of the two prefers M. Boss. I committed it also to the perusal of another friend, who out of curiosity desired it, but when he had looked upon two or three of his first propositions, he became prejudiced by reason of some greater obscurity in them than in those which M. Boss begins with, saying to me, that if he, writing more at large than M. Boss, did yet begin with more intricate propositions, he could not expect to find him in the rest of his book so clear and methodical as the other. I intended the last week to have returned your book with many thanks, but was disappointed, and so could not return it till now, which I do herewith by J. Stiles. I have sent you also ten shillings for that part of Kersey, which you sent me, and thirty shillings more for three other copies of the same, which I subscribed for. If you please to direct J. Stiles where he may receive the books and pay the money.

I thank you that you are pleased to remember me about what that most excellent author M. Huygens has lately published. I understand, by Mr. Oldenburg, that M. Slusius has some kind of information concerning my general method, which I made mention of to you. But though I must acknowledge your good will to me, in desiring Mr. Oldenburg to make it known to him, and see nothing in M. Slusius' reply but what is free and generous, yet I think it most proper for me to wave an answer, there being nothing that requires it. Concerning the expenses of being a member of the R. S. I suppose there hath been done me no unkindness, for I met with nothing in that kind besides my expectations. But I could wish I had met with no rudeness in some other things. And therefore I hope you will not think it strange, if, to prevent accidents of that nature for the future, I decline that conversation which hath occasioned what is past. I hope this, whatever it may make me appear to others, will not diminish your friendship to me.

Your humble servant,

I. Newton.

The year is not given in the date of the letter, but it must be 1673, from the circumstance that he mentions having received a part of Kersey's Algebra—for the first portion of that work was not published till about this period. See Phil. Trans. vol. viii. p. 6073.

CCLV.

NEWTON TO COLLINS.

Sept. 17, —73.

Sir,

I have here inclosed a testimony of my judgment concerning Mr. Dary his fitness for the place, which he sues for, and I wish him all success in it. The day after that I received your letter with one from him inclosed, I received another from him, wherein he desires my opinion about the relation of the lines one to another, which are drawn from the centre of an ellipsis to the angular points of a polygon inscribed into the same ellipsis, which consists of twenty-four equal sides. As it appears to me, their relation cannot be accurately known without an equation, which is four times decompounded of adfected cubic equations, and twice of quadratic ones, and by consequence would ascend to 324 dimensions; to compute which would be an Herculean labour, and when done it would be unmanageable. I doubt, therefore, that to decide the controversy between him and Mr. Gunton, recourse must be had to some mechanical examination of their assertions, but I leave it to you and Mr. Bond, to whom I understand it is referred. I must beg excuse that I have so long deferred to answer your letter, wherein another from Mr. Gregory was conveyed to me, but now I understand that Mr. Gregory is at London, and intends to make Cambridge in his way into Scotland, I shall not trouble you any further with discourses about the perspective, but refer it to our meeting, if Mr. Gregory will be pleased to favour me with a visit. I thank you for the little but ingenious tract of P. Pardies, and remain, Sir,

your obliged friend and servant,

IS. NEWTON.

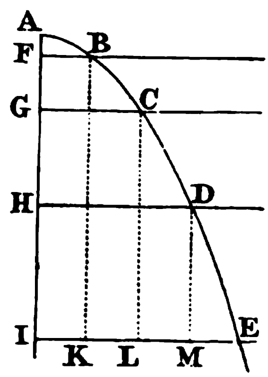
CCLVI.

NEWTON TO COLLINS.

Cambridge, June 20, 1674.

Sir,

I thank you for your kind present. Mr. Anderson's book is very ingenious, and may prove as useful if his principles be true; but I suspect one of them, namely, that the bullet moves in a parabola. This would be so indeed, were the horizontal celerity of the bullet uniform, but I should think its motion decays considerably in the flight. Suppose, for instance, a bullet shot horizontally from A, moves in the line AE, and AI being perpendicular to the horizon, in it take AF, AG, AH, AI, &c., in proportion as the square numbers 1, 4, 9, 16, &c.: and it is certain that if in one moment of time the bullet descend as low as F, in the next moment it shall descend as low as G, in the third as low as H, &c. And therefore drawing the horizontal lines FB, GC, HD, IE, the bullet at the end of the first moment will be somewhere in the line FB, suppose at B, and at the end of the second moment it will be somewhere in the line GC, suppose at C, &c. But that FB, GC, HD and IE are in arithmetical progression, (which is the condition of



the parabola,) seems not probable; for if it were so, the celerity of the bullet would increase, because the spaces AB, BC, CD, DE, described in equal times, are the latter bigger than the former; whereas I should rather think that the celerity decreases very considerably. And perhaps this rule for its decreasing may pretty nearly approach the truth; viz., letting fall the perpendiculars BK, CL, DM, &c., to make IK, KL, LM, &c., a decreasing geometrical progression. If you should have reason to speak of this to the author, I desire you would not mention me, because I have no mind to concern myself further about it.

As for the method of extracting the roots of literal equations, the root of this, $y^3+aay-b^3=0$, (which may be a form for all cubic equations,) may be thus extracted. Suppose l pretty nearly equal to the desired root, and put l in the quotient for the first term. Then proceed as follows:—

$$l-\frac{m}{n}-\frac{3lmm}{n^3}+\frac{m^3}{n^4}$$

For brevity's sake I here put $l^3+aal-b^3=m$ and $3ll+aa=n$.

$$l+p=y$$

$$\begin{array}{rr|l} & y^3 & l^3+3llp+3lpp+p^3 \\ + & aay & +aal+aap \\ - & b^3 & -b^3 \end{array}$$

$$-\frac{m}{n}+q=p$$

$$\begin{array}{rr|l} & p^3 & -\frac{m^3}{n^3}+\frac{3mm}{nn}q-\frac{3m}{n}qq+q^3 \\ + & 3lpp & +\frac{3lmm}{nn}-\frac{6lm}{n}q+3lqq \\ + & np & -m+nq \\ + & m & +m \end{array}$$

$$-\frac{3lmm}{n^3}+r=q$$

$$\begin{array}{r|l}
q^3 & -\frac{27l^3m^6}{n^9}+\frac{27llm^4}{n^6}r-\frac{9lmm}{n^3}rr+r^3 \\
-\frac{3m}{n}qq & -\frac{27llm^5}{n^7}+\frac{18lm^3}{n^4}r-\frac{3m}{n}rr \\
+\quad 3lqq & +\frac{27l^3m^4}{n^6}-\frac{18llmm}{n^3}r+3lrr \\
+\frac{3mm}{nn}q & -\frac{9lm^4}{n^5}\quad+\frac{3mm}{nn}r \\
-\frac{6lm}{n}q & +\frac{18llm^3}{n^4}-\frac{6lm}{n}r \\
+\quad nq & -\frac{3lmm}{nn}+nr \\
-\quad \frac{m^3}{n^3} & -\frac{m^3}{n^3} \\
+\frac{3lmm}{nn} & +\frac{3lmm}{nn}
\end{array}$$

There may be other ways of extracting the root of this equation: as for instance, if b be greater than a, the root may be thus expressed,

$$y=b^3-\frac{aa}{3b}+\frac{a^6}{81b^5}-\frac{a^8}{243b^7}, \&c.;$$

or thus, $y=\frac{b^3}{aa}-\frac{b^9}{a^8}+\frac{3b^{15}}{a^{14}}-\frac{12b^{21}}{a^{20}}$, &c., if a be considerably greater than b.

The extraction of the simple cube root may be done after the same manner, omitting only the second term aay. Or else it may be done as in numerical arithmetic. Suppose $y^3=a^3+b^3$, and that a is bigger than b, and let the divisor for finding the terms of the quotient be always triple the square of the first term, that is $3aa$. And the form of the work will be this:

$$a^3+b^3\ \left(\ a+\frac{b^3}{3aa}-\frac{b^6}{9a^5}+\frac{5b^9}{81a^8},\ \&c.\right.$$

$$\begin{array}{l}
a^3 \\
\hline
0\ +b^3 \\
\quad b^3+\frac{b^6}{3a^3}+\frac{b^9}{27a^6} \\
\hline
\quad 0-\frac{b^6}{3a^3}-\frac{b^9}{27a^6} \\
\quad -\frac{b^6}{3a^3}-\frac{2b^9}{9a^6}\ *+\frac{b^{15}}{81a^{12}}+\frac{b^{18}}{729a^{15}} \\
\hline
\qquad 0\ +\frac{5b^9}{27a^6}*-\frac{b^{15}}{81a^{12}}-\frac{b^{18}}{729a^{15}}
\end{array}$$

So if $y^3=c$, suppose any letter as a to be pretty exactly equal to the cube root of c, and putting $c-a^3=b$, or $a^3+b=c$, extract the cube root out of a^3+b, as in the former example, which root, (viz., $a+\frac{b}{3aa}-\frac{bb}{9a^5}+\frac{5b^3}{81a^8}$, &c.,) being once extracted, may be kept as a rule for extracting numeral cube roots. But yet (as you suggest) these infinite series are only useful, when the roots of equations cannot be attained accurately. For when they can be accurately attained, recourse must be had to other methods, this only performing it by approximation. Sir, I am

your obliged humble servant,

Is. Newton.

CCLVII.

DARY TO NEWTON.

Sir, Tower, 15 of Aug. 1674.

Although I sent you three papers yesterday, I cannot refrain from sending you this. I have had fresh thoughts this morning about those two sorts of equations, which we have lately bandied about, and I have attained an universal series for any equation of two cossic[a] notes. Truly it pleaseth me well; but yet I do hereby submit it to your censure.

To extract the root of an equation consisting of two several powers (or potestates) and an absolute number, by an approximation easily performed by logarithms. As for example $+z^p = az^q \pm n$.

In which example, and in all such like equations, you must observe that z is the unknown symbol or root sought, $p =$ the index (or power note) of the highest power, $a =$ the known number or coefficient of the middle term, $q =$ the index of the inferior power; $n =$ the absolute number or resolvend.

The rule is this: first guess at the root as nearly as you can, the nearer the better, (not for necessity but for accommodation,) and suppose that guess to be z.

Then observing the following series, you shall approach (from this supposed z) toward the true z, which is sought. Because every term in this series brings you nearer and nearer; for if your supposition be too great, every term in this series makes it less and less; or if your supposition be too little, every term in this series makes it greater and greater. So when you are pleased to make a cessation, the last term is that which you seek. The series follows:

$$\sqrt[p]{+az^q+n}=b \quad \sqrt[p]{+ab^q+n}=c$$
$$\sqrt[p]{+ac^q+n}=d \quad \sqrt[p]{+ad^q+n}=e$$
$$\sqrt[p]{+ae^q+n}=f \quad \sqrt[p]{+af^q+n}=g, \text{ \&c.}$$

In which series you must note that + only inti-

[a] Algebraic—from the Italian, in which the science was at first called Regula de Cosa.

mates the retaining of the proper signs, whatever they be, + or −.

Sir,

Pray do not count me troublesome, for I could not forbear but send this by Stiles the carrier, who is paid for the carriage. Pray remember me about the series of logarithms.

Your most humble and obliged servant,

MICH. DARY.

A true copy of that which I sent to Mr. Newton this morning.

CCLVIII.

NEWTON TO COLLINS.

Sir, Nov. 17, 1674, Cambridge.

Hearing that Mr. Kersey's book is out of press, I desire you would send me the fourth part. I have ordered John Stiles to satisfy you for it. Mr. Dary is very solicitous about mathematics. The rules, which I lately sent him for resolving equations by logarithms, to be communicated to you, he applies to quadratic equations; whereas they are only to be applied to equations, which have many intermediate terms wanting, four or five at least. And the more intermediate terms are wanting, the sooner they approach to truth. He is desirous of the best way of determining logarithms by the hyperbola, and the solution of another problem of the same kind, but I have nothing valuable to communicate therein, which you are not already acquainted with. So, with my thanks to you for all your kindness, I rest

your obliged and humble servant,

IS. NEWTON.

CCLIX.

NEWTON TO OLDENBURG.

Cambridge, Dec. 5, 1674.

Sir,

I am sorry you put yourself to the trouble of transcribing Fr. Linus's conjecture, since (besides that it needs no answer) I have long since determined to concern myself no further about the promotion of philosophy. And for the same reason I must desire to be excused from engaging to exhibit yearly philosophic discourses, but yet cannot but acknowledge the honour done me by your council, to think of me for one amongst that list of illustrious persons, who are willing to perform it, and therefore desire to have my thanks returned to them for the motion. If it were my lot to be in London for some time, I might possibly take occasion to supply a vacant week or two with something by me, but that's not worth mentioning.

If you think fit you may, to prevent Fr. Linus's slurring himself in print with his wide conjecture, direct him to the scheme in my second answer[t] to P. Pardies, and signify (but not from me) that the experiment, as it is represented, was tried in clear days, and the prism placed close to the hole in the window, so that the light had no room to diverge, and the coloured image made not parallel, (as in his conjecture,) but transverse to the axis of the prism.

Your humble servant,

Is. Newton.

[t] Phil. Trans. No. 110, p. 219. (Oldenburg's note.) Linn's letter with Oldenburg's reply, according to Newton's direction, are to be found together, Phil. Trans. vol. x. pp. 217—219.

CCLX.

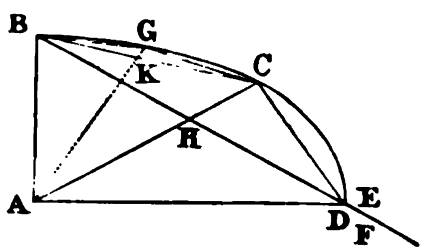
NEWTON TO DARY.[t]

Jan. 22, 1675.

Mr. Dary,

With my thanks for your problem, I should have sent you a continuation of the two series you desire, but I have not any computations of them by me, and perceive them so tedious to compute that I am constrained to defer them to another time. The series for the length of the ellipsis I computed when I sent it you, and sent you so much as I computed of it. But at present, instead of these I have sent you an approximation for the length of the ellipsis, which you may send Sir Anthony Dean instead of that you propounded, if you please.

Suppose AB, AD rectangular conjugate semidiameters of the ellipsis; BCD a quadrant of it; BHD its chord, bisected in H; draw AH abutting upon the ellipsis at C; join BC and CD; take BE = BC + CD, and EF = ⅓DE; and BF shall be the length of the quadrant quam proxime. This is derived from Hugenius's quadrature of the circle, and, I believe, approaches the ellipsis as near as his doth the circle.



[t] From a copy in Dary's handwriting, and indorsed "Mr. Isaac Newton's letter to Mich. Dary, Jan. 22, 1675."

In like manner, if you would know the length of any other arch as BGC, bisect its chord in K, draw AKG, and the chords BG, GC, and make $\frac{4BG+4GC-BC}{3}$ the length of the arch BC.

Thus you may find BC and CD generally, and the sum of them will give you the quadrant BCD, exacter than before.

Your loving friend,

I. NEWTON.

Dr. Barrow's Euclid I make no question but is safe enough, now I know it was delivered.

CCLXI.

NEWTON TO COLLINS.

Cambridge, July 24, 1675.

Sir,

I received your former letter as well as your later, and should have written to you sooner, but that I stayed to think of something that might satisfy your desire. But though I cannot hitherto do it to my own liking, yet that I may not rack your patience too much, I have here writ you what occurs to me, which is only about facilitating the extraction of roots. The former method might be applied to determine all by every thousandth, as well as by every hundredth root, but not with advantage, for it will require the extraction of roots to fourteen or fifteen places, besides a greater number of additions, subductions, and divisions, in those greater numbers. And therefore

I have rather sent you the following notes about extracting roots:

1. When you have extracted any root by common arithmetic to five decimal places, you may get the figures of the other six places by dividing only the residuum

$$\text{by}\left\{\begin{array}{l}\text{double the quotient}\\ \text{triple the square of quot.}\\ \text{quadruple the cube of quot.}\end{array}\right\}\begin{array}{c}\text{for}\\ \text{the}\\ \text{root}\end{array}\left\{\begin{array}{l}\text{square}\\ \text{cube}\\ \text{square square.}\end{array}\right.$$

Suppose B the quotient or root, extracted to five decimal places, and C the last residuum, by the division of which you are to get the next figure of the quotient, and D the divisor, (that is, $2B$, or $3BB$, or $4B^3=D$,) and $B+\frac{C}{D}$ shall be the root desired. That is, the same division by which you would find the sixth decimal figure, if prosecuted, will give you all, to the eleventh decimal figure.

2. You may seek the root, if you will, to five decimal places by the logarithms. But then you must find the rest thus:—

$$\text{Divide the propounded number}\left\{\begin{array}{l}\text{once}\\ \text{twice}\\ \text{thrice}\end{array}\right\}\text{by that root,}$$

prosecuting the division always to eleven decimal places, and to the quotient add the said root;

$$\left.\begin{array}{l}\text{once, and half}\\ \text{twice, and a third part}\\ \text{thrice, and a quarter}\end{array}\right\}\begin{array}{c}\text{of the}\\ \text{sum shall}\\ \text{be the}\end{array}\left\{\begin{array}{l}\text{square}\\ \text{cube}\\ \text{square square}\end{array}\right\}\begin{array}{c}\text{root}\\ \text{desired.}\end{array}$$

For instance,

$$\text{Let } A \text{ be the number and } B \text{ its}\left\{\begin{array}{c}\text{Q}\\ \text{C}\\ \text{QQ}\end{array}\right\}\text{root, extract-}$$

ed by logarithms unto five decimal places,

$$\text{and} \left\{ \begin{array}{l} 2)\ B + \dfrac{A}{B} \\ 3)\ 2B + \dfrac{A}{B^2} \\ 4)\ 3B + \dfrac{A}{B^3} \end{array} \right\} \text{shall be the} \left\{ \begin{array}{c} Q \\ C \\ QQ \end{array} \right\} \text{root desired}$$ [u].

Note; that you have according to my former direction, but 76 Q roots, and 88 C roots, and 94 QQ roots to extract, whereof ten are exact roots. But I think you will do well to let the table of QQ roots alone till you have done the other two; and then, if you find your time too short, print the Q and C roots without troubling yourself any further.

Sir, I am
your humble servant,
IS. NEWTON.

This letter is printed from a copy which is not in Newton's handwriting: but the amanuensis has added "Copia vera" at the end of it. "Root" or "roots" have been substituted for Rx, which is the abbreviation for radix used by the writer.

CCLXII.

NEWTON TO COLLINS.

Trin. Coll. Aug. 27. 1675.

Sir,

In the theorems that I sent you I perceive I committed a mistake, in transcribing them from the papers

[u] See the following letter.

where I had computed them. They should have been

$$2)\quad B+\frac{A}{B}=\sqrt{A}$$

$$3)\ 2B+\frac{A}{BB}=\sqrt[3]{A}$$

$$4)\ 3B+\frac{A}{B^3}=\sqrt[4]{A}$$

In words at length: to find the cube root of A to eleven decimal places, seek the root by logarithms to five decimal places, and suppose it B. Then square B, not by logarithms, but by common arithmetic, that you may have its exact square to ten decimal places, and by this square divide A to eleven decimal places, and to the quotient add $2B$; the third part of the quotient[v] shall be the root cubical of A to eleven decimal places. Your surest way will be to find first the whole series of the roots B by logarithms, and try whether it be regular by differencing it; then square those roots by Napier's bones, and last divide each number A by the correspondent square, and add $2B$ to each quotient, and try the resulting series again by differencing it, whether it be regular. If it be regular, I suppose, you know the differences will at last come to be equal. What is said of cubes is easily applicable to square-squares. I would have given you examples in numbers, but that I have lent my books of logarithms to a person, who is out of town.

Your humble servant,

IS. NEWTON.

I thank you for your intended present.

This is printed from a "copia vera" in the same hand

[v] This should be *sum* not the last letter, and the com-
quotient, as is evident both from mencement of the present.

as the preceding letter, and on the side of it is the following note.

"Mr. Collins,

"I have left with the maid your book of Briggs's Loga-"rithms, and would request your favour (if I might not be "too troublesome) to procure me the loan of Napier's Bones, "and the book of their use; I should in a few days return "them. "J. SMITH.

"I find all the roots (found by the longest radius of loga-"rithms) false and uncertain from the eighth place of the "decimal onwards; though the logarithm itself and the work "upon it be duly proved."

CCLXIII.

NEWTON TO OLDENBURG.

Cambridge, Decemb. 14, 1675.

Sir,

The notice you gave me of the Royal Society's intending to see the experiment of glass rubbed to cause various motions in bits of paper underneath*, put me upon recollecting myself a little further about it, and then remembering that if one edge of the brass hoop was laid downward, the glass was as near again to the table as it was when the other edge was laid downward, and that the papers played best when the glass was nearest to the table: I began to suspect, that I had set down a greater distance of the glass from the table than I should have done, for in setting down that experiment, I trusted to the idea I had of the bigness of the hoop, in which I might be easily

* See Birch's History of the Royal Society, vol. iii. pp. 250, 251.

mistaken, having not seen it of a long time. And this suspicion was increased by trying the experiment with an object glass of a telescope, placed about the third part of an inch from the table. For I could not see the papers play any thing near so well as I had seen them formerly. Whereupon I looked for the old hoop with its glass, and at length found the hoop, the glass being gone; but by the hoop I perceived, that when one edge was turned down, the glass was almost the third part of an inch from the table, and when the other edge was down, which made the papers play so well, the glass was scarce the eighth part of an inch from the table. This I thought fit to signify to you, that if the experiment succeed not well at the distance I set down, it may be tried at a less distance, and that you may alter my paper, and write in it an eighth part of an inch, instead of a half or a third of an inch. The bits of paper ought to be very little, and of thin paper. Perhaps little bits of the wing of a fly, or other light substances, may do better than paper. Some of the motions, as that of hanging by a corner, and twirling about, and that of leaping from one part of the glass to another without touching the table, happen but seldom, but it made me take the more notice of them.

Pray present my humble service to Mr. Boyle, when you see him, and thanks for the favour of the converse I had with him at spring. My conceit of trepanning the common ether, as he was pleased to express it, makes me begin to have the better thoughts on that he was pleased to entertain it with a smile. I am apt to think that, when he has a set of experiments to try in his air pumps, he will make that one to see how the compression or relaxation of a muscle will sink or swell, soften or harden, lengthen or shorten it.

As for registering the two discourses, you may do it, only I desire you would suspend till my next letter, in which I intend to set down something to be altered, and something to be added in the hypothesis, being, in the mean while, Sir,

your humble servant,

Is. Newton.

This letter is printed in the Gen. Dict. vol. vii. p. 783.

CCLXVI.

NEWTON TO OLDENBURG.

December 21, 1675.

Sir,

Upon your letter I took another glass, four inches broad and a quarter of an inch thick, of such glass as telescopes are made of, and placed it a sixth 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, and all was still, the motion of the papers would continue sometimes while I counted a hundred, every paper leaping up about twenty times, more or less, and down as often. I tried it also with two other glasses that belong to a telescope, and it succeeded with both, and I make no question but any glass 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 glass, you will fail in any I can send you. I am apt to suspect the failure was in the manner of rubbing: for I have observed that the rubbing variously, or with various things, alters the case. At one time I rubbed the aforesaid great glass with a napkin, twice as much as I used to do with my gown, and nothing would stir, and yet presently rubbing it with something else, the motions soon began. After the glass has been much rubbed too, the motions are not so lasting, and the next day I found the motion fainter, and difficulter to excite than the first. If the society have a mind to attempt it any more, I can give no better advice than 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,) and let this be rubbed, not with linen, nor soft nappy woollen, but with stuff, whose threads may rake the surface of the glass, suppose tamerine, or the like, doubled up in the hand, and this with a brisk motion as may be, till a hundred or a hundred and fifty may be counted, the glass lying all the while over the papers: then if nothing stir rub the glass with your finger ends half a score times to and fro, or knock your finger ends as oft upon the glass; 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 sixty or eighty may be counted, and then rub or knock again with your fingers, and repeat this till the electric virtue of the glass be so far excited as to take up the papers, and then a very little rubbing or knocking now and then will revive the motions. In doing all this, let the rubbing be always done as nimbly as may be, and if the motion be circular, like that of glass 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 being 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 myself till the last week, I suspect the air might raise the gold, and then a small attraction might determine it toward the glass, for I could not make it succeed.

As for Mr. Hooke's insinuation that the sum of the hypothesis I sent you had been delivered by him in his Micrography, I need not be much concerned 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 shall do it as briefly as I can, and that the rather, that I may avoid the savour of having done any thing unjustifiable or unhandsome towards Mr. 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 ethereal medium; that light is the action of this medium; that this medium is less implicated in the parts of solid bodies, and so moves more freely in them, and transmits light more readily through them; and that after such a manner as to accelerate the rays in a certain proportion; that refraction arises from this acceleration, and has sines proportional; that light is at first uniform; that its colours are some disturbances, 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, and retarding it on the other where blue appears; and that there are but these two original colours or colour-making modifications of light, which by their various degrees, or, as Mr. Hooke calls it, dilutings, produce all intermediate ones.

This rejected, the remainder of his hypothesis is that he has changed Des Cartes's pressing and progressive motion of the medium to a vibrating one, the rotation of the globuli to the obliquation of pulses, and the accelerating their rotation on the one hand, and 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 phenomena of thin plates, and added another explication of the colours of natural bodies, fluid and solid.

This I think is, in short, the sum of his hypothesis, and in all this I have nothing common with him, but the supposition that ether is a medium susceptible of vibrations, of which supposition I make a very different use: he supposing it light itself, which I suppose it is not. This is as great a difference as is between him and Des Cartes. But besides this, the manner of refraction and reflexion, and the nature and production of colours in all cases, (which takes up the body of my discourse,) I explain very differently from him; and even in the colours of thin transparent substances I explain every thing after a way so differing from him, that the experiments I ground my discourse on destroy all he has said about them, and 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 is common to us, that ether may vibrate, and so if he think fit to use that notion of colours arising from the various bigness 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, and 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; and that of plated bodies exhibiting colours, a phenomenon, for the notice of which I thank him: but he left me to find out and make such experiments about it, as might inform me of the manner of the production of those colours, to ground an hypothesis on; he having given no further insight into it than this, that the colour depended on some certain thickness of the plate, though what that thickness was at every colour, he confesses in his Micrographia, he had attempted in vain to learn; and therefore, seeing I was left to measure it myself, I suppose he will allow me to make use of what I took the pains to find out. And this I hope may vindicate me from what Mr. Hooke has been pleased to charge me with. Sir, I doubt I have already troubled you with too long a letter, and so break off abruptly.

Your humble servant,

Is. Newton.

This letter is printed in the Gen. Dict. vol. vii. p. 783, and in Birch's Hist. of R. S. vol. iii. p. 270, 278.

CCLXV.

NEWTON TO OLDENBURG.[w]

January 10, 1675–6.

Sir,

Concerning the experiment of the glass and papers, I should add these two to the former directions. One, that the glass be rubbed with a full handful of stuff, which may cover and rub all the glass at once; for thus its electric virtue will be more easily and vigorously excited than if rubbed with a little only, doubled up but once or twice. This rubbing with the stuff, I suppose, rarefies and diffuses the electric effluvia from the glass into the air, and 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 glass or table. If the experiment be tried with a glass three or four inches broad, set about one sixth of an inch from the table, and the papers of a thin sort of paper cut into triangular pieces, the sides of those triangles may not unfitly be about the twentieth or twenty-fifth part of an inch, more or less. It may be best tried with bits of several sizes put in at once, and 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 and the former directions observed, I cannot imagine how you

[w] There is no written address on this letter, but there is an indorsement on it in Oldenburg's handwriting of the days, on which it was received and answered.

should miss, though I cannot promise all 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 candour in acquainting me with Mr. Hooke's insinuations. It is but a reasonable piece of justice I should have an opportunity to vindicate myself from what may be undeservedly cast on me, and therefore, since you have been pleased to be my representative there, and I have no means of knowing what is done but by you, I hope you will continue that equitable candour; though I think the present business 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 Mr. Hooke 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 ether to vibrate, and yet whilst he grasps at all, he is likely to fall short of this too. That ethereal vibrations are light is his; but that ether may vibrate (which is all I suppose) is to be had from a higher fountain: for that ether is a finer degree of air, and air a vibrating medium, are old notions, and the principles I go upon. I desire Mr. Hooke to shew me, therefore, I say not only the sum 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 is his own. It is not likely he'll pretend I had from him the application of vibrations to the solution of the phenomena of thin plates: and yet all the use I make of vibrations is to strengthen or weaken the reflecting power of the ethereal superficies, which is so far from being in his Micrographia, that the last spring, when I told him of the reflecting power of the ethereal 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, lest 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: that as Des Cartes puts every globulus to be urged forward on one side by the illuminated medium, and impeded on the other by the dark one, so Mr. Hooke puts every vibration to be promoted at one end and retarded at the other by those mediums, and thence both alike derive two modifications of light on the two sides of the refracted beam for the production of all colours.

By Mr. Gascoigne's letter,[x] one might suspect that Mr. Linus tried the experiment some other way than I did, and 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 them to acquaint them that the prism casts from it several images. One is that oblong one of colours, which I mean, and this is made by two refractions only. Another there is, made by two refractions and an intervening reflexion, and this is round and 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, and that so much the more by how much unequaller the angles are, but yet not much unround, unless the angles be very unequal. A third image there is, made by one single reflexion, and this is

[x] Gascoigne's letter is printed in this collection, vol. i. pp. 221—224. An extract from this letter, beginning with this paragraph, is inserted in the Phil. Trans. vol. x. p. 503.

always round and colourless. The only danger is in mistaking the second for the first. But they are distinguishable, not only by the length and 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 and third move swiftly, and go always on the same way till they disappear, but the first moves slow, and grows continually slower till it be stationary, and then turns back again, and goes back faster and faster, till it vanish in the place where it began to appear. If, without darkening 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's beams, and then turn it about its axis, they "cannot miss of seeing the first image; which having "found, they may double up a paper once or twice, and "make a round hole in the middle of it about half or "three quarters of an inch broad, and hold the paper "immediately before the prism, that the sun may shine "on the prism through that hole; and the prism being "stayed and held steady 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 fifteen or twenty feet 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, and "a bluish green in the middle; and if they obscure "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 nobody into whose hands a prism shall happen, may find difficulty or trouble in trying it. But when Mr. Linus's friends have tried it thus, they may proceed to repeat it in a dark room with a less hole made in their window-shut. And then I shall desire that they will send you a full and clear description how they tried it, expressing the length, breadth, and angles of the prism, its position to the incident rays, and to the window-shut, the bigness of the hole in the shut, through which the sun shined on the prism, what side of the prism the sun shined on, and 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, and the length, breadth, and figure of the space there illuminated by that light, and the situation of each colour within that figure; and if they please to illustrate their description with a scheme or two, it will make the business plainer. By this means, if there be any difference in our way of experimenting, I shall be the better enabled to discern it, and give them notice where the failure is, and 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 tried by Mr. Linus, that I may have an opportunity to consider what there is in that, which makes against me. And because Mr. Gascoin seems to suspect that my directions sent Mr. 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 than I have followed above these seven years. As for my suspicion that Mr. 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 Mr. Gascoin's insinuation, as if I influenced the press in what concerns Mr. Linus and me. You know, Sir, I never spake nor hinted a syllable to you concerning printing or not printing any thing of Mr. 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 entertained 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 postscript to this purpose: that seeing Mr. Linus was designing something about light for the press, to prevent publishing his mistake you might, if you thought fit, signify to him, (but not from me,) that the experiment was tried otherwise than he suggested, and that in such and 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 Mr. Gascoin here animadverts, but was so far from being designed 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 wished they had been suppressed, for I doubted the printing them would make Mr. Linus unquiet, and so in the end create me trouble. As for his second letter, which you shewed me at London, I returned it again to you so soon as I had read it, and 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 waived it, Mr. Linus was like to make the more stir; to which I replied, that the business, 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, and by your procurement preparations were accordingly made for its trial at the next assembly of the Royal Society, as I understood by Mr. Hooke: but the day proved cloudy, and before another assembly I returned to

Cambridge, and from that time never inquired after nor regarded the matter further, till you sent me Mr. Linus's third letter. This is the history of Mr. Linus's business, so far as I know it; which I have set down that his friends may see he has not been dealt with obliquely, as they seem to apprehend. All that I think they can object to you is, that you were at a stand, because you could not engage me in the controversy, and to me, that I had no mind to be engaged: a liberty every body has a right to, and may gladly make use of, sometimes at least, and especially if he want leisure, or meet with prejudice or groundless insinuations. But I hope to find none of this in Mr. Gascoin. The handsome genius of his present letter makes me hope it for the future. In the mean time I desire, with him, that you would publish Mr. Linus's letters as soon as you can conveniently, to prevent further misapprehensions. Sir, I am

your obliged and humble servant,

Is. Newton.

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

In printing my former letter to Mr. Linus, you may leave out what I mention of Mr. Hill and Mr. Hooke, or at least put letters for their names; for I believe they had rather not be mentioned. If you have opportunity, pray present my service to Mr. Hooke, for I suppose there is nothing but misapprehension in what has lately happened.

This letter is printed in the Gen. Dict. vol. vii. p. 784.

CCLXVI.

NEWTON TO OLDENBURG.

Jan. 25, 1675-6.

Sir,

I received both yours, and thank you for your care in disposing those things between me and Mr. Linus. I suppose his friends cannot blame you at all for printing his first letter, it being written, I believe, for that end ; and they never complaining of the printing of that, but of the not printing that which followed, which I take myself to have been, per accidens, the occasion of by refusing to answer him. And though I think I may truly say I was very little concerned about it, yet I must look upon it as the result of your kindness to me, that you was unwilling to print it without an answer.

As to the paper of observations, which you move in the name of the Society to have printed, I cannot but return them my hearty thanks for the kind acceptance they meet with there ; and know not how to deny any thing, which they desire should be done. Only I think it will be best to suspend the printing of them for a while, because I have some thoughts of writing such another set of observations for determining the manner of the production of colours by the prism, which, if done at all, ought to precede that now in your hands, and will do best to be joined with it. But this I cannot do presently, by reason of some incumbrances lately put upon me by some friends, and some other business of my own, which at present almost take up my time and thoughts.

The additions that I intended, I think I must, after putting you to so long expectations, disappoint you in: for it puzzles me how to connect them with what I sent you; and if I had those papers, yet I doubt the things I intended will not come in so freely as I thought they might have done. I could send them described without dependance on those papers, but I fear I have already troubled the Society, and yourself, too much with my scribbling, and so suppose it may do better to defer them till another season. I have therefore at present only sent you two or three alterations, though not of so great moment that I need have stayed you for them; and they are these.

Where I say that the frame of nature may be nothing but ether condensed by a fermental principle, instead of those words write, that it may be nothing but various contextures of some certain ethereal spirits, or vapours, condensed as it were by precipitation, much after the manner that vapours are condensed into water, or exhalations into grosser substances, though not so easily condensible; and after condensation wrought into various forms, at first by the immediate hand of the Creator, and ever since by the power of nature, who by virtue of the command, "Increase and multiply," became a complete imitator of the copies set her by the Protoplast. Thus perhaps may all things be originated from ether, &c.

A little after, where I say the ethereal spirit may be condensed in fermenting or burning bodies, or otherwise inspissated in the pores of the earth to a tender matter, which may be, as it were, the succus nutritius of the earth, or primary substance, out of which things generable grow: instead of this, you may write, that that spirit may be condensed in fermenting or burning bodies, or otherwise coagulated in the pores of the earth and water, into some kind of humid active matter for the continual uses of nature, adhering to the sides of those pores after the manner that vapours condense on the sides of a vessel.

In the same paragraph there is, I think, a parenthesis, in which I mention volatile salt-petre; pray strike out that parenthesis, lest it should give offence to somebody.

Also where I relate the experiment of little papers made to move variously with a glass rubbed, I would have all that struck out which follows about trying the experiment with leaf gold.

Sir, I am interrupted by a visit, and so must in haste break off.

Your's,

Is. NEWTON.

This letter is printed in the Gen. Dict. vol. vii. p. 786.

CCLXVII.

NEWTON TO OLDENBURG.

Feb. 15, 1675-6.

Sir,

I thank you for giving me notice of the objection, which some have made. If I understand it right, they mean that colour may proceed from the different velocities which ethereal pulses or rays of light may have, as they come immediately from the sun. But if this be their meaning, they propound not an objection, but an hypothesis to explain my theory. For the better understanding of this, I shall desire you to consider, that I put not the different refrangibility of rays to be the internal or essential cause of colours, but only the means whereby rays of different colours are separated. Neither do I say what is that cause, either of colour or of different refrangibility, but leave these to be explained by hypotheses, and only say, that rays, which differ in colour, differ also in refrangibility, and that different refrangibility conduces to the production of colour no other way than by causing a different refraction, and thereby a separating of those rays, which had different colours before, but could not appear in their own colours till they were separated. Suppose red and blue powders (as Minium and Bice) were equally mixed, the compound would be neither a good red nor a good blue, but a middling dirty colour. Suppose further this mixture was put into water, and after the water had been well stirred, the powder left to subside; if the red was much more ponderous than the blue, it would subside fastest, and leave most of the blue to subside after it; and by consequence the heap would appear red at bottom and blue at top, and of intermediate colours between. Here then are various colours produced out of a dirty colour by means of different gravity, and yet that different gravity was not the internal cause of those colours, but only the cause of the separation of the particles of [the] several colours. And so it is in the production of colours by the prism: the different refrangibility of rays is no otherwise the cause of colour in this case, than the different gravity of the powders was in the other; it only causes a diverse refraction of the rays originally qualified to exhibit diverse colours, and by that diverse refraction they are separated, and, when separated, they must needs exhibit each their own colours, which they could not do while mixed. Had I supposed different refrangibility the internal cause of colours, it would have been strangely precarious, and scarcely intelligible; but to make it only the cause of separation of rays endowed with different colours is nothing but experiment, and all that I have asserted in my writings. In like manner, where I make different reflexibility the cause of colours, (as in the case of thin transparent plates,) I say not that it is their internal cause, but only the means of their separation. For I apprehend that all the phænomena of colours in the world result from nothing but separations or mixtures of difform rays, and that different refrangibility and reflexibility are only the means, by which those separations or mixtures are made.

This being apprehended, I presume you will easily see that you have not sent me an objection, but only an hypothesis, to explain my theory by. For to suppose different velocities of the rays the principle of colour, is only to assign a cause of the different colours, which rays are originally disposed to exhibit, and do exhibit, when separated by different refractions. And though this should be the true essential cause of those different colours, yet it hinders not but that the different refrangibility of the rays may be their accidental cause, by making a separation of pulses of different swiftness. Yea, so far is this hypothesis from contradicting me, that, if it be supposed, it infers all my theory. For if it be true, then is the sun's light an aggregate of heterogeneal rays, such [as] are originally disposed to exhibit various colours; then is the whiteness of that light a mixture of those colours, being the result of the mixture of those unequal colorific motions: then is there nothing requisite for the production of colours but a separation of these rays, so that the swiftest may go to one place by themselves and the slowest to another by themselves, or one sort be stifled and another remain: then must all the phænomena of colours proceed from the separations of these rays of unequal swiftness, because while they continue blended together, as in the sun's original light, they can exhibit no other colour but white; and lastly, then must various refrangibility and reflexibility be the instrumental causes of the phænomena of colour, those two being the proper means whereby difform rays are separated.

Were I to apply this hypothesis to my notions, I would say, therefore, that the slowest pulses being weakest, are more easily turned out of the way by any refracting superficies than the swiftest, and so, cæteris paribus, are more refracted: and that the prism, by refracting them more, separates them from the swiftest, and then they, being freed from the alloy of one another, strike the sense distinctly, each with their own motions apart, and so beget sensations of colour different both from one another, and from that which they begat while mixed together; suppose the swiftest the strongest colour, red; and the slowest the weakest, blue.

To all this I might add, concerning the different swiftness of rays, that I myself have formerly applied it to my notions in mentioning other hypotheses, as you may see in my answer to Mr. Hooke, sect. 4,[y] and I think also in the hypothesis I lately sent you. I say, I applied it in other hypotheses; for in this of Mr. Hooke, I think, it is much more natural to suppose the pulses equally swift and to differ only in bigness, because it is so in the air, and the laws of undulation are without doubt the same in ether that they are in air.

[y] See Phil. Trans. vol. vii. p. 5088.

Having thus answered, as I conceive, your objection in particular, I shall now, for a conclusion, remind you of what I have formerly said in general to the same purpose; so that I may at once cut off all objections, that may be raised, for the future, either from this or any other hypothesis whatever. If you consider what I said both in my second letter to P. Pardies and in my answer to Mr. Hooke, sect. 4,[z] concerning the application of all hypotheses to my theory, you may thence gather this general rule: That in any hypothesis where the rays may be supposed to have any original diversities, whether as to size, or figure, or motion, or force, or quality, or any thing else imaginable, which may suffice to difference those rays in colour and refrangibility, there is no need to seek for other causes of these effects than those original diversities. This rule being laid down, I argue thus. In any hypothesis whatever, light, as it comes from the sun, must be supposed either homogeneal or heterogeneal. If the last, then is that hypothesis comprehended in this general rule, and so cannot be against me: if the first, then must refractions have a power to modify light, so as to change its colorific qualification and refrangibility, which is against experience.

Since the writing of this I received your other letter. I thank you for your account of Mr. Berchenshaw's scale of music, though I have not so much skill in that science as to understand it well. If you should register the papers in your hand, before you return them, I would desire you to leave out the last paragraph of the hypothesis, where I mention Mr. Hooke and Grimaldo together: but since you are to receive

[z] See Phil. Trans. vol. vii. p. 5088, and the letter to Pere Pardies, ibid. p. 5014: the page in the Transactions is wrongly numbered 4014.

those papers again, (that of the observations at least; for the hypothesis I am more inclined to suppress,) I suppose it will not be necessary that you should put yourself to the trouble of registering them.

I remain, Sir,

your humble servant,

Is. Newton.

This letter is printed in the Gen. Dict. vol. vii. p. 786.

CCLXVIII.

NEWTON TO OLDENBURG.

Cambridge, April 26, 1676.

Sir,

I am now to return you thanks on a double account, the one for publishing my letter in your last Transactions, the other for motioning to get the experiment in controversy tried before the Royal Society. I could be very desirous, not to say ambitious, to have such a thing done, did I not fear I should be troublesome; and therefore I shall esteem it a great favour, if you please to get it done, being apt to think that Mr. Linus's friends will not otherwise acquiesce.

Yesterday I, reading the two last Philosophical Transactions, had the opportunity to consider Mr. Boyle's uncommon experiment about the incalescence of gold and mercury. I believe the fingers of many will itch to be at the knowledge of the preparation of such a mercury, and for that end some will not be wanting to move for the publishing of it, by urging the good it may do in the world; but in my simple judgment the noble author, since he has thought fit to reveal himself so far, does prudently in being reserved in the rest. Not that I think any great excellence in such a mercury, either for medical or chemical operations: for it seems to me that the metalline particles, with which that mercury is impregnated, may be grosser than the particles of the mercury, and be disposed to mix more readily with the gold, upon some other account than their subtilty, and then in so mixing their grossness may enable them to give the parts of the gold the greater shock, and so put them into a brisker motion than smaller particles could do: much after the manner that the saline particles, wherewith corrosive liquors are impregnated, heat many things, which they are put to dissolve, whilst the finer parts of common water scarce heat any thing dissolved therein, be the dissolution never so quick, and if they do heat any thing, (as quick lime,) one may suspect that heat is produced by some saline particles lying hid in the body, which the water sets on work upon the body, which they could not act on, whilst in a dry form. I would compare, therefore, this impregnated mercury to some corrosive liquor, (as aqua fortis,) the mercurial part of the one to the watery or phlegmatic part of the other, and the metallic particles, with which the one is impregnated, to the saline particles, with which the other is impregnated; both which I suppose may be of a middle nature between the liquor, which they impregnate, and the bodies they dissolve, and so enter those bodies more freely, and by their grossness shake the dissolved particles more strongly than a subtiler agent would do. If this analogy of these two kinds of liquors may be allowed, one may guess at the little use of the one by the indisposition of the other, either to medicine or vegetation. But yet because the way, by which mercury may be so impregnated, has been thought fit to be concealed by others that have known it, and therefore may possibly be an inlet to something more noble, not to be communicated without immense damage to the world, if there should be any verity in the Hermetic writers, therefore I question not but that the great wisdom of the noble author will sway him to high silence till he shall be resolved of what consequence the thing may be, either by his own experience, or the judgment of some other, that thoroughly understands what he speaks about, that is, of a true Hermetic philosopher, whose judgment (if there be any such) would be more to be regarded in this point than that of all the world beside to the contrary, there being other things beside the transmutation of metals, (if those great pretenders brag not,) which none but they understand. Sir, because the author seems desirous of the sense of others in this point, I have been so free as to shoot my bolt; but pray keep this letter private to yourself.

Your servant,

IS. NEWTON.

CCLXIX.

NEWTON TO COLLINS.

Cambridge, Sept. 5, 1676.

