(5) Flamsteed has written in the margin 'sent him hereon ye elongations of all & diameters which he has employed in his *Principia*'. See note (5) of Letter 315, p. 494, for the table of elongations given in *Principia*, III, 403, to which Flamsteed refers. At pp. 427–9 Newton summarized in Propositions 22 and 23 his work upon the inequalities of the Moon, and its extension to those of the satellites of ♃ and ♄.

(6) See note (1) above.

275   FLAMSTEED TO NEWTON
5 JANUARY 1684/5
From the original in the University Library, Cambridge.
In reply to Letter 274; for answer see Letter 276

<div align="right">The Observatory Jan 5   168$\frac{5}{4}$</div>

Worthy Sr

    I am heartily glad that my comunications are usefull to you & I intreate you whenever you thinke that any thinge in my power may serve you, you would freely command it. if you desire to have ye places of the comet calculated to ye dayes I mentioned tis a short businesse: I shall doe it from ye French observations which seeme sufficiently accurate & send them you. if you will give me leave to guesse at your designe I beleive you are endeavoring to define ye curve yt comet described in ye æther from your Theory of motion[1] & if my assistance in so usefull a designe might helpe I would lend it most willingly. As for ye motion of ♄ I have found it about 27′ slower in ye Antonicall[2] appearances since I came here, then Keplers numbers, & ♃ s about 14 or 15′ swifter as you will find by ye account of theire Conjunctions which I published in ye last yeares *transactions*,[3] the error in Jupiter is not allwayes the same, by reason ye place of his Aphelion is amisse in Kepler. nor is ye fault in ♄ always the same but lesse in ye Quadratures as it ought to be, yet the differences in both are regular & may be easily answered by a small alteration in ye Numbers as is found in ♄ by our New Tables[4] which Mr Halley made at my request & Instigation. I have corrected ♃ my selfe so yt hee has of late yeares answered my calculus in all places of his orbit but I have not beene strict enough to affirme that there is no such exorbitation as you suggest of ♄, but after ye next terme if not sooner I will inquire diligently. tho to confesse my thoughts freely to you I can scarce thinke there should be any such influence since the distance of ye planets from each other in those positions is neare four Radij of the *Orbis annuus* so that, in such yeilding matter as our æther, I can not conceave that any impression made by ye one planet upon it can disturbe ye motion of the other: but if you thinke that when they approach so neare each other theire attractive

powers exert themselves more vigorously, & either draw them nearer or thrust them further from each other then they would doe, were the planets at a greater distance, I can onely say that it seemes unlikely such small bodies as they are compared with ye Sun, the largest & most vigorous Magnet of our susteme, should have any influence upon each other at so great a distance. the largest magnets that have yet beene got out of our earth for ought I can understand have no influence either upon one another or a Needle at ye distance of 100 yards & You will easily conclude that ye space of forty Thousand diameters of our earth which is neare ye distance of ye 2 planets when in ♂ of ye ☉ beares a much greater proportion to theire diameters then 100 yards does to ye diameter of an ordinary Magnet.⁽⁵⁾ but these are onely suggestion & perhaps I mistake all this while the reason of your suspicion that ye one planet operates on ye other. I therefore must beg your pardon for this freedom & assure you that I am wholly unprejudiced by any præconceived opinion & ready to examine any hint that may be usefull or assist in ye restitution of theire Motions. I know Keplers distances of ♄ agree not with ye sesqualter proportion & that ♃s too ought to be amended & both must be altered before wee set upon ye enquiry whether ♃s motion had any influence on ♄s ye yeare before or after ye great conjunction, which I shall willingly trie as soone as our cold weather goes of & I can get a little leasure.