Sir,

I received the packet you sent, and return you the manuscript papers with my thanks for them, and for M. Frenicle's book. In your paper about Mr. Gregory, I have presumed to raze out two things, as you will perceive: the first, because though about five years ago I wrote a discourse, in which I explained the doctrine of infinite equations, yet I have not hitherto read it, but keep it by me: the last, because in my general method mentioned in your fourth section, I have occasion to make use of no other way of extracting the roots of adfected equations than that you are already acquainted with. If you should have occasion to see Dr. Pell, or (if he be not in London) to write to him at any time, pray present my service to him, and let him know, that, though I know not how far Mr. Gregory has improved the method of infinite series, yet, so far as I know any thing of it, I account it of no great advantage for resolving adfected equations in numbers. Some use it may have sometimes this way, but I neither invented it nor recommend it much for this end, but for extending Algebra to such sorts of problems, as the common ways of computing extend not to. And therefore, his method for resolving equations interfering so little with mine, I could wish, (even though they interfered much more,) that he would not stay to expect the publishing of mine, as I perceive by one of the papers you sent me he does. For I would not be an instrument of hindering the public so long from enjoying a thing so valuable. I have nothing in the press, only Kinkhuysen's Algebra I would have got printed here to satisfy the expectation of some friends in London, but our press cannot do it. This I suppose is the book Dr. Lloyd means: it is now in the hands of a bookseller here to get it printed; but if it do come out I shall add nothing to it. As for my paper I sent about infinite series, I know not whether it will be proper to print it. I leave it to your discretion. In my apprehension, it may do as well to suppress it; but if you think otherwise, I desire you would give me notice before it go into the press, because of altering an expression or two. Mr. Baker's patience, as well as his skill, I admire. His method I see is to find first *x*, the sum of the four quantities, and then the quantities severally, which I think is the method you were suggesting to me at London. The other problem, I think, I told you required no art, but much calculation, to resolve it, and therefore I have never thought of it since I saw you. There is nothing requisite to the solution but this: to find two equations expressing the nature of the two curve lines, supposing their bases coincident and their ordinates parallel, and putting the same letter, suppose *x*, for the bases in both equations, and another letter, suppose *y*, for the ordinates, to exterminate one of those letters. For the resulting equation will give you the several valors of the other letter, which valors limit all the intersection points of the two curves. I doubt I shall put you to too much trouble to transcribe Mr. Leibnitz's whole letter, if it be so long, and therefore I shall desire you to send me only a general account of it, with such passages as you think may concern me, if there be any thing that concerns me.

Sir, I am

your humble servant,

Is. NEWTON.

This letter is printed in the Gen. Dict. vol. vii. p. 787.

CCLXX.

NEWTON TO ———.

October 24, 1676.

Sir,

I doubt you have thought it long that I have not yet answered yours. Your letter out of the cider country I have communicated here. I know not yet what effect it will have. To M. Leibnitz's ingenious letter I have returned an answer, which I doubt is too tedious. I could wish I had left out some things, since to avoid greater tediousness I left out something else, on which they have some dependance. But I had rather you should have it any way, than write it over again, being at present otherwise encumbered. Sir, I am, in great haste,

yours,

Is. Newton.

I hope this will so far satisfy M. Leibnitz, that it will not be necessary for me to write any more about this subject; for having other things in my head, it proves an unwelcome interruption to me to be at this time put upon considering these things.

There is no address to this letter. It seems to have been written to Collins, from the subject of the preceding communication; and there are a number of figures in Collins's handwriting on the back.

CCLXXI.

COLLINS TO NEWTON.

Mr. Newton—Sir,

The letter herewith sent proclaims the author's great worth, knowledge, and candour, and the method therein seems unto me most admirable for geometric curves, wherein the ordinates are expressed by an equation, but how it will perform in mechanic curves, wherein there is no such habitude expressed, I humbly crave your sentiment. At first reading it was not immediately obvious how he came to find $y = \frac{2zr^2}{rr + zz}$. His design is, first out of these data, AQ $= r$ the radius, and Q1N $= z$, to find the square of the chord AD $= \frac{4r^4}{rr + zz}$. For the finding whereof, suppose the chord of the complement to the semicircle to be likewise drawn, and then there is given the ratio of these chords, such as r to z, and the sum of their squares $= 4rr$. Out of such data, by an analytical process, AD^2 is found $= \frac{4r^4}{rr + zz}$.

And then it holds

AN^2 : NQ^2 : : AD^2 : D1B1^2, which he calls y^2: that is $rr + zz : zz : : \frac{4r^4}{rr + zz} : \frac{4r^4 zz}{\overline{rr + zz}^2}$, the root whereof is $\frac{2zr^2}{rr + zz} = y$, as he makes it: and changing zz in the second term of the proportion for rr, by such means

A1B, or x, is found $= \frac{2r^3}{rr + zz}$, as he likewise makes it. Excuse me for troubling you with this impertinence.

I remain,

your most humble thankful servitor,

JOHN COLLINS.

Whereas he says, "habita ergo recta 1 B 1 D $= \frac{2zr^2}{rr + zz}$

"et recta 1 B 2 B $= \frac{4r^3 z\beta}{\overline{rr + zz}|^2}$

"habebitur valor rectanguli 1 D 1 B 2 B, multiplicatis "eorum valoribus in se invicem, habebitur inquam " $\frac{8r^5}{\overline{r^2 + z^2}|^3}$;" this should be $\frac{8r^5 zz\beta}{\overline{r^2 + z^2}|^3}$.

He says the ordinate NP is $\frac{8r^5 z^2}{\overline{r^2 + z^2}|^3}$; this should be the same.

Out of $r = \text{QA}$, and $z = \text{Q1N}$, he finds $x = \text{A1B}$ (which is supposed not to fall out of the line AQ, though the figure represents otherwise): thus,

$$r : z : : x : \frac{zx}{r} = 1\text{B}1\text{D}.$$

This squared, viz. $\frac{zzxx}{rr} = 2rx - x^2$, whence by reduction $x = \frac{2r^3}{zz + rr}$.

His calculus of the areal ordinate 1 N 1 P is faulty; but I hope ere long to send you the calculation true.

CCLXXII.

NEWTON TO COLLINS.

Cambridge, Novemb. 8, 1676.

Sir,

I doubt you think I have forgot to answer your last letter, and to return you thanks for the pains you took in copying out for me the large letters of those two ingenious persons, M. Leibnitz and M. Tschirnhaus. As for what you propound about the former's calculation, you have well corrected $\frac{8r^5}{\overline{rr+xx}|^3}$[c], by turning it to $\frac{8r^5xx\beta}{\overline{rr+xx}|^3}$ where it signifies an area, but the ordinate NP is rightly $\frac{8r^5xx}{\overline{rr+xx}|^3}$, it being produced by dividing the rectangle 1P1N2N3P. viz. $\frac{8r^5xx\beta}{\overline{rr+xx}|^3}$ by its base β. You seem to desire that I would publish my method, and I look upon your advice as an act of singular friendship, being, I believe, censured by divers for my scattered letters in the Transactions about such things, as nobody else would have let come out, without a substantial discourse. I could wish I could retract what has been done, but by that I have learned what is to my convenience, which is to let what I write lie by till I am out of the way. As for the apprehension that M. Leibnitz's method may be more general, or more easy than mine, you will not find any such thing. His observation, about reducing all roots to

[c] See above, p. 402.

fractions, is a very ingenious one, and certainly his way of extracting adfected roots is beyond it: but in order to series they seem to me laborious enough in comparison of the ways I follow, though for other ends they may be of excellent use. As for the method of transmutations in general, I presume he has made further improvements than others have done, but I dare say, all that can be done by it, may be done better without it, by the simple consideration of the ordinatim applicatæ: not excepting the method of reducing roots to fractions. The advantage of the way I follow you may guess by the conclusions drawn from it, which I have set down in my answer to M. Leibnitz; though I have not said all there. For there is no curve line expressed by any equation of three terms, though the unknown quantities affect one another in it, or the indices of their dignities be surd quantities, (suppose $ax^{\lambda} + bx^{\mu}y^{\sigma} + cy^{\tau} = 0$, where x signifies the base, y the ordinate, λ, μ, σ, τ, the indices of the dignities of x and y, and a, b, c, known quantities with their signs $+$ or $-$,) I say there is no such curve line, but I can, in less than half a quarter of an hour, tell whether it may be squared, or what are the simplest figures it may be compared with, be those figures conic sections or others. And then, by a direct and short way, (I dare say the shortest the nature of the thing admits of, for a general one,) I can compare them. And so, if any two figures expressed by such equations be propounded, I can by the same rule compare them, if they may be compared. This may seem a bold assertion, because it is hard to say a figure may or may not be squared or compà[red] with another, but it is plain to me by the fountai[n I] draw it from, thou[gh] I will not undertake to prove it to others.

The same method extends to equations of four terms, and others also, but not so generally. But I shall say no more at present, but that I am,

your's to serve you,

Is. Newton.

An extract from this letter is published in the Analysis per Quant. Series, but with interpolations.

CCLXXIII.

NEWTON TO OLDENBURG.

Nov. 18, 1676.

Sir,

I promised to send you an answer to Mr. Lucas this next Tuesday, but I find I shall scarce finish what I have designed, so as to get a copy taken of it by that time, and therefore I beg your patience a week longer. I see I have made myself a slave to philosophy, but if I get free of Mr. Linus's business, I will resolutely bid adieu to it eternally, excepting what I do for my private satisfaction, or leave to come out after me; for I see a man must either resolve to put out nothing new, or to become a slave to defend it.

But to let this pass, I beg the favour of you to let your servant convey this, enclosed, to Mr. Boyle, I not knowing well how to direct it to him.

Sir, I am

your humble servant,

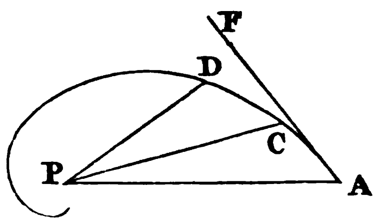
I. N.

CCLXXIV.

COLLINS TO NEWTON.

12 Oct. 1678.

A curve passing through the ends of the rays PA, PC, PD, and which are supposed to be in constant proportion, and make equal angles at the pole P, I call the rhumb spiral; it is desired to draw a



touch-line to the said curve, both by aid of a series and a geometrical approach; and this it is presumed, according to Dr. Wallis in Libro de Cycloide, cannot be done without straightening the curve.

Conversely if one ray, as PA, and the angle the touch line makes therewith, be given FAP, it is desired from these data to describe the spiral.

The rays PC or PD and PA may be conceived to be the tangents of half the complements of the latitudes of two places, numbered with the double arc or whole complement, and the angle DPA to be the difference of longitude, the drawing of a touch line to such spiral gives the angle of the rhumb from one place to the other; a problem of great use in navigation, and no construction or good approach for it, yet in lines not[d] known. Vouchsafe, therefore, to consider this most useful problem, and impart the

[d] This second negative is obviously an oversight. The meaning seems to be that no construction or linear approximation was yet known.

same as an appendage to your Letters to Leibnitz, which we hope you will consent to have printed in English.

CCLXXV.

NEWTON TO BOYLE.

Cambridge, Feb. 28, 1678-9.

Hon[rd] Sir,

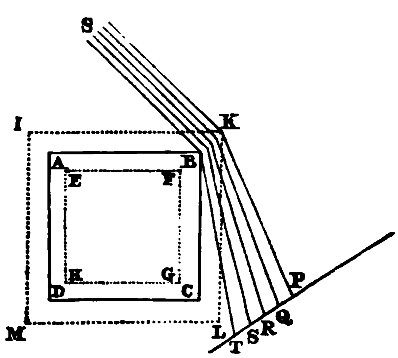
I have so long deferred to send you my thoughts about the physical qualities we spake of, that did I not esteem myself obliged by promise, I think I should be ashamed to send them at all. The truth is, my notions about things of this kind are so indigested, that I am not well satisfied myself in them; and what I am not satisfied in I can scarce esteem fit to be communicated to others, especially in Natural Philosophy, where there is no end of fancying. But because I am indebted to you, and yesterday met with a friend, Mr. Maulyverer, who told me he was going to London, and intended to give you the trouble of a visit, I could not forbear to take the opportunity of conveying this to you by him.

It being only an explication of qualities, which you desire of me, I shall set down my apprehensions in the form of suppositions, as follows. And first, I suppose that there is diffused through all places an ethereal substance capable of contraction and dilatation, strongly elastic, and, in a word, much like air in all respects, but far more subtil.

2. I suppose this ether pervades all gross bodies, but yet so as to stand rarer in their pores than in free spaces, and so much the rarer as their pores are less.

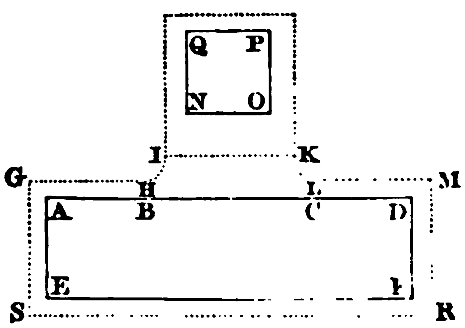
And this I suppose (with others) to be the cause why light incident on those bodies is refracted towards the perpendicular; why two well-polished metals cohere in a receiver exhausted of air: why mercury stands sometimes up to the top of a glass pipe, though much higher than thirty inches: and one of the main causes why the parts of all bodies cohere: also the cause of filtration, and of the rising of water in small glass pipes above the surface of the stagnating water they are dipped into: for I suspect the ether may stand rarer not only in the insensible pores of bodies, but even in the very sensible cavities of those pipes. And the same principle may cause menstruums to pervade with violence the pores of the bodies they dissolve, the surrounding ether, as well as the atmosphere, pressing them together.

3. I suppose the rarer ether within bodies, and the denser without them, not to be terminated in a mathematical superficies, but to grow gradually into one another, the external ether beginning to grow rarer, and the internal to grow denser, at some little distance from the superficies of the body, and running through all intermediate degrees of density in the intermediate spaces. And this may be the cause why light in Grimaldo's experiment, passing by the edge of a knife or other opaque body is turned aside, and, as it were, refracted, and by that refraction makes several colours. Let ABCD be a dense body, whether opaque or transparent, EFGH the outside of the uniform ether, which is



within it, IKLM the inside of the uniform ether which is without it; and conceive the ether which is between EFGH and IKLM to run through all intermediate degrees of density, between that of the two uniform ethers on either side. This being supposed, the rays of the sun, SB, SK, which pass by the edge of this body between B and K, ought in their passage through the unequally dense ether there, to receive a ply from the denser ether, which is on that side towards K, and that the more by how much they pass nearer to the body, and thereby to be scattered through the space PQRST, as by experience they are found to be. Now the space between the limits EFGH and IKLM I shall call the space of the ether's graduated rarity.

4. When two bodies moving towards one another come near together, I suppose the ether between them to grow rarer than before, and the spaces of its graduated rarity to extend further from the superficies of the bodies towards one another, and this by reason that the ether cannot move and play up and down so freely in the strait passage between the bodies as it could before they came so near together. Thus if the space of the ether's graduated rarity reach from the body ABCDFE only to the distance GHLMRS when no other body is near it,



yet may it reach further, as to IK, when another body, NOPQ approaches: and as the other body approaches more and more, I suppose the ether between them will grow rarer and rarer.

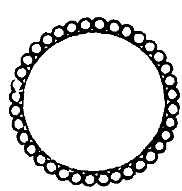
These suppositions I have so described, as if I thought the spaces of graduated ether had precise limits, as is expressed at IKLM in the first figure, and GMRS in the second: for thus I thought I could better express myself. But really I do not think they have such precise limits, but rather decay insensibly, and in so decaying extend to a much greater distance than can easily be believed or need be supposed.

5. Now from the fourth supposition it follows, that when two bodies approaching one another, come so near together as to make the ether between them begin to rarefy, they will begin to have a reluctance from being brought nearer together, and an endeavour to recede from one another: which reluctance and endeavour will increase as they come nearer together, because thereby they cause the interjacent ether to rarefy more and more. But at length when they come so near together that the excess of pressure of the external ether, which surrounds the bodies, above that of the rarefied ether, which is between them, is so great as to overcome the reluctance, which the bodies have from being brought together, then will that excess of pressure drive them with violence together, and make them adhere strongly to one another, as was said in the second supposition. For instance in the second figure, when the bodies ED and NP are so near together, that the spaces of the ether's graduated rarity begin to reach to one another, and meet in the line IK, the ether between them will have suffered much rarefaction, which rarefaction requires much force, that is, much pressing of the bodies together: and the endeavour which the ether between them has to return to its former natural state of condensation, will cause the bodies to have an endeavour of receding from one another. But, on the other

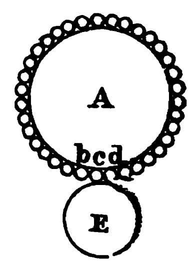
hand, to counterpoise this endeavour there will not yet be any excess of density of the ether, which surrounds the bodies, above that of the ether which is between them at the line IK. But if the bodies come nearer together, so as to make the ether in the midway line IK grow rarer than the surrounding ether, there will arise, from the excess of density of the surrounding ether, a compressure of the bodies towards one another, which when, by the nearer approach of the bodies, it becomes so great as to overcome the aforesaid endeavour the bodies have to recede from one another, they will then go towards one another, and adhere together. And on the contrary, if any power force them asunder to that distance where the endeavour to recede begins to overcome the endeavour to accede, they will again leap from one another. Now hence I conceive it is chiefly that a fly walks on water without wetting her feet, and consequently without touching the water; that two polished pieces of glass are not without pressure brought to contact, no, not though the one be plane, the other a little convex; that the particles of dust cannot by pressing be made to cohere, as they would do, if they did but fully touch; that the particles of tinging substances and salts dissolved in water do not, of their own accord, concrete and fall to the bottom, but diffuse themselves all over the liquor, and expand still more if you add more liquor to them; also that the particles of vapours, exhalations, and air do stand at a distance from one another, and endeavour to recede as far from one another as the pressure of the incumbent atmosphere will let them. For I conceive the confused mass of vapours, air, and exhalations, which we call the atmosphere, to be nothing else but the particles of all sorts of bodies, of which the earth consists, separated from one another, and kept at a distance by the said principle.

From these principles the actions of menstruums upon bodies may be thus explained. Suppose any tinging body, as cochineal or logwood, be put into water; so soon as the water sinks into its pores, and wets on all sides any particle, which adheres to the body only by the principle in [the] second supposition, it takes off, or at least much diminishes, the efficacy of that principle to hold the particle to the body, because it makes the ether on all sides the particle to be of a more uniform density than before. And then the particle, being shaken off by any little motion, floats in the water, and with many such others makes a tincture; which tincture will be of some lively colour, if the particles be all of the same size and density, otherwise of a dirty one. For the colours of all natural bodies whatever seem to depend on nothing but the various sizes and densities of their particles: as I think you have seen described by me more at large in another paper. If the particles be very small, (as are those of salts, vitriols, and gums,) they are transparent, and as they are supposed bigger and bigger, they put on these colours in order, black, white, yellow, red; violet, blue, pale green, yellow, orange, red; purple, blue, green, yellow, orange, red, &c.; as is discerned by the colours, which appear at the several thicknesses of very thin plates of transparent bodies. Whence to know the causes of the changes of colours, which are often made by the mixtures of several liquors, it is to be considered how the particles of any tincture may have their size or density altered by the infusion of another liquor.

When any metal is put into common water, the water cannot enter into its pores to act on it and dissolve it. Not that water consists of too gross parts for this purpose, but because it is unsociable to metal. For there is a certain secret principle in nature, by which liquors are sociable to some things and unsociable to others. Thus water will not mix with oil, but readily with spirit of wine or with salts. It sinks also into wood, which quicksilver will not; but quicksilver sinks into metals, which, as I said, water will not. So aqua fortis dissolves silver not gold, aqua regis gold and not silver, &c. But a liquor, which is of itself unsociable to a body, may, by the mixture of a convenient mediator, be made sociable; so molten lead, which alone will not mix with copper, or with regulus of Mars, by the addition of tin is made to mix with either. And water, by the mediation of saline spirits, will mix with metal. Now when any metal is put in water impregnated with such spirits, as into aqua fortis, aqua regis, spirit of vitriol, or the like, the particles of the spirits, as they, in floating in the water, strike on the metal, will by their sociableness enter into its pores and gather round its outside particles, and by advantage of the continual tremor the particles of the metal are in, hitch themselves in by degrees between those particles and the body, and loosen them from it, and the water entering into the pores together with the saline spirits, the particles of the metal will be thereby still more loosed, so as by that motion the solution puts them into, to be easily shaken off, and made to float in the water: the saline particles still encompassing the metallic ones as a coat or shell does a kernel, after the manner expressed in the annexed figure. In which figure I have made the particles round, though they may be cubical or of any other shape.



If into a solution of metal thus made, be poured a liquor abounding with particles, to which the former saline particles are more sociable than to the particles of the metal, (suppose with particles of salt of tartar:) then so soon as they strike on one another in the liquor, the saline particles will adhere to those more firmly than to the metalline ones, and by degrees be wrought off from those to enclose these. Suppose A a metalline particle enclosed with saline ones of spirit of nitre, and E a particle of salt of tartar contiguous to two of the particles of spirit of nitre, b and c, and suppose the particle E is impelled by any motion toward d, so as to roll about the particle c, till it touch the particle d: the particle b adhering more firmly to E than to A, will be forced off from A. And by the same means the particle E, as it rolls about A, will tear off the rest of the saline particles from A, one after another, till it has got them all, or almost all, about itself. And when the metallic particles are thus divested of the nitrous ones, which as a mediator between them and the water held them floating in it, the alcalizate ones crowding for the room the metallic ones took up before, will press these towards one another, and make them come more easily together: so that by the motion they continually have in the water, they shall be made to strike on one another, and then, by means of the principle in the second supposition, they will cohere and grow into clusters, and fall down by their weight to the bottom, which is called precipitation.

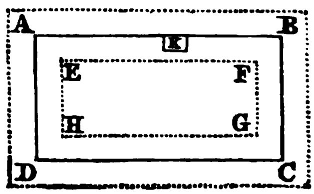


In the solution of metals, when a particle is loosing from the body, so soon as it gets to that distance from it where the principle of receding, described in the fourth and fifth suppositions, begins to overcome the principle of acceding described in the second supposition, the receding of the particle will be thereby accelerated, so that the particle shall, as [it] were, with violence leap from the body, and, putting the liquor into a brisk agitation, beget and promote that heat we often find to be caused in solutions of metals. And if any particle happen to leap off thus from the body, before it be surrounded with water, or to leap off with that smartness as to get loose from the water, the water, by the principle in the fourth and fifth suppositions, will be kept off from the particle, and stand round about it like a spherically hollow arch, not being able to come to a full contact with it any more; and several of these particles, afterwards gathering into a cluster, so as by the same principle to stand at a distance from one another, without any water between them, will compose a bubble. Whence I suppose it is that in brisk solutions there usually happens an ebullition.

This is one way of transmuting gross compact substances into aereal ones. Another way is by heat. For as fast as the motion of heat can shake off the particles of water from the surface of it, those particles, by the said principle, will float up and down in the air at a distance both from one another and from the particles of air, and make that substance we call vapour. Thus I suppose it is when the particles of a body are very small, (as I suppose those of water are,) so that the action of heat alone may be sufficient to shake them asunder. But if the particles be much larger, they then require the greater force of dissolving menstruums to separate them, unless by any means the particles can be first broken into smaller ones.

For the most fixed bodies, even gold itself, some have said, will become volatile, only by breaking their parts smaller. Thus may the volatility and fixedness of bodies depend on the different sizes of their parts.

And on the same difference of size may depend the more or less permanency of aereal substances in their state of rarefaction. To understand this, let us suppose ABCD to be a large piece of any metal, EFGH the limit of the interior uniform ether, and K a part of the metal at the superficies AB. If this part or particle K be so little that it reaches not to the limit EF, it is plain that the ether at its centre must be less rare than if the particle were greater, for were it greater, its centre would be further from the superficies AB, that is, in a place where the ether (by supposition) is rarer. The less the particle K, therefore, the denser the ether at its centre, because its centre comes nearer to the edge AB, where the ether is denser than within the limit EFGH. And if the particle were divided from the body, and removed to a distance from it, where the ether is still denser, the ether within it must proportionally grow denser. If you consider this, you may apprehend how, by diminishing the particle, the rarity of the ether within it will be diminished, till between the density of the ether without and the density of the ether within it there be little difference, that is, till the cause be almost taken away, which should keep this and other such particles at a distance from one another. For that cause, explained in the fourth and fifth suppositions, was the excess of density of the external ether above that of the internal. This may be the reason, then, why the small particles of vapours



easily come together, and are reduced back into water, unless the heat, which keeps them in agitation, be so great as to dissipate them as fast as they come together, but the grosser particles of exhalations raised by fermentation keep their aereal form more obstinately, because the ether within them is rarer.

Nor does the size only, but the density of the particles also, conduce to the permanency of aereal substances. For the excess of density of the ether, without such particles, above that of the ether within them, is still greater. Which has made me sometimes think that the true permanent air may be of a metallic original: the particles of no substances being more dense than those of metals. This I think is also favoured by experience; for I remember I once read in the Philosophical Transactions how M. Huygens, at Paris, found that the air made by dissolving salt of tartar would in two or three days' time condense and fall down again, but the air made by dissolving a metal continued without condensing or relenting in the least. If you consider then how by the continual fermentations made in the bowels of the earth there are aereal substances raised out of all kinds of bodies, all which together make the atmosphere, and that of all these the metallic are the most permanent, you will not perhaps think it absurd that the most permanent part of the atmosphere, which is the true air, should be constituted of these: especially since they are the heaviest of all others, and so must subside to the lower parts of the atmosphere, and float upon the surface of the earth, and buoy up the lighter exhalations and vapours to float in greatest plenty above them. Thus I say it ought to be with the metallic exhalations raised in the bowels of the earth by the action of acid menstruums, and thus it is with the true permanent air. For this, as in reason it ought to be esteemed the most ponderous part of the atmosphere, because the lowest, so it betrays its ponderosity by making vapours ascend readily in it, by sustaining mists and clouds of snow, and by buoying up gross and ponderous smoke. The air also is the most gross inactive part of the atmosphere, affording living things no nourishment, if deprived of the more tender exhalations and spirits that float in it: and what more inactive and remote from nourishment than metallic bodies?

I shall set down one conjecture more, which came into my mind now, as I was writing this letter. It is about the cause of gravity. For this end I will suppose ether to consist of parts differing from one another in subtilty by indefinite degrees. That in the pores of bodies there is less of the grosser ether in proportion to the finer than in open spaces, and consequently that in the great body of the earth there is much less of the grosser ether, in proportion to the finer, than in the regions of the air: and that yet the grosser ether in the air affects the upper regions of the earth, and the finer ether in the earth the lower regions of the air, in such a manner that from the top of the air to the surface of the earth, and again from the surface of the earth to the centre thereof, the ether is insensibly finer and finer. Imagine now any body suspended in the air or lying on the earth: and the ether being by the hypothesis grosser in the pores, which are in the upper parts of the body, than in those, which are in its lower parts, and that grosser ether being less apt to be lodged in those pores than the finer ether below, it will endeavour to get out and give way to the finer ether below, which cannot be without the body's descending to make room above for it to go out into.

From this supposed gradual subtilty of the parts of the ether, some things above might be further illustrated and made more intelligible, but by what has been said you will easily discern whether in these conjectures there is any degree of probability, which is all I aim at. For my own part, I have so little fancy to things of this nature, that had not your encouragement moved me to it, I should never, I think, have thus far set pen to paper about them. What is amiss, therefore, I hope you will the more easily pardon in

your most humble servant and honourer,

Is. NEWTON.

CCLXXVI.

NEWTON TO HALLEY.

Cambridge, March 14, 1696.

Sir,

I understand that a report has been some time spreading among the Fellows of the Royal Society, as if I was about the longitude at sea. For putting a stop to that report, pray do me the favour to acquaint them, (as you have occasion,) that I am not about it. And if the rumour of preferment for me in the Mint should hereafter, upon the death of Mr. Hoar, or any other occasion, be revived, I pray that you would endeavour to obviate it by acquainting your friends that I neither put in for any place in the Mint nor would meddle with Mr. Hoar's place, were it offered me. You will thereby oblige

your most humble and

most obedient servant,

Is. NEWTON.

CCLXXVII.

NEWTON TO HALLEY.

Sir, London, Feb. 11, 1696–7.

This morning Colonel Blunt, the King's first Engineer, was with me, and acquainted me with a design the King has to allow ten shillings per diem for two masters to teach Engineering (I mean the mathematical grounds of it) two hours each day, to those of the army who will come to hear them publicly, Engineers, and Officers, and others, who shall have the curiosity and capacity. I proposed you as a fit person to be one of the two, if you should think fit to accept of the thing. By bringing you acquainted with the Officers and making you known to the King, it may be a means of making way for something better. The Colonel will call on me seven or eight days hence for your answer. I am

your faithful friend to serve you,

Is. Newton.

I wrote to you the last post for an Engineer's place. I question you can have both.

CCLXXVIII.

NEWTON TO THE DUKE D'AUMONT.

Ordered May 27, 1714.

May it please your Grace,

The letter you were pleased to honour the Royal

Society with, came so late to their hands, that I could not sooner return you their thanks for the great humanity and civility, wherewith you have treated them. Your Grace's letter was read in a full meeting of the Society, to the great satisfaction and pleasure of all the members present.

Whenever any thing comes to their knowledge, which they may think acceptable to your Grace, they will take care to communicate it; and in the mean time desired me to signify to your Grace, how exceedingly you have obliged them.

I am
your Grace's most humble
and most obedient servant,
ISAAC NEWTON.

CCLXXIX.

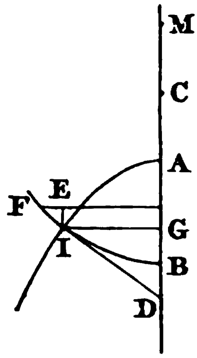
KEILL TO NEWTON.*

Honoured Sir,

I returned but last week to Oxford, but whilst I was in Northamptonshire, I began to think on M. Leibnitz's problem, and I think I have hit upon the solution of it, it not being difficult to any that understand fluxions. The method I used was this.

Suppose a series of curves of the same nature, which have all the same axis AB, and vertex A. Let AI be one of them, and BI the curve, which cuts it

* The post mark on this letter is Feb. 24. The subject shews that it was written in 1715.



at right angles. Because the curve AI is given, its subnormal GD will be given; but this subnormal is the subtangent of the curve BI, therefore FE : EI : : IG : GD, that is (calling AG x and IG y) $\dot{y} : -\dot{x} : : y : \text{GD}$; here $\dot{x}$ must be negative, for when y increases x decreases. By this analogy I get an equation, and from the equation I obtain a value of the latus rectum. This value of the latus rectum I put in its room in the equation, which expresses the nature of the curve proposed, and I have a new equation expressed by x, y, and their fluxions, which gives the nature of the curve required. For example, suppose the curve AI a parabola, whose equation is $2lx = yy$, then $\text{GD} = l$, and

$\dot{y} : -\dot{x} : : y : \frac{-\dot{x}y}{\dot{y}} = l$; this value of l being put in

the equation for the parabola gives $\frac{-2x\dot{x}y}{\dot{y}} = yy$ and

$-2x\dot{x} = y\dot{y}$, hence $a^2 - x^2 = \frac{1}{2}y^2$, or $2a^2 - 2x^2 = y^2$. Hence the curve BI is an ellipse, whose greater axis is double in power to its lesser.

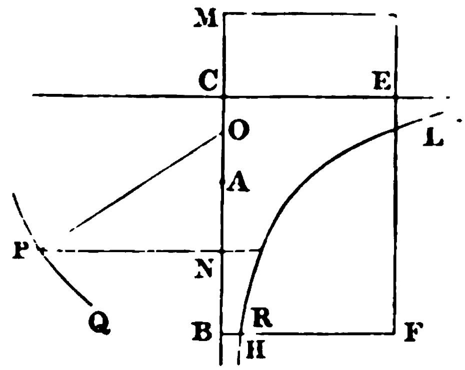
If the curves proposed are hyperbolas, whose centre is C, transverse axis $\text{AM} = 2a$, their equation is

$\frac{2lax + lx^2}{2a} = y^2$, and the subnormal is $\frac{la + lx}{2a}$, and $\dot{y}$:

$-\dot{x} : : y : \frac{la + lx}{2a}$; hence $l = \frac{-2ay\dot{x}}{\overline{a + x}.\dot{y}}$, which value of l

being put in the equation of the hyperbola gives

$\frac{-4a^2yx\dot{x} - 2ayx^2\dot{x}}{2a \times \overline{a + x} \times \dot{y}} = y^2$, and $\frac{-x^2\dot{x} - 2ax\dot{x}}{x + a} = y\dot{y}$, that is $-x\dot{x}-a\dot{x}+\frac{a^2\dot{x}}{a+x}=y\dot{y}$, or $\frac{2a^2\dot{x}}{a+x}=2x\dot{x}+2a\dot{x}+2y\dot{y}$, and taking the fluents, we have fluent $\frac{2a^2\dot{x}}{a+x}$ + a given quantity $=x^2+2ax+y^2$.[f] From which we draw the following construction. At the centre C, [with the] asymptotes C A, C E, describe the hyperbola L G H, whose rectangle inscribed is $=2a^2$; make BF perpendicular to BA, and equal to it, and make up the rectangle MF; in AB take any point N, and let NO be a mean proportional [between] NA and NM; at the centre O with the



[f] $AC=CM=a$, $AN=x$,

$2a^2=CN\times NG=\overline{a+x}\times NG, \therefore NG=\frac{2a^2}{a+x}$.

Fluxion of space BNGR is $\frac{-2a^2\dot{x}}{a+x}$,

$BA=b=BF$, and $MF=\overline{2a+b}\times b=2ab+b^2$,

$NO=\sqrt{NA\times NM}=\sqrt{x\times\overline{2a+x}}=\sqrt{2ax+x^2}$,

$OP^2=MF-BNGR=2ab+b^2+\int\frac{2a^2\dot{x}}{a+x}$,

$ON^2=2ax+x^2$,

$PN=y$,

$PN^2=OP^2-ON^2$,

$y^2=2ab+b^2+\int\frac{2a^2\dot{x}}{a+x}-2ax-x^2$

$2y\dot{y}=\frac{2a^2\dot{x}}{a+x}-2a\dot{x}-2x\dot{x}$, whence

$2x\dot{x}+2a\dot{x}+2y\dot{y}=\frac{2a^2\dot{x}}{a+x}$, which is Keill's fluxional equation.

radius OP, whose square is equal to the rectangle MF minus the hyperbolic space BNGR, describe the arch PQ, and let NG produced meet with it in P, the point P will be in the curve required.

The curve will be an oval figure whose greatest ordinate is at A.

Mr. Stirling, an undergraduate here, has likewise solved this problem. I have also received the solution of this and several other problems from Mr. Pemberton. If you please you may put mine in the Transactions.

I am, Sir,

your much obliged humble servant.

JOHN KEILL.

CCLXXX.

NEWTON TO THE COMMISSIONERS OF THE TREASURY.[f]

May it please your Lordships, Mint Office, 21 Sept. 1717.

In obedience to your Lordships' order of reference of Aug. 12th, that I should lay before your Lordships a state of the gold and silver coins of this kingdom in weight and fineness, and the value of gold in proportion to silver, with my observations and opinion, and what method may be best for preventing the melting down of the silver coin, I humbly represent that a pound weight of gold, 11oz. fine, and 1oz. alloy, is cut into 44½ guineas, and 1lb. weight of silver, 11oz. 2dwts. fine, and 18dwts. alloy, is cut into 62 shillings, and according to this rate, 1lb. weight of fine gold is

[f] The manuscript is not in Newton's hand, but it is paged with figures different from the body of the writing, and which resemble his.

worth 15lb. 6oz. 17dwts. 5grs. of fine silver, reckoning a guinea at 1*l*. 1*s*. 6*d*. in silver money; but silver in bullion exportable is usually worth 2*d*. or 3*d*. per oz. more than in coin, and, if at a medium such bullion of standard alloy be valued at 5*s*. 4½*d*. per oz., 1 lb. weight of fine gold will be worth but 14lb. 11oz. 12dwts. 9grs. of fine silver, in bullion, and at this rate a guinea is worth but 20*s*. 8*d*. When ships are lading for the East Indies, the demand of silver for exportation raises the price to 5*s*. 6*d*. or 5*s*. 8*d*. per oz. or above. But I consider not these extraordinary cases.

A Spanish pistole was coined for 32 reas, or 4 pieces of 8 reas each, usually called pieces of eight, is of equal alloy, and the 16th part of the weight thereof.

A doppe moida of Portugal was coined for 10 crusadoes of silver, and is of equal alloy, and the 16th part of the weight thereof. Gold is therefore in Spain and Portugal of 16 times more value than silver of equal weight and alloy, according to the standard of those kingdoms, at which rate a guinea is worth 22*s*. 1*d*. But this high price keeps their gold at home in good plenty, and carries away the Spanish silver into all Europe, so that at home they make their payments in gold, but will not pay in silver without a premium. Upon the coming in of a plate fleet, the premium ceases, or is but small, but as their silver goes away and becomes scarce, the premium increases, and is most commonly about 6 per cent., which being abated, a guinea becomes worth about 20*s*. 9*d*. in Spain and Portugal.

In France, a pound weight of fine gold is reckoned worth 15lb. weight of fine silver. In raising and falling the money, their King's edicts have sometimes varied a little; but I do not here consider them. By the edict of May, 1709, a new pistole was coined for 4 new louis, and is of equal alloy, and the 15th part of the weight thereof, (except the errors of their mint,) and by the same edict fine gold is valued at 15 times its weight of fine silver, and at this rate a guinea is worth 20*s.* 8½*d.* I consider not here the confusion made in the moneys in France by frequent edicts to send them to the mint, and give the King a tax out of them; I consider only the value of gold and silver in proportion to one another.

The ducats of Holland, and Hungary, and the Empire, were lately current in Holland among the common people, in their markets and ordinary affairs, at 5 guilders in specie and 5 stivers, and commonly changed for so much silver money in three-guilder pieces, as guineas are with us for 21*s.* 6*d.* sterling; at which rate a guinea is worth 20*s.* 7½*d.*

According to the rates of gold to silver in Italy, Germany, Poland, Denmark and Sweden, a guinea is worth about 20*s.* and 7, 6, 5, or 4 pence; for the proportion varies a little within the several governments in those countries. In Sweden gold is lowest in proportion to silver, and this hath made that kingdom (which formerly was content with copper money) abound of late with silver, sent thither (I suspect) for naval stores.

In the end of King William's reign, and the first year of the late Queen, when foreign coins abounded in England, I caused a great many of them to be assayed in the mint, and found by the alloys, that fine gold was to fine silver in Spain, Portugal, France, Holland, Italy, Germany, and the northern kingdoms, in the proportion above mentioned, errors of the mint excepted.

In China and Japan, one pound weight of fine gold is worth 9 or 10 pounds weight of fine silver, and in

East India it may be worth 12. And this low price of gold in proportion to silver carries away the silver from all Europe.

So then by the course of trade and exchange between nation and nation in all Europe, fine gold is to fine silver as 14⅘ or 15 to 1; and a guinea at the same rate is worth between 20s. 5d. and 20s. 8½d., except in extraordinary cases, as when a plate fleet is just arrived in Spain, or ships are lading here for the East Indies, which cases I do not here consider. And it appears by experience as well as by reason, that silver flows from those places where its value is lowest in proportion to gold, as from Spain to all Europe, and from all Europe to East India, China, and Japan, and that gold is not plentiful in those places, in which its value is highest in proportion to silver, as in Spain and England.

It is the demand for exportation, which hath raised the price of exportable silver about 2d. or 3d. in the ounce above that of silver in coin, and hath thereby created a temptation to export or melt down the silver coin, rather than give 2d. or 3d. more for foreign silver: and the demand for exportation arises from the higher price of silver in other places than in England in proportion to gold, that is from the higher price of gold in England than in other places in proportion to silver, and therefore may be diminished by lowering the value of gold in proportion to silver. If gold in England, or silver in East India, could be brought down so low as to bear the same proportion to one another in both places, there would be no greater demand for silver than for gold to be exported to India. And if gold were lowered only so as to have the same proportion to silver money in England, which it hath to silver in the rest of Europe, there would be no temptation to export silver rather than gold to any other part of Europe. And to compass this last, there seems nothing more requisite than to take off 10*d.* or 12*d.* from the guinea, so that gold may bear the same proportion to the silver money in England, which it ought to do by the course of trade and exchange in Europe. But if only 6*d.* were taken off at present, it would diminish the temptation to export or melt down the silver coin, and by the effects would shew hereafter, better than can appear at present, what further reduction would be most convenient for the public.

In the last year of King William, the dollars of Scotland, worth about 4 shillings and 6 pence half-penny, were put away in the north of England for 5 shillings; and at this price began to flow in upon us. I gave notice thereof to [the] Lords Commissioners of the Treasury, and they ordered the collectors of taxes to forbear taking them, and thereby put a stop to the mischief.

At the same time the louis-d'ors of France, which were worth 17*s.* 0¾*d.* a-piece passed in England at 17*s.* 6*d.*

I gave notice thereof to the Lords Commissioners of the Treasury, and his late Majesty put out a proclamation that they should go but at 17*s.*; and thereupon they came to the mint, and 1,400,000*l.* were coined out of them. And if the advantage of 5¼*d.* in a louis-d'or sufficed at that time to bring into England so great a quantity of French money, and the advantage of 3 farthings in a louis-d'or to bring it to the mint, the advantage of 9½*d.* in a guinea, or above, may have been sufficient to bring in the great quantity of gold, which hath been coined in the last fifteen years, without any foreign silver.

Some years ago the Portugal moidores were received in the west of England at 28*s.* a-piece; upon notice from the mint that they were worth only

27*s*. 7*d*., the Lords Commissioners of the Treasury ordered their receivers of taxes to take them at no more than 27*s*. 6*d*. Afterwards many gentlemen in the west sent up to the Treasury a petition that the receivers might take them again at 28*s*. and promised to get return for this money at that rate, alleging that when they went at 28*s*. their country was full of gold, which they wanted very much; but the Commissioners of the Treasury, considering that at 28*s*. the nation would lose 5*d*. a-piece, rejected the petition. And if an advantage to the merchants of 5*d*. in 28*s*. did pour that money in upon us, much more hath an advantage to the merchant of 9½*d*. in a guinea, or above, been able to bring into the mint great quantities of gold without any foreign silver, and may be liable to do it still, till the cause be removed.

If things be let alone till silver money be a little scarce, the gold will fall of itself. For people are already backward to give silver for gold, and will, in a little time, refuse to make payment in silver without a premium, as they do in Spain, and this premium will be an abatement in the value of the gold. And so the question is, whether gold shall be lowered by the government, or let it alone till it falls of itself for the want of silver money.

It may be said that there are great quantities of silver in plate, and if the plate were coined there would be no want of silver money. But I reckon silver is safer from exportation in the form of plate, than the form of money, because of the greater value of silver and fashion together; and therefore I am not for coining the plate, till the temptation to export the silver money (which is a profit of 2*d*. or 3*d*. an ounce) be diminished; for as often as men are necessitated to send away money for answering debts abroad, there will be a temptation to send away silver rather than gold, because of the profit, which is almost 4 per cent.; and for the same reason foreigners will choose to send hither their gold rather than their silver.

All which is most humbly submitted to
your Lordships' great wisdom.

Isaac Newton.

Observations upon the state of the coins of gold and silver,
Dated Oct. 22, 1718.

Obs. 1. Standard gold, before 6*d.* was taken from the guinea, was worth 3*l.* 19*s.* 9¼*d.* per ounce at the mint, and by taking 6*d.* from the guinea, became worth 3*l.* 17*s.* 11*d.* per ounce; and standard silver is there worth 5*s.* 2*d.* per ounce; but the demand for exportation hath raised both species above the price at the mint, and thereby hath carried out all the foreign silver for many years, and began to carry out some of the foreign gold the last November, and therefore raised the price of gold for exportation, above the mint price, before the 6*d.* was taken from the guinea. But it never raised it to above 4*l.* 0*s.* 6*d.* per ounce, nor kept it long at that price; for in March last, foreign gold fell down to 3*l.* 19*s.* 6*d.*, and in April to 3*l.* 19*s.*, and in May to 3*l.* 18*s.* 6*d.*, all which was below the old mint price; and therefore the price of foreign gold for exportation was raised the last winter by some other cause than the taking 6*d.* from the guinea, and the price of gold for exportation to foreign markets having been, ever since March, below the old mint price, the price was certainly too high.

Obs. 2. The price of gold for exportation depends upon our debts abroad, and answers to the course of exchange. When the exchange is lowest, the price of foreign gold is highest, and on the contrary: and thence the coinage of gold hath of late years been greater or less, accordingly as the course of exchange hath been lower or higher. In the year 1713, the course of exchange and the coinage grew high together; in the years 1714 and 1715 the exchange was highest, it being (for instance) with Amsterdam, from 36*s*. to 37*s*. and then the coinage was greatest; in the year 1716, the exchange was only from 35*s*. to 36*s*. with Amsterdam, and proportionably with other places, and the coinage abated accordingly; in the year 1717 the exchange with Amsterdam was only 34*s*. to 35*s*. 2*d*. and the coinage abated to one half of what it had been two or three years before; and in this present year the exchange hath been only 33*s*. 10*d*. to 34*s*. 10*d*. till within this fortnight, and this low course of exchange, together with the discouragement of the coinage of gold, by the taking 6*d*. from the guinea, hath carried out almost all the gold imported, and thereby hath had the same good effect for paying our debts abroad in gold and preserving our silver, which the bill proposed last session of parliament would have had, if it had passed into an act for stopping the coinage of gold. Whence those debts arose is difficult to understand, without more skill in trade than I can pretend to; but considering that a good part of the gold imported in the years 1713, 14, 15, and 16, was in French money and ducats, I suspect that after the war with France was at an end, great quantities of gold were sent hither to pay for stocks, until the interest of stocks was lowered by act of parliament; and since that discouragement some foreigners have been drawing their moneys back with the interest of their stocks, and some gold, this year, hath been sent to the mint in France, and some merchants are newly broke.

Obs. 3. The course of exchange was low in November last, before the 6*d*. was taken from the guinea, as [it] was afterwards in February last, and both times was at the lowest (being with Amsterdam at 33*s*. 10*d*.); and therefore the lowness of the exchange, last winter, arose not from the taking 6*d*. from the guinea, but from the debts we had abroad before the 6*d*. was taken off, which debts, if the coinage of gold had not been discouraged by taking 6*d*. from the guinea, might have remained unpaid, until they could have been paid in silver with more advantage to private persons.

Obs. 4. By the payment of our debts abroad in gold, the demand for exportation hath abated ever since February last, and the exchange hath risen gradually to 35*s*. and gold hath fallen down gradually to the mint price, and hath begun to come to the mint again, so that within a fortnight so much gold hath come to the mint as will make above 75,000*l*. Whence I gather, that whenever the exchange with Amsterdam is above 35*s*., it will bring gold to the mint, and would have brought gold to the mint in the years 1713, 14, 15, and 16, and part of 1717, although the 6*d*. had been taken off before, the exchange in all these years being, with Amsterdam, above 35*s*., and for the most part above 36*s*., and therefore in all the gold then coined, which was above 5 millions, the nation would have saved 6*d*. per guinea, had the 6*d*. been taken off before.

Obs. 5. The demand for exportation hath, ever since the 6*d*. was taken from the guinea, raised the price of silver about 3 times more than the price of gold, and sometimes 4 or 5 times more, or above; and therefore the temptation to export gold moneys hath all this year been 3 times less than the temptation to export silver moneys. And if this temptation hath not this year sensibly diminished the quantity of our silver moneys, it hath much less carried out our gold moneys. And all, or almost all, the gold which hath been exported this year hath been in foreign bullion, and by consequence the nation hath lost little or nothing by the exportation, because the bullion, being foreign, went out at the same price it came in at. Foreigners, or their agents, who receive guineas in payments, will lose 3*d*. per guinea exporting them, besides the danger they run by breaking the law. There hath been above 110,000*l*. imported in gold to be coined since Christmas, and the 6*d*. per guinea saved in all this coinage will recompense abundantly the loss of 6*d*. per guinea in all the guineas exported by foreigners; and therefore there is nothing in the objection, that in making payments to foreigners in guineas we lose 6*d*. per guinea, for we get the 6*d*. again in receiving back all the guineas, which they do not export.

Obs. 6. Since the demand for silver for exportation hath all this year been three times greater than that of gold, no gold would have been exported this year, had it not been for the want of foreign silver; and the exportation thereof hath prevented the exportation of the same value of foreign silver, as fast as it could have been procured, for paying debts abroad, and in the mean time hath saved the interest of the debts paid off.

Obs. 7. And this exportation hath been a further advantage to the nation by raising the course of exchange from 33*s*. 10*d*. to 35*s*.; for when the exchange is low, the nation loseth by it so much as it is under par: and if the debts, which have been paid in gold, had continued till they could have been paid in silver, they would have caused the exchange to continue low.

Obs. 8. And to restore the 6*d*. to the guinea would be to lose these advantages, and to give more by above 9*d.* in the guinea for all the gold, which shall be imported hereafter, than it is worth in foreign markets, and to receive the corrupt trade of exporting silver to buy gold abroad, and importing gold to buy silver at home.

An account of the gold and silver moneys coined yearly, from Christmas 1699 *to Christmas* 1717, *by weight.*

	Gold.				Silver.			
	lb.	oz.	dwt.	gr.	lb.	oz.	dwt.	gr.
1700	2701	4	14	11	4805	10	6	16
1701	26742	0	0	0	37477	0	0	0
1702	3642	0	0	0	114	6	0	0
1703	34	2	0	0	718	6	0	0
1704	0	0	0	0	4007	0	0	0
1705	104	0	0	0	429	7	15	0
1706	537	0	0	0	932	0	0	0
1707	607	0	0	0	1174	0	0	0
1708	1010	0	0	0	3751	0	0	0
1709	2468	0	0	0	25423	0	0	0
1710	3716	0	0	0	817	0	0	0
1711	9324	0	0	0	24768	0	0	0
1712	2855	0	0	0	1784	6	0	0
1713	13137	0	0	0	2333	0	0	0
1714	29526	0	0	0	1566	0	0	0
1715	39090	0	0	0	1643	0	0	0
1716	23765	0	0	0	1650	0	0	0
1717	15186	0	0	0	948	0	0	0
	174444	6	14	11	114341	6	1	16

The whole weight of the gold money coined in these eighteen years, after the rate of 44½ guineas to the pound weight troy, amounts unto 7,762,783 guineas. And the weight of all the silver moneys, after the rate of 3*l.* 2*s.* to the pound weight troy, amounts unto 354,458*l.* 18*s.* 5*d.*

CCLXXXI.

NEWTON TO HALLEY.

St. Martin's Lane by Leicester Fields,
Dr. Halley, Dec. 3, 1724.

I received from you formerly a table of the motions of the comet of 1680 in an elliptic orb[h]. You there put

The node ascendent in ♑ 2° 2′;

The node descendent ♋ 2° 2′;

The inclination of the plane of the orb to the plane of the ecliptic 61° 6′ 48″;

The perihelium of the comet in this plane ♐ 22° 44′ 25″;

The equated time of the perihelium, December, 7d. 23h. 9m.;

The distance of the perihelium from the ascending node in the plane of the ecliptic 9° 17′ 35″;

The axis transversus 138.29571, and the axis conjugatus 1.84812, the mean distance of the earth from the sun being 1.00000.

And in this orb you computed the places of the comet on November 3d. 16h. 47m. Nov. 5d. 15h. 37m., and Nov. 10d. 16h. 18m., as follows:

1680, Tempus verum.	Long. comput.	Lat. comput.
D. H. M.	° ′ ″	° ′ ″
Nov. 3 16 47	♌ 29 51 22	1 17 32 Bor.
5 15 37	♍ 3 24 32	1 6 9
10 16 18	15 33 2	0 25 7

The first of these three places you have inserted into the table of the motions of this comet in an elliptic orb,

[h] See the Astronomiæ Cometicæ Synopsis, as printed with Halley's Astronomical Tables.

which you have printed in your Astronomical Tables, where you treat De Motu Cometarum in orbibus ellipticis. I beg the favour of you to reexamine the two last of them, viz. those on Nov. 5d. 15h. 37m., and Nov. 10d. 16h. 18m.

In the same printed table, you have calculated the place of this comet upon March 9d. 8h. 38m., true time. I beg the favour of you to calculate its place in the parabolic orb also upon March 9d. 8h. 38m., true time, and send me its computed longitude, latitude, and distance from the sun. For I would add them to the table of the motion of this comet in a parabolic orb printed in the third book of the Principia Mathematica, pag. 459. ed. ii. By its distance from the sun, I mean the distance of its centre from the centre of the sun, in parts, whereof the radius of the orbis magnus is 1.00000. I am

your most humble servant,

ISAAC NEWTON.

CCLXXXII.

NEWTON TO ——.

Literas tuas amicissimas accepi, 26 Julii datas, et gratias reddo tibi maximas, quod exemplaria duo Optices ad D. Johannem Bernoulli meo nomine misisti, et eo pacto nos reconciliare conatus fueris; quod et fecisti, ut ex literis ejus intelligo. Nam D. Leibnitius epistolis aliquot, quas vidi, disertis verbis affirmaverat D. Bernoullium autorem esse Epistolæ ad ipsum 7 Junii 1713 datæ, et mox in Germania impressæ et per orbem literarium sparsæ. Sed cum ex literis D. Bernoulli jam acceptis intelligam ipsum non fuisse autorem, amicitiam ejus lubenter amplector et colo. Et eo nomine literas inclusas ad ipsum scripsi, quas oro ut ubi occasionem nactus fueris ad ipsum mittas. Oro etiam ut Academiæ vestræ gratias meas reddas ob munera Historiæ suæ annuatim ad me missæ, te curante. Sed et gratiæ meæ tibi ipsi debentur ob Ephemerides ad me missas. Antequam literis tuis responderem, cupiebam colloqui cum D^no^ Keil qui aberat in agro Northamptoniensi. Sed is jam in urbem hanc rediit, et quantum sentio, a litibus in posterum abstinebit.

Inspiciendo literas, quas D^nus^ Cotes, interea dum curaret editionem secundam libri mei Principiorum, ad me scripsit, observo quod schedæ primæ triginta septem, (id est usque ad paginam 296 inclusivæ,) impressæ fuerunt ante 30 Junii 1710 st. vet., id est antequam hæ lites cœperunt. Sed monente tandem D. Nicolao Bernoulli quod error aliquis admissus fuisset in Prop. x. lib. III., constructionem propositionis correxi, et correctam ei ostendi, et imprimi curavi, non subdole, sed eo cognoscente. Cætera in lucem prodierunt uti fuerant ante has lites impressa.

Per experimenta et mea, et ea D^ni^ Mariotti, (pag. 245. Traité du Mouvement des eaux,) et alia a D. Cotes facta et mecum communicata, constat, ex quantitate aquæ per datum foramen in fundo vasis, dato tempore, effluentis, quod velocitas ejus in foramine ea sit, quam corpus cadendo a dimidia altitudine aquæ in vase stagnantis acquirere potest. Sed aqua post exitum acceleratur, uti constat per alia experimenta.

Præter verba quæ ex libro Principiorum citasti, extant alia in scholio ad Prop. LXIX. lib. I, quibus clarissime constat me gravitatem corporibus essentialem minime fecisse. Sed spero quod contentiones hæ omnes in posterum cessabunt.

STRODE'S[1] CORRESPONDENCE.

CCLXXXIII.

STRODE TO COLLINS.

Maperton, near Wincanton,
Somersetshire, July 11, 1672.

Mr. Collins,

My ignorance in the choicest authors of astronomy hath begot you this trouble. Upon my reading and considering Mons^r Bullialdus I thought I had found a proposition worthy your and others' knowledge, and when I found my mistake I was resolved to have cried Peccavi, and to have said no more; but on the desire of the Dr. Beal, to whom I am very much obliged, I could not but give it him: but seeing it hath procured me so considerable of such matter from you, it no ways troubles me. Upon my perusing Bullialdus, in my paper book I inserted this; that whereas Bullialdus would have the middle motion of the planet to be accounted on the axis of the cone, I conceived that it could not be, for that the circle

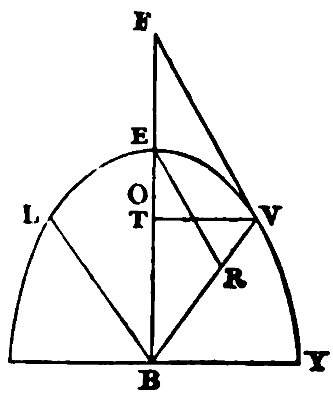
[1] Thomas Strode was born at Shepton Mallet in Somersetshire. He entered as a commoner of University College in 1642, and after travelling for a time in France, settled at Maperston. He published

"A short Treatise of the Combinations, Elections, Permutations, and Composition of Quantities, &c. London, 1678."

"A new Speculation of the difference of the Power of Numbers." Printed with the former.

"A new and easy Method to the Art of Dialling: 1. Containing all horizontal dials, all upright dials, reflecting dials, &c. 2. The most natural and easy way of describing the curves of the sun's declination on any place, &c. London, 1688."—Wood's Athenæ Oxon., iv. 448; but there is a query attached to the dates of both publications.

of the cone and the ellipse being in two several planes, intersecting each other in the centre of the ellipse, there would rise an inequality, from the angle of the middle motion made in the circle, to that in the ellipse, which by his manner of calculation is none; and therefore I suppose we must rather imagine the middle motion of the planet to be about the poles of the ellipse, and yet on the same plane to be accounted by equal angles. On this account, I suppose, it is that Mr. Streete corrects the anomaly. And for that which you say would be the greatest improvement of the conics, which you have not yet leisure to try, give me leave to insert these few problems, although I must confess I, being unacquainted with Slusius, do not apprehend what use you can make of that proposition, or whether one of these is it, or whether these may help you to the finding it.



Let $x =$ BE (6) semi-axis transver.

$s =$ BY (3) semi-axis conjug.

$b =$ BV ($\sqrt{21}$) semi-diam. transver.

$c =$ BL ($2\sqrt{6}$) semi-diam. conjug.

$p =$ BO ($3\sqrt{3}$) dist. poli a centro,

$r =$ Radius (1),

$\sigma =$ sine $\angle$ TBV ($\sqrt{\frac{5}{21}}$), $\pi =$ sine $\angle$ BRE ($9\sqrt{\frac{1}{126}}$),

BT $= 4$, BF $= 9$, FV $= \sqrt{30}$, tangent,

BR $= \frac{2}{3}\sqrt{21}$, ER $= \frac{2}{3}\sqrt{30}$, ordinate.

The two first problems are to find the angles VBE and BRE.

First Problem.

x, s, b, r given, quære σ, n.

$b\sqrt{xx-ss}$ (or bp) : $s\sqrt{xx-bb}$ :: r : σ

$3\sqrt{63}$ $\quad$ $3\sqrt{15}$ $\quad$ 1 $\quad$ $\sqrt{\frac{5}{21}}$,

(log. $\bar{1}.688375$.)

for $\sqrt{xx-ss}=p=\sqrt{36-9}=3\sqrt{3}$

$\sqrt{xx-bb}=\sqrt{36-21}=\sqrt{15}$.

Second Problem.

bc : xs :: r : n

$2\sqrt{126}$ $\quad$ 18 $\quad$ 1 $\quad$ $9\sqrt{\frac{1}{126}}$ (log. $\bar{1}.904057$,)

$b=\sqrt{21}$, $c=\sqrt{24}$, $bc=\sqrt{504}$ or $2\sqrt{126}$

Having b, x, s, then $xx+ss-bb=cc$.

If you try these propositions by trigonometry, you will find their exactness, for in the triangle BVT are known the sides. Having the angle BRE, whose sine is represented by n, [and] any two of these x, s, b, c; the other two may easily be found, and consequently the angle RBE, thus:

Third Problem.

r, $\quad n$, $\quad b$, $\quad x$ given; quære c et s.

1, $9\sqrt{\frac{1}{126}}$, b, $\quad 6$,

$rrxx-bbnn$: $xx-bb$ :: $rrxx$: cc

$36 \quad \frac{27}{2} \quad\quad 36 \quad 21 \quad\quad 36 \quad 24$

$\frac{45}{2} \quad\quad 15$

Multiply $nn=\frac{81}{126}$ in $bb=21$, $nnbb=\frac{1701}{126}=\frac{27}{2}$

Fourth Problem.

r, n, b, c given; quære x et s.

$$\frac{bb+cc}{2} \pm \sqrt{\frac{bbbb+cccc+2bbcc}{4} - \frac{bbccnn}{rr}} = \frac{xx\ (36)}{ss\ \ (9)}$$

$$\frac{45}{2} \qquad \frac{2025}{4} - 324 \text{ or } \frac{1296}{4} = \frac{729}{4}, \text{ whose square}$$

root is $\frac{27}{2}$.

I have made radius $=1$, and expressed the sine by a surd for the exact trial thereof.

If these problems are worth your observing, I shall, so soon as conveniently I may, (God willing,) give you the demonstration of them. After the same manner, as I verily believe, it may be done in an hyperbola, (but I had not time to try,) for I cannot call to mind any proposition in an ellipse, but there is the same in an hyperbola.

As to the problem then in hand, the way that you have done it is very true and ingenious, supposing ID in your first figure to be known, which I look on to be more difficult to find than either of its focal rays.

That the sun's almacanters delineated on an horizontal dial are hyperbolas, except when the sun is in the equator, I have heard or read. That they are like hyperbolas I know, for I have, by help of calculation, described [them], when I knew not what an hyperbola meant, but have never since tried them; but, being you do affirm it, I do no ways doubt of it, and you have given a curious way for finding their focus.

And whereas you write I am inclined to publish a treatise of the conics, &c., I will assure you I never had such an opinion of my own labours. I must confess somewhat above two years since, being with my Lord Bishop of Sarum, his Lordship commended the study of the conics, and lent me Gregory a S^{to} Vincentio de quadratura circuli, and commended Maginus and Dr. Wallis his works to me. And since that time I have been intent thereon, and have made it my business to choose out the most curious and needful propositions out of each author, which, for the most part, I have demonstrated by Algebra, and some few problems I have added of my own, but as yet they are not in any order, and that is the reason that at present I cannot demonstrate those I send. And whereas you write of my translating into Latin, &c., I must acknowledge my own inability. It was my unhappiness that by reason of our civil wars I had not sufficient time in the university to have perfected my Latin by reading good authors; and afterwards, my inclination led me to the study of the Mathematics.

I have never seen Slusius's Mesolabe, but as I apprehend him by the Phil. Transactions, he shews the resolution of cubic and biquadratic equations by conical sections, as M. Des Cartes hath done, which way, I must truly confess I never fancied. For after all our pains, there is nothing but a line drawn by an unequal hand for the solution of the problem, and it is exactness pleases. But if I understand from you that he treats of the conics, I shall take some care to have one.

I perceive, by the printed paper you sent me, that others are of my mind, that there is need of some treatise of Algebra in English; for those we have already are imperfect and deficient. I do not doubt but sufficient care will be taken, that this may not, although at present, by the paper, there seems one chapter wanting, in such a general book as this is intended to be, of shewing the divers methods used by our modern authors. If time give leave, I shall [shew] you what I mean, by transcribing half a sheet of paper out of a manuscript of six sheets that I have framed for my own use. If you please to enter my name for one of them, I shall take it as another favour from you to him, who desires to be,

Sir,

your most humble servant,

THO. STRODE.

CCLXXXIV.

STRODE TO COLLINS.

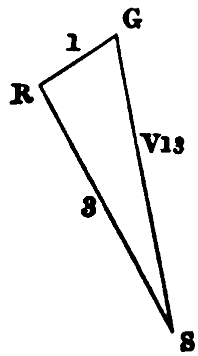
Maperton, Oct. 17, 1672.

Mr. Collins,

It is high time to give you some account what helps and assistances I have had from Kinkhuysen's books, which I should sooner have done, but I understood by Dr. Beal, from whom I received Gregory's Optica Promota, (wherein I find a resolution, but after another method, to my former doubt,) that for some time your troubles in removing have been so great that it would scarce afford you sufficient time for your necessary occasions. I have studiously gone over the treatise of the conics, and I find nothing there but what I formerly knew, and had analytically (although after another manner) demonstrated, except one proposition of the asymptotes of an hyperbola, which, to avoid tediousness, I have omitted. And that I have followed my own, and not his method, is on two accounts: first, for that I am like a tender mother over her only child, thinking none more beautiful, when possibly it is deformed; secondly, I would not give any occasion to any to accuse me of being a plagiary. There may seem to be [some] few propositions, that I have had from him, which I never saw in any other author. The first is, that if through the poles of any section a line is ordinately applied to its axis, it is the latus rectum of the axis of the said section. I have often admired that this was not observed by any author. I remember, about two years since, before I had read Mydorgius, I acquainted his Lordship, the Bishop of Salisbury, with this proposition, who then said it was in Mydorgius, but I could not find it there. But in Kinkhuysen's conics it lies couched among his equations concerning the focus.

The second is this, that the sum of the squares of any pair of conjugate diameters is equal unto the squares of both the axes of the ellipse, and that the difference of their squares is equal unto the difference of the squares of both axes in an hyperbola. The first part of this proposition is in Greg. a S^to^ Vincentio, and it is likewise in your first paper, but the second part I never saw saving in this author and my own papers; but it is not in his conics, but in his appendix to his other book, and in its demonstration he seems to me to be guilty of the same oversight as I was for several months, for taking that to be granted which ought to be proved. When I return you your books I shall shew you therein that I had not this proposition from Kinkhuysen. Besides, the different methods of our demonstrations, although both analytical, will appear by a question I proposed to

Dr. Beal about twelve months since, of a triangle RGS being given, whose sides are $GS = \sqrt{13}$, $RG = 1$, $RS = 3$, and it is required to find the transverse axis of that hyperbola, whose centre is G and RG part of one of its asymptotes, and GS a semidiameter of the said hyperbola, and RS a line touching the hyperbola in the point S; which cannot be resolved, as I conceive, numericè, without the assistance of this proposition, and by it it is easily performed.



I shall trouble you no more at present but to acquaint you that I have, after a sort, finished my conics, and that I have some few things else to transcribe, and that then I shall return up your books with many thanks. In the mean time I have sent you up a Cheddar cheese, which I suppose within this fortnight will come to your hands, which I desire you to accept from him, who is

your humble servant,

Thos. Strode.

[Postscript by Dr. Beal.]

Sir, since your last I wrote largely to you by post, but I fear it was miscarried, being sup[er]scribed to Mr. Andrewes', instead of Mr. Austin's, your letters of directions being all, at that time, in Mr. Strode's hands. And I hear so much of your great trouble, that 'tis against my conscience to increase it. If you can procure an ingenious person, who will carefully examine and correct Mr. Strode's [conics and] fit them for the press, he will send them to you, and promiseth to acknowledge the favourable assistance. But he saith, he is very unwilling his labours should be taken into another's, and drowned in another's name. In these terms he resolves to commit himself to your conduct, being most ready to offer his endeavours, but destitute of any assistants, or skilful neighbours, with whom he can communicate, and of other good books than as he is by you furnished. And 'tis considerable that he can do something without a guide or assistant at hand.

Sir, I pray God to bless you in all your labours, and in all that belongs to you.

Your very affectionate friend to serve you,

J. BEAL.

From Yeovil in Somersetshire,
Oct. 19, 1672.

Sir, in fit time I shall acquaint you of four defects in Mr. Strode's papers, which seem easy to be amended; in which if he gets assistance, it mayen courage him to go on in doing good service. He confesseth that for more composed problems he must refer to those in the highest form.

CCLXXXV.

STRODE TO COLLINS.

Maperton, May 31, 1673.

Mr. Collins,

I have herewithal sent you some of those books you sent me, whose names are subscribed, and I desire you to let me know the prices of the other books I have kept. I hope you have received that which I lately sent you by Dr. Beal. I have sent you a manuscript of what I have of geometry, possibly the fourth and fifth books may give you some divertisement: the other books are for those of an ordinary proficiency, or rather learners. In that of three continued proportions, I have brought them to half the number, which I formerly acquainted you withal, because I have left out these symbols, $\frac{d}{f} = bb \pm mm$, $\frac{h}{k} = bb \pm cc$, and $\frac{P}{R} = \frac{mm + cc}{mm - cc}$; the letters c, m, b, ∺, do signify three quantities in continued proportion to avoid tediousness. On the same account I have omitted these in oblique-angled triangles, where the three sides are expressed by b, c, h, viz. $g = b + h$, $n = c + h$, and $r = b + c + h$, and these have usually resolutions when l, or t have; and some other trivial propositions, and so have shortened the number of 518 propositions unto 168. The book of the conics, which is most desired, is most imperfect, for those propositions in the Appendix ought to be intermixed with the former, which I am unwilling to do until I find how it may be accepted, and then, if desired, I shall take the pains to transcribe it. I know not above one theorem more, which I conceive fit to add, and that is in Schootens' commentary on Des Cartes, page 351. That I have not taken notice of some of those propositions in yours of April 18th, is for that I have wholly declined all such propositions, that have respect to the cone, except those which give the demonstration of the chiefest properties of the sections, and those that I cannot demonstrate, as that theorem of Leautaud's, by you mentioned, as also of the description of a conic section, which shall pass through five given points, for I do not apprehend it; it seems to me like the passing of a circle through four given points, which accidentally may be done.

And for that problem of Mydorgius, where he gives no way for the calculation, it is performed in my 68th prop., and that prop. of any axis being given, how to apply several rational ordinates. Vide Append. 22 sect.

And where an hyperbola and an ellipse have the same axes as in the figure of my third proposition, the diameters of each, according to your desire, are found by my 65th prop. Let me desire your opinion of my demonstration in the 3, 9, 25, 49, 56, and 70th propositions, and of those in the Appendix. In the latter part of the conics, and in the Appendix, things are too confusedly put together, which I could not altogether help, being some of them but newly discovered, and in the transcribing, if occasion requires, must be helped. For the application of these curves to the finding of adfected equations, although I do, as I believe, fully apprehend what M. Des Cartes and his commentators have written, especially where the greatest unknown symbol exceeds not the biquadratic power, yet I shall not presume to do it, but shall be very glad to see it performed by some abler pen. If this find no acceptance among our chiefest mathematicians, yet let me as earnestly desire you to be as careful of it as if it did, (for it has cost me a great deal of pains,) and safely to return it to Dr. Beal, and you will oblige

your humble servant,

THO. STRODE.

Books returned.

Apollonius, 5, 6, 7 of the Conics.
Gregorii a S^to^ Vincentio, posthumus.
Maurolycus Apollonius.
Kinkhuysen's Conics, with an English translation.
Kinkhuysen's Gromatice.
Gregory's Optica Promota.
Bullialdus de Spiralibus.

Books detained.

Viviani de Maximis et Minimis.
Dulauren's Specimina Mathematica.
Dary's Miscellany, with two sheets of Algebra.

Dr. Barrow's Apollonius.
Kersey's Algebra, two first books.
A Philosophical Transaction.

As for that excellent paper of Dr. Barrow's, I shall not at present write any thing: possibly another trial may overcome the difficulties I find. The books of his, which you commend, his Optic and Geometrical Lectures, I have them by me, and I know where at any time to have Schooten's Miscellanies.

CCLXXXVI.

STRODE TO COLLINS.

Maperton, Sept. 25, 1673.

Mr. Collins,

In my last unto you, I acquainted you that I had lately sent you a couple of cheeses: possibly you may think them lost, but they are yet at my friend's house at Wincanton, for my servant came out two hours too late for the carrier, by whom they were intended to be sent, which I knew not until this week; so that they will not be carried until his next journey for London, which I suppose will be shortly—he does not keep certain seasons.

I have received yours of the 11th instant, and in it very acceptable papers, containing the demonstration of a very useful problem, and by the help of it I conceive I can give an easy resolution to the fitting any given section into any upright cone if feasible. Indeed there seems to me something of omission in its demonstration. Dr. Barrow's of the hyperbola I have at last conquered. I cannot apprehend in Mr. Gregory's that BN vel $MK \times BM = BG \times BF = EL \times LC = XX$.

I suppose that it should be $\frac{XX}{4}$. That the two first are equal is clear by 36 Euc. iii, and that $LE \times EC = \frac{XX}{4}$ in an ellipse, and that $EC \times CL = \frac{XX}{4}$ in an hyperbola, I know to be true, but not to demonstrate it; neither can I see how either of the two first are equal unto the two last. I shall take pains to transcribe that about the conics, and shall observe your directions therein. What to do about the other part I know not: I was about to insert these propositions, more than I have out of Euclid, and had wrought out brief; but, being I could not otherwise demonstrate them than he had done, I desisted.

10 Def. 13, 15, 18, 29, 32, 34, 35, 37, 38, 42 Euc. i.
All its second book analytically,
20, 21, 32 Euc. iii.
1, 2, 3, 4 Euc. vi.

There is a friend of mine, who intends to be in London the next week, whom I have desired to visit you, if you are at the farthing office, and pay you what I owe you.

Sir, I am your servant,

THO. STRODE.

CCLXXXVII.

STRODE TO COLLINS.

Maperton, April 20, 1675.

Mr. Collins,

This day fortnight I had the happiness to see Mr. Baker at my house, where he stayed until Saturday morning; his occasions would not permit him longer to stay. Here inclosed is his letter to you, and his statements on M. Tschirnhaus' rules, then written; but he desired me, if I thought fit, to transcribe it, which I have done. I doubt not but he hath hit on M. Tschirnhaus' way, and hath demonstrated all except the cubic equation, where $p=q=r$; but instead thereof hath given us another cubic, page 1, and biquadratic, page 5. The plano-quadratic is more difficult this than the common way. You will find some mistakes in Mr. Baker's paper, which put me to some trouble, as particularly he expressed this equation, page 5, $\pm\sqrt{\frac{p}{4}}+\sqrt{\frac{q}{4}}\pm\sqrt{\frac{p}{4}}-\sqrt{\frac{q}{4}}=x$, without making an universal surd over them thus as they ought $\pm\sqrt{\frac{p}{4}+\sqrt{\frac{q}{4}}}\pm\sqrt{\frac{p}{4}-\sqrt{\frac{q}{4}}}=x$. I took the boldness to mark them with an universal surd; afterwards reading it over too hastily I struck them out, but on another review I find they ought to stand.

Pag. 2. Eq. 32, 33, 34, I conceive 1600ª should be signed affirmatively.

Pag. 3. 13 and 14 Eq. are not right set down. It should be

$$\frac{p}{4}\pm\sqrt{\frac{pp}{16}-\frac{q}{4}+\frac{r}{2p}}\pm\sqrt{\frac{pp}{8}-\frac{q}{4}-\frac{r}{2p}\pm\frac{p}{2}\sqrt{\frac{pp}{16}-\frac{q}{4}+\frac{r}{2p}}}=x,$$

or

$$\frac{p}{4}\pm\frac{1}{2}\sqrt{\frac{pp}{4}-q+\frac{2r}{p}}\pm\sqrt{\frac{pp}{8}-\frac{q}{4}-\frac{r}{2p}\pm\frac{p}{4}\sqrt{\frac{pp}{4}-q+\frac{2r}{p}}}=x.$$

His mistake was for not subtracting the absolute quantity $\frac{r}{p}=b$ from the square of a, and in making the two last surds to be multiplied together. You will find most excellent all this work.

Sir, I am
your humble servant,
THO. STRODE.

CCLXXXVIII.

STRODE TO COLLINS.

Mr. Collins, Maperton, July 28, 1675.

I have hitherto forborne to return you my thanks for your civil respects in honour, for that I resolved in my first to answer your desire about Dr. Davenant's problem, and some unexpected occasions have twice or thrice hindered my resolutions of waiting on him, seeing I am tied to Thursdays. Sir, his method of resolution is in part, as I hinted, a tentative way, and there is no occasion of the sum of the cubes; which is thus, let $x^2 =$ sum of the four squares of four continued proportions. Let us conceive a to be the least, e the [secon]d, $\frac{e}{a} = F$, then $F^6aa + F^4aa + FFaa + aa = x^2$; suppose any square to be aa, and try it, dividing x^2 by it, then the quote is $F^6 + F^4 + FF + 1$; subtract 1, the remainder is $F^6 + F^4 + FF$. Now imagine that or some other square to be FF, then divide the remainder by it, the quote is $F^4 + FF + 1$; from whence substract 1, and divide the remainder by FF, the quote is $FF + 1$, from whence subtract 1. If the remainder is equal to FF, as is formerly supposed, the mark is hit; if not, you must try until you find it.

Let $x^2 = 3280$, then $F^6aa + F^4aa + FFaa + aa = x^2 = 3280$. Suppose $aa = 4$, the square of the first term, then $\frac{3280}{4} = 820$, and $820 - 1 = 819 = F^6 + F^4 + FF$. Let us imagine $FF = 9$, then $\frac{819}{9} = 91$, and $91 - 1 = 90 = F^4 + FF$; $\frac{90}{9} = 10$, and $10 - 1 = FF$, as was imagined and therefore true, and consequently the four terms are 4, 36, 324, $2916 = 3280$. Observe if either of the two remainders, 819, 90, are not divisible by $FF = 9$, then try another square. This would be a difficult way where a or F are fractions, or great numbers. I must needs confess such tentative ways please not my fancy.

Since my return I received a letter from Mr. Baker, who gives me an account that one Joannes Marcus Marci, in a Treatise de Proportione Motus, has demonstrated that motus projectorum is no parabola. I thank you for Barrow's book you lately sent. I have herewith returned Barrow's Apollonius, Archimedes, and Theodosius. I am

your humble servant,

THO. STRODE.

CCLXXXIX.

COLLINS TO STRODE.

Oct. 24th, 1676.

Mr. Strode,—Sir,

I have both yours of the 11th and 13th inst.; and received the tub of butter you were pleased to send [me], for which [I] return you my hearty thanks.

The admirable M. Leibnitz, a German, but a member of the Royal Society, scarce yet middle aged, was here last week, being on his return from Paris to the court of the Duke of Hanover, by whom he was importuned to come away, and refuse such emoluments as were offered him at Paris; but during his stay here, which was but one week, I was in such a condition I could have but little conference with him: for, being troubled with a scorbutic humour, or saltness of blood, and taking remedies for it, they made me ulcerous and in an uneasy condition. However, by his letters and other communications, I presume I perceive him to have outtopped our mathematics quantum inter Lenta[t], &c. His combinatory tables are specious and not numerical.

I would not have you think it my design to hinder the printing of your exercise De arte combinatoria, for the truth is, be a mathematical argument never so good, it is hard to find undertakers; and as to yours in particular, the same subject being handled by Pascal in his tract Du Triangle Arithmetique, and in the late French Algebra, entitled Elemens de Mathematiques, I could have wished yourself had been chancellor of their performances. I offered your tract to divers booksellers, who refused a concernment in it, but at last Mr. Broome, upon Dr. Beal's desire, is the undertaker, and it is at the press at Mr. Clarke's, a printer in Aldersgate-street, but not yet begun: you may be pleased to give your directions about a title and preface. Mr. Baker being with you satisfies me why I could not expect an answer of some large letters I sent him. I should gladly know what conveniences of intercourse you have, and would be willing that what is writ to either of you might be common to both. Mr. Dary hath three or four sheets of Algebra in the press about simple and compound interest and annuity questions, with logarithmical approximations for the roots of equations, which he prints at his own charge

[t] Velut inter ignes Luna minores. Qu.

for want of undertakers, as likewise doth Mr. Henry Bond, an ancient teacher of mathematics, his tract entitled, The Longitude found, viz. his tables and theory of the inclination and declination [of the] magnetical needle.

From France (by an English gentleman returning from Rome, with novelties thence, not yet taken up from the Custom-house) I have received one of Philip de la Hire's treatises of the Conic Sections, Gallice, in a new method of projections, wherein I am apt to believe there is something novel, and more than in others; and he is about to publish the same and more in Latin. The former, and also this, (if desired,) I hope ere long to send to you. Mr. Bernard, Professor of Astronomy at Oxford, now at Paris, writes that Borellius his Geom. Tract. de Motu Animalium, and Viviani de Loco Solido are now extant, and expected there; he is writ to, to procure and send them, of which, especially the latter, when arrived I hope to give you an account.

CCXC.

COLLINS TO STRODE.

8th of Feb. 1676-7.

Mr. Strode,—Sir,

I perceive, by a letter of yours to Mr. Broome, the books I sent are come to your hands. In this severe winter I have been troubled with an ebullition of blood caused by its thinness and saltness, whence ensued a great itching, to remove which taking physic before phlebotomy, it caused boils and an inflammation in my right arm, which hindered not only all correspondency with you ever since August last, but likewise my private affairs, being out of the farthing office, or public concern, on account of tin farthings that are to ensue.

Frenicle's Triangles, Dary's Interest, and De la Hire's Conics, be pleased to accept, and to return Pascal du T[riangle] A[rithmetique], it being none of mine. The French Algebra is common to be had at 12*s*. price. I do not find he takes notice of two late Dutch authors, namely, Kinkhuysen and Abraham de Graaf, each of which I much more esteem than the French author. As to your tract of Combinations, I looked upon it as worthy public view, but conceiving it not so proper to go alone as with your other works, as thinking it might not prove of common sale, and being loth it should be printed at a house not used to mathematical work, nor furnished with proper types, you mistake if you think I had any design to hinder its coming forth on any other account, saving only that I thought I might give you some hints further to enlarge the subject of figurate arithmetic with the following advancements.

1. Leibnitz asserts all equations relating to figurate numbers or angular sections may have their roots expressed in surds, and hath imparted something of that kind by what he hath already attained, and promised further to explain himself when he arrives at Hanover, being here in October on his return to the Duke of that name, under whom he will be in eminent employ.

2. There may be combinatory tables made in species, which may be of as much use in Algebraic affairs, as the table of sines in practical geometry, in which tables most difficult problems shall be found ready solved, or easily thence deducible, and hence canons derived for the surd roots of all equations. His notions about the same he offered to explain on condition any one would undertake the work.

3. How to add or interpole progressions of squares, cubes, or of any other ranks of numbers, whose last differences are equal, is already performed by aid of tables of figurate numbers, by Mercator, in his Logarithmotechnia, and by Dary, in his Gauging.

4. The said Mercator continues a table of logarithms thereby.

5. The tables of logarithms may be carried on, and such progressions added, on supposal of the use of such figurate numbers; but by quite evading the same, either without or by fitting equations to all such ranks.

6. More particularly if you can add the reciprocals of an arithmetical progression, or its squares, by aid of equations suited thereto, derived from the knowledge of figurate arithmetic, you know as much, even in this kind, as I have hitherto attained. But lest you should not apprehend my meaning, I have set down two such ranks:

$$\frac{1}{2} + \frac{1}{3} + \frac{1}{4} + \frac{1}{5} + \frac{1}{6} + \&c.$$

$$\frac{1}{4} + \frac{1}{9} + \frac{1}{16} + \frac{1}{25} + \frac{1}{36}$$

Where you see in the first rank, if the numerator be common, and the denominators in arithmetical progression, it is called the reciprocals of the arithmetical progression; the latter rank is called the reciprocals of the squares of an arithmetical progression. For the finding the present worth of an annuity of 100 per annum, to continue 100 years, discounting 6 per cent. per annum simple interest, I have added 100 terms in the following rank; viz.

$$\frac{10000}{106} \quad \frac{10000}{112} \quad \frac{10000}{118} \quad \frac{10000}{124} \quad \&c.$$

and found the sum of them to amount to (as I remember) 3200*l.* 0*s.* 1*d.* fere, which argues the absurdity of simple interest in relation to purchases. The explication of the method is not only scientific but large.

Mr. Broome hath your Treatise of Combinations, and is resolved to go forward with it, which I shall as much promote as in me lieth: these accessions may be proper enlargements for a following edition.

I remain ———,

This letter is dated 1676 in the original MS. It must, however, be 1676–7, although he speaks of not having written since August. The fact may have escaped his memory, and the date of some of the books mentioned, as well as of Leibnitz' visit to London, must be considered as determining the point.

CORRESPONDENCE WITH WALLIS.

CCXCI.

COLLINS TO WALLIS.

Jan. 2, 1665.

Reverend Sir,

The unexpected favour I received from you when at Oxford, cannot but make me mindful of those obligations of thanks I ought to return, as a testimony whereof vouchsafe to accept of a few books I herewith send, viz.

Mengoli Geometria Speciosa. Antimi Farbii Opusculum Geometricum. Caravagii Mediolanensis Applicationum Doctrina, which handles the limits of such equations as have but two nomes. Not that I think any man's works can add to your vast treasure of knowledge, but possibly may excite you to supply what they have omitted, or amend what they have but perfunctorily performed.

It hath pleased God still to reserve me, as a monument of his mercy, in the land of the living; but upon my return hither, I found wanting Mr. Anthony Thompson and Mr. Henry Sutton, two of the best mathematical instrument makers. And the quondam servant of Mr. Sutton, John Marke, being now returned, is about taking his master's house; he desires the presentment of his most humble service. We hope he may prove as good a workman as his deceased master, and that mite of knowledge I have attained to [I] shall most willingly serve him with.

Some new books are published in France; as Pascal du Triangle Arithmetique, &c. and some others, whereof I hope ere long to have an account, from the hands of an ingenious German gentleman now residing at Paris, and familiar, I presume, with M. Roberval, who, being on the contemplation of curves, perchance will have proposed to his consideration the curve in Wright's or Mercator's sea-chart, that represents the great arch of a circle, and with what plane such a curve may be cut out of the cylinder, in which Mr. Wright supposes the sphere inscribed; whether the said meridian line be not the same, or in the same ratio as Mr. Gunter's logarithm scale of tangents, beginning therein at 45 degrees, and counting each half degree a whole one; and the nature of that curve that may be made by raising the degrees of that meridian line as perpendiculars upon the degrees of the equinoctial in that chart, which are equal parts and the contrary; and this in order to the more easy description of the great arch's curve in that chart, or for the supply of the meridian line, it not yet being known (geometrically) to find the rhumb between two places of known latitudes and given difference of longitude. And if any thing come to my knowledge about these methods, I should be willing to communicate it, and humbly crave the favour, when your occasions draw you to London, to afford me the cognizance thereof, that I may have some further opportunity administered of expressing my gratitude. In the·interim, if you shall vouchsafe a line or two in return, it needs no other direction but to me, as an accountant at the Excise office in St. Bartholomew Lane, London, who desires to be accounted an admirer of your worth, and

your most affectionate servitor ———

CCXCII.

COLLINS TO WALLIS.

Reverend Sir, Feb. 28.

I have received yours dated the instant, with your enclosed exercise, and considerations about Mercator's, or rather Mr. Wright's sea-chart, for the use whereof [I] return most hearty thanks, but withal am sorry I should put you to so much trouble about such a subject, which, I presume, I as far understood as those authors left it, if not farther. This must redound to my crime and your goodness, who were pleased to divert your studies from other sublime speculations. You have discovered where the difficulty lies, &c. may it redound to your immortal fame to remove it! not that I presume to desire[i] [you to]

[i] The sentence in this place has been left incomplete by the erasures and interlineations introduced into it: it is therefore printed so as to express, as nearly as possible, what appeared to be the meaning it was intended to convey.

reveal your discoveries in such worthy speculations to myself, but to be reserved for your own opportunity to communicate to the learned; as many of which, as I ever had the happiness to converse with, account you willing lampada tradere. The latitudes of two places, and their difference of longitude given, to find the rhumb geometrically, (as far as the nature of such a problem admits,) is very useful, and therefore desirable, because seamen (expertus loquor) are much for delineations; hence the common tables of natural sines may perchance be sufficient for marine calculations, if others were wanting, and hence the common sea-chart, with parallel meridians equally divided, may supply the want of Wright [and] Mercator's chart. I confess, as a sciolist, I have adventured to my shame too far in this subject, and this made me not to send you my endeavours sooner, as blushing at so judicious a censure. Such a chart seems to be an evolution of the sphere. For admit a sphere, made by the conversion of that semicircle, that is the semibase of a cylinder, unrol itself in the concave of the said semicylinder, (cut through the axis,) with the axis of the sphere erect to the side of the parallelogram, it then evolves the sphere, and on this supposition it is not hard to delineate the curve that any rhumb line, the ecliptic or any other great circle, shall leave behind; but the geometrical speculations about such curves seem sublime.

Moreover that the meridian line of Mercator's chart should seem (as it doth) to be the same with the logarithm tangents, (viz. that the adding of natural secants should constitute a logarithm tangent, though to an unwonted ratio,) is Mysterium aliquod grande proposed long since to Mr. Briggs and Mr. Gunter, but not approved or disproved. See my Plane Sailing, herewith sent, the Navigation part, pp. 117, 44, 45, 60, 61,

62. And admitting that the meridian line were the same with a log. tangent, it would not want a geometrical way to describe it, by such curves as pure geometry will reject. But in regard I hope to have the honour to attend you at your arrival here, I cease enlargement, or to cumber you with some later thoughts of my own about the said chart, curves, or meridian line.

At the request of Mr. Sutton I wrote a despicable treatise of quadrants. His design was to demonstrate himself to be a good workman in cutting the prints of those quadrants, and thereby to obtain customers, mine to improve the prints by varnish, which I was certain I could accomplish twelve years since, to a better lustre than this I herewith send, together with a sheet of my book, which I now send, and the which, commaculated with dirt or ink, will be washed away without damage.

The first thing I published was about a quire of paper concerning Merchants' Accounts, which, upon later thoughts, I found myself unable to amend, and was reprinted in May last, but by reason of the contagious death of the bookseller not procurable yet. And among these luxuriancies I met with a dialling scheme of Mr. Foster's, and commented upon that; which it is too late to wish undone, as the author died and left it, but what improvements I have met with on that subject I shall be willing to manifest. I hope to be more wise and silent hereafter. Vouchsafe herewith also to receive and accept Broscius his larger treatise De Numeris Perfectis, with your own papers, and to account me one of your most thankful and

obliged servitors and admirers ———

CCXCIII.

COLLINS TO WALLIS.

Aug. 1, 1666.

Reverend Sir,

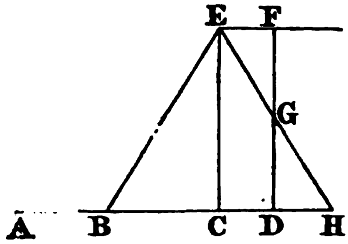
Upon the expectation of your arrival here, I have hitherto forborne to send you my book of Accounts, which is so eccentric to your studies as I thought it unworthy your acceptance; but notwithstanding have sent it herewith. That pasted quadrant was of Mr. Marke's fixing, but now he is more careful. As to my Navigation, it is altogether unworthy the view of a geometer, or that character you are pleased to afford it. I am informed you have had the view of an algebraic treatise of Bartholinus, entitled, Dioristice: in regard it is not to be had, my humble request is that you would vouchsafe me the title of it exactly with the time and place of its edition, as also your censure of the books following, if they have passed your view.

Josephi Hebræi Bibliotheca Mathematica, 8vo. Francofurti. Andræi Alexandri Mathemologium, folio. Sinclari Apologia pro Archimede et Euclide.