Such observations as you desire of ♃s Satellites you will find printed in Mr Oldenburgs *Transactions*⁽⁶⁾ Num. 96. pag 6095 from which I determined then the elongations of each satellite of ♃ in his semidiameters something lesse, then since I have made them on ye examination of theire Eclipses, but because it mey be some trouble to you to worke those observations I will endeavor to ease you as much as I can by giveing you ye elongations of each Satellite in minutes and seconds as they appear from ye Sun when ♃ is in his Meane distance from him with the same at our earth when hee is in his greatest & nearest distances, as also ye semidiameters of ♃ at our earth in those positions

		Satell.	1		2		3		4	
			′	″	′	″	′	″	′	″
Juxta Tabulas Carolinas distantiæ a Terra.										
♃ diam e propriis observationibus										
♃ᵃ Aphelij in ☍.☉ᵃ 420145 50′·36″	------ et Satellitum Elongationes		2·14		3·44¼		5·54½		10·11	
☌ ☉ 620238 34·16			1·30¾		2·32		4·00		6·54	
♃ Perihelij in ☍ ☉ 394932 53·50	inde deductæ extimæ e		2·22½		3·58½		6·17		10·50	
☌ ☉ 594985 35·44	propriis observationibus deductæ.		1·34½		2·38½		4·10		7 11½	

Elongationes autem a ♃ᵉ medio
e sole spectatæ satellitis -
ab iisdem derivatæ ------

1ᴵ 1·48   et Solis e Jove medio visi
2ᴵ 3·01   semidiameter. 3′·06″
*⁽⁷⁾ 3ᴵᴵ 4·16
* 4ᴵ 8·13½

These I have transcribed from a little Tract[8] I wrote about 11 yeares agone at Derby concerneing ye diameters of ye planets but never published: the observations from whence they were deduced are published in ye aforementioned place in ye *Transactions* but because ye minutes and seconds answering ye parts of my screw were not necessary to be added there they were omitted. I shall give you ym therefore here by which you may examine my determinations
A.D. 1672. Derbiæ

Martij 19. hor $7\frac{1}{6}$ limbus ♃ ˢ rem. a 4to satellite $1601 = 9' \cdot 34''$ ♃ ˢ semid
       27.   .   8   limbus ♃ ˢ rem a 4to rep.[9] $1591 = 9 \cdot 30$ perh[10] $64 = 0' \cdot 22''$,
       28.   .   8   Eadem distantia ---------- $1598 = 9 \cdot 33$

on ye 27th of March at 8h. ♃ was in ♍ 9° 22', his distance from our Earth 460900 such as ye earths meane distance from ye ☉ is 100000. the Satellit then wanted about $\frac{2}{5}$ of a semidiameter of its utmost elongation which was therefore now $9' \cdot 17''$ whence ye elongations in ye other distances before recited were derived, whereby tis evident, thus its distance from ♃ is not more then $2\frac{2}{3}$ semidiameters of ye Sun.

    The observations whereby ye elongations of ye 3d, 2d, & 1st were limited I made April 4. and 11th. 1673. I rely cheifly on ye latter, tho the former agree very well with them. The Measures were these

1673 April 27[11] hor $7\frac{1}{2}$ ♃ ˢ limbus rem. a 3° satellite $947 = 5' \cdot 40''$ ♃ ˢ semi-
                                                          diameter
                         a 2°   ---- $622 = 3 \cdot 43$ auferenda
                                         $66 = 0' \cdot 24''$
                         a 1°   ---- $405 = 2 \cdot 25$

the third satellit by my tables then wanted $\frac{2}{3}$ of a semidiameter of ♃ of its utmost elongation. the second neare $\frac{1}{2}$. & the first $\frac{1}{8}$. which, added to ye observed distances, correct by ye subtraction of ♃ ˢ semidiameter because they were measured from his remoter limbe, give ye present utmost elongations of
ye $\begin{cases} 3: & 5' \cdot 31'' \\ 2. & 3 \cdot 29\frac{1}{2} \\ 1 & 2 \cdot 05. \end{cases}$ ♃ ˢ distance from our earth was now 449863 such partes
as the earths meane distance from ye Sun is 100000. hence ye elongations in ye Tablet were derived, and by these you may examine them.