I never saw any of them; but the two latter are in the Marquis of Dorchester's library. May I presume a little further, I would likewise intreat your information concerning the manuscript of Galileus in your library, whether it be his Mechanic Problems which Mr. Salisbury, whilst living, complained he could not obtain. And when your occasions shall next invite you hither, if not too troublesome, another request would be to have a sight of the thin folio of Valerius, entitled Subtilium Indagationum liber primus, to compare with an approach some of our gaugers make use of to compute the segment of an upright cone between two parallel planes, whereof one passeth through the axis, which is as followeth:

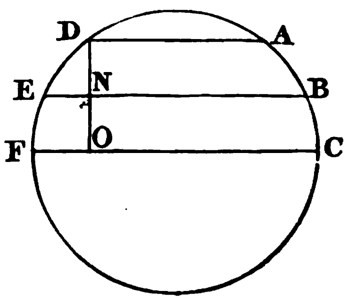
As the diameter of the base + parallel distance of those planes to the semidiameter of the base; so segment of the circumscribing cylinder between those planes to the segment of the cone.

In the annexed figure, if EBH be a triangle through the axis, AB a semidiameter of the base, EC plane passing through axis, FD the other plane; AD:AB::so segment cylinder CF to segment cone EGDCE.



And for the second segment of a sphere, to shun the use of the area of a segment of a circle some use the proportion following:

Let a sphere be cut by three parallel planes, one passing through the centre as FC, the other two above it, as EB, DA, and let a fourth plane, from D to O, be erect to the former, the approach used is, as the solidity of the central zone CADFC is to the solid on the half segment DFOD, so is the solidity of BADEB (which represents a zone excluding the centre) to the second segment DEND.



With what safety, and how far these approaches may be used, I have not abilities sufficient to judge. Vouchsafe not to be offended that I trouble your judicious eyes therewith, and account me one of those that are desirous to obtain the aspect of your favour, and to manifest himself, Sir,

your much obliged servitor,

J. COLLINS.

CCXCIV.

WALLIS TO COLLINS.

Oxford, Aug. 7, 1666.

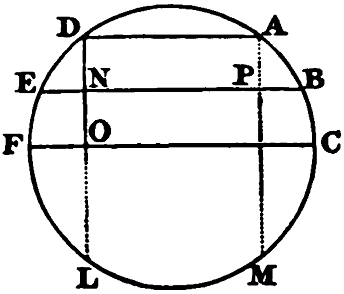
Sir,

I thank you for yours of the 2nd instant, and your book of Merchants' Accounts, which you were pleased to send with it; which I have perused, and find very full and satisfactory as to what it undertakes. I am sorry I am not able to give you a better account in the particulars of your letter. The books you mention, whether I have seen all of them or not I cannot tell, (for I have so ill a memory at names, that I can undertake but little for it,) but if I do not much misremember, I saw some of them, if not all, in my Lord Brounker's hands, more than a year ago; when I did only slightly look them over, having then no more time than only so to do. I do not remember that in those I looked on, I did take notice of the contents of them as very considerable for any thing of new discoveries, but rather illustrating or putting into some new form things before known, which made me less concerned as to inquiring further after them. That of Bartholinus's Dioristice, I do not remember that I have seen at all; but he is like enough to perform well what he undertakes, being formerly a scholar of Schooten, and well understanding his way of (as he calls it, Cartesian) geometry. I think the other three were amongst those my Lord Brounker shewed me, and perhaps that of Bartholinus also.

The manuscript of Galileo, which you saw with me, is not his Mechanic Problems, but rather an introduction to them, which (that I might the better give you an account of it) I have since read over. Its subject is about the utility of mechanic engines, wherein it consists; which he shews to be chiefly in this, that they do exchange strength for time, performing the same in so much a longer time as it is with less strength. In shewing which, he explains the balance, the lever, the wheel, the pulley, the screw, and particularly Archimedes' water-screw, and the force of a stroke; referring in the close to his Mechanic Problems, which, he says, should be annexed to this discourse.

That of Valerius, if I forget it not, I intend to bring with me next time I come to London, that you may see it, as you desire; when it will be I cannot well tell. I passed through London not long since, both as I went into Kent and as I came back again; but I made so short a stay both times, and lodged so far out of town when I was near it, that I could not, as I desired, get so much spare time as to make you a visit, which therefore (with some other things I would not willingly have omitted) I was forced to let alone.

The method, you mention, of approach, in the second segment of a sphere, may perhaps come pretty near the matter, so as to serve a gauger's occasions, where a preciseness is not intended, but it always gives the segment END bigger than indeed it is. For it is manifest, to the first view, that the segment FOD doth decrease towards D in a much greater proportion than the intermediate segment OA; whereas the method you mention supposeth them to lessen proportionally.



I have formerly, according to the method I shewed you, computed such a segment as NLMP, supposing the sphere's semidiameter 1.00000, and OD = 0.75000, ON = 0.50000, (that is, R = 1, ON = $\frac{1}{2}$, OD = $\frac{3}{4}$;) and found the content 2.93040 proxime, (that is, more than 2.93039, and less than 2.93040). The whole operation is too large to insert, but you may, at your leisure, computing it according to the method you mention, find how near that comes to the truth, and thereby make an estimate of the goodness of it. The post hour approaching allows me not time this morning to do it.

Nor can I give you any estimate of the other rule of approach, for the segment of [a] cone between two parallel planes perpendicular to the base, which will require a longer time to examine.

What I said of the fixing of the pasted quadrant was only an intimation, which might be of use to Mr. Marke, in order to rendering them more exact. Nor shall I now add more than that I am

your friend to serve you,

JOHN WALLIS.

CCXCV.

COLLINS TO WALLIS.

Reverend Sir,

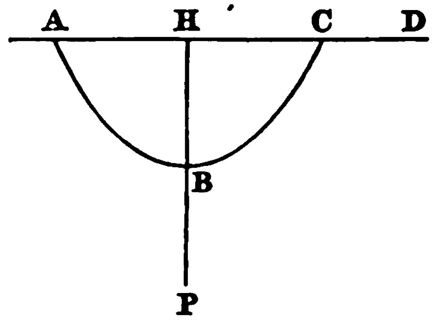
Mr. Pitts, a stationer in Little Britain, (of whom I can give no other than a good character,) understanding from Mr. Branker, a late fellow of Exeter College, who now lives with Lord Brereton in Cheshire, that you had prepared sundry tracts for the press, is very desirous to undertake any of them, and to treat with you to that purpose, promising they shall come out on very good paper like Schooten's Miscellanies, and the cuts to be suitable thereto, which may be done by Mr. Marke, and indeed, passing by morning and night, and at noon too, if need require, I should afford my endeavour to have it carefully corrected. The said Mr. Pitts hath sent, and presents you with as much of Dr. Pell's delayed book as is yet printed, and intends to send the rest when finished. We are very sorry to hear of your losses by robbery: if money were so lost, it might have been prevented by paying it here on the account of Sir Thomas Pennystone, farmer of the excise, and receiving it there of him. I have enclosed sent a note of some books lately come forth, and hope to procure some of them ere long. Talks there are as if some about Hamburgh had discovered a series of the ratio of polygons inscribed [in] and circumscribed about a circle, which would be of great use in making the sine or tangent of any arc in the quadrant on demand; but, as far as I know, the rank remains to be completed, and I presume it cannot be expected so well from any other as yourself.

This easily presents itself,

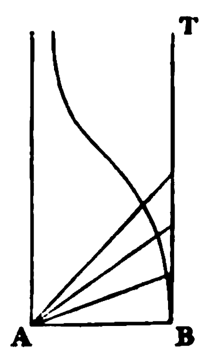
	Triangle.	Square.	Pentagon.	Hexagon.
Inscribed	1	2		3
Circumscribed	4	4	4	4

but the continuance and completing of the rank will beget admiration.

If my presumption offend not, I would add that there might be many sorts of conchoid lines. As let ABC be a parabola, whose base AC is indefinitely extended; let this parabola slide along ACD drawing the ruler HP after it, which always lies over the points P and H, and



where the parabolical line and this line or ruler HP intersect, let there be points made in a curve, which will be conchoidal or asymptotic, of which there may be innumerable kinds. Now such a curve being proposed, how to find the figure and the pole is not yet handled. But I have a suspicion that the meridian line of Mercator's sea chart may be made by some such kind of curve. For let AB be radius, BT a line of tangents, and supposing the first degree of Mercator's meridian line equal to one degree of tangent, with every 10 degrees of meridian on B as a centre, let every 10 degrees of secant be intersected, and a curve passing through those intersections will resemble a conchoid line. Vouchsafe to excuse



the trouble I put on your patience. If you should chance to consider these things, and make some useful discovery therein, I doubt not but in time you will make it redound to the public benefit of learning.

I remain ———

CCXCVI.

COLLINS TO WALLIS.

Feb. 2, 1666–7.

Reverend Sir,

I received yours, and communicated it to Mr. Pitts, who very much honours your advice, and thanks you for it; but if there be any other that is willing to bargain for the said impression, he is not desirous to interpose for these reasons.

1. He is engaged in the Dr.'s book already: but chiefly, the impression is double the number that ought to have been of a mathematical book, the best whereof, though sure of sale, are but slow. Mr. Briggs's Arithmetica Logarithmica, being too numerous an impression, has been tendered about the streets at 1*s*. 6*d*. each. The like I say of Mr. Barrow's Euclid. Mr. Sutton and myself, as Mr. Marke well knows, have bought divers of them at 1*s*. a book in quires.

2. There are sundry tracts of Algebra expected; first from beyond sea, as that of Chaveau; one promised, the manuscript whereof is by Hudden, Ab Ovo ad Montem (Mala?); an entire posthumous treatise of this nature left by Tacquet, which Meursius of Antwerp intends to print; two volumes in folio of Renaldinus in the press at Florence; Fermatius; lastly, the third volume of Des Cartes is said to contain all of his that is tolerable, and mentioned by Borellius in his life.

At home Mr. Kersey hath a laborious treatise in English ready for the press, which is promised by Mr. Stephens, who will undertake it when paper is more reasonable.

The said Mr. Kersey hath made notes on the Clavis [h], and, to say the truth, doth not admire any thing in it, save what concerns the tenth and succeeding Elements of Euclid.

Mr. Bunning, an aged minister, near Nuneaton in Warwickshire, hath commented on the Clavis, which he left with Mr. Leybourne to be printed; but one Mr. Anderson, a knowing weaver, told Mr. Bunning that the Clavis itself, and his comment thereon, were immethodical, and the precepts for educing the roots of an adfected equation maim and insufficient.

Mr. Pitts upon supposition, as above, is notwithstanding willing to deal for the impression, provided there be an engagement that it shall not be reprinted till the impression be sold; and because it is already common, that he may have liberty to increase it with such comments or explications as he shall be advised (by yourself, instar omnium, if willing) by his friends to be annexed to it; if also there be not another Clavis or Introduction ready for the press, prepared by Mr. Gilbert Clarke, (which we shall endeavour to know,) who lives with Sir Justinian Isham, within seven miles of Northampton, who in the preface of a treatise concerning some experiments in philosophy, intimates he wrote a comment on the Clavis, which lay long in the hands of a printer, by whom he was abused, meaning Leybourne. Mr. Cocker, our famous English graver and writer, now a schoolmaster at Northampton, about

[h] Oughtred published his Arithmeticæ et Numeris et Speciebus Institutio, &c., the Clavis alluded to in this and the following letters, in 1631.—See notes on Oughtred's correspondence, vol. i.

half-a-year since, directed a friend of his, that came up, to see me, who intimated that Mr. Clarke, finding his notes obstructed, had prepared either a new Clavis or a method of equations, and further enlargements upon Des Cartes. Mr. Dary thinks he hath of late so mended his precepts for the obtaining the roots of an equation, that they shall undoubtedly give the quality of every assumed figure in the root, whether too great or too little, and render the least root in the equation, or discover the impossibility thereof; and the like he can do for the greatest, which I hope by some other opportunity to transcribe, and send them to be submitted to your judicious censure. As concerning the book of Dr. Pell's scholar, I think the Dr. did little concern himself in it till the introduction was past, and to speak plainly, I account that introduction much worse than Principia Matheseos Universalis. I have not been yet concerned in the correcting of any part of that book, and have observed many errors therein. I perceive you have had the happiness to light upon the same manner of effecting cubic equations in your letter to Lord Brounker that I have heard the Dr. sometimes mention, to wit, by waving indented lines to cross a right line, by which waving lines I presume he meant the diameters of the parabolasters when the ordinates are inclined, and in regard the Dr. is not likely in this treatise to publish something else I have heard him discourse of, I think fit to suggest it.

1. That any equation being proposed, he can, by his general doctrine of limits, determine how many of its roots are possible.

2. I have seen two long scrolls, or tables of numbers, relating to a biquadratic equation, having all the powers extant; in one, the resolvend or homogeneum

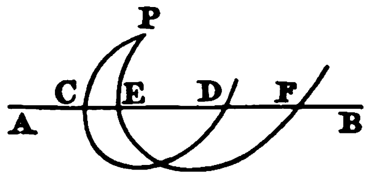
comparationis hath been kept constant, and the coefficient of any one term at a time altered successively in an arithmetical progression, till one or more of the roots have lost their possibility, and the said table hath given the roots very near. The other keeping the coefficients constant hath given the roots all along when the resolvend hath been an arithmetical progression. And such tables, he saith, are not very hard to make in relation to any equation, and that he disuseth the general method.

I presume these things have been considered by yourself, and therefore I claim pardon for my impertinences, and return my utmost thanks for your manifold favours, subscribing myself

your most affectionate humble servitor ——

For the effecting all manner of equations in lines, I have heard it thus in general described:

By the doctrine of limits, he assumes a limited line AB and finds a point above it from whence for each pair of roots issues a curve as PCD, PEF; which crossing the right line at C, E, D,



F, give AC, AE, for affirmative roots, and AD, AF, for negative roots; and if it be an equation of an odd degree, as cubic, the point F might fall beyond B, shewing an impossibility of another root; and if a curve do not touch at all, two roots lose their possibility: and all these may be one continued indented curve, and then methinks they might be the diameters of the parabolasters at the end of Dr. Wallis his Arithmetic.

What else might be expected from Dr. Pell, whose

Idea of Mathematics[i] is extant in Durie's Reformed School, printed in 1651; where, page 42, he saith, according to his prescribed method he can deduce not only all that ever is to be found in our antecessors' writings, and whatsoever they may seem to have thought on, but also all the mathematical inventions, theorems, problems, and precepts, that it is possible for the working wits of our successors to light upon; and, page 45, to determine all the number of problems that can be conceived concerning any thing propounded, and for ex[er]cise in these methods my Lord Brereton informs me hath wrote a quire of paper on this question.

CCXCVII.

WALLIS TO COLLINS.

Oxford, Feb. 5, 1666-7.

Sir,

In answer to yours of the 2d instant, concerning the proposition I made about Mr. Pitts his dealing for the impression of Mr. Oughtred's Clavis, I have not much more to say than what I [have] expressed already. It was only a motion of my own, which Mrs. Lichfield, who hath the impression to sell, knew not of. And I took my rise only from the overture your letter mentioned from Mr. Pitts, of his willingness to print mathematic[al] books. And I thought it seasonable enough to inform him of this, which is already printed, which (in the scarcity of books since the fire) might be vending, while others are but in preparing. Whether the

[i] The Idea of Mathematics was published in London, A.D. 1650.

number be too great, or the book not so vendible, the bookseller, who understands his trade, is a more competent judge than I. But for the goodness of the book in itself, it is that (I confess) which I look upon as a very good book, and which doth in as little room deliver as much of the fundamental and useful part of geometry (as well as of arithmetic and algebra) as any book I know; and why it should not be now acceptable I do not see. It is true, that as in other things so in mathematics, fashions will daily alter, and that which Mr. Oughtred designed by great letters may be now by others be designed by small; but a mathematician will, with the same ease and advantage, understand *Ac*, and a^3 or *aaa*. Nor will Euclid or Archimedes cease to be classic authors and in request, though some of their considerable propositions be, by Mr. Oughtred and others, delivered now in a more advantageous way, according to men's present apprehensions. And the like I judge of Mr. Oughtred's Clavis, which I look upon (as those pieces of Vieta who first went in that way) as lasting books and classic authors in this kind; to which, notwithstanding, every day may make new additions. But this is beside the business, for a bookseller is to judge that a good book which sells well, whatever we students judge of them. If Mr. Pitts think it convenient for him to deal for the impression, as it is, and let me know on what terms he is willing to take it, Mrs. Lichfield is my neighbour, and I can easily propose it to her, and give him an answer. As to the particulars he mentions, I think it reasonable that she should engage not to put forth another impression till he may have a reasonable time for the putting off of this: how many years that is to be judged at, booksellers can better judge than I. And as for adding any thing to it to be sold or bound with it, I think she hath no reason to be against it, provided it be not so as to become a prejudice to her copy. But I confess, as to my own judgment, I am not for making the book bigger; because it is contrary to the design of it, being intended for a manual or contract; whereas comments, by enlarging it, do rather destroy it. I should rather advise those, who may publish any thing that way, to do it as a work of their own, than as a comment on this. For if Mr. Oughtred had intended it to be large, he could with more ease have made it much bigger than it is. But it was by him intended, in a small epitome, to give the substance of what is by others delivered in larger volumes. I told you in my last what price she expects for it, as I have formerly understood from her, viz. 40*l.* for the impression, which is about 9½*d.* a book. If they agree about the price, I believe other things will be easily enough accommodated. If he will have any thing else adjoined to it, the paper and print of that is to be further agreed on.

I thank you for your advertisement of books expected, resting, in haste,

your loving friend,

JOHN WALLIS.

Mrs. Lichfield's daughter is now in London, and lodgeth at Mrs. Stevens's house in Castle Yard, in Holborn, over against Magpie Court. Mr. Pitts, if he please, may speak to her about it, or let me know his mind here, which he please.

CCXCVIII.

COLLINS TO WALLIS.

Worthy Sir,

I have yours in answer to what was objected against the Clavis. It was not my intent to disparage the author, though I know many that did lightly esteem him when living, some whereof are at rest, as Mr. Foster and Mr. Gibson. I do not search atramentum in nive, but my design was to acquaint you with the argument of certain books, whereby the Author might be improved, divers of which I might presume you had not heard of as being scarce. Nor is the Author or any man blamed for making a collection of things already known. Collection, translation, and illustration of matters scarce, exotic, and obscure, cannot but have its encouragement. You grant the author is brief, and therefore obscure, and I say it is but a collection, which, if himself knew, he had done well to have quoted his authors, whereto the reader might have repaired. You do not like those words of Vieta in his theorems, ex adjunctione plano solidi, plus quadrato quadrati, &c., and think Mr. Oughtred the first that abridged those expressions by symbols; but I dissent, and tell you 'twas done before by Cataldus, Geysius, and Camillus Gloriosus[k], who in his first decade of exercises, (not the first tract,) printed at Naples in 1627, which was four years before the first edition of the Clavis, proposeth this equation just as I here give it you, viz. $1ccc + 16qcc + 41qqc - 2304cc - 18364qc - 133000qq - 54505c + 3728q + 8064N$ æquatur 4608,

[k] Exercitationum Mathematicarum Decas prima, Nap. 1627, and probably Cataldus' Transformatio Geometrica, Bonon. 1612.

finds N or a root of it to be 24, and composeth the whole out of it for proof, just in Mr. Oughtred's symbols and method. Cataldus on Vieta came out fifteen years before, and I cannot quote, that as not having it by me.

As for Geysius, he published an Algebra and Stereometria divers years before the first edition of the Clavis was extant in Mr. Harriot's method, out of which Alsted took what he published of algebra in his Encyclopædia, printed in 1630, the year before the Clavis was first extant (see Christmannus and Raymarus). Mr. Harriot's method is now more used than Oughtred's, and himself in the esteem of Dr. Wallis not beneath Des Cartes. Dr. Hakewill, in his Apology, tells you Harriot was the first that squared the area of a spherical triangle; and I can tell you, by the perusal of some papers of Torporley's it appears that Harriot could make the sine of any arch at demand, and the converse, and apply a table of sines to solve all equations, and treated largely of figurate arithmetic. His papers fell into the hands of Sir Thomas Aylesbury, father to the Lord Chancellor's lady, where I hope they still are, unless they had the hard fate to be lent out, before the fire, and be burned, as some have said.

Concerning the obscurity and brevity of Des Cartes I am of your mind, though he saith, page 395, 3rd vol. that he hath done the work of an architect, and leaves it to carpenters and masons to finish.

As for what Mr. Oughtred hath done on the table of powers, I willingly suppose he had not seen Geysius or Faulhaber, whom Des Cartes visited, out of whose algebraic works hard copious matters may be taken, viz. out of his

Arithmetischer Wegwyser, 1613, Germanice;

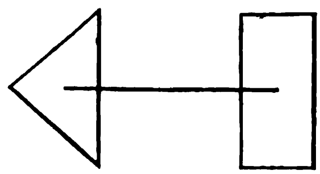
Flores Algebraici, about figurate arithmetic, Germanice;

Academia Algebræ, Germanice;

Ansa Artis mirabilis, Latine;

Miracula Arithmetica and other small tracts, Germanice.

And as for Mr. Oughtred's method of symbols, this I say to it; it may be proper for you as a commentator to follow it, but divers I know, men of inferior rank that have good skill in algebra, that neither use nor approve it. One Anderson, a weaver, who hath solved these problems; The sides of a trapezium and its area given, to situate the figure which amounts to an equation of the eighth power; divers problems about the Tactions of spheres; and this problem found in the Mathematical Exercises of Anderson the Scot, A cube is the greatest solid of a square base that can be inscribed in a sphere, let a solidity less than that be proposed, and let it be required to find a parallelepiped of a square base equal thereto, that may be inscribed in the same sphere; the Exercises find two of a greater or lesser base, and lesser or greater length. This problem the weaver hath improved, and applied to the gauging of casks part cut below the heads, by producing the planes of the parallelepipedons, so that their sides shall cut off, (viz. each parallelepiped twelve,) second segments in the whole equal, which he distinguishes in all cases, as well when the base is a rectangle as a square. Mr. Dary, the tobacco cutter, a knowing man in algebra, hath performed this problem: Let any two plane figures, (as an ellipse and trapezium[1],) howsoever posited in a parallel position, have a right



[1] The word is not expressed, but a figure is roughly drawn in this place, which seems to have this meaning.

line extended between them in the nature of an axis, about which let a plane move round, which will cut both figures, and where on their verge or extremity they are so cut, let right lines be joined and they will be clothed with a surface; he can compute or cube the solid of given bases contained under that surface, and hence finds Mr. Oughtred's rules in the Circles of Proportion about tapering timber insufficient. I might name Wadley, a lighterman, and may acquiesce in these men's judgments, or at least in Dr. Pell's, who hath said it is unworthy the present age to continue it, as rendering easy matters obscure. Is not A^5 sooner wrote than *Aqc*? Let *A* be 2. the cube of 2 is 8, which squared is 64: one of the questions between Maghet Grisio and Gloriosus is whether $64 = Acc$ or *Aqc*. The Cartesian method tells you it is A^6, and decides the doubt.

As to the third objection, about the defect of argument, and fourth, about the improvement of the general method, they cannot properly concern the author, nor is he to be blamed for not publishing what probably he knew not, which yet, in good part, was then extant in Gerrard and Vieta de Recognitione et Emendatione Æquationum; but those works of Vieta came out piecemeal, most of them at his own dispose, and thence became almost unknown and unprocurable.

The aim of those objections was not to disparage the author, but to incline you to supply the defect of him, that his book, together with yours, might be of the more durable esteem, and not be undervalued, (as that author now is by Mr. Hooke and Dr. Croone,) as wanting the most material parts of algebra.

I agree with you, the author is not to be rejected; he was, without doubt, a very learned divine and mathematician, and one that did much good in his generation. I know no man that would willingly be without his book, and certainly it had been a great detriment to learning to have wanted it.

About the resolution of adfected equations in numbers, neither Dr. Pell, nor Albert Girard, if alive, will think it fully done unless as soon as one root is known the equation be depressed and all the rest found; on which account Albert Girard in his Invention Nouvelle en l'Algebre, printed at Amsterdam in 1629, finds fault with Stevin, Vieta, and all his predecessors, for not giving so many answers as the index of the highest power denotes. Wherewith Dr. Wallis agrees, saying he doth first find, by the doctrine of limits, whether an equation be possible, and in what bounds an answer is to be found, and then supposing a root doth by positions frame a table that shall give the first and second figure of the root, afterwards finds the rest without hesitation by the general method, and expresseth the impossible roots just as Albert Girard doth.

These impossible roots, saith Dr. Pell, ought as well to be given in number as the negative and affirmative roots, their use being to shew how much the data must be mended to make the roots possible, and give points or bounds in delineations, shewing how much a curve must pass beneath or beyond a given right line, by aid whereof the roots are found.

Now I am come to your latter letter, and will tell you the truth. I never learned Greek, nor more of Latin than an ordinary schoolboy, and do not so well understand it as to be able to espy a fault, (and because I find defects in others, you may think I have high conceits of myself,[1]) nor am I at all conceited

[1] This parenthesis has been added in an interlineation, which will account for its not harmonising well with the rest of the sentence.

of any thing I have done, nor would be sorry if they were all burned, being toys done in ignorance and haste; that which was of most use, to wit, an Introduction to Merchants' Accounts, lately reprinted, underwent the fate. In my very Paper of Interest, the precept for equations of payments will discover the time, when one sum at simple interest shall amount to any other proposed, but I deny that there can be any such thing as an equation of payments, or a purchase of an inheritance at simple interest.

I doubt not but your mechanic inventions would be very acceptable, and worth the owning. I have been troubled with smoking chimneys, but never read what Lucar in his Gunnery, or Des Cartes in his second volume of Letters, p. 504, saith as to the remedy.

CCXCIX.

COLLINS TO WALLIS.

Reverend Sir,

I received yours of the 5th instant, in answer to mine of the 2nd preceding, since the writing whereof Mr. Pitts, having conference with Mr. Thompson, a bookseller, who lived in Paul's church-yard, and now in Little Britain, was by him informed that he was importuned, since the fire, to treat with Mrs. Lichfield for the impression, who offered it for 36*l.*, and the said Mr. Thompson would not give more than 32*l.* for it, the rather because it is printed upon worse paper than the former impression; he having one of them here did also shew it. I prevailed with them to meet, being near together, and Mr. Pitts declared his unwillingness to interpose without the consent of Mr. Thompson, who offered and desired him to take what part or share thereof he would. I cannot prevail with them to bid more for it; they say it is not a book so much inquired for here as in the universities, and they both doubt it will not sell without a comment; and Mr. Thompson says he was long possessed of Mr. Clarke's comment, who would freely have imparted it to any one to print, and presumes he may have it again if he request it, and affirms it is very large, and will make above twenty sheets. And this they agree upon, if they may have liberty to print that comment here by itself, and to sell it apart or bound up with the book, as they see convenient, and that the book be not reprinted so long as they have an hundred books left, they will then give 32*l.*, and no more, for the impression. This is not different from what Mr. Thompson saith himself formerly designed, and doth think the widow will be inclinable to, forasmuch as her daughter was lately with him about it. Now as to the book itself, Dr. Croone and Mr. Colwall can attest that the late Mr. Foster of Gresham College seldom heard it mentioned, but took occasion to utter his dislike of it, and Dr. Croone hath formerly said as much to Mr. Thompson. By reason whereof, in anno 1649, I asked Mr. Foster what authors he would advise unto; he replied, that the Algebra of Schubelius, (out of which Mr. Bunning hath taken some of his notes,) Stifelius, Clavius, Dibuadius, Stevin, did fully handle the surds and Euclid's irrational lines; that Harriot, Herigone, Des Cartes, and Ghetaldus sufficiently [handle] the specious and exegetic part, not mentioning Vieta or Mr. Oughtred, whose works might then be scarce, and not so large as now, Vieta not being then in one volume.

I have likewise seen other small good compendiums, as Vaulezard's, and Hume's epitome of Vieta, Durett, Beaugrand, Henrioone, Lantz, Minher, Cardanus, in 8vo. Leotaud's Geometria Practica, which is rather a small tract of Conics and the problematic part of speculative geometry; to which might be added some I have not seen, as Andreas Arzett's Clavis Mathematicæ, in English, Billingsby's Idea, Gibson's Syntaxis, Barlaam in Dutch, which I understand not, Sybrant Hantz, Frans Vander Huips, &c. All these are in 8vo. or 12mo. In 4to. in 1661, came out Gerard Kinkhuysen's Algebra, after his analytic conics, which I presume to be an excellent introduction. He was the first author of the problem of the three sticks in Des Cartes, and hath published in his conics his own solution thereof. I shall mention one more in high Dutch, which I have not seen; Academia Algebræ.

Now concerning Mr. Anderson the weaver, a reserved person, I never had any papers or so much as a theorem from him. Mr. Leake accounts him very able in algebra and solid geometry, and one, if he may be believed, that hath exalted the fifteen problems of Vieta, of tactions of circles to tactions of spheres, who affirmed, above four years since, that he found the second segments of a sphere or spheroid by inscription of a parallelepipedon, whose sides produced cut off twelve second segments. I have met with nothing of such a tendency except in Anderson's first decade of Mathematical Exercises, at Paris, 1619; where he proposes to find a parallelepiped with a square base equal to a given solid that is less than a cube inscriptible in a sphere, and finds two such parallelepipeds the $\left\{\begin{matrix}\text{one}\\ \text{other}\end{matrix}\right\}$ of a $\left\{\begin{matrix}\text{greater}\\ \text{less}\end{matrix}\right\}$ base, $\left\{\begin{matrix}\text{less}\\ \text{greater}\end{matrix}\right\}$ height than the cube, and these being equal, their planes produced cut off twelve second segments of the one equal to twelve of the other. I want abilities to drive it home, and doubt whether any person living hath done it either before or so well as yourself, but hope my suggesting of what others, if they have done, keep to themselves may be no offence to you who will impart it. In my letter about the meridian line, I forgot to mention a curve that may be supposed to pass through every tenth degree of secant, if crossed with the extents of the length of every tenth degree of arch, the centre for the compasses being the extremity of the radius, which may be of some use in the plane chart: the query is, whether it is yet considered or treated of by geometers. Mr. Pitts nor myself have reason to think your motion unseasonable at such a time as this is, but return you hearty thanks for your trouble in it, as also for your hint that somewhat else might be preparing whilst the Clavis is selling; any thing of yours will be most acceptable to the learned, and to the stationer also, whom I hope and believe will deal candidly, and put your works, as they deserve, into a more splendid garb than those already extant. May your endeavours therefore have good success, and yourself happiness! So wisheth

your obliged and thankful servant ———

CCC.

WALLIS TO COLLINS.

Oxford, Feb. 15, 1667-8.

Sir,

I have this evening received Leotaud's Cyclomathia, which you sent me, and have read over that part of his second book, which is particularly directed against me, and some of the former propositions which by their titles seemed most to concern the business, as the 4th, 5th, 6th, 7th, (and some others,) with their demonstrations. And I find, as my last did presage, that (beside the cavilling at some phrases and expressions, which he would strain to such a sense as to make appear absurd) there is nothing material in it that seems to need any answer. The whole stress of what he saith depends on what he repeats out of Clavius, that the angle of contact and the right-lined angle are heterogeneal and not capable of proportion, which they would found on 5th Def. 5th Euclid, which they would have to be, not a definition of homogeneal quantities, (which is the whole drift of that definition,) but of such homogeneal quantities as have proportion one to another, supposing (contrary to the 4th definition) that, of homogeneal quantities, some have and some have not proportion to one another. To which I had spoken so fully in my treatise De Angulo Contactus, cap. 5, 6, 7, 8, as that the reading of those chapters is answer enough to all of this kind; of all which he takes very little or no notice. And if we should grant that to be the meaning of the definition, as he would have it, which indeed it is not, yet this would serve him, at the most, but only as to the angle of contact, not as to that of a semicircle, or other segment of a circle. For it is manifest that both a right angle may be so multiplied as to exceed any angle of a segment, and that the angle of any segment may be so multiplied as to exceed a right angle. And therefore these at least must be such homogeneal quantities as bear proportion one to another by that definition. Which granted doth directly overthrow his whole hypothesis, as himself is aware. And though I had urged this clearly and strongly enough in my Treatise, especially cap. 7, yet of this, because it did pinch too close, he takes no notice.

For all those arguments drawn from the 1 prop. 10 Euclid, and 2 prop. Archimedis de Sphæra et Cylindro, (which are the foundation of all those demonstrations, whether of the ancients or of the moderns,) which proceed, as they speak, by way of exhaustion, which do all suppose, that it is a sufficient proof of equality to prove that quantities differ less than by any assignable part, he thinks it a sufficient evasion to say, that a right angle may exceed that of a semicircle by such an excess, as is less than an infinitesima pars of either. Which evasion, if it be allowed, will as well elude all those demonstrations by way of exhaustion, or by inscription and circumscription of right-lined and curve-lined figures, so frequent amongst both the ancients and the moderns. For how easy is it to answer, if this be allowed, to that of Archimedes, for instance, de dimensione circuli, that a circle is bigger, or less if you will, than the triangle which he proposeth as equal to it, but not by any assignable quantity, but by somewhat that is less than the infinitesima pars thereof? And the like to all demonstrations of that kind.

To my last argument from optics he hath no other evasion but to deny, that in speculis curvis angulus incidentior est æqualis angulo reflectionis; who, I presume, is the first that ever did deny it. To an argument of Gregory, or Ainscomb, from that of Euclid, si auferatur plusquam dimidium, atque ex reliquo plusquam dimidium, &c., (1 El. 10.) he tells, (ad prop. 7.) that it is not to be left to the demonstrator to make such ablations by what way he thinks fit, but that his adversary is to direct in what way such ablations shall be made; which discovers so great a weakness, as if either he did not know what it is to demonstrate, or else meant, of design, to prevaricate. But I suppose he might have been engaged, unawares, in his former examen against Ainscomb or Gregory, which rather than to retract, he resolved to defend as well as he might, and was obliged to take me in by the way. But enough, at present, from

Sir,

your friend and servant,

JOHN WALLIS.

CCCI.

WALLIS TO COLLINS.

Oxford, Feb. 27, 1667–8.

Sir,

Yours, Feb. 25, I received this morning. The account which you give me of Dulaurens his Algebra answers my expectation. For by the glorious title which was represented, and the great weight laid upon a slight problem, and that so lamely proposed that it was hard to pick out what he meant, made me think there was no great matter to be expected from him. The problem you mention about the length of a line cutting an ellipse given in specie, and cutting one of the axes with an angle given, you will observe to be none of mine, but one proposed, it seems, by such another algebraist as Dulaurens. And it may be solved by the same method in an hyperbola, as I have done in the ellipse, with little more than changing the signs + and – : and in a parabola still more easily. The straightening a curve was done by Mr. Neil, (and after him by Dr. Wren and my Lord Brounker,) a good while before Henrat, (and I suppose both proceeded upon the grounds I mentioned in my Arithm. Infinit.;) and it was commonly known to divers of our English mathematicians before Henrat's came abroad.

With my answers to Leotaud's cavils I perceive you are satisfied. I confess I thought Leotaud (though I had not read much of him) had been a better geometer till I saw that piece. But since I find that he assents to opinions in geometry "venerationis gratia," I shall take him to be as orthodox in geometry as in divinity, confining his opinions in the one to father Clavius, as in the other to the Pope's authority. Of Slusius I have a much better opinion, and did you not tell me that he is reprinting this summer, I should have desired you to have bought his works for me, (because [I] have them not,) and sent them down. But I think I shall see you at London about the middle of Easter term. Fabri's Optics I have not yet had leisure to examine. I doubt also he admits much in mathematics "venerationis gratia," as well as Leotaud, which makes him insist on the earth's stability, but it is in him more pardonable, because here the Pope's authority is interposed, having condemned the Copernican hypothesis of heresy; but whether an angle of contact and of a circular segment be homogeneous or heterogeneous to right-lined angles, the Pope hath not yet determined.

Mr. Gregory's piece about the quadrature of the circle, ellipse, and hyperbola, I have looked over; and for aught I discover, upon a slight view of it, (not having strictly examined every proposition,) it seems to be truly enough performed according to the method proposed, and his prop. 32 shews his way of calculating an hyperbolic space. I am of your opinion that it would advance the sale [of] the book to have a clear and easy method laid down for the operation, for any space proposed; but I think it best to be done by himself, who is most master of his own notion therein, and hath already (I presume) considered of abbreviating methods; for to calculate a table after the method of this 32d proposition would be a long work.

What you mention of solving equations of the third, fourth, or superior degrees trigonometrically, I do well enough approve of. For the process of angular sections, resolving themselves into such equations, the one will be but the inverse of the other. This at present from

yours, &c.

JOHN WALLIS.

CCCII.

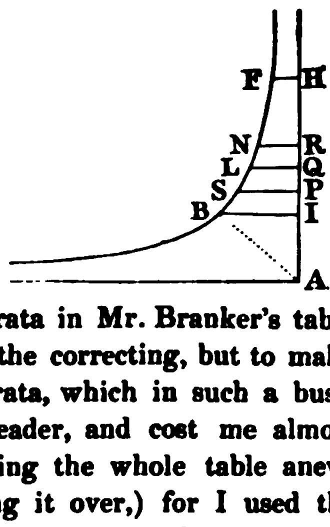
WALLIS TO COLLINS.

Oxford, July 21, 1668.

Sir,

I received yours of July 14, and thank you. What I wrote of Mr. Mercator was not intended to his disadvantage at all, but the contrary: for though I know him not, I have a good esteem for him. What concerns the logarithms I did so far only consider, (as I read it over once in some haste,) whether what was done were well, and it seemed to me so to be, and so much as would be sufficient for a good expedite way of calculating logarithms; but I did not so study it as to consider whether or how it might be further improved, for the whole was but an afternoon's work.

Only having found the quadrature of the hyperbola a little lame, (that is, not so full as [I] wished it were,) I did that night consider how it might be improved, and the next morning wrote to Mr. Oldenburg, because I was unwilling to leave to foreigners the perfecting of that which was by ours carried on so far. That this expedient in squaring the hyperbola must need afford a proportional help in the logarithms is evident, because they depend one upon the other; and his last proposition, of finding a sum of logarithms, may be much expedited from some other principles that I have not room here to mention. In sum, if (as I put it) we make AH = 1, AI = IB = b, HI = A, and the plane BIHF = pl; then is $pl - b^2 + b^3 =$ BIPS + BIQL + BIRN, &c., to BIHF.



What I wrote of the errata in Mr. Branker's table was not to find fault with the correcting, but to make a supply of his table of errata, which in such a business is material to the reader, and cost me almost as much pains as calculating the whole table anew, (saving the time of writing it over,) for I used the same method to examine as I would have done to calculate a new one. I can say nothing to the comparing of Mr. Gregory's with that [of] M. Huygens, because I do not remember that I have seen that of Huygens; but wonder that Huygens should write against him unprovoked, being himself an ingenious modest man, as I have hitherto apprehended him; and certainly Gregory's piece is not contemptible. The other particulars you mention of Mr. Gregory's are good, and I suppose likely enough to be true.

In printing my things, I had rather you make use of Mr. Oughtred's note of multiplication, ×, than that of ✳; the other being the more simple. And if it be thought apt to be mistaken for X, it may [be] helped by making the upper and lower angles more obtuse ⋈.

The post is going, so that I can add no more, but that I am

yours, &c.,

JOHN WALLIS.

CCCIII.

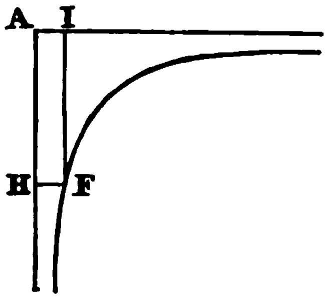
WALLIS TO COLLINS.

Oxford, Aug. 25, 1668.

Sir,

I received yours of the 15th instant. I shall be very glad of any happiness for Mr. O., and that you mention in particular, if your news hold. I perceive they make no great haste at the press, and I wish it were more; for it is no advantage either to the bookseller or the book to hang too long in hand. The running title on the several heads is easily added, being the same with that of each chapter, which being found in the beginning of the chapter, serves all along till the next chapter. Only the number of the proposition handled in that page is to be changed.

The rule you mention, out of the French Transactions, though I have not particularly examined it, seems to me doubtful; for by that it will not be possible that any such hyperbolic space can be less than (or so little as) the number answering to the logarithm 0.3622156868 continually added, (unless I misapprehend your meaning;) the parallelogram A H F I, (which I suppose you mean by the parallelogram of the hyperbola, as you describe it,) being in all places equal, and therefore its measure, 1.0000000000, being a standing measure.



Pray let them make what dispatch may be at the press, which (do what they can) will be considerably retarded by passing of letters and papers to and fro: which is that which I would have prevented, (beside your own trouble,) by having it done here if it had been convenient. It shall stay with me as little as is possible. I am obliged to you for your care; and rest

yours, &c.

JOHN WALLIS.

CCCIV.

WALLIS TO COLLINS.

Oxford, Sept. 8, 1668.

Sir,

I do not return the sheet sent by the last post, because there is not much to be altered; and therefore I would not charge the postage. But,

Pag. 20, line 11, for "continuais," put "continuatis."

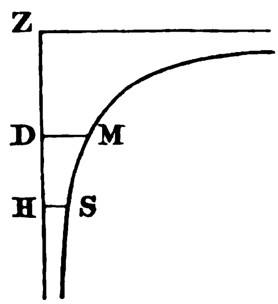
Pag. 22, at Prop. 16, I would have I: : in three places taken down from the first line into the third; and in the end put out *mn* before I[m]; and in the mar-

[m] Each of these alterations is particularly stated; but it seemed sufficient to print the general description.

gin, fig. 14, 15, (supposing that both figures are to be cited, as, I think, here they are); so at Prop. 17, page 23, (if the three figures are to be cited,) let it be fig. 16, 17, 19. But I am very suspicious of mistakes in the citations of figures in the margin, which made me desire in my last that you would look over the copy, and compare it with the draughts of the schemes, to see whether they answer aright to them or no. For I have no means here to compare them.

I do not understand why the sign of multiplication × should more trouble the convenient placing of the fractions than the other signs + − = > : : .

As to other things in some of your late letters. I find that M. Huygens his rule, for the measure of an hyperbolic space, is the same with Mr. Barrow's, as I have signified in a letter to M. Huygens, which perhaps my Lord Brounker hath shewed you. For whereas one mentions the difference of logarithms of ZD, ZH, the other of HS, DM, it makes no alteration; for the proposition being in both the same, the difference of logarithms will be so too. And I find Huygens's constant logarithm 0.3622156868 to be the logarithm of 2.3025853, which is the termination of what you call elementum logarithmicum. Anderson's book [which] you mention, is not yet come to hand. That which I could wish altered in Mr. Mercator's logarithmotechnia was his manner of expression of the composition of ratios. For composition be a word used by Euclid sometimes for addition, sometimes for multiplication, and there being in him two compositions of ratios, the one mentioned in Def. 14, Lib. 5, which is by addition



of the exponents, as when $\frac{3}{2}+\frac{2}{2}=\frac{5}{2}$, the other by multiplication of the exponents, Def. 5, Lib. 6, as where $\frac{3}{2}\times\frac{2}{2}=\frac{6}{4}$, which ambiguity hath caused some confusion, especially where the latter is called an addition of ratios. Clavius[n], and Gregory St. Vincent, and divers others, to avoid this inconvenience, have, for distinction sake, called the former composition by addition, the latter composition by multiplication; with which most writers, who speak distinctly, have used to comply: of which I have spoken at large in what I have writ against Meibomius, and against Mr. Hobbes's fourth dialogue, and elsewhere. Now Mr. Mercator resuming the old term of addition,) which I could wish antiquated, (for that which is indeed a multiplication, reduceth us to the same confusion, which hath been endeavoured by distinguishing to be avoided. For certainly sextuplex is as much the double of triplex as 6 is the double of 3: not an addition in either place, but a multiplication. And what Euclid means by proportion duplicate, triplicate, quadruplicate, &c. Def. 10. Lib. 5, is but what we now call by other names, square, cube, square-square, &c. And consequently what in Mr. Mercator is called numerus rationum I should rather have called exponentem potestatis exposite rationis, and consonantly elsewhere, which differs only in expression, not in substance, from what he means all along. And so, though 0 be indeed the exponens potestatis (pro indice exposita) æqualitatis; yet not 0 but 1 is exponens rationis æqualitatis, as

[n] Against this part of the letter there is written in the margin,

-3	-2	-1	0	1	2	3
$\frac{1}{r^3}$	$\frac{1}{r^2}$	$\frac{1}{r}$	$\frac{1}{1}$	$\frac{r}{1}$	$\frac{r^2}{1}$	$\frac{r^3}{1}$

2 is the exponent of duplicity. Which, if you please, you may shew him, that he do not misapprehend me.

What you say of the French possibly pretending that Mercator's quadrature was grounded on Lalovera, there is no reason of that pretence, there being much more reason to say that Lalovera's (if the same for substance) was grounded on my Arithmetica Infinitorum, where, in Prop. 88 and 95, the ground is laid geometrically, and the rest but an arithmetical calculation deduced from it, as you see in my letter about that quadrature. But that book is a book, which the French, though it's like enough they make good use of it, do not desire the world should take notice of. That the rectifying a parabolical line and squaring the hyperbola do mutually give one another is no new thing, but what hath been discoursed between me and M. Huygens by letters many years ago; the whole mystery of rectifying curve lines being laid down in my Scholia ad Prop. 38. Arithm. Infin., and further prosecuted in my Cycloide, pag. 90, &c. And I have not yet seen any thing of that kind, but what are but particular instances of what is there delivered in general.

Mr. Mercator's instruments, mentioned in yours about a month ago, I have yet not spoken of to Dr. Wren, (having but once had opportunity, and then I forgot it,) but he being then in London, and for a good while after, I suppose you had there opportunity to discourse it with him.

As to Mr. Gregory, I do admit to be demonstrated, in Prop. 11, that which he there undertakes, viz. that the sector ABIP, is not (as he speaks) analytically composed of the triangle ABP and the trapezium ABFP, or that the converging series there proposed is not analytically composed, and therefore not capable of such a determination. But I do not yet find, (though

I have again sought for it) that Prop. 10 or 11, or any where else, he doth offer any demonstration, that if ABIP be not analytically composed of ABP and ABFP, it is not possible there can be any other converging series imagined, of which it may be so composed; or that, in case such a converging series cannot be found, it is not possible that there can be any other way of squaring the circle by analytical operations. For though it is like enough the thing may be true, (and therefore I do not reject it,) yet, I say, I do not see where he doth offer to demonstrate it: and in his epistle, pag. 5, he seems to me to intimate so much, " verum est me hanc demonstrationem integram ad phrasem generalem geometricorum non reduxisse." And therefore I did think it was not to be exacted of him to have demonstrated what he did not undertake to demonstrate; which was a sufficient answer to M. Huygens his exceptions. But if it be any where done I shall be willing enough to be shewed it; that is, the strength of this consequence demonstrated,—— If ABIP be not analytically composed of ABP and ABFP, then it is not possible there can be any other way of giving a straight-lined figure equal thereunto by analytical operations, viz. addition, subtraction, multiplication, division, and extraction of roots. This at present from,

Sir,

yours, &c.

JOHN WALLIS.

CCCV.

WALLIS TO COLLINS.

Oxford, Sept. 10, 1668.

Sir,

The business of this is to desire you to do me the favour to transcribe two or three propositions out of my papers, (which, I suppose, are with my Lord Brounker,) to send away with the enclosed letter to Lalovera. They are those about the cycloid, and are (as I remember) Prop. 18, 20, and 21, De calculo centri gravitatis, which in those papers is called the fifth chapter, if I mistake not the numbers: you will however find them to be these. The first of them gives the measures and centre of gravity of the cycloidal plane and the parts thereof, after which is another of the figure of sines, right and versed, or the solids thereof, which you may omit. Then the next, Of the measures and centres of gravity of the solids of the cycloid and its parts. The last, Of the surfaces of those solids. These you may please to transcribe (carefully) with these titles, De Calculo Centri Gravitatis Prop. 18, and so of the rest. They will contain (I suppose) about two sheets of paper, or more; and copy out the scheme which refers to them, (I think one scheme serves them all). Only for the explication of the symbols, if in the first of these three propositions they be not expressed, but only with a general reference, Retentis symbolis ut in præcedentibus aliquot propositionibus, you must then there add the explication; Nempe, &c., and take them out of the former proposition. I think they are these: Diameter $A\alpha =$

$2R$, peripheria $ABaA = P$, arcus $BA = a$, $Ba = \alpha$, chorda $BA = c$, $Ba = \chi$, sinus rectus $BV = s$, versus $AV = v$, $Va = 2R - v = y$, $VC = x$, $a + s = f$, $a - s = e$; if there be any more pray add them. I shall send you, by Moor the carrier, one of my books De Cycloide, to be sent with it, that being too big to come by the post. If the thing be despatched time enough, it may be sent by the same hand that carries my letter to M. Huygens, which perhaps my Lord Brounker may have shewed you.

One Theodorus Riccius, who lies at Mr. Edward Roberts's house, near York House, and takes in post letters; this Riccius is very suddenly going directly to Paris, if not gone already.

My sending of these to Lalovera, is to let him see that we had not our numbers from him, though possibly they may agree with his; which they must do if both be right. I do not know whether my Lord Brounker [may] have yet had time to examine those parts of my papers where these propositions are contained. If he see reason not to send these papers, or not yet, I shall therein be guided by him. While I am writing this, I receive Anderson's book of Gauging. What the geometry of it may be, I cannot tell; but the language is barbarous enough, I see at the first view. Excuse this trouble given you by,

Sir,

your loving friend,

JOHN WALLIS.

I wrote to you by the last post, directed to Mr. Pitts's house.

CCCVI.

WALLIS TO COLLINS.

Sept. 26, 1668. Oxford.

Sir,

When I opened yours of Sept. 22, I thought I had had two new sheets, but when I considered them, they were none of what I expected, but one, E, (which I had seen before,) and one of Mr. Gregory. I have (of my own) B, C, and E, as they be wrought off, but not D. I can hardly be of your opinion that they (at this rate) [will] have finished fifty sheets before next Easter term.

I am glad to hear that Dr. Pell promiseth better rules for the logarithms than those of Mr. Mercator. But, in all tabular operations of this kind, we must be content with a prope verum; for numbers will not admit of an accuratè.

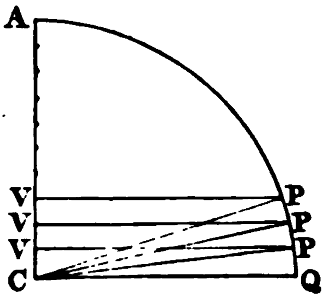
Mr. Gregory's piece, that you send the first sheet of, deals pretty roundly with Hugenius. I wish yet that he will consider whether there be not a mistake, when he intimates that Hugenius challengeth that quadrature of the hyperbola to have been his. I confess his words seem to speak roundly so much, but I took his meaning to have been, not to challenge this quadrature of the hyperbola to be his, which is the same with that of the circle and ellipse, to which certainly he can make no pretence that I know of; but only, that the connexion of the hyperbola's quadrature with the business of the logarithms was before known to him, which may be true enough; for it hath been known likewise to a great many more, ever since the book of Gregorii de Sancto Vincentio. And it was in print in my Commercium Epistolicum, Epist. 39, 40, long before the Royal Assembly was in being, and therefore before M. Hugenius can pretend to have discovered it to the Royal Assembly, not as a thing then new or first discovered by me, but as a thing sufficiently known; and I quote St. Vincent for it. Nor is that it (I suppose) which Mr. Gregory, or Mr. Mercator, pretend to be new, in what they now publish. For I suppose neither of them are ignorant that this was formerly known, nor Mons^{r}. Hugenius neither. And yet I think there is nothing else to which he can make any pretence; at least as I did favourably understand his words. If he pretend to more of it, I doubt he is the more to blame. What he saith was by him made known to the Royal Assembly about the weight of the air, with respect to this of the hyperbola's quadrature, I know nothing of it, nor do I remember that I ever heard any word spoken to that purpose, and so can say nothing to it.

There is no haste of sending to Lalovera, till we hear whether he be dead or alive. Yet I could be content I had one of his books, De Cycloide, for myself, for that I have must be returned to Mr. Oldenburg, and of his Tetragonismus, (for I have neither of them,) if they be to be had. I do not find but his method (for the main) may be sound, but perplex enough, and his figures not clear, nor so fitted as to be understood without difficulty; and in some of his calculations there be some mistakes; but I believe I shall hardly take the pains to examine his methods so particularly as to make myself master of them, because I think I have methods of my own much more clear and easy; and I believe you will think so by that time what you have of mine is printed. As for my pieces, if those who are concerned in the copies of them will print all or any of them single, I am not averse from it; but I cannot well do it without them.

What you ask of the mistakes of Anderson, I confess I did not so narrowly examine it as to note the mistakes as I went, reading it very cursorily over at once before I laid it by; and though in the reading I took notice of some, yet I did neither charge my memory with them, nor write them down. That of the parallelogram to the parabola as 4 to 3, which should have been as 3 to 2, I remember (now you mention it) that I did take notice of; and of that in the beginning of pag. 69, which is true in parabolas, (meaning it, as I suppose he doth, of a segment cut off by a straight line,) but not of the hyperbolas or ellipses. Some cases there are in which it may be true of these, but not universally. He might as well have said that segments of circles, for the circle is but one species of an ellipse, as a square is of a parallelogram, if on the same base, are as their altitudes, and if of the same altitude, they are as their bases, and consequently segments of circles (how unlike soever) in proportion compounded of that of their bases and that of their altitude. But it is much otherwise.

As to what Mr. Gregory desires, I know not what to say more than I have done: you know what I have said to you, and, I suppose, you saw what I say to Hugenius himself; That, at the eleventh proposition, against which he excepts, as much is demonstrated as is affirmed, and that more was not there to be expected, &c. I cannot say it was there proved, (for it is not so much as affirmed,) that there can be no other way of squaring the circle analytically. Nor when M. Huygens makes this objection is there any supplement to that purpose in the reply. The paper which he sent me, how cogent soever, is no part of what is either in the book or in the reply. To the truth of the proposition I have made no exception. The strength of both the arguments in the last paper I had considered before he had sent it; for I apprehended the substance of it, suggested to my thoughts from what is said in his preface, pag. 5, Sicut numeri fracti nunquam procedent ex integrorum additione, &c. sed tantum ex divisione; et numeri incommensurabiles, &c. tantum ex radicum extractione, ita quantitates non analyticas nunquam ex analyticarum additione, subductione, multiplicatione, divisione, radicum extractione, sed ex sexta hac operatione, &c. Whence I easily foresaw it might be argued, in like manner as, quorum alterum est, alterum non est cum eodem tertio commensurabile, ea non sunt commensurabilia inter se: so also, quorum alterum est, alterum non est cum eodem tertio synanalyticum, ea nec ita sunt inter se; which is the foundation of both his arguments, and may be of most. This, because it seemed to me a reasonable postulatum, so as that I saw not any thing solidly to be objected against, and the consequences from thence such as I knew not how to avoid, I did (without his deducing the demonstration) say that I saw nothing to the contrary but that the proposition might be true. And of the same mind I am like to be, till either I do discover myself, or somebody else shews me, somewhat to the contrary that as yet I see not. If to what he is now printing he think fit to add the contents of his last paper, and if withal he add that the strength of it was already in his preface, in the place to which I refer, (which I think all that is in the whole book to that purpose,) it will be a good additional to his former reply. And if Hugenius rest satisfied with[out] making any further reply, I think Mr. Gregory hath done his work, and hath the better end of the staff. After this is done, I think it will not be necessary for others to appear as seconds in the business, unless upon further provocation; because it may occasion further heats and animosities between the two societies, (that of ours and that of theirs,) than the business doth deserve, and seeking occasions of revenge and disparaging each other, which may prove of ill consequence. What I have said to M. Huygens myself may possibly have as good an influence on him, as if I should more reproach him in public; yet, if my Lord Brounker think fit, (who in the first testimony is as much or more concerned as I,) that something briefly to this purpose be inserted, That the persons concerned in the former testimony of that book, notwithstanding the objections made by M. Huygens, to which Mr. Gregory hath made his reply, do not see any cause to recede from their former favourable opinion of it, I shall not be averse from it. But I think there will be no need of it, and perhaps the issue may be as well without it.

About the segment of circles; it's true I give no other measure of them, than by the versed sine, the right sine, and the arch and semidiameter. You say that you take nothing as known (besides the radius, for so I understand you) but the versed sine. But this being given, the right sine is known also ($s^2 = 2vR - v^2$) and then the arch by the tables of sines. But if you would have a table computed for segments of spheres and circles, according to the several proportions of the versed sines, I know of no more expedient way at present than this. Supposing the radius CA divided into any number of equal parts. Suppose 10, 100, 1000, 10000, or as many as you please, CV, VV, &c. and consequently CV, CV, CV. &c, are 1, 2, 3, &c. of such parts, and therefore squares of VP, VP, VP, &c., (which represent the circles of the hemisphere,) are R^2-1, R^2-4, R^2-9, &c.; and the aggregate of all them, (multiplied into one of those equal parts of the radius,) or so many of them as you shall need,



represents the whole hemisphere, or such a part thereof as you need: that is, they are thereunto as the square of radius to the circle[m]. And the square roots of them $\sqrt{R^2-1}$, $\sqrt{R^2-4}$, $\sqrt{R^2-9}$, &c. are the lines VP, and the aggregates of them, or so many of them as you need, (multiplied by the altitude of one such part,) gives you the area of the whole quadrant, or such a part as you need. The table is easily calculated, wanting for each place but one extraction of the square root, and one addition of two numbers, after a subduction of the successive squares out of the square of radius. And the result of the work will be either greater or less than the just, according as you take the inscribed or circumscribed figure; and the middle between both will be truer than either. I add here a specimen.

The remainder of this letter does not appear in the collection; it was probably written on the part which contained the address, which is also missing; but there can be little doubt of its having been intended for Collins.

[m] The meaning of the writer is by no means clearly expressed, and there was the more difficulty in ascertaining it from inaccuracy in the MS. This has been corrected; and he seems to wish to convey, that the sum of the solids is to the volume of the hemisphere as $1:\pi$.

CCCVII.

WALLIS TO COLLINS.

Oxford, Novemb. 3, 1668.

Sir,

I thank you for yours of Octob. 26, and the book you sent with it, which I received last Saturday toward night. I thought to have sent you the enclosed by the last post, but before I had transcribed it, (thinking fit to keep a copy of it,) I was otherwise diverted. Mr. Gregory is certainly in the wrong, and therefore I am sorry to see him write at that rate he doth. And I could have wished, but that it is now too late, that his angry preface and the first leaf of his book had been suppressed. I wrote him a large letter of Octob. 22, when I knew not of this preface, enclosed in one to my Lord Brounker. Whether it came to his hands before he went out of town I know not. If not, I desire you will send it after him, with this enclosed, which you may, if you have opportunity, shew my Lord before it goes. I would be content to have his Lordship's sense upon the former, and upon this whole matter.

The series for the circle, answerable to that of the hyperbola, is the same which I sent you a while since. For that, which in the ellipse answers to the asymptotes of the hyperbola, are the two equal diameters, which in the circle are any two cross diameters intersecting at right angles, answering to asymptotes so crossing. And the ordinates to one of these diameters answer to the ordinates to the asymptotes, terminated in the curve.

I was not against printing a comment on the Clavis Math., if any think fit to do it; but only that I thought it not necessary, and would swell a manual into a volume. What piece I shall else print I have not yet determined; but an Introduction to Algebra I have not yet ready. That at London gets on so slowly, that, if I had been aware of it, I would never have given way to print it there; and I doubt I must yet be forced to have it finished here. The post is going, and I can add no more, but that I am

yours, &c.

J. W.

CCCVIII.

WALLIS TO COLLINS.

Jan 19, 1668–9.

Sir,

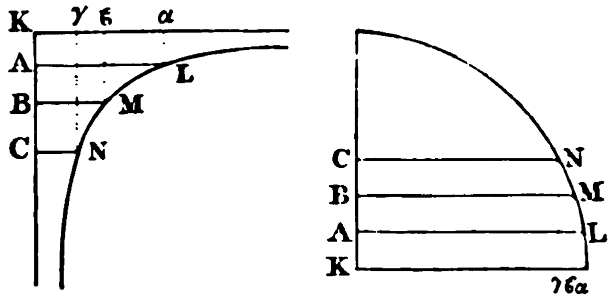
What I suppose Mr. Oldenburg intends in the next Transactions, though it contain divers of the principles on which I proceed in my hypothesis of motion, is not intended as any summary of my book now printing; nor is it at all in that method, however upon the same principles. To the other question, the chapter De centro gravitatis must stand where it doth, and is not to be removed. The next, De calculo centri gravitatis, though I once thought of taking it out there, and putting it by itself; yet considering that cannot be done without altering the numbers of the figures, and, in pursuance of that, going over the whole work anew, in which the figures are cited over and over again many times in the same page, it would make so much work to make that alteration all along, and would be subject to so many mistakes, that I think there will be a necessity of letting it stand where it doth, and proceeding in some following chapters as they were at first designed, without dividing it into two parts. But what you speak of putting out these three or four chapters alone cannot at all be; they being necessarily connected with what is to follow, de vecte, cochlea, trochea, tympano, &c., and that de motuum acceleratione et percussione, which must all go together, because they frequently cite propositions out of the precedent chapters. If that De calculo centri gravitatis be taken out, it is all [that] can be done, and that not without much trouble for the reasons mentioned. The series of which you inquire in pag. 398 of Lalovera de cycloide I have looked upon, but it's complicated so with other things, that I see not how to give an account of it without reading over most of the book, nor can it well be otherwise understood; which at present, having many things on my hands at once, I am not in a capacity to do. And I should rather give an account of the things from my own principles than study to be perfect in his; his whole method all along being somewhat perplexed. Though (because I find the results for the most part agree with mine) I take it to be sound though dark. But the general design [of] these I take to be to shew how by having the sum of lines, making the plane of those figures, the circle and hyperbola, he proceeds to a sum of squares to find the solid ungula, or the moment of that plane; and so to the sums of cubes, to find the moment of that ungle, and so on. Or, which is equivalent, from the squaring of a plane, whose lines are as the lines in an hyperbola or circle, to the squaring of a second, third, fourth, &c. plane, whose lines are in the duplicate, triplicate, quadruplicate, &c. proportion of those lines, which is the method he prosecutes throughout his book. But I have not taken the pains to make myself master of those methods, because those of my own are, to me at least, less perplexed and more simple than those of his, and bring about the same effects with more ease and clearness. If to others it shall seem otherwise, they will have their choice to use which they please.

But his design there is not at all like that series which we have about the hyperbola, which I suppose you aim at. You may, with my humble service to him, signify so much to my Lord Brounker, from

your friend to serve you,

JOH. WALLIS.

I wrote to you by the last post in answer to a former letter. The series in that letter designed, is the series for the circle answering to that we have for the hyperbola.



The centre in each being K, the asymptotes in the hyperbola answering to the two conjugate equal diameters in the ellipsis or circle; and the points A, B, C, &c. in one, corresponding to those in the other; the rectangles Aα, Bβ, Cγ, &c. in the one being equal, suppose B^2, B^2, B^2, or $\frac{B^2}{a}a$, $\frac{B^2}{b}b$, $\frac{B^2}{c}c$, &c.: in the other, $a\sqrt{B^2-a^2}$, $b\sqrt{B^2-b^2}$, $c\sqrt{B^2-c^2}$, &c. supposing the angles at K to be right angles. And the lines AL, BM, CN, &c., in the one are $\frac{B^2}{a}$, $\frac{B^2}{b}$, $\frac{B^2}{c}$, &c. in the other $\sqrt{B^2-a^2}$, $\sqrt{B^2-b^2}$, $\sqrt{B^2-c^2}$, &c. And for the calcule of the consequent parallelograms KL, AM, BN, &c. you are in the one to divide, in the other to extract a square root, and then to multiply the result into one of those parts KA, or AB.

CCCIX.

WALLIS TO COLLINS.

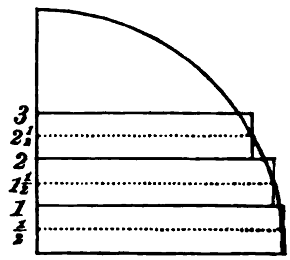
Jan. 1668-9. Oxon.

Sir,

To yours of Jan. 12 I had before answered, as to so much as concerns Mr. Gregory's letter, in mine of Jan. 8, sent in a parcel directed to Mr. Pitts. And I have since sent his letter up to Mr. Oldenburg enclosed in one of Jan. 12, with an answer. As to Dr. Newton's design of calculating a table for segments of circles, I do not know any more convenient or expedite way than according to a specimen, which I have heretofore sent you. What may best serve the gauger's turn I shall not determine; but otherwise I should think it much better to make the radius equal to 1, with as many places as you please for decimal fractions, and consequently the square of radius, rather than the semicircle[n], equal to 1. And then, for every number in your table, you will need but one subduction, one addition, and one extraction of the square root to a very few places, which, to those versed in it, is done

[n] The semicircular *area,* not the arc, is intended: this appears by what follows.

presently, and sooner than so much of division. And when he is a little onward in his work, he will presently see how great leaps he may safely take without danger of missing one unit in what decimal place he please. So that I do not question but he may dispatch a great many places in a very short time, without any rectification by the third, fourth, or fifth differences, which, in so quick a work as this is, I think will be of very little advantage. Yet if he would go the other way to work, he must, at convenient distances, make essays whether the first, second, third, fourth, or fifth differences will serve the turn; and then it will be easy to apply them in such manner as Torporley, if I remember the name aright, in the manuscript you shewed me, or as Mercator, in his Logarithmotechnia, prop. 12, directs. A table thus computed is easily applied to the other hypothesis of making the semicircle equal to 1, by dividing any place you have occasion to use, or all, if you would so reduce the whole table, by 1.57080, the proportion of the semicircle to the square of radius being as 1.57080 (proxime) to 1. I should only direct this alteration in the former directions of September last, that whereas I take the ordinates in the quadrant $=\sqrt{R^2-a^2}$, and take a successively $=1$, 2, 3, &c. or $=0$, 1, 2, 3, &c., the one answering to the circumscribed figure, the other to the inscribed, it will be more accurate to take it $=\frac{1}{2}$, $1\frac{1}{2}$, $2\frac{1}{2}$, $3\frac{1}{2}$, &c., which will be the middle lines of the parallelograms partly inscribed and part circumscribed. As for instance; if $R=1$, and this [is] divided into 10000 parts; then $a=0.00005$, $a=0.00015$, $a=0.00025$, &c. And then each of these $\sqrt{R^2-a^2}$ multiplied in the altitude of one such part, that is into 0.0001, gives you the parallelogram, partly inscribed part circumscribed, answering to the first, second, third, fourth, &c. parts of the erect radius, reckoned from the transverse radius, or the base. And when you have finished the table for all the $\sqrt{R^2-a^2}$, you may begin to collect the aggregates, either from the top or from the bottom, which you please; and the aggregates, (because to be multiplied by 0.0001,) are to be depressed four places downwards in the table. I have given here a short specimen, which is easy to continue at pleasure. I add only that I am



your friend to serve you.

JOHN WALLIS.

R^2-a^2	$\sqrt{R^2-a^2}$
0.9999999975	0.99999999875
0.9999999775	0.99999998875
0.9999999375	0.99999996875
0.9999998775	0.99999993875
0.9999997975	0.99999989875
0.9999996975	0.99999984875
0.9999995775	0.99999978875
&c.	&c.

Summa in 0.0001.	Numeri tabulares.	Alt.
0.99999999875	0.000099999999875	0.0001
1.99999998750	0.000199999998750	0.0002
2.99999995625	0.000299999995625	0.0003
3.99999989500	0.000399999989500	0.0004
4.99999979375	0.000499999979375	0.0005
5.99999964250	0.000599999964250	0.0006
6.99999943125	0.000699999943125	0.0007
&c.	&c.	

CCCX.

WALLIS TO COLLINS.

Oxford, March 23, 1668–9.

Sir,

I gave no answer to your former letter, because I have yet had no time to prosecute that inquiry; and for the same reason I can say little to that I received yesterday. Your notion therein, as to the particular small parts, is true; but it will not hold as to the aggregate of such parts. For though it be true, that as each of the (sines or their small parallelograms) GH, to GK, so GK, to the respective (tangent or its little parallelogram) GF; yet it will not follow, that therefore all the GH's, to as many GK's, so those GK's, to all the GF's, the proportions of GH to GK being every where various.

It is	1	:	100	::	100	:	10000
	2	:	100	::	100	:	5000
	3	:	100	::	100	:	$3333\frac{1}{3}$
	4	:	100	::	100	:	2500
But not	10	:	400	::	400	:	$20833\frac{1}{3}$

So that I do not yet see how to make that advantage which seems to be designed hereby. As to Mr. Hobbes's undertakings, it will be time to speak to them when I see the performance.

I have sent this morning, by More's wagon, a bundle of books directed to Mr. Pitts, in which is contained as is below expressed. You may please to consider of them with Mr. Pitts what price to put upon them; or of a like value in other books. I find, since I came home, that I had some of my Cycloides left; so that you need not be solicitous of returning me that you had. But I wish I had the two papers of cuts and the printed sheets after N, which is the last I have. I am, Sir,

your friend to serve you,

JOHN WALLIS.

5. Opera Mathematica, pars prima { Oratio inauguralis. Opus arithmeticum. Contra Meibomium.

9. Pars altera .... { De angulo contactus. De sectionibus conicis. Arithmetica infinitorum. Observatio eclipseos.

12. Commercium epistolicum.
4. De cycloide, &c.
8. Elenchus Geometriæ Hobbianæ.
6. Due correction, &c.
3. Hobbiani puncti dispunctio.

To each of these works is added, the size in which it is printed, the description of the types, number of sheets, &c.

CCCXI.

COLLINS TO WALLIS.

June 17, 1669.

Reverend Sir,

I yesterday left with Mr. Pitts to be sent to you, (and he saith it was delivered to the Oxford wagoner, that went on this morning,) to wit, Dr. Newton's book of gauging, and Anderson's three sheets, which be pleased to accept. Dr. Newton is said to have made 600 numbers more in readiness for the further enlargement of his table. When you write to Mr. Peter Schooten I should be glad you would vouchsafe to put a few queries to him, to wit, What became of Golius his manuscripts, particularly of Vieta's Harmonicon Cœleste, and some remains of Anderson the Scot, which were sent by Sir Alexander Hume over thither to be printed; what mathematical book it is that Hudden hath now in the press at Amsterdam; whether Vossius intends to enlarge his father's book De Scriptoribus Mathematicis, (which the preface saith is the most crude piece that ever Gerard Vossius published;) how he approves of those books here unknown and not to be had, viz. one entitled Slot en Sleutel, (the lock and key,) and of Martin Wilkins his Officina Algebræ, Belgice, which hath above three hundred problems solved in it; and whether the said Schooten will take care to buy, procure, and leave with such persons, as he shall be directed to, some few such copies as I possibly, for the use of the Society, may send to him for; his money shall be advanced or reimbursed to him either at Amsterdam or Middleburgh. One John Jacob Ferguson hath lately, in 1667, at the Hague, published, in low Dutch, a book entitled Labyrinthus Algebræ, wherein he solves cubic and biquadratic equations by such new methods as render the roots in their proper species, when it may be done, to wit, in whole or mixed numbers, fractions, or surds, either simple, compound, or universal, and likewise improves the general method; thereby accomplishing as much as Hudden, in annexis Geometriæ Cartesianæ, seemed to promise about it. This part is translated by Mr. Old, and by me almost transcribed, which, annexed to Kinkhuysen's Introduction, (with your help, advice, or assistance, which the Lord Brounker may possibly crave, the books being translated at his Lordship's desire,) will render the said Introduction very acceptable. I hope, ere long, to send you both to peruse in the * * *

CCCXII.

WALLIS TO COLLINS.

Oxford, June 24, 1669.

Sir,

I received and thank you for the two books you sent, and did yesterday look them slightly over, and find divers things proper enough for the subjects they handle. In that of Dr. N. I find the press was not well corrected, by reason of which many errata have scaped, as well as some mistakes in the calculation, divers of which are amended in the table of errata. But I find (as I did suspect by Dr. N.'s discourse with me, and told him of it) that, in his table of segments, he did not take his data precisely enough to bear a calculation of so many places. For in his table of ten places the three last figures are not accurate; as I found by examining some few by accurate computation. As for instance,

At number		instead of	the accurate numbers are
versed sine..	250	0.1955011110	0.195501109478 −
	750	0.8044988890	0.804498890522 +
	490	0.4872684781	0.487268452900 +
	510	0.5127315219	0.512731547100 −

which agree but to seven places of decimals: supposing the whole diameter =1000 and the circle =1, which are his suppositions.

But however seven places are as much as the usual tables of sines, &c. reach unto, and is abundantly sufficient for the business it is designed for.

I find a small mistake in his preparatory table, in pag. 2, 3, of the sheet K, next before that of segments, where numbers should have been

First number 0.01745329252 – not 0.01745329259
Last number 1.74532925199 + 1.74532925900

and all the intermediate numbers proportionable.

It's possible this small undiscovered mistake in taking the first number of this table might influence the whole table of segments which follows, if (as I supposed he did) he calculated that table upon the credit of this. It were not amiss, if he were advertised of it before he proceed with the rest of his numbers.

When I write next to Van Schooten I will propose the queries you mention. Mr. Wing's new book I have not yet seen, and therefore can give you no opinion upon it. But Dr. Wren, who hath more considered that subject than I have done, can much better satisfy you in that question than I.

I sent yesterday, by Mrs. Margaret Lichfield, another parcel of copy, directed to Mr. Pitts: she lodgeth at Mr. Moxon's in Russel Street. If you or Mr. Pitts go that way, before she send it, you may there have it. There remain about two such parcels of the chapter we are now about. I suppose, before this time, Mr. Oldenburg hath given you one of my books in answer to that of Mr. Hobbes. No more at present, but that

I am
your friend to serve you,
JOHN WALLIS.

CCCXIII.

WALLIS TO COLLINS.

Oxford, Jan. 11, 1669–70.

Sir,

To yours of the 8th instant. I think it would be convenient to acquaint Mr. Boyle that we are preparing to send, and know whether he think fit to send any or all of his own pieces, in return for those which Borelli hath sent him, at least those which he hath not. Of those in the catalogue you send me, I think it may be convenient to send (beside those of Mr. Boyle) Hooke's Micrography, Lower's two books, Wing's and Streete's Astronomy, Hobbes contra fastum geometrarum, Merret's Pinax, Willis's Pathologia cerebri, (his former books Borelli hath,) Needham's Disquisitio, Uleg Beig's Catalogue, Wilkins' Universal Character, Evelyn's Sylva and Pomona, Beveridge's Chronology, Glanville's Progress, &c. Mercator's Logarithmotechnia, Gregory's Exercitationes, The History of the Royal Society, and what more of them you please.

And moreover, a collection of the Transactions, if Mr. Oldenburg have not sent them already; and Barrow's pieces, all of them, which I find not in your list.

Amongst mine, let my Grammatica Practica linguæ Anglicanæ be one.

I shall speedily prepare a letter to send him. But consider what answer we are to give to what he moves, of sending over some copies of his to be sold, whether any booksellers here will take off any number of copies.

Pray let Mr. Oldenburg have the two books he desires. I like well enough of Flamsteed's design, but I would not have him too severe with Streete, who, I think, hath deserved well in astronomy. Mr. Horrox his name is truly spelled Horrockes, not Horrox, which I could wish to be preserved, at least in some places, in the printed books; though (since he hath been pleased so to put it) it may in Latin elsewhere be written with *x*. I sent yesterday to Mr. Oldenburg, by Bartlett's coach, some more of my last papers against Mr. Hobbes, and a cut supernumerary to perfect what I sent you before.

I had lately a letter from Peter Van Schooten, who longs to hear of some here willing to part in the impression of my things at Leyden. He tells me that young Golius being very sick, and himself scarce so well as to go abroad, he hath not yet inquired of the book sent by Sir Alexander Hume to Golius his father, but will do [so] on the first occasion; that he knows of nothing of Hudden's in the press, but that he had long since spoken of a book near finished, treating of algebra a capite ad calcem, but believes that Hudden, being full of business and but slow in writing books, (and I may add, who doth not so much as understand Latin,) he doth not think it to be finished or in the press, and if ever, it will be in Dutch; that Martin Wilkins his Officina Algebræ hath nothing in it but vulgaria; Slot en Sleutel he knows not; that of Vander Huyp's he knows nothing extant but the book you mention; that he will very readily serve you in buying what books there you shall send for, (mathematic books being there, in their auctions, to be had very cheap,) and will, if you give him leave, desire of you the like favour at London, when he shall know how to direct a letter to you; that letters will come to him safe and post free if directed thus:—

Myn Heer Willem Vander Cruck Procureur,
in's Graven Hage om te behandigen aende
Professor Van Schooten te Leyden,

and this in a cover thus directed:

Myn Heer myn Heer Bisdommer Commissaris in's Graven Hage,

and the letter given to the Secretary of [the] Holland Ambassador at London, to send.

I have here received twelve copies of mine De Motu, but in one of them the sheet I is torn, and the last leaf of all is wanting. I wish, with the perfecting of this, he would send me all the printed sheets after Cc in the second alphabet, together with the first sheet. He hath made the year in the title page 1670, whereas I directed it to be 1669. I wish they would make more haste at the press. I have had, I think, but two sheets since I was in London. Of Mr. Houghton's son I can say no more than I did. Dr. Crosse hath had much occasion to know both the father and the son, being his next neighbours and of long acquaintance.

I am yours, &c.

JOHN WALLIS.

CCCXIV.

WALLIS TO BORELLI.

Clarissimo, doctissimoque viro D. Johanni Alphonso Borellio, in Academia Pisana Matheseos Professori, Messanæ Siculorum agenti, Johannes° S.P.D.

Tradantur Messanæ in Sicilia.

Agnosco (vir clarissime) me tibi pluribus nominibus obstrictum esse, qui me et literis et libris tuis locuple-

° This is taken from a copy in Collins's hand writing, and the surname is omitted; but from the contents and the place at which it is dated, it is evidently written by Wallis.

tasti. Quem primum miseras librum unum et periisse putaveram, tandem postliminio accepi. Literas item binas, et cum secundis librorum molem majorem. Ex quibus, quos honoratissimo Boylio designaveras, Londinum propediem ad illum remittendos curabam: qui plurimas tibi gratias rependit, quod eum hoc honore affeceris. Unisque mihi reservatis singulorum exemplaribus, reliqua (quippe præter Apollonii unicum, erant singulorum, si memini, terna exemplaria) Londinum mittebam ad D. Johannem Collins, virum Mathematicum, et diligentem scriptorum mathematicorum exquisitorem. Eundem ego rogavi meo nomine ad te remittere meorum omnium quæ non habes exemplaria, (gratitudinis meæ ob acceptos tuos qualecunque indicium,) una cum his literis, et catalogo librorum præcipuorum apud nos nuperis annis editorum, quæ rem mathematicam præsertim spectant, atque etiam physiologiam, (nescio an non D. Boyle ad te missurus[p] sit libros aliquot.) Simulque petit [idem Johannes Collins] ut in posterum librorum mathematicorum non tuorum tantum, sed et ab aliis editorum, exemplar unum aut alterum ad illum mittere dignari velis, quæ vel numeratis pecuniis rependet, vel (si tu id malis) libris aliis hinc mittendis. Quippe libros in Italia, et oris adjacentibus editos, nos vix accipimus, aut etiam ne omnino, atque in eum finem hos nominatim recensendos a me petiit, quos ut mittas rogat.

Alexandri Marchetti exercitationes mechanicas, jam perfectas.

Eschinardi centuria problematum opticorum.

Renaldini Geometriam, seu tractatum de curvis Medicæis, determinationi et solutioni æquationum inservientibus.

[p] In MS. a D. Boyle ad te missuros sit libros aliquot, which is evidently wrong. Collins possibly made some oversight in copying this part of the letter.

Riccii tractatum ejusdem argumenti ; si extat.

Antanalisi di Grisio, contra Maghetto.

Il compendio delle regale trigonometriche, e centuria di problemi, di Cavalerio.

Griembergeri de luce et refractionibus opus posthumum nuper editum.

Ejusdem speculum ustorium ellipticum, cum appendice ad praxin sectionum conicarum, et consectariis de circulorum contactibus, et sectionibus angularibus.

Ejusdem novam cœli perspectivam.

Mengoli tractatum de additione fractionum seu quadraturas arithmeticas.

Hodierna opera mathematica omnia.

Et si quid tuorum extat præter illa quæ jam scripsisti.

Idemque, ni fallor, etiam hac vice tibi mittet libros aliquos, alios post missurus quos petieris. Vale.

Scribebam Oxonii 13 Jan. 1669–70, stilo Angliæ.

Citius misissem nisi quod commoda navigii occasio non occurrebat.

Books sent by Mr. Boyle.

A continuation of New Experiments Physico-Mechanical, touching the spring and weight of the air and their effects. The first part. Oxford, 1669, 4to.

Some considerations touching the usefulness of experimental philosophy. Oxford, 1664, 4to.

Of absolute rest in bodies. London, 1669, 4to.

Hydrostatical paradoxes. Oxford, 1666, 8vo.

Ths origin of forms and qualities. Oxford, 1666.

Sent by John Collins.

Slusii Mesolabium.

De respirationis usu primario, auctore Malachia Thruston. London, 1670, 8vo.
Tractatus de Urim et Thummim.
Barrovii Optica.

CCCXV.

WALLIS TO COLLINS.

Oxford, July 23, 1670.

Sir,

I have now yours of July 14, with the book inclosed, and six books from Mr. Pitts. If Mr. Bee will barter for the book you mention, you may please to make that bargain for me. If there be need of taking any of the first part from Mr. Pitts to that purpose, I shall satisfy him either in money or books. I thank you for the book of that new Dutch engine, which promiseth fair, but how it will perform we cannot judge, unless he would open the covered box and shew what is within it. I could wish, if it were not too much trouble, that, before the copies be dispersed, these faults were mended with a pen:

Pag. 560, lin. 28, after latus rectum $\frac{h^2}{h^2-\eta^2}. L$

add $+ \frac{\eta^2}{h^2-\eta^2}. T.$

Pag. 561, lin. 5, after latus rectum $\frac{h^2}{\eta^2-h^2}. L$

add $+ \frac{\eta^2}{\eta^2-h^2}. T.$

Pag. 565, lin. 23, for $\frac{\eta^2-h^2}{L}$ make it $\frac{\eta^2-h^2}{L}. T$, (the letter T being omitted;) but the two former are the more considerable, and should have been put into the table of errata, if I had continued it so far.

It should have been

in the first place $\frac{h^2}{h^2-\eta^2}. L + \frac{\eta^2}{h^2-\eta^2}. T.$

in the latter . . . $\frac{h^2}{\eta^2-h^2}. L + \frac{\eta^2}{\eta^2-h^2}. T.$

Excuse the frequent trouble you have from

your friend to serve you,

JOHN WALLIS.

It's very possible there may be some other like errors, either by the printer's mistake or mine; but I [have] not yet had time to peruse the whole. If you find any, you will do me a courtesy to mark them and give me notice.

CCCXVI.

WALLIS TO COLLINS.[q]

Oxford, Aug. 4, 1670.

Sir,

I am sorry those of the Society have no better satisfied Mr. Pitts in the price of the book, which I am very sensible was both troublesome and chargeable to print. I have written to Mr. Oldenburg concerning it. You may forbear presenting any more as from me, save only that to my Lord Brounker and that to Mr. Boyle; because I would not forestall his market. The Theatrum Machinarum you may let alone, if they put it at such unreasonable terms. My third part I

[q] The direction of this letter is lost; but there can be little doubt of the person to whom it is addressed.

mean shall be very short; and when that is done, I think I shall rest a while. But something must be done of a third part, because it is promised; and without it the other two are but an imperfect work. I thank you for the book you sent me by the carrier, which I shall inquire after when the wagon comes in.

You may add, if you please, in my book, pag. 565, line 11, after conjugatus: adde, sin curvam tangat, perinde est ad utrumvis casum referas: quippe tum hyperbolæ degenerant in opposita triangula, quorum communis vertex est O, punctum contactus, evanescente axe transverso; which case I wonder how I missed. And pag. 556, line 24, after genetricis; adde, aut etiam hyperbolam hanc ubivis tangat.

The post is going: I add only that I am

your friend to serve you,

JOHN WALLIS.

CCCXVII.

COLLINS TO WALLIS.

March 21, 1671.

Reverend Sir,

I have yours of the instant, wherein you mention the printing of Mr. Merry's exposition of Hudden's rules about reducing compound equations into their components, (the MS. which he is willing to communicate,) concerning which I have this to say, that Mr. Merry did explain only some of those rules, to wit,

omitting the rest; that I do believe that if a treatise of that nature, and about finding the roots and limits of equations, [it] would be very acceptable, I am sure, to many here: for of two mathematical clubs here, one is a large one consisting of divers ingenious mechanics, gaugers, carpenters, shipwrights, some seamen, lightermen, &c., whose whole discourse is about equations. Nor doth Mr. Kersey treat of the roots or limits of equations of any high degree, and therefore [it] might be another treatise apart to be sold with his, the printing whereof is now in agitation, collecting what is scattered in Hudden, Bartholinus, Dulaurens, and the Dutch writers, Kinkhuysen, Ferguson, &c. well digested in Latin, [or] especially in English, into which it might afterwards be translated. Neither may we doubt of considerable improvements concerning limits. I send you two papers, the one of Slusius, which he sent to Mr. Oldenburg, who need not know that I have imparted the same, the other of Mr. Gregory. Neither of them come up to what I have heard discoursed by Dr. Pell, to wit, that he finds the limits of high equations made by the multiplication of the known roots ascendendo, first precisely limiting each degree in the order of the scale, as first the quadratic equation, then the cubic, and so on.

After he hath the limits of an equation, then giving any homogeneum, he affirms he can fall upon the logarithm of the root quam accuratissime; now I shall speak my sense of it.

This figure (pl. 6, fig. 1,) may represent the curve of a cubic equation you formerly calculated, in which if AQ be a root when the homogeneum is $=0$, and DA the homogeneum when DC represents a pair of equal roots; if we shall suppose another homogeneum given as AE, and a root found thereto by trial somewhat near the truth as FG, it seems probable that parabolasters may be made to pass through the points

C and G, so as the one to fall within AG and the other without, so that the ordinate HI shall be less and the ordinate HK greater than the complement or difference between the root sought and the root or limit DC. The performance whereof depends chiefly on the drawing of a line to touch the curve AOC at the point O, if the touch lines of those parabolasters do the one fall within, the other without, the said touch line, the quæsitum is thence easily obtained.

I incited Dr. Barrow to this method in relation to the quadrature of the circle, and he met with good success in it, as you may see in his Geometrical Lectures, page 103.

If AQ be bisected by the line CK, it is scarce worth inquiry whether the parallels to AQ be bisected by the curve ACQ, but I think they are not.

Another inquiry may be whether these curves that are described by making the roots of adfected equations perpendiculars or ordinates to their homogenea may not be described by aid of a rank of continual proportionals, as in Dr. Barrow's curves, pag. 135, or by the pure powers of an arithmetical progression, seeing the pure biquadratic parabolaster at the end of your former works was described from an adfected cubic equation. Lastly, that which is most desirable in algebra is an easy method for obtaining the roots of high adfected equations, and that will be performed by one series for all cubic equations, another for all biquadratics, &c., ad inf. only varying the signs and using due caution, and those series are found or made by extracting the roots of adfected equations in species and not in numbers.

CCCXVIII.

WALLIS TO COLLINS.

Oxford, Jan. 25, 1671–2.

Sir,

In yours of [the] 23rd instant I was a little surprised at your mention of Dr. Worthington's death, whom I saw well at Hackney about Michaelmas last, and thought nothing but that he had been so still. A bundle of letters of Crabtree to Horrox I saw, but know not that there was any of Gascoigne among them; nor do remember that I ever saw any thing of his. The papers that I had of Horrox and Crabtree I have so reduced in the extract I made, that I do not think there is any thing material omitted. The papers themselves I returned all to Mr. Oldenburg, not keeping one single paper by me, how inconsiderable soever. What afterwards became of them I can give no account, save only those extracts of my own, which you sent me to revise, and I have returned them to you. The papers you have from Mr. Moore I shall be willing enough to see, and if they be different from such as are already collected, as I suppose they are, it will be fit to add them to the rest, either of Horrox or of Crabtree's letters.

I am not against the reprinting of my treatises in parts. I have desired Dr. Marshall, who is now in Holland, to put that to an issue, whether they will or will not print them; and if not suddenly, to bring back with him those I had sent fitted for the press, and then shall go in hand with them as soon as may be. Only I thought those small pieces you have, if they be worth it, might be doing in the mean time.

I am very glad of the improvements of the microscope, both by Mr. Newton and Mr. Hooke. I have no acquaintance with the former, but to the latter you may present my services, and that I could wish he would not only in a cypher, but more at large commit it to writing, and either keep in his own hands, or leave, sealed up, with Mr. Oldenburg, as a public person, till he be ready to publish something of it. However, that he would not suffer himself by the multitude of businesses to be diverted from perfecting his design, which doth but too often prejudice things of this nature.

The pieces you mention of Dr. Barrow, [and] Mr. Newton, I shall be willing enough to see in print. As to that of Kinkhuysen, I know not whether Mr. Newton were not better, and he might with as much ease, publish what he hath as a treatise of his own, rather than by way of notes on him. The design of Poterius I like well, as I did that of Mr. Graves to the same purpose. Lalovera's third appendix I have not seen. What are those remains of his I know not, but look on him as a learned man and very good mathematician, only somewhat intricate, and not so clear in his delivery of his notions. I am willing enough those treatises be published in England, for 'twill be no disreputation to our nation to put forth what France is not willing to venture upon, provided that it do not hinder the printing those of our own nation, who I doubt not have things as considerable as theirs, which lie by the wall for want of publishing. Of Slusius I have a very good opinion, and of what he doth. Dr. Barrow's method for tangents I do not well remember, not having read it lately, and but slightly when I did.

My own, for the most part, is such as that I use in my conic sections, prop. 23, 30, 36, and elsewhere often, which is very natural, and easily applied in most cases, at least where the ordinates, or what is equivalent thereunto, are known. And this is my method for Maxima and Minima. Another way I have deducible from my De Motu, cap. x, prop. 6, which is from the composition of motion, very easily applicable where curves are supposed generated by a compound motion. Both which are not hard to draw up into a general method if there be occasion. I have lately written to P. Bertet an answer to a letter of his, and one to Mr. Oldenburg, concerning a synopsis sent him by P. Pardies. I presume, if you desire it, he will shew you both.