    Sr I have not had the happinesse of Mr Pagetts company this Christmas tho hee promised it me; the hard weather perhaps prevented him as it did me from goeing to London so I have not yet had the happinesse of ye perusal of your papers. I am very well pleased however to heare that you intend to oblige us

with ye publication of them next terme when I hope to have ye use of them [not]⁽¹²⁾ being obliged to any but your selfe for it. Sr I am ever

<div align="center">Your affectionate Freind & Servant</div>

<div align="right">JOHN FLAMSTEED</div>

I wish you an happy yeare pray give the same to Mr Crompton from me when you see him & let him know I am mindfull of his concerne & my promise.

To
Mr Isaack Newton at
  his Chamber in Trinity
    Colledge in Cambridge
      these
        present

*⁽⁷⁾ 3 d. 4. 46 rectius 4.47
  4.   8. $13\frac{1}{2}$ 8.$12\frac{2}{3}$

NOTES

(1) At a meeting of the Royal Society on 10 December 1684 'Mr HALLEY gave an account, that he had lately seen Mr NEWTON at Cambridge, who had shewed him a curious treatise, *De Motu*; which, upon Mr HALLEY's desire, was, he said, promised to be sent to the Society to be entered upon their register. Mr HALLEY was desired to put Mr NEWTON in mind of his promise for the securing his invention to himself till such time as he could be at leisure to publish it. Mr PAGET was desired to join with Mr HALLEY' (Birch, IV, 347).

In August 1684, when Halley visited Newton at Cambridge, he had learnt that Newton had proved that for a body moving in an ellipse under an attractive force directed to a focus the law of force was that of the inverse square. In November Paget brought to Halley from Newton a paper containing the demonstration. Thereupon Halley paid Newton a second visit at Cambridge, and afterwards reported the facts to the Royal Society, as recorded above. See Letters 278 and 285, pp. 415 and 431.

There are various conjectures as to the paper brought by Paget to Halley, but probably it was the Tract *De Motu*, printed in Rigaud's *Historical Essay on the first publication of Sir Isaac Newton's Principia* (Oxford, 1838) and again in Rouse Ball's *Essay on Newton's Principia* (London, 1893). Certainly the paper was a forerunner of the *Principia*; and we have Newton's own testimony that he had embarked on the latter in November or December 1684; for he writes 'The Book of Principles was writ in about 17 or 18 months, whereof about two months were taken up with journeys, & the MS was sent to ye R.S. in spring 1686 [and presented on 28 April 1686]; & the shortness of the time in which I wrote it, makes me not ashamed of having committing some faults' (in a rough draft among the Macclesfield papers, printed by Rigaud, *op. cit.* p. 92 and Rouse Ball, *op. cit.* p. 59). For full discussions of the points in doubt, see Rigaud, pp. 15–16, 77; Edleston, p. lv; Rouse Ball, pp. 30–2 and More, *Newton*, pp. 301, 302.

Flamsteed guessed right; the theory was applied to a comet's path in the *Principia*, Book III.

(2) The meaning of the word is doubtful. It may be a slip for 'anatonicall', from the Latin *anatonus = ἀνάτονος*, 'stretching upwards'.

(3) *Phil. Trans.* **13** (1683), 244–58.

(4) Writing to Abraham Sharp on 11 February 1709/10, Flamsteed remarked: 'I must add concerning Saturn that, whereas Sir Isaac Newton suggested to me that all the planets increased in their bulk continually by an accession of matter from the æther about them, this now seems not probable. Mr Halley had told him that the motions of Saturn were slower this last 100 years much than formerly. I have tables of Saturn by me of his making, presented to Sir Jonas Moor, wherein he makes Saturn's motion in 100 years 26 minutes slower than 'tis in the *Caroline tables*. Now if the planets grow slower in their motions they must consequently remove further from the sun, and there is no reason for their removing further from the sun except they increase in bulk and weight: but I do not find that Saturn moves any slower now than he did almost 2000 years agone, which makes me think our earth and the other planets have gained little or nothing from the tayls of comets, and that the fumes from them have filled our orbit from the sun as far as the orb of Venus with that matter which causes the light we see in the moonless nights about the time of the vernal equinox, of which Mr Ffatio has given an account' (from an original draft in Greenwich Observatory library: for the whole letter see Baily, *Flamsteed*, p. 274).