I am,
your faithful friend to serve you,
JOHN WALLIS.

I would very fain that Mr. Hooke and Mr. Newton would set themselves in earnest for promoting the designs about telescopes, that others may not steal from us what our nation invents, only for our neglect to publish them ourselves.

CCCXIX.

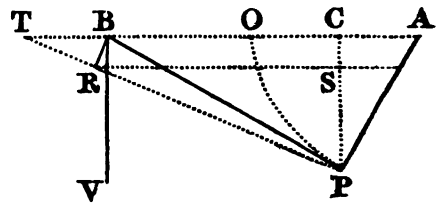
WALLIS TO COLLINS.

Oxford, Feb. 13, 1671–2.

Sir,

I forbore to answer yours of Feb. 3, because I did not till Saturday night receive from my son the things you left with him, viz. The Assembly's reasons, &c.

the beginning of Horrox, to the end of X, and the manuscript copy; for all which I thank you. In the printed sheets, I find divers literal faults, but such as are too apt to pass, where the author is not at hand; errors in numbers I have not yet, in those few that I have perused, observed any. The print about Augustine's works I did shew Mr. Bernard, with the rest of the letter, but he had before shewed me the like, sent him in a letter from Mons^{r}. Quesnel's, to whom I have, on that occasion, sent a letter about it, addressed to Mr. Oldenburg by the last post, with whom you may see it. I thank you for the two Propositions of Dr. Barrow, the latter of which is virtually included in what I presume as [a] Lemma to my later method, out of my treatise of the angle of contact, that the direction of a curve in each point is the same with that of its tangent, and consequently compounded of the same motions. Mr. Wase I have not yet spoken with, but shall inform him as you direct about Perault's translation of Vitruvius, as also of the Greek MS. of Architecture. I am sorry for the loss of Mr. Horrox's papers, in Mr. Brook's hands, by the fire. What Shakerley's tables are, I know not. My son is not good at transcribing, especially of what he so little understands as mathematics, so that we must wait till more leisure for those papers; nor is there any great haste. Mr. Townley, as to his question, I suppose may receive satisfaction from Stevinus in his Sparto-statics. What I said of it in the place mentioned, was only to seclude that consideration from what was then in hand. Nor was I willing to engage in too many diversions, because it would swell the book too much. But, supposing the thread not capable of stretching, &c., if the direct descent in BV to the direct ascent in PC, be as the weight P to the weight V, (or what is equipollent to it) they are in equilibrio; and the descent of V, equal to the shortening of PB, meanwhile P ascends in the arch of a circle PO (whose centre is A) equiva-



lent to that in the tangent PT obliquely, and this equivalent to a direct ascent in PC; whence by calculation we may gather the proportions. For BP pulls obliquely, and therefore weaker than RP, in reciprocal proportion to their lengths, (supposing BRP a right angle,) therefore $PB : PR :: V : \frac{PR}{PB}.V$, the strength of V at B in comparison to what it would have at T or R. Then $PC : PT :: PS : PR :: \frac{PR}{PB}.V :$ $\frac{PR^2}{PS \times PB}.V \left(=\frac{PT \times PR}{PC \times PB}.V\right) = P$, that P and V in this position [may] be equipollent. Excuse me if I have in haste miscomputed, for I have scarce time to consider it lest I lose the post. I am

yours to servé you,

JOHN WALLIS.

CCCXX.

NOTES OF COLLINS FOR WRITING TO WALLIS AND VERNON.

16 March, 1671–2.

Dr. Wallis,

His letter of the 9th instant received, by which see hath sent up the papers.

Much of Horrox's letter of 12 Dec. 1640, in Dissertation against Hortensius.

Those papers of Gascoigne's Dioptrics and Theories doubted not worth printing. Bp. Wilkins says he never saw any papers between Gascoigne and Crabtree.

Flamsteed hath some of Gascoigne's.

Mr. Vernon hath notice about three weeks since, in whose hands the books are at Paris.

Booksellers or Ogilby intend to print the Dutch Naval Architecture, to which might be subjoined his exercise de Cono Cuneo.

Mr. Vernon sent me one of Billy's Diop.

I desired some more, (and Lalovera's exercise against Meibomius,) that I might present you with one, but am glad he hath prevented [me]; mine with Kersey whose pains rest unprinted.

Vernon his of received. Fermat's papers embraced and explained by Dr. Pell; a Transaction should have been sent, but Mr. Oldenburg intends it; since that received the bundle sent by Dr. Aglionby, cont. 3 vol. Des C. Epistles, 2 Poisson on the Mus. and Mech., Labbæi Bibl., Pascal du Triangle Arithm.

Pardies Geometry not so much to be esteemed here; want price of books a P. Bertet.

A note sent three weeks since about Jolley's bundle; my own table of interest to be reprinted by Sir Samuel; an account of Mengolus desired and one to be brought over.

Lalovera and Poisson common; if he obtains Pascal and Desarque's Conics, and Grienbergerus, good use may be made of them here in what we are to publish.

CCCXXI.

WALLIS TO COLLINS.

Oxford, March 27, 1672.

Sir,

I had newly sent away my letter to you by the last post, when upon a review of yours to me I began to suspect an error [of] mine by misapprehending the nature of the curve; which therefore if you please to return me, that I may a little better consider of it, I will see to mend it by the next. Meanwhile I have been reading Poterius de Ponderibus, &c., which you sent to Mr. Bernard, who imparted it to me. How accurate he hath been in his collections I know not, for we have for the most part but his own assertions, not his authorities. His reductions are mostly to French measures, not to English, which makes it more proper to have been printed in France. And in our English measure he is grossly out, making our foot less than it ought to be by at least an inch and a half, supposing the French foot to be truly taken. For he makes the proportion of the Paris foot to ours to be as 1560 to 1302, which I have myself found by comparing them to be as 16 to 15; theirs containing of ours $12\frac{4}{5}$ inches proxime, which by him should be more than $14\frac{1}{3}$. And I fear therefore that he may be alike mistaken [in] others. He makes the height of Goliath about $15\frac{1}{3}$ French feet, which is above a perch, or $5\frac{1}{4}$ English yards, by his computation; about three times the height of an ordinary person, and must therefore be strangely disproportionate in bigness to his height, by what we have in Galileo's Dialogues de Motu, pag. 129. But enough of this at present. I am,

Sir,

yours to serve you,

JOHN WALLIS.

CCCXXII.

WALLIS TO COLLINS.

Stoke juxta Guilford, Maii 13, 1672.

Ad tuas Maii 9 datas quo respondeam, hæc habe.

Figuram tangentium C A a O, pl. 6, fig. 2, (conchoidi congenerem) complere intelligantur æqualibus intervallis dissitæ tangentes Va, sinibus versis AV (adeoque et complementorum rectis CV $= x$) arithmetice proportionalibus convenientes; adeoque, posito radio CA $= r$, et sinu recto VB $= s = \sqrt{r^2 - x^2}$, erit V$a = b$ $= \text{AS} = \frac{sr}{x} = \frac{r}{x}\sqrt{r^2 - x^2}$; et posito VD $= a$, adeoque CD $= x \mp a$, DO $= \frac{r}{x \mp a} \cdot \sqrt{r^2 - x^2 \pm 2xa -} = \frac{r}{x \mp a} \times \sqrt{s^2 \pm 2xa -}$. Curvam A$a$O tangat aTF, occurrens in F rectæ CA, abscindens VF $= f$, adeoque DF $= f \pm a$, et (propter FV : FD : : Va : DT,) DT $= \frac{f \pm a}{f} \times \frac{r}{x}\sqrt{r^2 - x^2}$ $= \frac{f \pm a}{fx} . rs$. Ergo (propter DT $\overset{=}{\underset{<}{>}}$ DO,) $\frac{f \pm a}{fx} rs \overset{=}{\underset{<}{>}} \frac{r}{x \mp a} \times \sqrt{s^2 \pm 2xa -}$. Et (sumptis quadratis) $\frac{f^2r^2s^2 \pm 2fr^2s^2a}{f^2x^2} \; \frac{}{-} +$ $\overset{=}{\underset{<}{>}} \frac{r^2s^2 \pm 2r^2xa -}{x^2 \mp 2xa +}$; adeoque $f^2r^2s^2x^2 \pm 2fr^2s^2x^2a$

$\mp 2f^2r^2s^2xa \pm \substack{=\\>\\<} f^2r^2s^2x^2 \pm 2f^2r^2x^3a -$. Hoc est $\pm 2fr^2s^2x^2a \mp 2f^2r^2s^2xa \pm \substack{=\\>\\<} \pm 2f^2r^2x^3a -$; et divisis omnibus per $\pm 2fr^2xa$, positoque D in V, $s^2x - fs^2 = fx^2$, seu $s^2x = fs^2 + fx^2 = fr^2$, et $f = \frac{s^2}{r^2}x$; unde punctum F determinatur. Atque hactenus in epistola de tangentibus jam edita.

Petis jam ut velim punctum contrarii flexus eadem methodo determinare: nempe quo ita sumatur V*a*, ut infra hanc sit DT < DO, sed supra DT > DO. Est autem (per jam ostensa) FV : FD : : $f : f \pm a : : \frac{s^2x}{r^2} : \frac{s^2x}{r^2} \pm a$ $: : s^2x : s^2x \pm r^2a : :$ V*a* $\left(= \frac{rs}{x}\right)$: DT $\left(= \frac{s^3xr \pm sr^3a}{s^2x^2} = \frac{s^2xr \pm r^3a}{sx^2} \right)$, adeoq. $\frac{s^2xr \pm r^3a}{sx^2} \substack{<\\>} \frac{r}{x \mp a} . \sqrt{s^2 \pm 2xa - a^2}$;

et (dividendo utrinque per $\frac{r}{sx^2}$,)

$s^2x \pm r^2a \substack{<\\>} \frac{sx^2}{x \mp a}\sqrt{s^2 \pm 2xa - a^2}$; et (multiplicando per $x \mp a$,) $s^2x^2 \pm r^2xa \mp s^2xa - r^2a^2$ $(= s^2x^2 \pm x^3a - r^2a^2)$ $\substack{<\\>} sx^2\sqrt{s^2 \pm 2xa - a^2}$, et (sumptis quadratis) $s^4x^4 \pm 2s^2x^5a + x^6a^2 - 2s^2x^2r^2a^2 \mp 2x^3r^2a^3 + r^4a^4 \substack{<\\>} s^4x^4 \pm 2s^2x^5a - s^2x^4a^2$; hoc est $x^6a^2 - 2s^2x^2r^2a^2 \mp 2x^3r^2a^3 + r^4a^4 \substack{<\\>} -s^2x^4a^2$, seu $x^6 - 2s^2x^2r^2 \mp 2x^3r^2a + r^4a^2 \substack{<\\>} -s^2x^4$, hoc est $x^6 + s^2x^4 - 2s^2r^2x^2 \substack{<\\>} \pm 2x^3r^2a - r^4a^2$. Et propterea (posito D in V, quo fiat $a = 0$, adeoque evanescat æquationis pars posterior, simulque excessus defectusve,) erit $x^6 + s^2x^4 - 2s^2r^2x^2 = 0$, adeoque $x^4 + s^2x^2 = 2s^2r^2$, seu (propter $x^2+s^2=r^2$,) $x^2r^2=2s^2r^2$, seu $x^2=2s^2$, adeoque $r^2=s^2+x^2=3s^2=\frac{3}{2}x^2$, et $2r^2=3x^2$, seu $\frac{2}{3}r^2=x^2$.

Adeoque sumpta $CV=x=r\sqrt{\frac{2}{3}}$, ducta $V\alpha$, designabit α punctum contrarii flexus.

Intelligatur jam Figura tangentium, non (ut prius) ad radium AC, sed ad arcum ABQ in rectam expansum, applicata (pl. 6, fig. 2, and 3). Hujusque tum tangentes tum punctum contrarii flexus inquirantur. Figura hæc a præcedente in hoc differt, quod ordinatarum tangentium $V\alpha$, DO, (nunc $B\alpha$, βO,) intervallum, quod prius fuerat VD, jam erit æqualis respectivo arcui æque alto $B\beta$, hoc est (in partibus exiguis) $B\tau$. Est autem (per Prop. cap. 1, De Motu) $\tau\tau.\ BV=DD.\ CA$, puta $ts=or$, adeoque $\frac{or}{s}=t$ $=\tau\tau=B\beta$. Sed $DD:\tau\tau::VF:B\phi$, hoc est $o:\frac{or}{s}::os$ $:or::s:r::VF\ (=f=\frac{s^2}{r^2}x):B\phi\ (=\phi=\frac{r}{s}.\frac{s^2}{r^2}.x=\frac{s}{r}.x)$.
Atque hinc determinabitur punctum ϕ.

The conclusion of this letter is omitted, as being recalled in Letter CCCXXIV.

CCCXXIII.

WALLIS TO COLLINS.

Sir, June 8, 1672. Oxford.

I send you, here inclosed, those heads of a letter, (which you desired, put into Latin, with a postscript of my own; and, in the other part of this sheet, the equivalent designations by sines, tangents, secants, &c.

as you desired. That the figure of tangents applied to the arch stretched out into a straight line, hath no contrary flexure, I am well satisfied, and can demonstrate it; so that in the last of those four operations in my letter of May 13, 1672, there is a mistake; but the three first I take to be sound. I am

yours to serve you,

JOHN WALLIS.

Sinuum rectorum et versorum, tangentiumque et secantium, pro arcubus angulisve expositis, eorumque complementis *'Ισοδυναμία.*

Esto R radius, S sinus rectus, Σ sinus rectus complementi; T tangens, τ tangens complementi; s secans, σ secans complementi; V sinus versus, υ sinus versus complementi. Erit

$$S = \sqrt{R^2 - \Sigma^2} = \frac{\Sigma T}{R} = \frac{T}{R}\sqrt{R^2 - s^2} = \frac{TR}{s}$$

$$= \frac{TR}{\sqrt{R^2 + T^2}} = \frac{R}{s}\sqrt{s^2 - R^2} = \frac{\Sigma R}{\tau} = \frac{R}{\tau}\sqrt{R^2 - S^2}$$

$$= \frac{R^2}{\sigma} = \frac{R^2}{\sqrt{R^2 + \tau^2}} = R \backsim \upsilon = \sqrt{2VR - V^2}.$$

$$\Sigma = \sqrt{R^2 - S^2} = \frac{S\tau}{R} = \frac{\tau}{R}\sqrt{R^2 - \Sigma^2} = \frac{\tau R}{\sigma} = \frac{\tau R}{\sqrt{R^2 + \tau^2}}$$

$$= \frac{R}{\sigma}\sqrt{\sigma^2 - R^2} = \frac{SR}{T} = \frac{R}{T}\sqrt{R^2 - \Sigma^2} = \frac{R^2}{s}$$

$$= \frac{R^2}{\sqrt{R^2 + T^2}} = R \backsim V = \sqrt{2\upsilon R - \upsilon^2}.$$

$$T = \frac{SR}{\Sigma} = \frac{SR}{\sqrt{R^2 - S^2}} = \frac{R}{\Sigma}\sqrt{R^2 - \Sigma^2} = \sqrt{s^2 - R^2} = \frac{R^2}{\tau}$$

$$= \frac{R^2}{\sqrt{\sigma^2 - R^2}} = \frac{SR}{R \backsim V} = \frac{R}{R \backsim V}\sqrt{2VR - V^2}.$$

$$\tau = \frac{\Sigma R}{S} = \frac{\Sigma R}{\sqrt{R^2 - \Sigma^2}} = \frac{R}{S}\sqrt{R^2 - S^2} = \sqrt{\sigma^2 - R^2} = \frac{R^2}{T}$$

$$= \frac{R^2}{\sqrt{s^2 - R^2}} = \frac{\Sigma R}{R - v} = \frac{R}{R - v}\sqrt{2vR - v^2}.$$

$$s = \frac{R^2}{\Sigma} = \frac{R^2}{\sqrt{R^2 - S^2}} = \sqrt{R^2 + T^2} = \sqrt{R^2 + \frac{R^4}{\tau^2}} =$$

$$\frac{R}{\tau}\sqrt{\tau^2 + R^2} = \frac{\sigma R}{\tau} = \frac{\sigma R}{\sqrt{\sigma^2 - R^2}} = \frac{TR}{S} = \frac{TR}{\sqrt{R^2 - \Sigma^2}}$$

$$= \frac{R^2}{S\Sigma}\sqrt{R^2 - \Sigma^2} = \frac{R^3}{S\tau} = \frac{R^2}{R - V} = \frac{R^2}{\sqrt{2vR - v^2}}.$$

$$\sigma = \frac{R^2}{S} = \frac{R^2}{\sqrt{R^2 - \Sigma^2}}\ \sqrt{R^2 + \tau^2} = \sqrt{R^2 + \frac{R^4}{T^2}} =$$

$$\frac{R}{T}\sqrt{T^2 + R^2} = \frac{SR}{T} = \frac{SR}{\sqrt{s^2 - R^2}} = \frac{TR}{\Sigma} = \frac{TR}{\sqrt{R^2 - S^2}}$$

$$= \frac{R^2}{\Sigma S}\sqrt{R^2 - S^2} = \frac{R^3}{\sigma T} = \frac{R^2}{R - v} = \frac{R^2}{\sqrt{2VR - V^2}}.$$

$$V = R \mp \Sigma = R \mp \sqrt{R^2 - S^2} = R \mp \frac{SR}{T} =$$

$$R \mp \frac{R^2}{\sqrt{R^2 + T^2}} = R \mp \frac{R^2}{s} = R \mp \frac{S\tau}{R} =$$

$$R \mp \frac{\tau R}{\sqrt{R^2 + \tau^2}} = R \mp \frac{\tau R}{\sigma} = R \mp \frac{R}{\sigma}\sqrt{\sigma^2 - R^2} =$$

$$R \mp \sqrt{2vR - v^2}.$$

$$v = R \mp S = R \mp \sqrt{R^2 - \Sigma^2} = R \mp \frac{\Sigma R}{\tau} =$$

$$R \mp \frac{R^2}{\sqrt{R^2 + \tau^2}} = R \mp \frac{R^2}{\sigma} = R \mp \frac{\Sigma T}{R} =$$

$$R \mp \frac{TR}{\sqrt{R^2 + T^2}} = R \mp \frac{TR}{s} = R \mp \frac{R}{s}\sqrt{s^2 - R^2} =$$

$$R \mp \sqrt{2VR - V^2}.$$

$T, R, \tau,$ ∺ ; $s, R, \Sigma,$ +++ ; $S, R, \sigma,$ +++.

$$T\tau = s\Sigma = S\sigma = R^2 = s^2 - T^2 = \sigma^2 - \tau^2 = s^2 + \Sigma^2.$$

CCCXXIV.

WALLIS TO COLLINS.

June 14, 1672. Oxford.

Sir,

In mine of June 8, I told you there was a mistake in mine of May 13, Prob. 4. It is in those words "propter $B\beta = B\tau$, &c." which though it were before rightly enough assumed in infinite exiguis, yet may not there, where $B\beta$ is to be designed as of any length whatever, and is not (so taken) $= \frac{r}{s} a$, but should be otherwise designed. Which designation, because it would be troublesome, I wave, and choose this process, to be substituted in the room of that there.

Denique, quo in hac etiam curva (ubi peripheriarum tangentes $B\alpha$, (Pl. 6, fig. 2, and 3.) ad AQ quadrantem, extensum rectam, applicantur,) designetur punctum (si quod sit) contrarii flexus, puta α; considerandum est, punctum ϕ hoc casu esse omnium altissimum; manifestum utique est, prout ab α, puncto contrarii flexus, quantulumcunque removeatur punctum contactus, sive sursum versus A, sive deorsum versus O, descensurum protinus punctum ϕ; quod de quovis contrarii flexus puncto, in hujusmodi quavis curva, facile est ostensum. Sumptis itaque ut prius $VC = x$, adeoque $V\alpha = B\alpha = \frac{sr}{x}$, et (quod ante demonstratum est Prob. 3.) $B\phi = \frac{sx}{r}$, et posita $BQ = c$, $Q\phi = \frac{sx}{r} + c$. Et consequitur, sumpta quantumvis minuta $VD = a$, adeoque (in infinite exiguis)

$B\beta = B\tau = \frac{ra}{s}$ (per Prop. 13, cap. 5, De Motu) et $Q\beta = c \mp \frac{ra}{s}$; et (substituta $x \mp a$ pro x, adeoque $\sqrt{r^2 - x^2 \pm 2xa -} = \sqrt{s^2 \pm 2xa -}$ pro $s = \sqrt{r^2 - x^2}$, et $\frac{x \mp a}{r} \sqrt{s^2 \pm 2xa -}$ pro $\frac{x}{r}.\ s = B\phi$, et $c \mp \frac{ra}{s} + \frac{x \mp a}{r}.\ \sqrt{s^2 \pm 2xa -}$ pro $c + \frac{xs}{r} = Q\phi$,) erit $c + \frac{xs}{r} > c \mp \frac{ra}{s} + \frac{x \mp a}{r}.\ \sqrt{s^2 \pm 2xa -}$, adeoque $\frac{xs}{r} \pm \frac{ra}{s} \left(= \frac{xs^2 \pm r^2 a}{rs} \right) > \frac{x \mp a}{r} \sqrt{s^2 \pm 2xa -}$, et $\frac{xs^2 \pm r^2 a}{sx \mp sa} > \sqrt{s^2 \pm 2xa -}$, et $\frac{x^2 s^4 \pm 2xs^2 r^2 a +}{x^2 s^2 \mp 2xs^2 a +} > s^2 \pm 2xa -$; et $x^2 s^4 \pm 2xs^2 r^2 a \pm$ $> x^2 s^4 \pm 2x^3 s^2 a \mp 2xs^4 a \pm$, seu $\pm 2xs^2 r^2 a \pm 2xs^4 a \pm$ $> \pm 2x^3 s^2 a \pm$, et consequenter (divisis omnibus per $\pm 2xs^2 a$,) $r^2 + s^2 = x^2$, Quod quidem (propter $r^2 - s^2 = x^2$) fieri non potest nisi sit $s = 0$, quod in solo verticis puncto A contigit. Adeoque (extra ipsum verticis punctum A) nullum erit punctum contrarii flexus.

Simili item processu demonstrabitur Probl. 2, (de curvæ puncto contrarii flexus, ubi applicantur tangentes $V\alpha$ ad AC diametrum;) nam, posito α puncto contrarii flexus, cui respondeat $VC = x$, $VB = s$, $V\alpha = \frac{sr}{x}$; $VF = \frac{s^2 x}{r^2}$, (per Probl. 1,) et $CF = x + \frac{s^2 x}{r^2} = \frac{r^2 + s^2}{r^2} x$; sumptaque quantumvis exigua $VD = a$, adeoque $CD = x \mp a$, et substitutis $x \mp a$ pro x, $x^2 \mp 2xa +$ pro x^2, $r^2 - x^2 \pm 2xa -$ $(= s^2 \pm 2xa -)$ pro s^2, adeoque $\frac{r^2 + s^2 \pm 2xa -}{r^2}$ in $x \mp a$ pro $\frac{r^2 + s^2}{r^2}.\ x$, erit (propter F, hoc casu, omnium supremum,) $\frac{r^2+s^2}{r^2}\cdot x$ $> \frac{r^2+s^2 \pm 2xa -}{r^2}$ in $x \mp a$, et $xr^2+xs^2 > xr^2+xs^2 \pm 2x^2a$ $\mp r^2a \mp s^2a \pm$, seu $\mp 2x^2a > \mp r^2a \mp s^2a \pm$. Et consequenter (divisis omnibus per $\mp a$), $2x^2 = (r^2+s^2=)$ $2r^2-x^2$, seu $2r^2=3x^2$, et $\frac{2}{3}r^2=x^2$, adeoque $x=r\sqrt{\frac{2}{3}}$; item propter r^2-x^2 $(=r^2-\frac{2}{3}r^2=\frac{1}{3}r^2)=s^2$, erit $s=r\sqrt{\frac{1}{3}}$; item $x^2=2s^2$, et Va $\left(=\frac{s}{x}r\right)=r\sqrt{\frac{1}{2}}$. Omnino ut ante demonstratum fuerat altera methodo.

In the same letter of June 8, I sent you a postscript to that therein inclosed for Borellius;) in which postscript I desire you to blot out that parenthesis, (sed illud omnium maxime cui debetur solidi centrum gravitatis,) for I perceive those words liable to be mistaken in another sense than I intended them.

I add no more at present but that I am

your friend to serve you,

JOHN WALLIS.

CCCXXV.

WALLIS TO COLLINS.

Oxford, July 18, 1672.

Sir,

I was a little surprised when in a letter from my son I found myself called upon for Mr. Flamsteed's Theory of the Moon, which I thought I had (for so I intended) presently returned to you after I had received and perused it. But being in the hurry of removing to another house and settling there, and the business of

Plate 6

Fig 1 p 52[illegible]

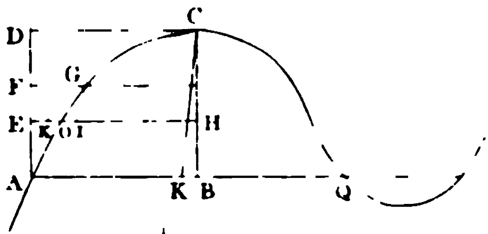


Fig 3 p 5[illegible]

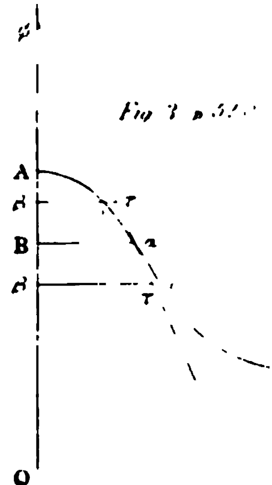
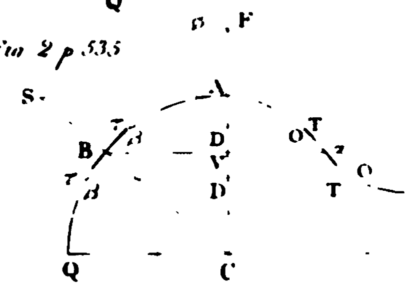


Fig 2 p 535



the Act withal coming on, I find I had neglected it, and having laid it out of sight, it was quite out [of] my mind, (save as of a thing already dispatched,) till this letter minded me to make search after it. Otherwise it should not have been so long ere you received it.

I find that in his table of Radices mediorum motuum, he gives it double, for the meridian of London in the page, and for that of Derby in the paper glued on, that you may take your choice. But I think it best to put both in, being but a very short table, the one suiting to tables for London, and the other with his own tables. Thus:

Motus Medii.

Ann. Chr. ineunte.	Pro Merid. Londini.												Pro Merid. Derbiæ.											
	Lunæ.				Apogæi.				☊ Retrog.				Lunæ.				Apogæi.				☊ Retrog.			
	s	°	′	″	s	°	′	″	s	°	′	″	s	°	′	″	s	°	′	″	s	°	′	″
1	4	12	25	55	9	13	46	59	8	28	33	16	4	12	28	40	9	13	47	00	8	28	33	15
1601	1	29	38	40	3	29	51	00	1	25	46	35	1	29	41	25	3	29	51	01	1	25	46	34

&c. &c.

And I have ordered the phrase all along to this purpose.

If you desire not to be named in the Epilogue, as you mention, you may only leave out those words in the title of it, "ad Io. Collins, editionem urgentem scriptus ab J. H. clarissime vir," and at the end "attamen recedens, &c." through which I have drawn a line with black lead, and may all be spared. Yet the date may stand.

I have done nothing about his letter, which therefore I return as it came, because I have not had time well to examine the contents of it, and because himself may better draw into form so much of it as may be proper to publish, nor am I willing to detain this longer from you.

The astronomical part of his tables I do not meddle with, because the[re is no] judgment to be made of it without comparing it with observations, which is a [matter I] have not leisure to attend, and which I presume he, who more attends it, [has] done already. I have only altered the phrase, or Latin, in some places, and but sparingly. He refers, Epil. p. 3, to a letter of Crabtree, about Horrox's hypothesis of the moon, which therefore I suppose you print with the rest.

When I was come thus far, casting my eye over the calculation before I sent it away, I find that in his Tab. 5, the greatest excentricity is 66854, the least, 43619, and yet, at the foot of Tab. 6, he makes their difference 22235, which should be 23235; which I should without scruple have altered there, had I not found the same again, Epil. p. 7, or rather instead of it 2223½; and accordingly, p. 12, the proportional part 22″, (which should be but 21″,) and Æquat. Subd. 16′ 01″ (for 16′ 00″) and so onwards. Which, though it be but an inconsiderable difference, yet it shews he did at first mistake 22235 for 23235.

On this acconnt, at the foot of Tab. 6, must be put, Excentricitatis differentia 232.35 qualium radius 10000.00. And Epil. pag. 7, lin. 28, for 2223½, (not 2323½ or 2323.5, but) 23235, (for in such parts is the difference of excentricities, præcept 3, taken out of Tab. 5). The other little differences in Part. proport. ad Æquat. Subd. &c. scarce amounting to one whole second, need no emendations. But in case 22235 (not 23235) should chance to be the true number for the difference of excentricities, then is the whole Tab. 5 (for excentricities) amiss, and all that depends upon it. I have therefore forborn to make these emendations mentioned, but left the numbers as they are, because it may perhaps be necessary to consult

Mr. Flamsteed about it, unless you can otherwise be sure whether the mistake be in that single number, or in the whole table.

This accident hindered me from sending by the last post, as I intended. The papers being too big for the post, I intend to send to-morrow morning by Moor's coach, at the Saracen's Head without Newgate.

In the mean time, I have yours of the 16th instant, and thank you for the two books you mention. The rest of the papers I will despatch as soon as I can, resting

yours to serve you,

JOHN WALLIS.

The half sheet has been torn off on which the address to Collins was written, and he has folded up what remains, and has directed it to Flamsteed, with the address "For Mr. William Lechford, ironmonger, in Derby."[a]

The amended arrangement of the table, which Wallis suggests, appears in the Opera Posthuma, p. 476, edit. 1673. But the first number in the tables differs from that given by Wallis, being 1 instead of 4. Flamsteed has also availed himself of the other corrections.

CCCXXVII.

WALLIS TO COLLINS.

Oxford, July 30, 1672.

Sir,

In pursuance of yours of the 27th instant, I send inclosed what you there desire; the title-page of the whole; some insertions to the preface; a note of errata for all as far as the end of Crabtree's Obser-

[a] See Flamsteed's directions in this respect in letter CLIX.

vations, amongst which there is one concerning the transposition of the letter omitted, where I have left a blank (to be supplied by you) of the page where it is to be found; and a title for that letter, where it is to come in: all which I have put into one leaf because of the postage. That letter of Dec. 20, 1638, I send you as I find it, in three leaves. The leaf wanting, (pag. 101, 102,) whether it had any thing more than the scheme I cannot tell. By the mark at the bottom of pag. 106, there seems to be somewhat wanting, and to be inserted out of some papers so marked. What it is I know not, for I find no such paper; perhaps it was on the back side of that which had the scheme, or else on some paper pinned on, which is lost. I would have those three leaves preserved, to be joined with the rest of the copy, which I keep together. What errata will be in the pieces of Flamsteed, which I know not, may be added to the end of these I send. Whether the title-page I mentioned for pag. 339 be inserted or not, is not much material. If you insert Townley's letter, it may come next after this of Horrox; but in the praxis thereupon, I remember there were divers mistakes, which, if you print it, should be corrected in the calculation. The correction of 2323.5 and 23235, instead of 2223.5, you will take care to amend as I directed. In the bottom of one of the tables it must be 2323.5 qualium radius 100000.0; but in the precepts it must be (at least in one place) 23235, because it answers to the parts taken out of a table answering to the radius 1000000 not 100000.0; instead of which, as I remember, it is written 2223½, which, being the first term in the golden rule, will make the fourth to be ten times as big as it should be.

I thank you for the two books of Fabri, which you were pleased to send me. I received them on

Saturday, with the papers of Crabtree, but remember that I have formerly perused both of them, but did not admire either of them. What Flamsteed intends about Venus in Sole might very well have been joined with these, that so all of Horrox might have been together. But in this I suppose we must be guided by the bookseller. The graved figures you have not sent me, and therefore I could not examine how well they answer the copy, which I presume you will do. I know not what to add more at present but that I am

yours to serve you,

JOHN WALLIS.

CCCXXVII.

COLLINS TO WALLIS.

12 August, 1672.

Reverend Sir,

You may perhaps think it strange that Horrox's Opera Posthuma are not by this time finished, to which 'tis answered, that the compositor hath been somewhat remiss, and this vice, as it happens, proves a virtue. It was Mr. Flamsteed's design that the letter of the 20 of December, 1638, should be wholly omitted, and one written by Crabtree to Mr. Gascoigne, the 21 of June, 1642, printed in its stead, and to go before his lunar numbers, to which end he sent up a translate thereof, whereas we have hitherto taken it for granted that Crabtree died but a little while after Horrox. This letter is the same you translated out of Mr. Moore's papers, but we did not know it to be such. It is rendered with this Prologue.

Johannes Flamsteedius Lectori.

Theoria illa lunæ quam tunc primum inventam amico suo familiari et studiorum socio Gul. Crabtrio in epistola data Dec. 20, 1638, Horroxius descripserat, rudis erat admodum ac impolita, nec secundis suis cogitationibus, cœlis, aut calculis, testante Crabtrio, omnino congrua, ut nedum ab exercitationibus suis, sed etiam a sequenti Theoriæ descriptione (ab epistola Crabtrii ad eruditissimum Gul. Gascoignium, data 21 Junii 1642, desumpta) satis liquet, quam Latinitate donatam ipsis tabulis hic præfationis loco præpono.

Had we known this sooner, six shillings might have been saved in composing that letter of 20 Dec. 1638. But the more immediate cause of this letter is this. Mr. Slingsby, my master, a member and the secretary of the Council of Plantations, upon the changing of that Council into a Council of Trade and Plantations, conceiving the secretary's office to be too troublesome for his administration, leaves it, hath disposed of one of his clerks, and recommends or puts me into the Farthing Office, to undertake to deliver out all that are coined. As he is Mint Master, he wants a clerk for his own occasions: I mentioned Mr. Houghton's son to him, who by this means will be in a hopeful way of preferment. At present, if he want an employment, I would advise him to accept this: there will be diet and lodging, and a salary or allowance for clothes, &c.; but what Mr. Slingsby intends to give, or Mr. Houghton to demand, I cannot say. Be pleased to do both the Messrs. Houghton and myself the kindness as to make them acquainted therewith, and to desire them to give an answer hereto. There will be no attendance at table, or such menial services expected: if the young man can be spared, and be of a good conversation, I should think it worth his while to put it to an adventure to come up. Direct your letter, as before, unto him who is

your obliged and most humble
affectionate servitor———

It will be seen by the following letters that Wallis remonstrated strongly against the omission of Horrox's own account of his lunar theory; yet Flamsteed seems resolutely to have adhered to the determination of making Crabtree's letter alone the preface to the tables. The emendations offered in the next letter are therefore only employed in part; vide Opera Posthuma, p. 467; nor is the letter of December 20 given as an addendum, according to the request of Letter CCCXXIX. This might also be gathered from Wallis's own expressions in Letter CCCXXX. The mistake about the death of Crabtree appears in the introductory epistle, and at p. 338. Crabtree's letter is dated in June by Collins; this should have been July.

CCCXXVIII.

WALLIS TO COLLINS.

Sir, Oxford, Aug. 15, 1672.

I have yours of Aug. 12 this morning, but am still of opinion that Mr. Horrox's own letter should yet be printed, to which that of Mr. Crabtree's may be well subjoined. The words in Flamsteed's Preface, "rudis admodum ac impolita, nec omnino congrua, &c." are too hard; let them rather be thus: "rudis adhuc erat et impolita, nec secundis suis cogitationibus, cœlove ipsi, (testante Crabtrio,) satis congrua, ut ex suis ipsius exercitationibus, ejusdemque theoriæ traditione ex Crabtrii epistola (ad eruditissimum Gascoignium data 21 Julii, 1642) desumpta, satis patet. Nos utramque epistolam, Latinitate donatam, Tabulis hisce, præfationis loco, præfigimus."

As to the latter part of your letter; I did, as you desired, acquaint Mr. Houghton with it, who thanks you for your kindness therein to him and his son, and shall be very ready to serve you in Oxford in any thing he may; but his son God hath otherwise disposed of, who hath been dead ever since November last. But in his stead give me leave to recommend one to Mr. Slingsby by you, whom I know very fit for such an employment, and will, I believe, accept of it, a very civil person, and genteel, pretty well in years, who, while he was heretofore an apprentice in London, was his master's cashier for divers years with very good approbation, and whom, at the expiration of his time, his master would have taken to be partner with himself. He hath since traded for himself for many years; but hath of late (for some reasons) left it off, is a widower, out of present employment, his children disposed [of], writes a very fair hand, understands accounts, will be very faithful and honest. I have written to him this morning, but being beyond London it will be towards the end of next week before it will be with him; within a few days after, I believe he will be with you at London. If you please to make stay of the place till he can come, and then give him your assistance therein, you will oblige, Sir,

yours to serve you,

JOHN WALLIS.

CCCXXIX.

WALLIS TO COLLINS.

Sir, Oxford, Oct. 26, 1672.

I have received the remaining sheets of Horrox this evening, but no letter with them; wherein I find that his letter of Dec. 20, 1638 is still omitted, which I am yet of opinion should not have been. Instead of which we have only a complaint that it was nought, and that Mr. Flamsteed hath put a better in the room of it, whereas doubtless any reader would (I am sure I should) be glad to see what it was, since the letter is the subject of the whole discourse of the two last pieces; and reference is so often made to it. And yet (if I do not much mis-remember, for I have no copy of it) it contains a much more clear account, and more intelligible, of his hypothesis, than all that here we have of it. For that I did clearly apprehend at first, and this is such as without good attention you shall hardly find out the meaning, or what it is he would have. But cer[tainly] it is not so bad that we need be ashamed of it. For [that the] numbers be not accurate is not to the purpose at all, for they were by himself afterward corrected, but the hypothesis is there much more clearly laid down. Or if not, yet it is his own, and it is the text of what all this is but the comment. It should, since it was left out in its due place, have come in next to the title-page of O o o, and I would yet, if I may be heard, have it either there inserted, or else subjoined at the end, as omitted in its due place. And I know no reason at all why Mr. Flamsteed should be peremptory in it; since it is, if I mistake not, a better account of the hypothesis, though not of the numbers, than all that he hath said about it. And what he says of it, pag. 467, 470, is but to reproach it. If he do not think it fit to come in amongst his papers, let it, for my justification, that I may not be charged with unfair dealing in omitting this letter in its due place, be subjoined to the errata, with this title:

"Addendum, pag. 323, lin. 12.

Ex Epist. Dec. 20, 1638. Toxtethæ."

I mean Mr. Flamsteed no hurt in it, but only would prevent that blame, which will be laid upon him, and me too, that we suppress Horrox's account of his own hypothesis, and then reproach it; whereas it were more fair to publish it as it is, and if there be need, excuse it. I am

your friend to serve you,

JOHN WALLIS.

CCCXXX.

WALLIS TO COLLINS.

Sir, Oxford, Novemb. 14, 1672.

I have yours of Octob. 26 and Novemb. 7; what concerns Apollonius I have acquainted Mr. Bernard with, who hath written, he tells me, his mind concerning it to Mr. Scot, which I think is to this purpose, that he thinks it more proper to print Apollonius at large before the Epitome of him. For printing the other first would rather endanger the loss of the author himself; it having been found already in experience, that Commandine having printed the translation before the original, hath so far hazarded the loss of the original, as that to this day it is not published.

I am glad Mr. Kersey's book is so forward, and shall be willing enough to see the whole of it abroad. As to what you mention to follow it, I shall not be wanting, as to my part, when there is occasion, and am well pleased that others are so forward in order to it. I wish we find not a stop at the press, which we meet with too often in mathematical books. I am sorry your public employments come to no better account, but that, as you mention, you think of quitting them.

As to Horrox's book, I think I have all; at least, I want not the Epistle Dedicatory. Mr. Flamsteed's tables I find are very frequently either miscomputed or misprinted; his epochas and mean motions are easily amended, if the reader consider them so as to correct them before he use them, else they may produce inconveniences in the use of them. His prosthaphæreses are not so easily amended, as not proceeding equally. I could wish he would himself look over them and note the errata. That letter omitted, was (I think) ill left out, because there is so frequent reference made to it, but itself nowhere to be found, which makes the thing lame. If Mr. Flamsteed would not have had it printed, he should have avoided the naming of it, and referring to it; but if it cannot be done, it must be as it may. But if it be not here inserted, I doubt whether it will be so proper to put it in another piece.

As to what concerns Mr. Streete, it is not any thing of what passed my hands, but only Mr. Flamsteed's additions, which I saw indeed, but did not think so fit for me to alter as to the substance of them; and I think some passages of reflection on him might better have been spared. But I would not have it proceed to animosities between them, and writing of books against one another, which may prove of ill consequence. And when he shall do any thing by way of animadversion on Venus in Sole, I hope he will abstain from all hard expressions. About Crabtree's death, soon after Horrox's, mention is made both in the Epistle and in the close of his letters, and it was according to the instructions I had at the time I drew them up, nor did you advertise me to the contrary till at least that at the end of the letters, ("of a few days,") was printed off, which in the Epistle is but "not long after." But that he lived till about

1652 or 1653 I was not aware till now, and had I known it in time, it ought to have been amended. The original mistake was certainly in Dr. Worthington's papers to me, giving me the particulars of the narrative of Horrox's life; and I wonder he should be so much mistaken.

Your conjecture of M. Huygens I suppose may be true enough. But since we are promised, as I take it, to see it in Slusius's method for tangents, we need be less solicitous about it till we see that. And what you say out of Slusius, that the business of Monachos, of Tangents, of Maxima et Minima, is in effect the same, is very true, and, as I remember, you may find the same said in one of my larger draughts of that concerning tangents, sent to Mr. Oldenburg; but I was forced to omit that with other things, to bring it into compass short enough for the Transactions.

I am

yours to serve you,

JOHN WALLIS.

CCCXXXI.

WALLIS TO COLLINS.

Sir, Oxford, March 15, 1672–3.

I received this day from M. Schooten from Leyden a letter, but of an ancient date, (29 Novemb. 1672,) wherein he desires me to inform him what that mistake is in Hudden's resolving compounded equations, which you told me Mr. Merry had taken notice of, and which did influence divers following propositions. I know not how to inform him, unless I have it from you. If

Mr. Merry think fit to have them informed of it, and if you have the opportunity of learning it from him and acquainting me with it, I shall upon the next occasion transmit it to them. I could be content that piece of Mr. Merry were published with those of Mr. Kersey, now in the press; for so far as I could guess, by the slight view of it which I had, I take it to be well done.

I was shewed, a few days since, a short treatise of Dialling, instrumentally, by a triangular engine, made (I think) by one William Perkes. If you know the person or the thing, you may let me have an account of it. I am

yours to serve you,

John Wallis.

CCCXXXII.

COLLINS TO WALLIS.

Reverend Sir, March 27, 1673.

Since my last, wherewith I sent you Dr. Barrow's epitome of Apollonius, &c., I have got into my hands Mr. Merry's Explication of Huddenius, but cannot find therein where he takes notice of any mistake. Mr. Merry resides at his father's house in Leicestershire, and having learned how to write to him, I accordingly intend to send to him about it. About the constitution of incomplete equations, it is easy to observe that many of the roots lose their possibility; so likewise if I make the coefficients of an affirmative equation a geometrical progression, and multiply the same by a binomial, whereof one part is the coefficient of the second term of the said equation, all the intermediate powers are destroyed, as here,

$$\begin{array}{l} a^5+2a^4+4a^3+8a^2+16a+100=0 \\ \qquad\qquad\qquad\qquad\qquad\quad a-\ 2 \\ \hline a^6 \quad * \quad * \quad * \quad +68a-200 \end{array}$$ the product is

which will have but two roots[b]; the true one $+2$ and one false one; so that I needed not have troubled you about that which I partly understand already, but however may possibly be better informed by your answer. . . . . . . of what authors and writings a second volume may suc[ceed] Mr. Kersey's, I herewith give you an account. The Lord Brounker is willing that the rules in Hudden, de Beaune, Bartholinus, Dulaurens, and the Dutch authors Ferguson, Brasser, Verstay, about the depressing, dividing, and limiting of equations, should be digested and illustrated with schemes and numerical examples: and unless you please to undertake the trouble of doing it gratis, will procure Mr. Dary (who is well versed in those authors and matters, and at present very poor and void of employment) an allowance of six pounds to undertake the same, to which possibly somewhat more may be added by the good intentions of others; wherein however your frequent advice and assistance will be seasonable, and very acceptable unto all your friends here, but more particularly unto

your humble thankful servitor,

JOHN COLLINS.

$$\begin{array}{l} a\ -4 \\ a^5 \\ \hline a^6-4a^5 \end{array}$$

as in this a^6-4a^5 which can have but two different roots, (as likewise the locus of the equation will make

[b] Collins had originally written 683 in the place of 100; had he followed out his own plan, and made it 32 (or 32 a^0), his result would have been a pure equation $a^6-64=0$.

evident,) and may be conceived to have six, if you admit the root of the fifth power of *a* to be so many times repeated———

CCCXXXIII.

WALLIS TO COLLINS.

Oxford, March 29, 1673.

Sir,

I have received yours of the 22d instant with the book and papers therein mentioned. The book I have yet had no time to consider and compare with Apollonius, of which it is an epitome, but presume it to be well done. The title-page and first sheet, with whatever epistles &c. are intended to be prefixed, I suppose are not yet finished, at least you have not sent it with the rest that I have, beginning at the sheet B. I thank you for that, and also for the papers of extracts out of Brasser's and Ferguson's Algebra. It is very true that those cubic equations, of which you give instance, may very well fall under Cardan's rule, notwithstanding the impossibility of $\sqrt{-}$, (the square root of a negative quantity,) in such cases as the binomial cube will admit of a cubic root's extraction, ($\sqrt[3]{a \pm \sqrt{-b}}$,) because then the impossible part of the root, that is $\sqrt{-}$, comes in the addition to be extinguished, of which I was before very well aware. But the main difficulty is extracting that cubic root, which all binomials do not admit. Yet this difficulty is not so great as may at first view be thought, nor is it greater in this case than in other cases of the same rule, where such $\sqrt{-}$ doth not occur. I find by your paper that they have a method, which what it is I know not, but guess it to be the same with that of Des Cartes in his Epistles, Part 2, Epist. 43. But long before I had seen any thing of Des Cartes, or knew any thing of Cardan's rule, (at least by that name,) I had long since, while I was yet but a young algebraist, in the year 1648, found out the same by my own inquiries, of which I give account in my preface to that against Meibomius. And, meeting then with the difficulty of extracting the cube root of a binomial, I took this easy and obvious way to expedite myself, of which I did in the same year, 1648, give an account, at his request, to Mr. Smith, then mathematic professor at Cambridge, in answer to a letter of his; which is only by reducing the surd into its rational and irrational parts, or (which is the same) by clearing so much of it as is rational from the note of radicality. As, for instance, for $\sqrt{243}$ I would put $9\sqrt{3}$, and by the same reason, for $\sqrt{-243}$ I would put $9\sqrt{-3}$. For by this means instead of $\sqrt[3]{10 \pm \sqrt{-243}}$, (which is your first instance,) I should put $\sqrt[3]{10 \pm 9\sqrt{-3}}$, and am presently sure that either $\sqrt{-3}$, or some multiple of it by a rational number (integer or fracted) is the irrational nome of the binomial root: (as, in the present case, $2\frac{1}{2} \pm \frac{1}{2}\sqrt{-3}$,) and what that rational multiplier is, will not be hard to discover, seeing that we are sure enough, from the construction of a binomial cube. Supposing the root to be $a + \sqrt{b}$, the cube must be $a^3 + 3a^2\sqrt{b} + 3ab + b\sqrt{b}$, of which the first and third parts are contained in one, and the second and fourth parts in the other member of the binomial: or, if the root should chance to be $a + e\sqrt{b}$ the cube will be $a^3 + 3a^2e\sqrt{b} + 3ae^2b + e^3b\sqrt{b}$, in which e will not be hard to find out.

For the logarithms of the hundred and first century, I have seen in it, in those books of Mr. Briggs in folio, and directions (I think from thence) to fill up others. And by those directions it is presumed that Vlacq supplied those centuries that Mr. Briggs left unfinished. What those directions are, I do not particularly remember, but I have either seen them there, or at least been told there are such.

The rule to reduce a biquadratic equation to a cubic is that I suppose which Des Cartes hath in his Geometry, lib. 3, pag. 79. editionis 1659, but without any demonstration of the grounds of it: and all his commentators have been so kind as not to give us any account of the grounds of it. Of this also, in the year 1648, in answer to a letter of Mr. Smith, which was the first occasion of my sight of Des Cartes's Geometry, then extant only in French, being by him desired to give him an account of it, I (because he saith nothing of the way whereby he came at it) set myself to find out a rule to do it, which proved to be the same with his, and in demonstrating it I did his also, which I have since communicated to Dr. Twysden and others, but is somewhat too large here to insert, but you may command it when you please. What you say of so multiplying the unknown root of the cubic equation, as that thereby the cube of one third of the coefficient shall be always made greater than the square of half the resolvend, I am very ready to believe, though I have not yet considered it. But even without this, I think that all cubic equations, and consequently biquadratics, may easily be brought under Cardan's rules, (at least I do not yet see what should hinder it, since that the impossibility of $\sqrt{-}$ hinders it not,) unless where the binomial cube will not admit of an extraction of its root. As for Dulaurens, I do not look upon him to have any great matter, but what he hath from others, and what notions he hath were but crude and undigested, and of which he was not at all master. Frenicle I take to be very good at numeral questions, such as those of Diophantus, to which he had a peculiar genius, and did accordingly apply himself; so that (as sir Ken. Digby hath told me) Fermat and Des Cartes did both give him the preeminence therein. The method you speak of, as material in algebra but wanting, of taking such roots as that the intermediate powers may vanish, is to be thus ordered. If the roots be so taken, partly affirmative and partly negative, that their aggregate equal 0, the second term doth vanish; if all their rectangles, the third vanishes; if all their solids, the fourth; and so on. As if $a+b-c-d+e=0$, $ab-ac-ad+ae-bc-bd+be+cd-ce-de=0$; $-abc-abd+abe+acd-ace-ade+bcd-bce-bde+cde=0$; and so on. But what are the best means to come at these, I have not considered, nor have I at present time so to do; but am

yours to serve you,

JOHN WALLIS.

What I wrote concerning Mr. Merry was only to know of him what that particular mistake was in Hudden, which he says doth influence divers following propositions. This Schooten desires to know. But as to the whole of Mr. Merry's piece, reduced into a fit method, I judge [it] very fit to be printed. I am glad that piece of Mr. Kersey, now in the press, finds so good encouragement.

You may please to keep this by you; for what I now write hastily in answer to the particulars of yours, it is very likely I may forget, keeping no copy of it by me, as seldom I do of what I write to you, but possibly there may be occasion of reviewing it, if you should desire any thing of this to be drawn up in form.

CCCXXXIV.

WALLIS TO COLLINS.

April 8, 1673.

Sir,

Yours of March 27 came not to me till March 30 at night; before which time I had answered yours of March 22: to which I have but this to add, that the method there cited out of Des Cartes' Epistles was before published by Schooten in the Appendix to his Commentaries on Des Cartes' Geometry in both editions, 1649 and 1659, which I was not aware of when I wrote that letter. My method in my letter mentioned served me without difficulty, for *e*, being always either an integer or half an integer, cannot be hard to find; and it serves indifferently for cubic binomials, or those of a higher power.

Yours of March 27 I have forborn to answer, expecting the papers therein mentioned, which are not yet come to hand, though I have several times sent to the carrier to inquire after them. The method of depressing biquadratic equations to quadratic by the help of a cubic, mentioned in my last, is this, being the sum of what I wrote to Mr. Smith, then mathematic professor in Cambridge, Nov. 28, 1648. Des Cartes in his Geometry, (p. 383 of the French edition, that is,) p. 79, edit. 1659, having (by casting out the second term) reduced his biquadratic equation to this form $x^4 \ldots pxx \ldots qx \ldots r = 0$, doth by help of this cubic equation $y^6 \ldots 2py^4 \left.\begin{matrix} +pp \\ \ldots 4r \end{matrix}\right\} y^2 - qq = 0$, resolve it into these two quadratics $xx - yx + \frac{1}{2}yy \ldots \frac{1}{2}p \ldots \frac{q}{2y} = 0$

$$xx + yx + \tfrac{1}{2}yy \ldots \tfrac{1}{2}p \ldots \frac{q}{2y} = 0$$

adding the signs of + or − according to certain directions there given.

For finding out the reason of his method, which he was pleased to conceal, I suppose with him the biquadratic to be made up of two quadratics, wherein (waving his terms $\frac{1}{2}yy$, $\frac{1}{2}p$, &c., because I presume them to be the result of some more simple operations) I take these two of my own,

$$xx - ax \ldots b = 0$$
$$xx + ax \ldots c = 0$$

where I do, as at pleasure I may, put the first term affirmative; and (because else the second term in the biquadratic would not be wanting) the second term, in one negative, and in the other affirmative, and with the same coefficient in both; the last term I leave uncertain to be supplied with + or − as it shall happen, for which, whatever it be, I put $\pm$[b], and for the contrary $\mp$. These multiplied together to make up the biquadratic,

$$\begin{array}{l} xx - ax \pm b = 0 \\ xx + ax \pm c = 0 \\ \hline x^4 - aaxx \pm bax \pm b \times \pm c \\ \quad \pm bxx \;\; \mp cax \\ \quad \pm cxx \\ \hline x^4 \pm pxx \;\; \pm qx \pm \;\; r = 0 \end{array}$$

[b] In the margin of a transcript is written, "Schooten, p. 340, Edit. 1659:" whose symbols for $\pm$ and $\mp$ are here used in the manuscript.

whence it appears that...... $aa \pm p = \pm b \pm c$

$$\pm \frac{q}{a} = \pm b \mp c$$

and therefore.............. $aa \pm p \pm \frac{q}{a} = \pm 2b$

$$aa \pm p \mp \frac{q}{a} = \pm 2c$$

that is .... $\frac{1}{2}aa \pm \frac{1}{2}p \pm \frac{q}{2a} = \pm b$

$$\frac{1}{2}aa \pm \frac{1}{2}p \mp \frac{q}{2a} = \pm c$$

which, supposing a to be known, gives the values of b and c, with their signs; that is,

$$xx - ax + \tfrac{1}{2}aa \pm \tfrac{1}{2}p \pm \frac{q}{2a} = xx - ax \pm b = 0$$

$$xx + ax + \tfrac{1}{2}aa \pm \tfrac{1}{2}p \mp \frac{q}{2a} = xx + ax \pm c = 0.$$

So that $\frac{1}{2}aa$ is still affirmative, $\frac{1}{2}p$ doth still retain the sign of p in the biquadratic, and $\frac{q}{2a}$, when it helps to express b, (that is, where it is $-ax$,) retains its sign, but where it helps to express c, (that is, where it is $+ax$,) it changeth the sign, which in the biquadratic it had; which agrees with Des Cartes his directions. So that now it appears that my a is the same with his y.

Then for finding a or y; because

$$\tfrac{1}{2}aa \pm \tfrac{1}{2}p \pm \frac{q}{2a} = \frac{a^3 \pm pa \pm q}{2a} = \pm b$$

$\frac{1}{2}aa \pm \frac{1}{2}p \mp \frac{q}{2a} = \frac{a^3 \pm pa \mp q}{2a} = \pm c$, their product is

$$\frac{a^6 \pm 2pa^4 + ppaa - qq}{4aa} = \pm b \times \pm c = \pm r$$

that is, $a^6 \pm 2pa^4 + ppaa - qq = \pm 4raa.$

that is, $\left.\begin{matrix}a^6 \pm 2pa^4 + ppaa \\ \mp 4raa\end{matrix}\right\} - qq = 0$, where a^6 and p^2 are always affirmative, because of multiplying like signs, qq always negative, because of multiplying unlike signs; $2p$ retains the sign of p, because multiplied into $+aa$; and $4r$, because of transposition, hath in this the contrary sign to what it had in the biquadratic: and these rules also agree with those of Des Cartes in the place cited. By which cubic equation having found the value of a^2 or y^2, and consequently of a or y, by it the two quadratic equations are resolved, and their four roots are the four roots of the biquadratic. The post is going; I can only add in haste, that I am yours to serve you,

JOHN WALLIS.

CCCXXXV.

WALLIS TO COLLINS.[c]

Sir, Oxford, April 12, 1673.

An equation of this form $a^2 - xa = Æ$ I soon discerned would, by adding $\frac{1}{4}x^2$ on both sides, become a perfect square $a^2 - xa + \frac{1}{4}x^2 = Æ + \frac{1}{4}x^2$, whose root is indifferently $a - \frac{1}{2}x$, or $\frac{1}{2}x - a = \sqrt{Æ + \frac{1}{4}x^2}$, and consequently $a = \frac{1}{2}x + \sqrt{Æ + \frac{1}{4}x^2}$, or $a = \frac{1}{2}x - \sqrt{Æ + \frac{1}{4}x^2}$, which I since find to be Dr. Pell's method of resolving quadratic equations.

In cubic equations I began to improve what I might in them, and I took out of his[d] store two cubic equa-

[c] This letter is printed from what had been copied into a folio book, and the handwriting is neither that of Wallis or Collins. The same book contains copies of the two preceding letters; and it may be right to add, that by a comparison of them with the originals several sentences were found to be omitted.

[d] "Mr. Oughtred" is written in the margin.

tions, such as might be expressed in two terms, as these, [e]$\mathfrak{z} + 3ÆZ = Z^3$ and $\mathfrak{x} - 3ÆX = X^3$, (where A and E are two quantities, A the greater, E the lesser, Z the sum, X the difference, $Æ$ the rectangle, $\mathfrak{z}$ the sum of their cubes, and $\mathfrak{x}$ the difference of their cubes;) that is by transposition $Z^3 - 3ÆZ = \mathfrak{z}$, and $X^3 + 3ÆX = \mathfrak{x}$. In which cubic equations $\frac{1}{3}$ of the coefficient known is the rectangle of two quantities, the sum or difference of whose cubes is the absolute quantity known, and the root sought is, in the first the sum, in the latter the difference of those quantities; so that now it amounts to this problem, The rectangle of two quantities, with the sum or difference of their cubes being known, to find the quantities.

This inquiry succeeded not amiss, for $\frac{Æ}{A} = E$, $\frac{Æ^3}{A^3} = E^3$, $A^3 + \frac{Æ^3}{A^3} = A^3 + E^3 = \mathfrak{z}$, $A^6 + Æ^3 = \mathfrak{z}A^3$, $\mathfrak{z}A^3 - A^6 = Æ^3$, which is a quadratic equation whose root is A^3. And in like manner $\frac{Æ}{E} = A$, $\frac{Æ^3}{E^3} = A^3$, $\frac{Æ^3}{E^3} + E^3 = A^3 + E^3 = \mathfrak{z}$, $Æ^3 + E^6 = \mathfrak{z}E^3$, $\mathfrak{z}E^3 - E^6 = Æ^3$, the root of which quadratic equation is E^3. That is, according to his method of resolving quadratic equations, $\frac{1}{2}\mathfrak{z} \pm \sqrt{\frac{1}{4}\mathfrak{z}^2 - Æ^3} = \frac{A^3}{E^3}$, the sum of whose cubic roots is $A + E = Z$, the quantity sought in the former of the cubic equations. In like manner $\frac{Æ}{A} = E$, $\frac{Æ^3}{A^3} = E^3$, $A^3 - \frac{Æ^3}{A^3} = A^3 - E^3 = \mathfrak{x}$, $A^6 - Æ^3 = \mathfrak{x}A^3$,

[e] $\mathfrak{z}$ and $\mathfrak{x}$ are here substituted for the forms of Z and X with the upper part of the preceding lines in them curved down, to represent which no modern types could be procured.

$A^6 - xA^3 = Æ^3$, the root of which quadratic equation is $A^3 = \sqrt{\frac{1}{4}x^2 + Æ^3} + \frac{1}{2}x$. So also $\frac{Æ}{E} = A$, $\frac{Æ^3}{E^3} = A^3$, $\frac{Æ^3}{E^3} - E^3 = A^3 - E^3 = x$, $Æ^3 - E^6 = xE^3$, $E^6 + xE^3 = Æ^3$, the root of which quadratic equation is $E^3 = \sqrt{\frac{1}{4}x^2 + Æ^3} - \frac{1}{2}x$. And having thus found A^3, E^3, the difference of their cubic roots is $A - E = X$, the quantity sought in the latter of the cubic equations.

Whence I framed to myself these two rules in an equation of this form, $Z^3 - 3ÆZ = x$, the root is $\sqrt[3]{\frac{1}{2}x + \sqrt{\frac{1}{4}x^2 - Æ^3}} + \sqrt[3]{\frac{1}{2}x - \sqrt{\frac{1}{4}x^2 - Æ^3}} = A + E = Z$. In this equation, $X^3 + 3ÆX = x$, the root is $\sqrt[3]{\sqrt{\frac{1}{4}x^2 + Æ^3} + \frac{1}{2}x} - \sqrt[3]{\sqrt{\frac{1}{4}x^2 + Æ^3} - \frac{1}{2}x} = A - E = X$.

I was not displeased at this my good success upon the first attempts of a young algebraist; and the rather because I did not know but that I was the first that had made this discovery, though since I find that Cardan had been before me.

Yet two or three things there were that troubled me. First, I found that in the former of these two forms, $\sqrt{\frac{1}{4}x^2 - Æ^3}$ would become impossible whensoever $Æ^3$ (the cube of $\frac{1}{3}$ of the coefficient) was greater than $\frac{1}{4}x^2$, (the square of $\frac{1}{2}$ of the absolute quantity,) yet durst not conclude the case impossible, because, in the addition, this impossible quantity was to vanish.

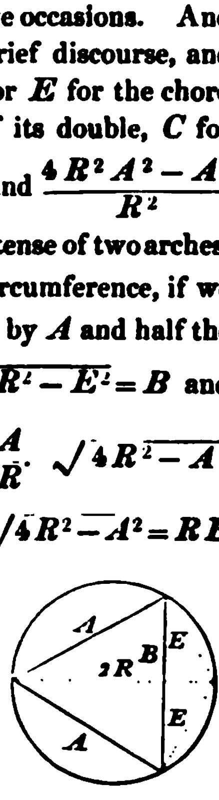
Next there was another form of cubic equations, $BZ - Z^3 = D$, which I would fain have brought under one of these forms, or have found another for it, but could not satisfy myself therein. For though, by transposition, $Z^3 - BZ = -D$ would be the same with the former of those, save that the absolute quantity is negative, but the square of its half the same as if it were affirmative, $Z^3 - 3\,Æ Z = -x$, yet it seemed inconvenient to make the sum of the cubes a negative quantity, and even thus, the same inconvenience would happen that I mentioned but now, viz. that $\sqrt{\frac{1}{4}x^2 - Æ^3}$ would often prove an impossible quantity, for I had not then confidence enough to introduce, without example, this notation, $\sqrt{-N}$ for $\sqrt{\frac{1}{4}x^2 - Æ^3}$, in this case.

There was yet a third difficulty, which I conquered; to extract the root of a binomial cube. As if $L^3 + 12L = 112$, that is $X^3 + 3\,Æ\,X = x$, and therefore $\sqrt[3]{\sqrt{\frac{1}{4}x^2 + Æ^3} + \frac{1}{2}x} - \sqrt[3]{\sqrt{\frac{1}{4}x^2 + Æ^3} - \frac{1}{2}x}, = X$, is $\sqrt[3]{\sqrt{3136 + 64}\,(= \sqrt{3200}) + 56} - \sqrt[3]{\sqrt{3200} - 56} = L$, where is $\sqrt{3200} + 56 = A^3$, and $\sqrt{3200} - 56 = E^3$. Of which binomial and apotome, the cubic roots are to be extracted for the quantities A, E, whose difference $A - E = X = L$, is the quantity sought. In order to which extraction I depress the irrational part $\sqrt{3200}$ by dividing 3200 by the greatest square number I can, prefixing its square root to the surd root of the quotient, (which serves also for other binomials as well as cubical,) that is, because $\frac{3200}{1600} = 2$ I find $\sqrt{3200} = 40\sqrt{2}$, whence it is certain that the surd part of the cubic root is $\sqrt{2}$, or at least some multiple of it by a rational number, (which will not be hard to find, as being an integer, or at least the half of an integer.) For supposing the binomial root of this cube $40\sqrt{2} \pm 56$ to be $a\sqrt{2} \pm e$, of which the cube is $2a^3\sqrt{2} \pm 6a^2e + 3ae^2\sqrt{2} \pm e^3$, it is manifest that one of the nomes contains the first and third part, and the other the second and fourth part, viz. $2a^3\sqrt{2} + 3ae^2\sqrt{2} = 40\sqrt{2}$, (and consequently $2a^3+3a\epsilon^3=40$), and $6a^2\epsilon+\epsilon^3=56$, whence a and ϵ will not be hard to find, if the binomial cube do admit of such a root. The roots in the present case are $2\sqrt{2}+2=A$, $2\sqrt{2}-2=E$, and therefore $A-E=4=X=L$, the quantity first sought.

I may now add, though I durst not then be so venturous without a precedent, that the same method serves for those cases which brought me to impossible or but imaginary quantities. As, if I had $L^3-63L=-162$, that is, say I, $Z^3-3ÆZ=-z$, and therefore $\sqrt[3]{\frac{1}{2}z+\sqrt{\frac{1}{4}z^2-Æ^3}}+\sqrt[3]{\frac{1}{2}z-\sqrt{\frac{1}{4}z^2-Æ^3}}=Z$, is $\sqrt[3]{-81+\sqrt{6561-9261}}+\sqrt[3]{-81-\sqrt{6561-9261}}$, that is $\sqrt[3]{-81+\sqrt{-2700}}+\sqrt[3]{-81-\sqrt{-2700}}=L$. Where is $-81+\sqrt{-2700}=-81+30\sqrt{-3}=A^3$, and $-81-30\sqrt{-3}=E^3$, whose roots we will suppose to be $-a\pm\epsilon\sqrt{-3}$, so is $-a^3+9a\epsilon^2=-81$, and $3a^2\epsilon\sqrt{-3}-3\epsilon^3\sqrt{-3}=30\sqrt{-3}$, (and consequently $3a^2\epsilon-3\epsilon^3=30$, or $a^2\epsilon-\epsilon^3=10$,) where we shall find $a=\frac{9}{2}$, $\epsilon=\frac{1}{2}$, and accordingly $-\frac{9}{2}+\frac{1}{2}\sqrt{-3}=A$, $-\frac{9}{2}-\frac{1}{2}\sqrt{-3}=E$, and $A+E=-9=Z=L$, the quantity sought. Or if I had at first put it $L^3-63L=+162$, I had by the same steps found $+\frac{9}{2}+\frac{1}{2}\sqrt{-3}=A$, $+\frac{9}{2}-\frac{1}{2}\sqrt{-3}=E$, and $A+E=9=Z=L$, the quantity sought.

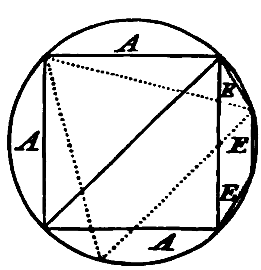
But without this addition I was well content with my success so far, and proceeded, for my further exercise, where Mr. Oughtred ends his Clavis, to the business of angular sections.

In order to this, waving Mr. Oughtred's method in this place, I chose to make use of a theorem, which he mentions elsewhere: That of quadrilaterals in a circle the rectangle of the diagonals is equal to the two rectangles of the opposite sides, which I since find to be a lemma made use of by Ptolemy on like occasions. And in pursuance thereof I drew up a brief discourse, and found that putting R for radius, A or E for the chord of a single arch, B for the chord of its double, C for its treble, &c., $\frac{A}{R}.\ \sqrt{4R^2-A^2}=B$, and $\frac{4R^2A^2-A^4}{R^2}$ $=B^2$, and because B is equally the subtense of two arches, which together make up the whole circumference, if we suppose half the one to be subtended by A and half the other by E, it will as well be $\frac{E}{R}.\ \sqrt{4R^2-E^2}=B$ and $\frac{4R^2E^2-E^4}{R^2}=B^2$ and consequently $\frac{A}{R}.\ \sqrt{4R^2-A^2}$ $=B=\frac{E}{R}.\ \sqrt{4R^2-E^2}$, that is, $A.\ \sqrt{4R^2-A^2}=RB$ $=E.\ \sqrt{4R^2-E^2}$, and $4R^2A^2-A^4$ $=R^2B^2=4R^2E^2-E^4$. So that a quadratic equation of this form must needs have two roots and both affirmative, for whether A^2 or E^2 be the quantity sought, the unknown quantities are the same. And because by transposition $4R^2A^2-4R^2E^2=A^4-E^4$, and dividing both by A^2-E^2, $4R^2=\frac{A^4-E^4}{A^2-E^2}=A^2+E^2$, (where A, E, are the subtendants of two arches which together make up $\frac{1}{2}$ the circumference, and therefore contain a right angle, whose hypotenuse is $2R$), hence follows that known and useful theorem, that in a right-angled triangle the square of the hypotenuse is equal to the two squares of the other sides.



In like manner I found $B^2=A^2+AC$, that is,

$\frac{4R^2A^2-A^4}{R^2}=A^2+AC$, or $\frac{3R^2A^2-A^4}{R^2}=AC$, and $C=\frac{3R^2A-A^3}{R^2}=3A-\frac{A^3}{R^2}$. And because C is equally the subtense of two arches, which together make up the whole circumference, if we suppose a trient of the one subtended by A and the other by E, it must as well be $3E-\frac{E^3}{R^2}=C$,



and consequently this equation, $3R^2A-A^3=R^2C=3R^2E-E^3$, must necessarily have two affirmative roots, the known quantities being the same whether A or E be sought. And moreover, by transposition, $3R^2A-3R^2E=A^3-E^3$, and, dividing both sides by $A-E$, $3R^2=\frac{A^3-E^3}{A-E}=A^2+AE+E^2$, so that A, E, being the subtendants of two arches, which together make up $\frac{1}{3}$ of the circumference, and consequently contain an angle of 120°, that is, $\frac{2}{3}$ of two right angles, whose subtense is $\sqrt{3R^2}$, the side of a regular triangle inscribed in the circle, hence it follows, that in a triangle, whose legs contain an angle of 120°, the square of the base is equal to the two squares of the legs and a rectangle of them.

But I further observed, that if the single be more than a trient, and less than two trients, whose subtendant Z I suppose to be the chord of a trient increased by one of the arches, A or E, the subtendant of the triple, which in this case will be more than the whole circumference, will be C, the same with that of the triple of the arch A or E, for the triple of $\frac{1}{3}+A$ is $1+3A$, that is, one whole circumference with the triple of the arch A, and therefore will begin and end at the same point, as if we had only taken the triple of A, and therefore the same equation must also have a third root Z. But because in this case Z is greater than $\sqrt{3R^2}$, the side of a regular triangle inscribed, $\frac{3R^2Z^2-Z^4}{R^2}=ZC$ will be a negative quantity, and consequently taking C as before, Z must be a negative root, and therefore $-3Z+\frac{Z^3}{R^2}=C$.

So that now we have $3R^2A-A^3=3R^2E-E^3=R^2C=-3R^2Z+Z^3$, a cubic equation of three roots, $+A, +E, -Z$, or, changing the signs, $-3R^2A+A^3=-3R^2E+E^3=-R^2C=3R^2Z-Z^3$, whose roots are $-A, -E, +Z$, the quantities A, E, Z, being chords drawn from any one point of the circumference to the three angles of the inscribed equilateral triangle, which all have the same C, the subtense of the triple arch. And by the same steps we find, by the quadrisection of an arch or angle, a biquadratic equation of four roots; by the quinquisection a sursolid equation of five roots, &c.; being so many chords drawn from the same point to all the angles of an inscribed regular tetragon, pentagon, &c.

Which gave me satisfaction in two things; first, that cubical equations, and those of higher powers, are capable, as I before suspected, of more roots than two; secondly, that those cubic equations which before brought me to an impossible construction, (viz. the square root of a negative quantity,) were not impossible cases, for in these equations R^6, the cube of $\frac{1}{3}$ of the coefficient is greater (at least not lesser) than $\frac{1}{4}R^4C^2$, the square of half the absolute quantity, (that is, $4R^2$, the square of the diameter, greater, or at least not lesser, than C^2;) and yet all these are possible cases.

So that in equations of this form $Z^3 - 3ÆZ = \pm x$, though that case do happen, which was the difficulty above mentioned, we are not to conclude the equation impossible; but when by the former method we are fallen upon this impossible construction, it will in this way be found possible, like as on the contrary, what is in that method possible, (without being reduced to $\sqrt{-N}$,) is impossible in this way; (for the one requires the cube of $\frac{1}{3}$ of the coefficient not to be less, the other not to be bigger, than the square of $\frac{1}{2}$ the absolute quantity,) save only where the said cube and square be equal, for then $\sqrt{\frac{1}{4}x^2 - Æ^3} = 0$ is extinguished, and it is indifferent to whether of the two methods it be referred. And these methods carry their limitations with them; for those of the first sort have but one real root and two imaginary, those of the latter sort have three real.

And, as before, for an angle of 120°, or $\frac{2}{3}$ of two right angles, so here for an angle of 60°, or $\frac{1}{3}$ of two right angles, (for such is that contained by ZA, or ZE, and the subtendant thereof $\sqrt{3R^2}$,) because $-3R^2Z + Z^3 = R^2C = 3R^2A - A^3$, that is, $3R^2Z + 3R^2A = Z^3 + A^3$, and, dividing both sides by $Z + A$, $3R^2Z = \frac{Z^3 + A^3}{Z + A} = Z^2 - ZA + A^2$, it follows that in a triangle whose legs contain an angle of 60°, the square of the base is equal to the squares of the two legs, wanting the rectangle of them.

In like manner from the equations for quadrisection, we gather the proportion of the square of the base to the squares of the legs, when the angle is $\frac{3}{4}$, $\frac{2}{4}$, $\frac{1}{4}$, of two right angles. And the like from the quinquisection, when the base subtends $\frac{4}{5}$, $\frac{3}{5}$, $\frac{2}{5}$, $\frac{1}{5}$, of two right angles. And so consequently for other sections.

After this progress, I found in Des Cartes's Geometry my two first rules for cubic equations, cited by him as Cardan's rules, but without any demonstration or reason of them given. And afterwards, consulting Cardan, I found in him a demonstration, but so intricate and perplexed, that I did not despise my own. My other method, by the trisection of an angle, I found he had also prosecuted with a like issue, and had met with just the same difficulties and obstructions that I had done: so that I now believed those obstructions not to arise from a defect in my search, but from the nature of the thing. I found also my negative roots owned by him under the name of false roots, though they are indeed as true as the other; and a plurality of roots in superior equations. And it was some satisfaction that I had in my first attempts lighted on those successes, which I found so great masters to esteem as things of good worth.

That which I most valued in his method, and which pleased me best, was his way of bringing over the whole equation to one side, making it equal to nothing, and thereby forming his compound equations by the multiplication of simples, from thence also determining the number of roots, real or imaginary, in each. This artifice, on which all the rest of his doctrine is grounded, was that which most made me to set a value on him, presuming it had been properly his own; but afterwards I perceived that he had it from Harriot, whose Algebra was published after his death in the year 1631, six years before Des Cartes's Geometry in French in the year 1637: and yet Des Cartes makes no mention at all of Harriot, whom he follows in designing his species by small letters, and the powers of them by the number of dimensions, without the characters of *q*, *c*, *qq*, &c.

Des Cartes his rule for depressing a biquadratic to a cubic equation is this. Having (by casting out the second term) reduced his biquadratic to this form, $x^4 \ldots pxx \ldots qx \ldots r = 0$, (leaving blanks for the signs $+$, $-$, to be supplied as the case may require,) he bids us write for it this cubic equation, $y^6 \ldots 2py^4 \left.\begin{matrix} +pp \\ \ldots 4r \end{matrix}\right\} yy - qq = 0$, by help of which he resolves the former into these two quadratics,

$$xx - yx + \tfrac{1}{2}yy \ldots \tfrac{1}{2}p \ldots \frac{q}{2y} = 0$$

$$xx + yx + \tfrac{1}{2}yy \ldots \tfrac{1}{2}p \ldots \frac{q}{2y} = 0$$

supplying the vacant places with $+$, $-$, according to certain precepts then given.

To discover the grounds of this; supposing (with him) the biquadratic to be made by the multiplication of two quadratic equations, waving his terms of $\frac{1}{2}yy$, $\frac{1}{2}p$, &c., as presuming them to be the results of more simple operations, I took these two of my own, $\left.\begin{matrix} xx - ax \ldots b = 0 \\ xx + ax \ldots c = 0 \end{matrix}\right\}$ wherein, taking in both, as I might do at pleasure, the first terms affirmative, the second terms I take negative in the one and affirmative in the other, and both with the same coefficient, (not but that two quadratics in whatever form would by multiplication make a biquadratic,) but because, if either of the signs were the same in both, or the coefficient not the same, the second term (which is supposed, if any were, to be already taken away by a precedent operation) would not be wanting in the biquadratic, and because the signs of b, c, are yet uncertain, as well as their values unknown, for the respective sign of each, be it $+$ or $-$, I put $\pm$, and $\mp$ for the contrary, and multiplying the one into the other, take their product to be equal to the biquadratic proposed.

$$\begin{array}{l} xx - ax \pm b = 0 \\ xx + ax \pm c = 0 \\ \hline x^4 - a^2x^2 \pm b\,a\,x \pm bx \pm c \\ \quad \pm b\,x^2 \mp c\,a\,x \\ \quad \pm cx^2 \\ \hline x^4 \pm px^2 \pm q\,x \pm r = 0 \end{array}$$

whence it appears that $aa \pm p = \pm b \pm c$, and $\frac{\pm q}{a} = \pm b \mp c$, and therefore

$$a^2 \pm p \pm \frac{q}{a} = \pm 2b, \text{ that is } \tfrac{1}{2}a^2 \pm \tfrac{1}{2}p \pm \frac{q}{2a} = \pm b$$

$$a^2 \pm p \mp \frac{q}{a} = \pm 2c, \qquad \tfrac{1}{2}a^2 \pm \tfrac{1}{2}p \mp \frac{q}{2a} = \pm c.$$

which, supposing a to be known, gives the value of ...b and ...c with their signs, that is,

$$xx - ax + \tfrac{1}{2}aa \pm \tfrac{1}{2}p \pm \frac{q}{2a} = xx - ax \pm b = 0$$

$$xx + ax + \tfrac{1}{2}aa \pm \tfrac{1}{2}p \mp \frac{q}{2a} = xx + ax \pm c = 0$$

so that here $\tfrac{1}{2}aa$ is still affirmative, $\tfrac{1}{2}p$ doth still retain the same sign that p had in the biquadratic, but $\frac{q}{2a}$ when it helps to express b, (that is, where we have $-ax$,) retains the sign of q in the biquadratic, but in that equation where it helps to express c, (that is, where it is $+ax$,) it changeth its sign, which is in brief the sum of Des Cartes his precepts, pag. 81, and argues my a to be the same with his y.

Then for the finding of a or y;

$$\text{because } \tfrac{1}{2}a^2 \pm \tfrac{1}{2}p \pm \frac{q}{2a} = \frac{a^3 \pm pa + q}{2a} = \pm b$$

$$\tfrac{1}{2}a^2 \pm \tfrac{1}{2}p \mp \frac{q}{2a} = \frac{a^3 \pm pa \mp q}{2a} = \pm c$$

their product is $\frac{a^6 \pm 2pa^4 + p^2a^2 - q^2}{4a^2} = \pm bx \pm c = \pm r,$

that is, $a^6 \pm 2pa^4 + p^2a^2 - q^2 = \pm 4ra^2$, that is,

$$\left.\begin{matrix} a^6 \pm 2pa^4 + p^2 \\ \mp 4r \end{matrix}\right\} a^2 - q^2 = 0,$$ (which is

Des Cartes's equation for finding y,) in which a^6 and p^2 are always affirmative, because of multiplying like signs; q^2 always negative, because of multiplying unlike signs; $2pa^4$ retains the sign of p, because of $\pm pa$ multiplied into $+a^3$; and $4r$, because of transposition, hath the contrary sign to that of r in the biquadratic: which agree with Des Cartes's precepts, p. 79. By which equation, having found the value of a, or y, by it, (that is, by some one of the three) we find the roots of the two quadratic equations the same with those of the biquadratic.

And this is the sum of what I formerly wrote to Mr. Smith for his satisfaction, and now repeat for yours. And, by the like method, other compounded equations may be reduced to more simple ones.

CCCXXXVI.

WALLIS TO COLLINS.

Oxford, May 6, 1673.

Sir,

I have yours of April 18 and May 3. As to what you say of multiplying the unknown root, &c. I supposed you had meant some other kind of multiplication in your former letter, by some way changing the form of the equation or the like. For barely changing the value of the unknown root, whether by addition, subtraction, multiplication, or division, will not at all help the matter, to make the designation by Cardan's rules any whit more possible, if before it were not. Of which I was well aware long ago, having considered that single change of value of the root. For instance; if it so fall out that the root, as the question is proposed, be to be thus expressed:

$$\sqrt[3]{a+\sqrt{-n}}+\sqrt[3]{a-\sqrt{-n}}$$

If we add or subduct b, or by b do multiply or divide, the result will be

$$+b+\sqrt[3]{a+\sqrt{-n}}+\sqrt[3]{a-\sqrt{-n}}$$
$$-b+\sqrt[3]{a+\sqrt{-n}}+\sqrt[3]{a-\sqrt{-n}}$$
$$b\sqrt[3]{a+\sqrt{-n}}+b\sqrt[3]{a-\sqrt{-n}}$$
$$\frac{\sqrt[3]{a+\sqrt{-n}}+\sqrt[3]{a-\sqrt{-n}}}{b}$$

in all which $\sqrt{-n}$ remains as before: and whether this change of the root be made while unknown, or when it becomes known, the importance will be the same.

If any thing can be done of this nature, it must be such as may change the proportion of $\sqrt{\frac{coeff.}{3}}$ and $\sqrt[3]{\frac{absol.}{2}}$.

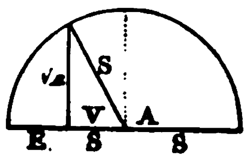
But the truth is, it needs no help. For I was of opinion from the first, that a negative plane may as well be admitted in algebra as a negative length, both being in nature equally impossible; for there can no more be a line less than nothing than a plane less than nothing, both being but imaginable; and if we suppose such a negative square, we may as well suppose it to have a side, not indeed an affirmative, or a negative length, but a supposed mean proportional between a negative and positive thus designable, $\sqrt{-n}$, or rather $\sqrt{-n^2}$, that is $\sqrt{+n \times -n}$, a mean proportional between $+n$ and $-n$. Only, though I had from the first a good mind to it, I durst not without a precedent, when I was so young an algebraist as in the history my late letter reports, take upon me to introduce a new way of notation, which I did not know of any to have used before me. And it was not without some diffidence that I ventured on $2\sqrt{3}$, instead of $\sqrt{12}$, not having then met with any example of a number so prefixed to a surd root; but I found it so expedient, not only for the discovering the root of a binomial, whether quadratic, cubic, or others, but for the adding and subducting of commensurable surds, that I resolved to use it for my own occasions, before I knew whether others would approve of it or no; especially having found in the first edition of Oughtred's Clavis, (for I had not then seen the second,) one instance or two for it, to justify myself if it should be questioned. But since that time it is grown more common, and I perhaps have somewhat contributed thereunto.

And had I but known of any precedent, (as since in Harriot I find one, and I think but one $\sqrt{-dddddd}$,) I should not have scrupled to follow it; but I was then too young an algebraist to innovate without example. Since that time I have been more venturous, and I find now that others do not scruple to use it as well as I.

This imaginable root in a quadratic equation I have had thoughts long since of designing geometrically, and have had several projects to that purpose. One of them was this:

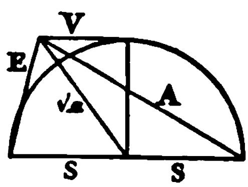
Supposing a quadratic equation $2SA - A^2 = Æ$, or (which is equivalent) $A^2 - 2SA + Æ = 0$.

If $S\left(=\frac{A+E}{2}\right)$ be bigger than $\sqrt{Æ}$; that is $S^2 > Æ$, the roots are $S \pm \sqrt{S^2 - Æ} = \begin{cases} A \\ E \end{cases}$, putting .... $S = \frac{1}{2}Z = \frac{A+E}{2}$ and ...... $V = \frac{1}{2}X = \frac{A-E}{2}$,

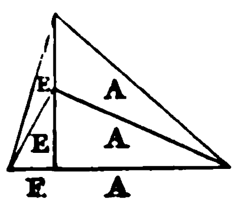


where $V\ (=\sqrt{S^2 - Æ})$, added to and taken from S, yields $S + V = A$, $S - V = E$, that is [the roots are] $S \pm \sqrt{+V^2}$.

But if $Æ$ be bigger than S^2, the roots are $S \pm \sqrt{S^2 - Æ}\ (= S \pm \sqrt{-V^2})$, where $\sqrt{Æ}$, which was the sine, now becomes the secant, and V, that was the cosine, is now the tangent. For $S^2 - Æ = V^2$, the difference of the planes S^2 and $Æ$, the greater is to be expressed by the hypotenuse, and the less by the perpendicular.



And so while $\sqrt{Æ}$, the supposed latus, becomes the hypotenuse, A and E, the supposed segments of the base, become the crura of the triangle, and whether A, E, be the segments of the base, or the crura, their difference of squares is still the same.



But (to pass this) in the cubic equation, as $aaa - 18a - 8 = 0$, when you have by Cardan's rules found one root, the other two are presently had; for, having found -4 to be one of the roots, the other are had by depressing it to a quadratic.

$$
\begin{array}{l}
a+4)a^3-18a\ -8(a^2-4a-2=0\\
\quad a^3+\ 4a^2\\
\hline
\quad\quad -\ 4a^2-18a-8\\
\quad\quad -\ 4a^2-16a\\
\hline
\quad\quad\quad -\ 2a-8\\
\quad\quad\quad -\ 2a-8\\
\hline
\quad\quad\quad\quad .\ \ .
\end{array}
$$

of which quadratic equation the two roots are $2+\sqrt{6}$ (affirmative), $2-\sqrt{6}$ (negative).

And so in all the others. But this, if I mistake not, you will always find, that where you do not meet that inconvenience of $\sqrt{-n}$, (the root of a negative square,) in the operation for the first root by Cardan's rules, you will certainly meet with it in this second operation for the other two roots, and vice versa. That is, in one of those operations, but never in both, you will always find it; save only where the cube of $\frac{1}{3}$ coefficient is equal to the square of $\frac{1}{2}$ homog. absol., wherein two of the three roots are coincident.

To my former letter of April 12, (which is at your disposal,) I would [add] a little consonant to what I now write; but have not time at present to write it, nor to take any copy of this. Resting

yours to serve you,

JOHN WALLIS.

The beginning of next week I think to be in London.

CCCXXXVII.

WALLIS TO COLLINS.

Oxford, Sept. 27, 1673.

Sir,

I have been otherwise so full of business that I have not had leisure to answer yours of Sept. 9; but my son hath for you, (if you please to call at his lodgings as you pass by,) the remainder of what I have of Angular Sections, which you may add to what you have already, and also that of abbreviating fractions or ratios, which you delivered to the Dean of Rochester,[f] and he to me, and I now return to you.

Mr. Bernard tells me he hath written (or means to write) to you about the twenty books he had. He hoped to have put off more, but some accident hath obstructed it for the present, by the absence of one beyond sea, who, had he been here, would have much conduced to it.

I do a little wonder that Mr. Gunton should find a difficulty in that which I thought had been very clear. But I do not there so much directly demonstrate [Des] Cartes's rule, as find out and demonstrate one of mine own, which in the issue proves to be the same with his. My whole process is this.

1. I suppose two quadratic equations, in both of which the root unknown is x, and both of them so reduced as to have the highest term affirmative, and the whole equal to nothing.

2. The coefficient of the second term in one I will suppose to be a, and in the other α; and for their

[f] Very Rev. John Castilion.

signs, whatever they be, I put $\pm$ and for the contrary $\mp$.

3. The absolute quantity, in that of a I make b; in that of a I make c: and their respective signs $\pm$, the contrary $\mp$.

4. So have I these two equations:

$$xx \pm ax \pm b = 0,$$
$$xx \pm \mathrm{a}x \pm c = 0,$$

which being multiplied the one by the other are to produce a biquadratic, in which, for the known quantities, I will put n, p, q, r, and for their respective signs $\pm$, and for the contrary $\mp$. That is,

$$x^4 \pm nx^3 \pm px^2 \pm qx \pm r = 0$$

5. I actually multiply those two quadratic equations, and suppose the product of that multiplication to be this biquadratic equation, whence I have the values of n, p, q, r, expressed by the symbols a, a, b, c. Thus,

$$\begin{array}{l} xx \pm ax \pm b = 0 \\ xx \pm \mathrm{a}x \pm c = 0 \\ \hline x^4 \pm ax^3 \pm bx^2 \\ \quad \pm \mathrm{a}x^3 \pm a\mathrm{a}x^2 \pm \mathrm{a}bx \\ \qquad\qquad \pm cx^2 \pm acx \pm b \times \pm c \\ \hline x^4 \pm nx^3 \pm px^2 \pm qx \pm r = 0 \end{array}$$

6. Hence it follows that $\pm n = \pm a \pm \mathrm{a}$, and $\pm p = \pm a\mathrm{a} \pm b \pm c$, and $\pm q = \pm \mathrm{a}b \pm ac$, and $\pm r = \pm b \times \pm c$.

7. I will now suppose the second term of the biquadratic to be wanting.

8. That is, $n = 0$, and therefore $\pm a \pm \mathrm{a} = 0$, which cannot be but that a, a must be equal and with contrary signs.