Sir Jonas Moore died in 1679 so these might be the tables referred to above.

(5) Cf. Flamsteed's remarks on magnetic attraction in Letter 250.

(6) *Phil. Trans.* 8 (1673), 6094–5.

(7) Newton's mark indicating a footnote that he placed immediately below the signature at the end of the letter. He has also lightly dotted in a 4 over the figure 1 of the third entry 4′. 16″; thus '.4.'.

(8) See a MS. tract which he wrote on the apparent diameters of the planets (Greenwich Observatory MSS. vol. 41, 220). 'In this year [1673] also, as I remember, I wrote a small tract in English concerning the true diameters of all the planets, and their visible [*sic*], when at their nearest distance from our Earth, or their greatest remove from it: which I sent to Mr. Newton in the year 1685, who has made use of it in the 4th book of his *Principia*' (memorandum by Flamsteed), see Baily, *Flamsteed*, p. 33.

(9) A contraction for *repetito* (repeated)?

(10) Meaning Jupiter's semidiameter at *perihelion* (below it is taken as 24″). The '64' and (below) '66' presumably refer to readings on his micrometer screw.

(11) An error for April 11.

(12) A short word has been overlaid by the wax that sealed the letter.

276 NEWTON TO FLAMSTEED[1]

[12 JANUARY 1684/5]

From the original in the Bodleian Library.
In reply to Letter 275; for answer see Letter 277

[12 January 1684/5]

Sr

Whilst I was concerned that you should be so long without ye sight of those papers I received a letter from Mr Paget by wch I understood he has been laid up sick of an ague. I am writing to him to transmit ye papers to you as soon as

he has a convenient opportunity. In my last[2] I made an allowance for ye distance of Jupiter & Saturn one from another diminishing their virtue in a duplicate proportion of ye distance. But yet I spake there but at randome not knowing their virtues till I had your numbers for Jupiter, by wch I understand his vertue is less then I supposed. But I am still at a loss for Saturn. I have not at all minded Astronomy of some years till on this occasion wch makes me more to seek. I cannot meet wth Hygen's book[3] of Saturn. Mercator[4] & another or two wch I have consulted leave me as wise as I was. I find Saturns ring is to his body in breadth as 9 to 4 & Hygens makes ye ring in Saturns nearest distance 68″ long at most, that is in his meane distance from ye sun about 1′: But it is ye dimension of ye orbit of ye Satelles about him that I want. Now I am upon this subject I would gladly know ye bottom of it before I publish my papers. I believe you can tell me what Hygenius measures are, or if there have been any other since assigned more exactly. For by Hygenius large measures of ♃ I suspect he may have assigned ye apparent diameter of ye ring of ♄ too large. Your information[5] about ye error of Keplers tables for ♃ & ♄ has eased me of several scruples. I was apt to suspect there might be some cause or other unknown to me, wch might disturb ye sesquialtera proportion. For ye influences of ye Planets one upon another seemed not great enough tho I imagined ♃'s influence greater then Your numbers determin it. It would ad to my satisfaction if you would be pleased to let me know the long diameters of ye orbits of ♃ & ♄ assigned by your self & Mr Halley in your new tables, that I may see how the sesquiplicate proportion fills ye heavens together wth another small proportion wch must be allowed for. I thank you for your kind offer of calculating the places of ye Comet from ye French observations to ye days you mentioned. I do intend to determin ye lines described by ye Comets of 1664 & 1680 according to ye principles of motion observed by ye Planets, & should be glad of your help as to those places of ye latter, if I shal not give you too much trouble.[6] Sr I am

Your most obliged Friend to serve you

I. NEWTON.

For Mr John Flamsteed at ye
   Observatory in Greenwich
      neare
      London

NOTES

(1) At the top of the page Flamsteed has written: 'Mr Paget was not Master till Apr. 1682 therefore this wrote about 85 or Jan 85/6.' The name 'Mr Paget' occurring on the third line is underlined. The postmark is JA/14, from which the date at the head of the letter has been conjectured.