9. Therefore $\pm \mathrm{a} = \mp a$, and $\pm a\mathrm{a} = \mp a^2$, and $\pm \mathrm{a}b = \mp ab$.

10. Therefore whenever the second term in the biquadratic is wanting, or taken away, the two quad-

ratics must be supposed in this form, and thus multiplied, viz.

$$\begin{array}{l} xx \pm ax \pm b = 0 \\ xx \mp ax \pm c = 0 \\ \hline x^4 \pm ax^3 \pm bxx \\ \quad \mp ax^3 - a^2xx \mp a \times \pm bx \\ \qquad \pm cxx \pm a \times \pm cx \pm b \times \pm c \\ \hline x^4 \quad * \quad \pm px^2 \pm qx \pm r = 0 \end{array}$$

11. Hence therefore a in one of the quadratics hath the sign $+$, and in the other $-$; (and it is indifferent in whether of the two we put it;) I will suppose [it] in the former, where I make b the absolute quantity, to be $-a$, and in the latter, whose absolute quantity I call c, to be $+a$. And then the work will stand thus:—

$$\begin{array}{l} x^2 - ax \pm b = 0 \\ x^2 + ax \pm c = 0 \\ \hline x^4 - ax^3 \pm bx^2 \\ \quad + ax^3 - a^2x^2 \pm abx \\ \qquad \pm cx^2 \mp acx \pm b \times \pm c \\ \hline x^4 \quad * \quad \pm px^2 \pm qx \pm r = 0 \end{array}$$

12. Therefore $\pm p = \pm b \pm c - a^2$, that is $a^2 \pm p = \pm b \pm c$.

13. And $\pm q = \pm ab \mp ac$, that is $\frac{\pm q}{a} = \pm b \mp c$.

14. Which, added to the former, makes $a^2 \pm p \pm \frac{q}{a} = \pm 2b$.

15. And subducted from it makes $a^2 \pm p \mp \frac{q}{a} = \pm 2c$.

16. That is $\frac{1}{2}a^2 \pm \frac{1}{2}p \pm \frac{q}{2a} = \pm b$ and $\frac{1}{2}a^2 \pm \frac{1}{2}p \pm \frac{q}{2a} = \pm c$.

17. Therefore, supposing a to be known, (p and q with their signs being given in the biquadratic proposed,) we have the value of b and c with their signs.

18. We may therefore now, (having the values of $\pm b$, and $\pm c$,) for the two quadratic equations, substitute these equivalent, viz.

$$x^2 - ax + \tfrac{1}{2}a^2 \pm \tfrac{1}{2}p \pm \frac{q}{2a}, \text{ for } x^2 - ax \pm b,$$

$$x^2 + ax + \tfrac{1}{2}a^2 \pm \tfrac{1}{2}p \mp \frac{q}{2a}, \text{ for } x^2 + ax \pm c.$$

19. In these equations it is manifest that $\frac{1}{2}a^2$ hath always the sign $+$, and $\frac{1}{2}p$ doth, in both of them, keep its own sign, which it had in the biquadratic, but q doth in the former (where we have $-ax$) keep his sign, and in the latter (where we have $+ax$) changeth its sign.

20. That is; if in the biquadratic it were $+p$, it must be so here in both equations: if there it were $-p$, it must be so here.

21. And [if] in the biquadratic it were $+q$ it must here be $+\frac{q}{2a}$ in the former, and $-\frac{q}{2a}$ in the latter: if in the biquadratic it be $-q$, it must here be $-\frac{q}{2a}$ in the former, and $+\frac{q}{2a}$ in the latter.

22. These being just the same equations, and the same rules with those of Des Cartes, pag. 81, save here we have a instead of his y, shews that his y is the same quantity with our a, and his rule depends on the same principles with ours.

23. It remains therefore only to find the value of his y, or our a, which is thus done:

24. Because (by § 16) $\frac{1}{2}a^2 \pm \frac{1}{2}p \pm \frac{q}{2a}$, that is $\frac{a^3 \pm ap \pm q}{2a}$, is equal to $\pm b$, and $\frac{1}{2}a^2 \pm \frac{1}{2}p \mp \frac{q}{2a}$, that is

$\frac{a^3 \pm ap \mp q}{2a}$, is equal to $\pm c$; therefore the product of those multiplied the one by the other is equal to $\pm b \times \pm c$, that is (by § 6) to $\pm r$.

25. That multiplication performed will stand thus:

$$\begin{array}{l} a^3 \pm pa \pm q \text{ (divided by } 2a) = \pm b \\ a^3 \pm pa \mp q \text{ (divided by } 2a) = \pm c \\ \hline a^6 \pm pa^4 \pm qa^3 \\ \quad \pm pa^4 \qquad + p^2a^2 \pm p \times \pm qa \\ \qquad \mp qa^3 \qquad \pm p \times \mp qa - q^2 \\ \hline a^6 \pm 2pa^4 \quad * \quad + p^2a^2 \quad * \quad - q^2 \text{ (divided by } 4a^2) \\ \quad = (\pm b \times \pm c =) \pm r. \end{array}$$

26. That is, $a^6 \pm 2pa^4 + p^2a^2 - q^2 = \pm 4ra^2$.

27. That is, $a^6 \pm 2pa^4 \left.\begin{array}{l} + p^2 \\ \mp 4r \end{array}\right\} a^2 - q^2 = 0$.

28. This is therefore a cubic equation, whose root is a^2; in which it is manifest that p^2 is ever affirmative, q^2 negative, $2p$ keeps its sign + or − as it was in [the] biquadratic, and $4r$ changeth its sign; that is, if there it were +, it is here −, if there −, it is here +.

29. This being the same equation and the same rules for finding a, with those of Des Cartes (p. 79) for finding y, the demonstration is the same for both.

30. Having therefore this biquadratic proposed; (as in § 10) $x^4 * \pm px^2 \pm qx \pm r = 0$, if according to the rules of § 28, we constitute the cubic equation of § 27 $a^6 \pm 2pa^4 \left.\begin{array}{l} + p^2 \\ \mp 4r \end{array}\right\} a^2 - q^2 = 0$, which, being resolved, gives the value of a; we have then, (by § 16) the values of b and c, with their signs,

$$\pm b = \tfrac{1}{2}a \pm \tfrac{1}{2}p \pm \frac{q}{2a}$$

$$\pm c = \tfrac{1}{2}a \pm \tfrac{1}{2}p \mp \frac{q}{2a}$$

and thereby the two quadratic equations of § 10,

$$x^2 - ax \pm b = 0,$$
$$x^2 + ax \pm c = 0,$$

each of which has two roots, (real or imaginary,) which (being extracted) are the four roots of the biquadratic proposed.

All this, though in fewer words, was clearly enough contained in the former letter of April 12, 1673.

About the beginning of the term I think I shall be in London. I could wish that in the mean time you would inquire of those mathematicians who about the year 1657 were in London, and used to meet at Gresham College at Mr. Rooke's Lecture, what they do remember of Mr. Neale's giving a straight line equal to a crooked, which was done about June or July 1653. But M. Huygens will needs have it, that Heaurat, in the year 1659, was the first inventor of it, and that he had his hints from M. Huygens; and he in pursuance of finding a straight line equal to the cycloid, occasioned by that of Dr. Wren, who in the year 1658 (they confess) was the first inventor of.

As to the account with Mr. Pitts; I find the enclosed paper of your writing, which may give you some light on it. I am

yours, &c.

JOHN WALLIS.

CCCXXXVIII.

COLLINS TO WALLIS.

Reverend Sir,

Mr. Scot the bookseller intending for Oxford, I thought fit to acquaint you that he is the person concerned in the printing of Dr. Barrow's abridgment of the ancient geometers, particularly of Archimedes, some fragments whereof the Dr. hath not meddled with, as namely the Arenarius, concerning which he saith that the copy is so corrupt, that without more time than he can allow to it, he can make nothing of it. Perhaps there may be manuscripts at Oxford, which may help to correct it, and he thinks it a good subject for some of Mr. Bernard's lectures, there being divers astronomical matters in it, and others of kin to his profession. Besides what is here mentioned, the Dr. hath said nothing concerning the mechanics at the end of the edition of Rivaltus, nor any thing about the Lemmata at the end of Borellius' Explication of the three latter books of Apollonius's Conics; about which your advice and assistance will be obliging and enriching to the commonwealth of learning. I have caused my servant to transcribe Mr. Merry's explication of Hudden's Rules, and as soon as the copy is examined (which I hope will not be long) you may expect the original to be sent to you from

your humble, obliged, thankful servitor,

J. C.

CCCXXXIX.

WALLIS TO COLLINS.

Sir, Aug. 24, 1674. Oxford.

The two books you mention in yours of Aug. 19, I received two days since, on Saturday, Aug. 22, and thank you for them: the one is Mr. Anderson's and Mr. Streete's book of Gunnery, the other, you tell me, is a small piece of Mr. Gregory De Motu Projectorum, &c., at the end of Mr. Mather's Animadversions on

Mr. Sinclair's Ars nova et magna, &c. They pursue, as you observe, different hypotheses; and I will not deny but that, as you intimate, Mr. Anderson may have derived his principles from my writings: for my Prop. 8. Cap. 10. Mechan. pursues the same hypothesis (of Galilæus, Torricellius, and others), which supposeth projection to be compounded of an uniform motion, (impressed from the projector,) and an uniformly accelerated (from gravitation), without taking notice of the resistance of the air. And according to this hypothesis, (as I there shew,) such a parabola doth arise as Mr. Anderson supposeth. But the other might as well derive his from thence also; for in the scholium of that proposition I shew my reasons why I do not acquiesce in that hypothesis, because that former motion, which they suppose uniform, must needs be continually retarded by the resistance of the medium, and because otherwise a bullet ought to strike with the same force at the greatest distance as close by the mouth of the piece, which all experience doth contradict. So that, as I then did, so I still do, incline to the latter hypothesis, which supposeth it compounded of two motions, the one retarded, the other accelerated. And Galilæus himself, (who, I think, was the first that suggested the hypothesis,) though as to short distances he speaks as if the resistance of the air might be safely neglected, as too much perplexing the calculation, (wherein he is followed by Torricellius and others,) yet in his Dialogues De Motu he doth, as to great distances, put great weight upon it. And practical cannoneers, I am told, find the random of a bullet very different from the parabola, which that hypothesis doth establish: and I think in reason it ought to be so, which very much disturbs all tables calculated on that hypothesis. Nor do I think that Torricellius, &c.

did take that for the true hypothesis, but only made use of it as the more easy, and as exact enough as to some of the more gross inquiries, though not for all those niceties that are here deduced from it. The particular calculations and constructions I have not examined in either, but suspect Mr. Gregory's curve VTP, fig. 6, will be found not to be, as he supposeth, a parabola.

As to what you suggest of my abridging the Mechanics, &c. of Archimedes, of which Dr. Barrow says nothing, I shall hardly have leisure to do any thing of that kind time enough; and I think it better if Dr. Barrow would do it himself, that so the whole may be more uniform, as done by the same hand. As to the abridging of Pappus, &c. though I am not against the work, yet I had much rather hearken to the printing those ancients at large, in Greek and Latin, which hath yet never been done, and the authors thereby in danger of being lost. I have no more time, but to tell you that I am

yours to serve you,

JOHN WALLIS.

Let Mr. Oldenburg see this.

The account of the subsequent editions, as well as of the MS. of the Arenarius, will be found in a Paper read to the Ashmolean Society at Oxford by Professor Rigaud, Nov. 11, 1836.

CCCXL.

WALLIS TO COLLINS.

Sir, Oxford, Sept. 1, 1676.

I told you, in mine of yesterday, that the method of reducing fractions to lesser denominations, (quam-proxime æquales,) mentioned in yours of Aug. 19, I liked well, if it would give all the approaches, of which I doubted. I have since considered it, and find it will not. For instance, taking the proportion of the diameter to the perimeter of a circle to be as 10000000000 to 31415926536, proxime; the quotients thence to be found in your way, (which I suppose is the same with that of Dr. Davenant,) are these,

$$\left.\begin{matrix}3+\\4-\end{matrix}\right|\;3.\begin{matrix}7+\\8-\end{matrix}\;\left|\;3.7.\begin{matrix}15+\\16-\end{matrix}\;\right|\;3.7.15.\begin{matrix}1+\\2-\end{matrix}\;\left|\;3.7.15\;1.\begin{matrix}292+\\293-\end{matrix}\right|$$

which, according as you make use of the lesser or greater quotient in the last place, give us the approaches as in these two tables, and no more;

1	3 +	1	4 −
7	22 −	8	25 +
106	333 +	113	355 −
113	355 −	219	688 +
33102	103993 +	33215	104348 −[h];

and in each table the proportions are, alternately, one too big, another too little. But if you consult my papers, which you have on this subject, you will find some hundreds which are here omitted: and particularly, if it were proposed to give the nearest proportion to this in numbers not greater than 100000, there are 279 approaches nearer than any which this method gives. The proportions here given are only the first and second of every of my orders: the quotients are the multipliers noted at the beginning of each of my orders. This at present from

yours,

JOHN WALLIS.

[h] In the third volume of the Transactions of the Royal Asiatic Society of Great Britain and Ireland, (1834,) there is a very curious paper of Mr. Whish on the Hindu quadrature of the circle: and at p. 511 he quotes a passage from the Tantra Sangraha which gives the ratio of 33215 to 104348 for that of the diameter to the

CCCXLI.

WALLIS TO COLLINS.

Oxford, Sept. 11, 1676.

This is an English letter of the same date with that which follows, and containing nothing but what is there more fully stated in Latin.

CCCXLII.

WALLIS TO COLLINS.

Oxoniæ, Sept. 11, 1676.

Aderat hic apud me, anno præterito, Germanus quidam, nomine Tschirnhaus, hoc est, (sono pariter et significatu) literis Anglicanis Churn-house (i.e. ædificium quo solet agitato lactis cremore confici butyrum). Eundem credo, cujus literarum apographum, (Parisiis datarum Sept. 1, 1676,) a te cum tuis (Londini datis Sept. 9,) hesterna nocte accepi. Æquatio inibi prima, (ut ille ingeniose monet,) nempe $x^3 - pxx + qx - r = 0$, abjecto secundo termino, recepto more (hoc est, quem Harriotus noster primus tradidit, in ejus posthumo opere algebraico, Londini edito anno 1631; et Cartesius postmodum adoptavit,) si porro sit $q = \frac{pp}{3}$, etiam tertio carebit. Processus utique hic erit.

$$x^3 - pxx + qx - r = 0.$$

circumference of a circle. He does not mention Wallis's other numbers, which however are not quite so close.

33215 : 104348	: :	1 : 3.14159265392
The true ratio. .		1 : 3.14159265358
33102 : 103993	: :	1 : 3.14159265301

Pone $x = z + \frac{1}{3}p$;

$$\text{adeoque } xx = z^2 + \tfrac{2}{3}pz + \frac{p^2}{9}$$

$$x^3 = z^3 + pzz + \tfrac{1}{3}ppz + \frac{1}{27}p^3$$

$$\begin{array}{rcl} x^3 & = & z^3 + pzz + \tfrac{1}{3}ppz + \frac{1}{27}p^3 \\ -pxx & = & \quad -pzz - \tfrac{2}{3}ppz - \frac{1}{9}p^3 \\ +qx & = & \qquad\quad + qz + \frac{1}{3}pq \\ -r & = & \qquad\qquad\quad - r \\ \hline & & 0 = z^3 \;\; * \;\; -\tfrac{1}{3}ppz - \frac{2}{27}p^3 \\ & & \qquad\qquad + qz + \frac{1}{3}pq \\ & & \qquad\qquad\qquad - r \end{array}$$

Manifestum itaque est, si $\frac{1}{3}pp = q$, terminum tertium evaniturum; propter $-\frac{1}{3}ppz + qz = -\frac{1}{3}ppz + \frac{1}{3}ppz = 0$ Hujusmodi innumera exempla videas in Merrii nostri scripto quod mecum aliquando communicasti.

In æquatione secunda, quam ille sic exponit "in æquatione $x^4 - px^3 + qx^4 - rx^3 + s = 0$, auferendo secundum terminum, et ponendo $q = \frac{3pp}{4}$, duo termini $-px^3$ et qx^4, supponendo vero $q = \frac{2r}{p} + \frac{pp}{4}$ termini $-px^3 - rx^3$, seu secundus et tertius, se destruent," aliquid erratum esse suspicor, et sic restituendum, "in æquatione $x^4 - px^3 + qxx - rx + s = 0$, auferendo secundum terminum, et ponendo $q = \frac{3pp}{8}$ tolletur item tertius; ponendo vero $q = \frac{2r}{p} + \frac{pp}{4}$ tolletur terminus quartus." Quod simili processu, quo supra, comprobabitur.

In æquatione tertia $x^3 - pxx + qx - r = 0$, si sit

$q = \frac{3r}{p} + \frac{2pp}{9}$, (sic enim lego quod scriptum est $\frac{2pp}{q}$) radices sunt in progressione arithmetica, simili processu (quem descriptum videas in annexa chartula[1]) elicietur.

In æquatione quarta $x^5 + px^4 + qx^3 + rxx + sx + t = 0$, (ne mireris tot affirmativa æquari nihilo,) radices intelligendæ sunt negativæ omnes. Quippe si ponamus $x = -1$, $x = -2$, $x = -3$, $x = -4$, $x = -5$, æquatio ex omnibus composita erit $x^5 + 15x^4 + 85x^3 + 225xx + 274x + 120 = 0$. Operationem habes cum reliquis, in annexa chartula adscriptam.

Æquatio quinta $x^4 = pxx + q$ est quadratica ex radice plana, cujus radix (recepto more educta) est $xx = \frac{1}{2}p + \sqrt{\frac{1}{4}pp + q}$ (seu potius $xx = \frac{1}{2}p \pm \sqrt{\frac{1}{4}pp + q}$; gemina enim est, altera affirmativa, altera negativa;) adeoque $x = \pm\sqrt{\frac{1}{2}p + \sqrt{\frac{1}{4}pp + q}}$ (nam et hic radix gemina est). Sed et $x = \pm\sqrt{\frac{1}{2}p - \sqrt{\frac{1}{4}pp + q}}$ (nam et hic etiam est gemina radix). Adeoque expositæ æquationis biquadraticæ, radices sunt omnino quatuor.

In æquationibus septima et duodecima (quas ut huic quintæ æquipollentes ponit) radices habentur resolvendo quadratum binomium; (per Oughtredi Clavem, cap. 6, § 11). Nempe si quadrati binomii $\frac{1}{2}p + \sqrt{\frac{1}{4}pp + q}$ nomen majus $\sqrt{\frac{1}{4}pp + q}$ habeatur pro $z = A^2 + E^2$ (summa quadratorum), et nomen minus $\frac{1}{2}p$ pro $2AE = 2A \times E$ (duplo rectangulo) erit $\frac{1}{4}z^2 = \frac{1}{16}pp + \frac{1}{4}q$, et $AE^2 = \frac{1}{16}pp$, adeoque $\frac{1}{2}z \pm \sqrt{\frac{1}{4}z^2 - AE^2} = \sqrt{\frac{1}{16}pp + \frac{1}{4}q} \pm \sqrt{\frac{1}{4}q} = \frac{A^2}{E^2}$, adeoque $x = A + E =$

$$\sqrt{\sqrt{\tfrac{1}{16}pp + \tfrac{1}{4}q} + \sqrt{\tfrac{1}{4}q}} + \sqrt{\sqrt{\tfrac{1}{16}pp + \tfrac{1}{4}q} - \sqrt{\tfrac{1}{4}q}},$$

seu (ut ille habet in æquatione septima)

[1] These operations are written on a blank leaf of the English letter; but it did not seem necessary to print them.

$x=\sqrt{-\frac{1}{2}\sqrt{q}+\sqrt{\frac{q}{4}+\frac{pp}{16}}}+\sqrt{\frac{1}{2}\sqrt{q}+\sqrt{\frac{q}{4}+\frac{pp}{16}}}$, nam lego $+\sqrt{\frac{q}{4}+\frac{pp}{16}}$, quod in posteriori loco scriptum est $-\sqrt{\frac{q}{4}+\frac{pp}{16}}$. Verum si in eodem binomio quadrato, $\frac{1}{2}p+\sqrt{\frac{1}{4}pp+q}$, habeatur $\frac{1}{2}p$ (nomen minus) pro $\varkappa$, et $\sqrt{\frac{1}{4}pp+q}$ (nomen majus) pro $2Æ$, quod erit quadratum imaginarium impossibile; quippe in vero quadrato summa quadratorum partium major est, saltem non minor, duplo rectangulo sub partibus radicis, habebitur (simili calculo) $\frac{1}{2}\varkappa \pm \sqrt{\frac{1}{4}\varkappa^2 - Æ^2} = \frac{A^2}{E^2} = \frac{1}{4}p \pm \sqrt{-\frac{1}{4}q}$. Adeoque $x=A+E=\sqrt{\frac{1}{4}p+\sqrt{-\frac{1}{4}q}}+\sqrt{\frac{1}{4}p-\sqrt{-\frac{1}{4}q}}$ (ut in ipsius æquatione duodecima). At vero (quod nescio tamen an ille animadverterit) non æquipollent hæ designationes. Est enim x æquationis duodecimæ imaginaria radix impossibilis quadrati, (quod satis innuit illud $\sqrt{-\frac{1}{4}q}$.) Sed x æquationis septimæ est veri quadrati radix vera. Nec valet (quod arguit ille) utriusvis ductu in se idem restitui quadratum; videmus enim $-x$ et $+x$, in se ductas, idem restituere quadratum $+xx$; nec tamen sunt inter se æquales. Neque tamen repudio plane, ut prorsus inutilem, hanc designationem $\sqrt{-q}$, sed eam jam fere a triginta annis adhibeo; (ut et ante me Harriotus, in opere supra citato; ut non mirer eos qui nuperius id usurpaverint;) nam qua ratione $-x$ usurpamus pro litera ablativa, eadem et $-xx$ pro ablativo plano usurpari poterit; (nam negativa linea, puta quæ minor sit quam nihil, pariter est in se impossibilis atque planum negativum: utrumque tamen imaginari licet, hujusque quadrati imaginarii imaginaria radix censenda erit. Atque hanc (si memini) Harriotus etiam adhibet in designandis radicibus, (per Cardani regulam,) hujusmodi æquationum $x^3 = -px + q$, aut etiam $x^3 = +px + q$ quoties cubus trientis p major est quam quadratum semissis q.

Æquationis sextæ $x^3 = px + q$, radix $\sqrt[3]{\frac{q}{2} + \sqrt{\frac{qq}{4} - \frac{p^3}{27}}} + \sqrt[3]{\frac{q}{2} - \sqrt{\frac{qq}{4} - \frac{p^3}{27}}}$ est per regulam Cardani.

Reliquas non examinavi, sed existimo illas (ut præcedentium aliquas) pariter inventas, adhibitis abjectione secundi termini, et Cardani regulis, (admissis etiam imaginariis radicibus quadrati negativi,) et binomiorum resolutionibus, atque tum demum observando quænam adhibitæ conditiones faciant ut alius aliquis terminorum simul evanescat. Quod et Huddenium fecisse arbitror in exquirendis regulis suis pro reducendis æquationibus, et Merrium nostrum. Estque adhuc campus satis amplus, si quis exspatiari velit pluribus inquirendis.

Quod spectat tres, quas habet figuras; invenias, credo, ea quæ hic dicta sunt consequi ex eis quæ habentur in tractatu meo de Sectionibus Angularibus, (anno 1648 primum scripto,) quem apud te habes, (quamquam jam anni tot elapsi sint quibus illum non viderim, ut vix meminerim quid inibi habeatur,) ubi inter alia hoc saltem reperias. A peripheriæ quovis puncto, ad singulos inscriptæ figuræ cujusvis æquilateræ angulos ductæ rectæ, radices sunt æquationis totidem dimensionum quot sunt inscriptæ latera, alternatim affirmativæ et negativæ; omnesque simul additæ se perimunt. Quadraturam circuli quod spectat, de cujus possibilitate aut impossibilitate quærit; existimo illud a me jam olim demonstratum esse, in mea Arithmetica Infinitorum prop. 190, cum scholio subjuncto.

Nempe rationem circuli ad quadratum diametri, non posse numeris designari juxta receptas notandi formas, puta numeris integris aut fractis, aut potestatum radicibus surdis, aut etiam radicibus æquationum vulgo receptarum. Quippe quo hoc fieret, eo rem reductam esse ostendo, ut numerus impar secaretur in duos æquales integros, atque ut æquationes algebraicæ adhibeantur, inter lateralem et quadraticum, inter quadraticum et cubicum, (et sic de cæteris,) intermediæ, (quæ itaque radices habeant plures quam unam sed pauciores quam duas, plures quam duas sed pauciores quam tres, &c.) quorum utrumque est impossibile. Sed nova designatione seu notatione opus esse. Puta, sicut in serie geometrica, 1, 2, 4, 8, 16, &c. (ubi continuus multiplicator idem est, nempe 2,) terminus inter 1 et 2 intermedius, cum veris numeris, ne quidem fractis, accurate scribi possit, insinuetur utcumque radicalitatis (ut loquuntur) nota $\sqrt{2}$, sec $\sqrt{1 \times 2}$. Ita in serie quam dicas hyper-geometricam, 1, 6, 30, 140, 630, &c. (ubi continui multiplicatores non sunt unus aliquis numerus, sed $\frac{6}{1}, \frac{10}{2}, \frac{14}{3}, \frac{18}{4}$, &c., quorum tum numeratores tum denominatores sunt arithmetice-proportionales,) si terminus inter 1 et 6 intermedius, (qui numeris ne fractis quidem, sed neque surdis lateribus $\sqrt{\ }$ designari possit,) utcumque insinuetur hujusmodi nota [1]$M\,1 \mid 6$, dico, ut 1 ad $M\,1 \mid 6$, sic semicirculus ad quadratum diametri. Vel in serie hyper-geometrica, 1, $\frac{3}{2}$, $\frac{15}{8}$, $\frac{105}{48}$, $\frac{945}{384}$, &c. (cujus continui multiplicatores sunt $\frac{3}{2}$, $\frac{5}{4}$,

[1] This is not the exact symbol used by Wallis, but the nearest which the types would afford.

$\frac{7}{6}, \frac{9}{8}$, &c.) medius inter 1 et $\frac{3}{2}$, sic designatur $M\,1\left|\frac{3}{2}\right.$, seu $\square$; dico ut 1 ad $M\,1\left|\frac{3}{2}\right.$, sic circulus ad quadratum diametri. Sed et, ut terminus ille $\sqrt{2}$, in progressione geometrica, quamquam veris numeris non possit accurate designari, possit approximatione in infinitum eo perveniri ut differentia sit omni data minor: ita similiter $M\,1 \mid 6$, et $M\,1\left|\frac{3}{2}\right.$, seu $\square$, (aliaque istiusmodi;) puta $\square = \frac{9.25.49.81.121.\ \&c.,\ \text{sumptis quadratis imparibus in infin.}}{8.24.48.80.120.\ \&c.,\ \text{sumptis eisdem quadratis unitate minutis}}$.
Adeoque, posito circulo $=1$, erit quadratum diametri $=\frac{9}{8}\times\frac{25}{24}\times\frac{49}{48}\times\frac{81}{80}$, &c.; positoque quadrato diametri $=1$, erit circulus $=\frac{8}{9}\times\frac{24}{25}\times\frac{48}{49}\times\frac{80}{81}$, &c, seu $\frac{2}{3}\times\frac{4}{3}\times\frac{4}{5}$ $\times\frac{6}{5}\times\frac{6}{7}\times\frac{8}{7}\times\frac{8}{9}\times\frac{10}{9}\times\frac{10}{11}\times\frac{12}{11}$, &c. vel etiam

$$1\frac{1}{2\frac{9}{2\frac{25}{2\frac{49}{2\frac{81}{2}}}}}$$

ope fractionis denominatorem habentis continue fractum in infinitum, fractionum vero singularium numeratores, 1, 9, 25, 49, &c. quadratos impares. Eademque quantitas, variis adhuc modis, per approximationes in infinitum, seu (ut loquuntur) per series infinitas, designari potest. Sed hæ (quantum scio) sunt omnium primæ, viginti abhinc annis editæ. Quique ex eo tempore de seriebus infinitis commentati sunt, hinc credo (si non immediate, mediate saltem, alii ab aliis incitati) sumpserunt ansam. Sed de his hactenus. Qui plura velit, tractatum ipsum adeat, qui totus est de seriebus infinitis. Tu interim vale.

CCCXLIII.

WALLIS TO COLLINS.[k]

Oxoniæ, Sept. 16, 1676.

Quam ante paucos dies mittebam, Sept. 11 datam, Anglice scriptam, hic habes inclusam, quoniam id expedire visum est, Latine redditam (D. Tschirnhausium quod spectat) sensu saltem eandem, si non et verbis. Interea temporis, hesterna nocte, accepi a te, pariter cum tuis Sept. 14 datis, literarum D. Leibnitii apographum. Ad quarum tamen singulos apices ut statim descendam nolim expectes. Non modo quod longa sit epistola, et rerum profundarum plena; sed (ut quod res est dicam) quamquam nuperis de seriebus infinitis meditationibus videar ego facem prætulisse in mea Infinitorum Arithmetica, (ante viginti annos edita,) quæ de seriebus infinitis tota est; ex eo tamen tempore aliis avocatus studiis, non multum temporis eisdem promovendis impendi nuper, nec in promptu habeo quid ab aliis nuperius scriptum sit. Mercatoris quadraturam hyperbolæ probe memini, ingeniosam quidem et feliciter præstitam; deque ea nonnihil commentatus in lucem ipse dedi. Quid porro ea de re edidit Gregorius memini me legisse quidem quamprimum prodiit, sed jam fere oblitus sum. Quid Newtonus præstitit ne vidi quidem, nec (ante tuas receptas literas) de Leibnitii hac in re meditatis quidquam. Oportet itaque ut ea recolem omnia antequam judicium de his aut illis fecero. Sunt utique omnes viri magni, de quibus non est temere pronunciandum. Leibnitii seriem pro circuli quadratura,

[k] This, with the addition in English, is written on the same sheet, and was evidently sent by the same post to Collins with the preceding letter, (No. cccxliii.)

mihi prius inauditam,) jam primum perpendi, quæ et ingeniosa est, et a meis plane diversa. Quippe ego (ut reliquas taceam) hanc seriem infinitam multiplicando exhibeo, $\frac{8}{9} \times \frac{24}{25} \times \frac{48}{49} \times \frac{80}{81}$ &c. seu $\frac{2}{3} \times \frac{4}{3} \times \frac{4}{5} \times \frac{6}{5} \times \frac{6}{7} \times \frac{8}{7} \times \frac{8}{9} \times \frac{10}{9}$ &c..... Addendo, hanc $\frac{1}{1} - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \frac{1}{9}$ &c. pro magnitudine circuli; posito quadrato diametri..... Qua methodo ad hanc pervenerit nondum scio. Video autem, re ad calculum reducta alternatim excedi et defici a Culenii numeris 0.785398 + ; sed tarde admodum eo accedi utrinque. Nam ubi ad $-\frac{1}{91}$ perventum est, habetur pro minori justo 0.779964 + : ubi ad $+\frac{1}{93}$, habetur pro majori justo 0.790716 + ut ne quidem primores duæ notæ 0.78 adhuc compareant. Longum utique esset, calculo detegere, siquid erratum foret. Non autem est cur admodum dubitem quin res in tuto sit.

De numero impossibili $\sqrt{1 + \sqrt{-3}} + \sqrt{1 - \sqrt{-3}} = \sqrt{6}$ quid dicendum censeam, videns ex eis quæ ad D. Tschirnhausii literas dicta sunt. Omnino quidem verum est $\sqrt{1 + \sqrt{-3}} + \sqrt{1 - \sqrt{-3}}$ in se ductam restituere quadratum 6; adeoque hujus radicem quadantenus esse, non tamen tanquam quadrati veri, sed monstrosi et imaginarii tantum, utpote quod habeat duplum rectangulum, majus summa quadratorum partium radicis. Vide annon dicendum potius $\sqrt{1 + \sqrt{-3}} + \sqrt{1 - \sqrt{-3}} = -\sqrt{-6}$.

Æquationes biquadraticas ad cubicas, et quadricubicas ad sursolidas, et quæ ab octava nona decimaque potestate denominatas, ad denominatas a septima, (et similiter de denominatis ab alia quavis potestate numero composito notata ad denominatas a proxime inferiori potestate numero primo, seu incomposito, notata reducendis,) apud Cartesium, (ni male memini,) alicubi traditum est. Sed operoso id labore factum iri, in superioribus potestatibus, non est ambigendum. Regulas quod spectat pro reducendis æquationibus, laudabilem operam in ea re posuerunt Huddenius, Merrius, et alii. Sed operosum est negotium rem illam penitus exhaurire. Ego quidem, operoso calculo, quem in tractatu meo De Motu solus sustinui, satis lassatus, contentus sum a labore calculi aliquantisper abstinere, et successuris aliis lampadem tradere. Quæ alia sibi expedienda proposuit Vir Cl. absolvat precor bonis avibus. Tu interim vale.

Sept. 16, 1676.

I have sent you these letters in Latin, that you may have them ready, if you should have a mind to transmit them. I received your packet of Sept. 12. The papers, if you can spare them, I shall not judge useless. But if you desire to have them returned, I will, upon notice of it, transcribe them and return your originals. I would be content to have a copy of Mr. Newton's letter, which you mention, having a very good esteem of the person, though no acquaintance with him. Mr. Baker's, I doubt, is too long to transcribe. I had rather see it in print; and Mr. Newton's also. As to our correspondence with the French, I like it best when it is done in print; they being apt to be disingenuous, in claiming all for their own which they have from hence, without owning whence they have it. The calculation of Leibnitz's letter, (which you say is amiss,) I have not had time yet to examine.

I am your's, &c.

JOHN WALLIS.

CCCXLIV.

WALLIS TO COLLINS.[1]

I should have sent this sooner (for I wrote it presently after I came home) but forgot it.

Root 1st.	Cubic equation.	
$a-1$)	$a^3-15a^2+54a-\ 40$	$(a^2-14a+40$
$a-2$)	$a^3-15a^2+54a-\ 56$	$(a^2-13a+28$
$a-3$)	$a^3-15a^2+54a-\ 54$	$(a^2-12a+18$
$a-4$)	$a^3-15a^2+54a-\ 40$	$(a^2-11a+10$
$a-5$)	$a^3-15a^2+54a-\ 20$	$(a^2-10a+\ 4$
$a-6$)	$a^3-15a^2+54a\mp\ 0$	$(a^2-\ 9a\pm\ 0$
$a-7$)	$a^3-15a^2+54a+\ 14$	$(a^2-\ 8a-\ 2$
$a-8$)	$a^3-15a^2+54a+\ 16$	$(a^2-\ 7a-\ 2$
$a-9$)	$a^3-15a^2+54a\pm\ 0$	$(a^2-\ 6a\mp\ 0$
$a-10$)	$a^3-15a^2+54a-\ 40$	$(a^2-\ 5a+\ 4$
$a-11$)	$a^3-15a^2+54a-\ 110$	$(a^2-\ 4a+10$
$a-12$)	$a^3-15a^2+54a-\ 216$	$(a^2-\ 3a+18$
$a-13$)	$a^3-15a^2+54a-\ 364$	$(a^2-\ 2a+28$
$a-14$)	$a^3-15a^2+54a-\ 560$	$(a^2-\ 1a+40$
$a-15$)	$a^3-15a^2+54a-\ 810$	$(a^2\mp\ 0a+54$
$a-16$)	$a^3-15a^2+54a-1120$	$(a^2+\ 1a+70$
$a-17$)	$a^3-15a^2+54a-1496$	$(a^2+\ 2a+88$

[1] This has not been a separate letter, but has been folded up to be communicated in some other way. It has no date, but was found between the leaves of the preceding letter. It was impossible to print the numbers exactly as they stand in the manuscript, because an octavo page would not contain them; all the quantities in the second table being written as continuations of the several lines in the first.

Root 2.	Root 3.	that is,	Root 1.	Root 2.	Root 3.
$7 - 3$	$7 + 3$		1	4	10
$6\frac{1}{2} - \frac{1}{2}\sqrt{57}$	$6\frac{1}{2} + \frac{1}{2}\sqrt{57}$		2	2.7251 −	10.2749 +
$6 - \sqrt{18}$	$6 + \sqrt{18}$		3	1.7575 −	10.2425 +
$5\frac{1}{2} - 4\frac{1}{2}$	$5\frac{1}{2} + 4\frac{1}{2}$		4	1	10
$5 - \sqrt{21}$	$5 + \sqrt{21}$		5	0.4194 +	9.5826 −
0	9		6	0	9
$4 - \sqrt{18}$	$4 + \sqrt{18}$		7	−0.2425 +	8.2425 +
$3\frac{1}{2} - \frac{1}{2}\sqrt{57}$	$3\frac{1}{2} + \frac{1}{2}\sqrt{57}$		8	−0.2749 +	7.2749 +
0	6		9	0	6
$2\frac{1}{2} - 1\frac{1}{2}$	$2\frac{1}{2} + 1\frac{1}{2}$		10	1	4
$2 - \sqrt{-6}$	$2 + \sqrt{-6}$		11		
$1\frac{1}{2} - \frac{1}{2}\sqrt{-63}$	$1\frac{1}{2} + \frac{1}{2}\sqrt{-63}$		12		
$1 - \sqrt{-27}$	$1 + \sqrt{-27}$		13		
$\frac{1}{2} - \frac{1}{2}\sqrt{-159}$	$\frac{1}{2} + \frac{1}{2}\sqrt{-159}$		14	impossible.	impossible.
$- \sqrt{-54}$	$\sqrt{-54}$		15		
$-\frac{1}{2} - \frac{1}{2}\sqrt{-279}$	$-\frac{1}{2} + \frac{1}{2}\sqrt{-279}$		16		
$-1 - \sqrt{-87}$	$-1 + \sqrt{-87}$		16		

Differences [of the cubic equations.]				Differences [of the quotients.]		
+ 40				40		
	+ 16				−12	
+ 56		−18		28		+2
	− 2		+6		−10	
+ 54		−12		18		+2
	− 14		+6		− 8	
+ 40		− 6		10		+2
	− 20		+6		− 6	
+ 20		∓ 0		4		+2
	− 20		+6		− 4	
± 0		+ 6		0		+2
	− 14		+6		− 2	
− 14		+12		− 2		+2
	− 2		+6		− 0	
− 16		+18		− 2		+2
	+ 16		+6		+ 2	
∓ 0		+24		0		+2
	+ 40		+6		+ 4	
+ 40		+30		4		+2
	+ 70		+6		+ 6	
+ 110		+36		10		+2
	+106		+6		+ 8	
+ 216		+42		18		+2
	+148		+6		+10	
+ 364		+48		28		+2
	+196		+6		+12	
+ 560		+54		40		+2
	+250		+6		+14	
+ 810		+60		54		+2
	+310		+6		+16	
+1120		+66		70		+2
	+376				+18	
+1496				88		

It appears by this, that, supposing an adfected cubic equation, fitted to one series of roots, arithmetically proportional, as 1, 2, 3, &c., the other two ranks of roots answering thereunto will not be arithmetically proportional. But the rank of coefficients in the middle terms of the quadratic equations, containing these two latter ranks of roots, will so be. And also, of the absolute numbers in those quadratic equations, the second differences will be equal, like as they would have been, if in all of them there had been the same coefficient of the middle term, and one rank of roots, as 1, 2, 3, &c. Note also, that if in any such series of equations, as here in cubics, one rank of roots be arithmetically proportional, then will the series of aggregates of all the rest, which is the coefficient of the second term of the subordinate equation, (as here of the quadratic,) be arithmetically proportional also: but decreasing, if those first did increase, and contrariwise, increasing if they did decrease. And consequently, if the proposed series be of quadratic equations, whereof one rank of roots be arithmetically proportional, the other will be so too: for the series of aggregates is no other than the other series of roots.

CCCXLV.

WALLIS TO COLLINS.

Oxford, Feb. 22, 1676–7.

Sir,

I have received yours of Feb. 16, and the book with it, but have not had time yet to peruse the book, or consider the latter part of your letter, which concerns Leibnitz. I am glad to find by yours that you are in pretty good health, of which I have been very solicitous ever since our abrupt breaking off our last commerce, which I did a while forbear, (though some particulars of a former letter of yours remained unanswered,) expecting from you an extract of Mr. Newton's letters; and, after that, I have been much of the time absent from home.

As to what you say of Mr. Pitts desiring somewhat to be added to Horrox's book; he shall have from me the problem you mention, if he please, or what other things of mine you have. And I shall consider of some other things. I have been solicited to publish my theory of the tides in Latin; the thing I translated many years since for Mr. Oldenburg. That, if you think fit, may be added there; and, if you will, my tract of Gravitation translated, with some enlargement. Kepler's problem you speak of is solved (by the cycloid) by Dr. Wren, in what of his is subjoined to mine De Cycloide. You may, if need be, extract that and join [it] with Gregory's papers.

The problem of mine above mentioned was proposed to me by Dr. Wren, to be effected in order to find the distance of comets. Mr. Halley's problem, with his leave, you may doubtless (notwithstanding its being in the Transactions) print in another language.

As to Mr. Flamsteed; I am obliged to him for thinking my things worth translating, and giving himself the trouble about it. You may add of mine what else is proper to the subject. What the letter of mine is, which you mention, about cubic equations, I have forgotten. I shall be willing to see those translations before they be printed. If any things of mine in Latin you would have in English, or the contrary, I can give it you, almost with the same pains as transcribing it.

The French Elements of Algebra, of which Mr. Smith, you say, said I had made some remarks on it: it was, I suppose, a mistake of his. I have indeed read it cursorily, and perhaps I told him that most of it was not new, but transcribed from others, with very little of new addition; only put into another order, and much of it tedious enough. But I committed no remarks on it to writing.

I should have said all this, and more, by the last post, but deferred, hoping to have got more time by this than I find I have; and therefore can give you but this hasty scribble, that you may not be solicitous whether I have received yours or no. I may add more at more leisure. From

yours to serve you,

JOHN WALLIS.

CCCXLVI.

WALLIS TO COLLINS.

Oxford, April, 1677.

Sir,

I send you herewith a packet with these particulars in it.

1. A discourse concerning the Julian period, and those of which it consists.

2. A small tract concerning continual proportionals decreasing infinitely continued.

3. A tract concerning combinations and aliquot parts.

4. A little discourse concerning a series of equations fitted to a series of roots arithmetically proportional: and about impossible roots.

5. A solution of the problem you mention; to find

a line which shall be cut in a given proportion, by four others in a given position.

6. A large discourse concerning algebra.

I desire you to preserve them, as I hope you do those others you have of mine; for of the last of these I have no copy at all, of some others, (those especially that you find by the view to be newly written,) none so perfect. But of some, which have been long written, I have better copies.

The last of them proves longer than I intended, yet to what is mentioned in the last page I thought to have made further additions, but that I was loth to keep it too long from you. And what you shall yet think proper to be added concerning late improvements which I have omitted, if you mind me of it, may yet be added. You may mind me also of the names of ancient algebraists of our own before Vieta. Such I have seen, but have forgot their names.

De la Hire's Conics I have not thoroughly perused, but shall return to you very shortly: it is not a perfect treatise of Conics, but contains some general remarks concerning them, which he deduceth mostly from what he calls the harmonic section of a line.

What you think fit of these to put to the press, either by themselves or with the books you mention, you may freely do it; only I would be content to review it before it be printed. This at present from

your's to serve you,

JOHN WALLIS.

Is Mr. Merry a person yet living, or is he not?

CCCXLVII.

WALLIS TO COLLINS.

Oxford, Oct. 8, 1677.

Sir,

I received yours of Sept. 20, and the packet therein mentioned soon after. Your considerations on Des Cartes I like well, and they are but faintly answered by Tschirnhaus. I suspect, as I formerly intimated, that of such communications of things not published they make but ill use beyond sea; and particularly that Comiers' new notion of two mean proportionals, &c., was borrowed from those papers, where you mention, page 1, Mr. Newton's making use of two movable angles. And no doubt but many other particulars therein imparted will shortly be published as French inventions.

At your note K, I desire you to consider if there be not somewhat miswritten. For to change the affirmative roots into negatives, and negatives into affirmatives, we are to change the signs of those places where the number of dimensions in the coefficient (not in the root) is odd; which agrees with Des Cartes's rule. And at L, the rule you blame, if understood of all roots, as well real as imaginary, (and of like roots repeated so oft as they occur,) I think to be true: but if of possible roots only, and of like roots reckoned but once, it cannot hold. At N, I think you mistake Mr. Neale's invention; that which you mention, of straightening the quadratic parabola, upon supposition of squaring the hyperbola, was done by Huygens and myself; but that of Mr. Neale is straightening the semi-cubical parabola without supposing the squaring an hyperbola. If he have done other also, it is more than I know. The letter of mine, which soon after you mention, I am willing should be printed with the rest, notwithstanding my ingrafting part of it into that of Harriot, which then, as to that part, may be abridged. What you say of John Geysius I have sought for in Alsted's Encyclopædia, but cannot find it in that book, which we have in the public library, which, as I remember, is printed at Herborn, 1620. I suppose, therefore, you mean some later edition, which was, it seems, about 1631. I return you with this the sheets you sent, with a note of the errata, such as I hastily observed in once reading over. Some smaller I omit to mention, but you will find them corrected in the margin of the sheets; which sheets therefore I would have preserved, to amend others by; and the titlepage is on the other side of them. I am still of opinion that Mr. Newton should perfect his notions, and print them suddenly. These letters, if printed, will need a little review by himself; for there be some slips in hasty writing them. This in haste from,

yours, &c.

JOHN WALLIS.

END OF VOL. II.

ERRATA IN VOL. II.

Page 53, line 5. "Sint ZD, DE, EH. &c." This does not correspond to the diagram. The error exists in the MS.

Page 130, line 7, over the second and third columns of figures supply the sign of minutes; over the fourth, that of seconds.

Page 141, lines 16 and 20, supply over the last column of figures in each place the sign of minutes.

Page 368, line 2 from bottom, in note, *for* Linn's *read* Linus's.