A SMALL "TWO-BODY" CORRECTION
TO KEPLER'S 3/2 POWER RULE

If Jupiter and the Sun interact, then

$$P_J^2 \propto r_{JH}^3 \; \frac{1}{1 + \dfrac{[a^3/P^2]_J}{[a^3/P^2]_H}}$$

...It would add to my satisfaction if you would be pleased to let me know the long diameters of the orbits of Jupiter and Saturn by yourself and Mr. Halley in your new tables, that I may see how the sesquiplicate proportion fills the heavens together with another small proportion which must be allowed for....

Newton to Flamsteed
12 January 1684/5

(2) See Letter 274.

(3) *Systema Saturnium* (1659).

(4) *Institutionum Astronomicarum libri duo* (London, 1676). See Letter 8, note (2), vol. I, p. 16.

(5) See Letter 275.

(6) See note (1), Letter 275.

277 FLAMSTEED TO NEWTON
27 JANUARY 1684/5
From the original in the University Library, Cambridge.
In reply to Letter 276

The Observatory Jan: 27. 1684/5

Sr

I receaved your papers from Mr Paget before your last[1] without date came to hand. but a benifice haveing beene bestowed upon me in the meane time I have not had leasure to peruse it yet. being provideing for a short Jorney to see it. however I have not failed to examine Hugens but can not find any thing for your satisfaction in him. but remembring yt Mr Halley had corrected ye Motion of $\hbar$ˢ Satellit in our *Transactions*[2] I turnd to them & found that hee states the utmost Elongation of ye Hugenian satellit from $\hbar$ˢ center. Nine diameters of ye body or 4 of the ring whose diameters I shall give you as deduced from my Derby observations. by which:

	of $\hbar$	of ye Ring
in ye remotest distance.	19″·10‴	43″·06‴
The diameters in ye meane ---------	22 ·15	50 ·02
in ye Nearest --------	26 ·36	59 ·51. which

is lesse yn Hugens make it very sensibly. Hence:

The greatest elongation of ye Satellit from $\hbar$ in his Meane distance is 3′·20″ but the semidiameter of ye ⊙ viewd from $\hbar$ is at the same distance 1′·41″. So yt ye radius of the Orbe of this satellit is but 2 semidiameters of the Sunn. I am calld away excuse this brevity of Sr

Your affectionate Freind & Servant

JOHN FLAMSTEED

I have considered what would be ye change of ye visible place of ♄ admitting him thrust out of his orbite & find it would scarce be sensible tho observed in ye Quadratures but it would be something sensible in ♃ if hee be so thrust out of his path:

J. F.

To
Mr Isaac Newton
  at his Chamber in Trinity College
    Cambridge these
      present.

(3)

NOTES

(1) Letter 276.

(2) *Phil. Trans.* **13** (1683), 82.

(3) The triangular mark indicates the London Penny Post which was introduced by William Dockwra (see *D.N.B.*) in 1683 and later taken over by the Government. The above form of the postmark was used during the period 1684–1711. S (Southwark), TV (Tuesday).

· ·

278 NEWTON TO ASTON[1]
23 FEBRUARY 1684/5
From a copy in the Letter book of the Royal Society

[Sir]

The designe of a Philosophick Meeting here Mr Paget when last with us pusht forward, and I concurred with him, and engaged Dr More[2] to be of it, and others were spoke too partly by me, partly by Mr Charles Montague[3] but that which chiefly dasht the buisiness was the want of persons willing to try experiments, he whom we chiefly relyed on, refusing to concern himself in that kind him self, And more what to add further about this buisiness I know not, but only this that I should be very ready to concurre with any persons for promoting such a designe so far as I can doe it without engaging the loss of my own time in those thinges.

I thank you for entring in your Register my Notions about Motion.[4] I designed them for you before now but the examining severall thinges has taken a greater part of my time then I expected, and a great deale of it to no purpose. And now I am to goe into Lincolnshire[5] for a Month or six weeks. Afterwards I intend to finish it[6] as soon as I can conveniently &c

Is. NEWTON.

Cambridge Febr. 23th. 1684/5