

272 NEWTON TO PAGET<sup>(1)</sup>

DECEMBER 1684

The letter has not been found. For answer see Letter 273

## NOTE

(1) We learn of this letter from one (Letter 273) which follows next. In it Newton sends two messages to Flamsteed: first, an offer of the perusal of certain papers; and from Flamsteed's reply (see Letter 273, note (8) and Letter 275, note (1)) we gather that they are the papers on motion, that led to the writing of the *Principia*; secondly, a request for the determination of the distances and periods of the satellites of Jupiter.

## 273 FLAMSTEED TO NEWTON

27 DECEMBER 1684

From the original in the University Library, Cambridge.

In reply to Letter 272; for answer see Letter 274

Honored Sr

*The Observatory December: 27. 1684.*

Mr Halley delivered me a note wherein you desire me to give you ye places of 2 fixed Stars<sup>(1)</sup> in ye foot of Perseus, & Mr Paget soone after a letter from you wherein you kindly offerede me ye perusall of your papers for which I humbly thanke you. Mr Pagit promised to be here on Tuesday last but the severity of that dayes weather I suppose prevented him else I had sent you by him the places of the Stars, which I now transmit<sup>(2)</sup> with ye distances whence I derive them. Octob: 23. 1680 at 9<sup>h</sup>.p.m. I measured the distance of ye bright star of  $\Upsilon$  from ye brighter of ye 2 marked  $\zeta$  in Bayer  $25^{\circ} \cdot 03' \cdot 40''$  from ye lesser,  $O$ ,  $23^{\circ} \cdot 09' \cdot 15''$ . and at 9<sup>h</sup>. 50' betwixt Aldebaran &  $\zeta$ ,  $18^{\circ} \cdot 02' \cdot 35''$ . but from it to  $O$ ,  $19^{\circ} \cdot 36' \cdot 35''$  allowing about 15" for ye contraction by refraction I state their distances & find their places to ye begining of ye yeare 1681<sup>(3)</sup> as followes:

Lucida	$\Upsilon$ a	$\zeta$ , $25^{\circ} \cdot 03' \cdot 50''$	} Long * 8 28.40.16. Lat Sept. $11^{\circ} 17' \frac{1}{2}$	} the Tyconick differ of longitude betwixt these ** is $4 \frac{1}{2}$ too big his latitudes well.
Palilicium	a	$\zeta$ , $18 \cdot 02 \cdot 50$		
Lucida	$\Upsilon$ ab	$O$ , $23 \cdot 09 \cdot 30$	} Long * 8 26.41.48. Lat Sept. $12 \cdot 08 \frac{1}{2}$	
Palilicium	ab	$O$ , $19 \cdot 36 \cdot 50$		

Long diff:  $1 \cdot 58 \cdot 28$  ipsarum inde distantia  $2^{\circ} \cdot 06' \cdot 46''$  I had proceeded to calculate ye places of ye comet from your observations but that I find you make ye observed distance betwixt ye 2 stars but  $1^{\circ} \cdot 46' \cdot 06''$ . which<sup>(4)</sup> is one third of a degree lesse then my calculus gave it, which tho I was very well satisfied in, yet for greater assurance the next night after I had made it I measur'd yt distance with my sextant but had a very bad assistant: yet by severall trialls concluded

it could not be more yn  $2^{\circ} \cdot 07' \cdot 10''$ . nor above  $\frac{1}{2}$  a minute lesse, which agrees altogeather with ye calculation. I would entreat you therefore examine your notes againe & then I shall either give you ye places of ye comet calculated from your observations or if you desire it rather, from ye french who give its right Ascensions & Declinations on ye 27 & 28 of Feb. & 8th of March, our stile. from which they are easily deducible. Monsieur Cassini saw & observed it neare ye same stars. he gives a map of them in his booke<sup>(6)</sup> & the comets path amongst them, whereby I should have concluded that you mistooke one of your stars for another that lies more to ye north & is lesse yn either  $\zeta$  or  $O$ , but that ye distance is too small for your measure as mine is too big.

In your letter to Mr Paget you further require my determinations of ye distances of 2 $\downarrow$ s satellits from him & their periods. I gave their utmost elongations in Num 96. of Mr Oldenburg's *transactions*.<sup>(6)</sup> but haveing since that time made many observations of their eclipses I find they require them something larger, & that they are as exactly in sequialte proportion to their periods as it is possible for our senses to determine. I give you here the times<sup>(7)</sup> of their revolutions to 2 $\downarrow$ s in his meane distance for you well know yt when hee is on his Aphelium they are made swifter, when Perihelium slower; & with them their utmost elongations from 2 $\downarrow$ s center in his semidiameters & ye millesme parts of them

	Revolutio 1			Revoluciones 5			Elong. a 2 $\downarrow$ sd 2 $\downarrow$ . mill	partibus qualium extima est 100.000						
	di.	h	"	dies.	h	"		2 $\downarrow$ semid.						
Revolutionum	1.	1	18	28	36	8	20	23	00	5	578	1 elong.	22	377
tempora ad Jovem.	2.	3	13	17	54	17	18	29	30	8	876	2 . . . .	35	642
	3.	7	03	59	36	35	19	58	00	14	159	3 . . . .	56	855
	4.	16	18	05	03	83	18	25	15	24	903	4 . . . .	100	

In ye *transactions* of November<sup>(8)</sup> last I published ye Eclipses of the Satellits for all ye next yeare any where visible. I find I can answer all the Eclipses of ye 1st that have beene carefully observed within lesse yn 2 minutes of time, the 4th has not faild me much more nor the 3d above thrice as much, but the 2d will not be brought to so neare a rule but that it will exorbitate much more. yet is not the error halfe so greate as wee frequently find in ye best lunar tables.<sup>(9)</sup> I use their motions altogeather æquable only allowing Roemers æquation of light,<sup>(10)</sup> without which allowance the error of my tables would be above 10' minutes of time. now it seemes strange ye moones motion should be so perplexed with inæqualitys & these, for ought I can perceave yet, except in ye 2d, wholly free from them & I have some reason to thinke ye errors I meet with in my Numbers for ye 2d Satellit may partly proceed from my haveing allowed its orbit to lie in the same plaine with ye orbits of ye other 3, whereas I have some observations yt

will scarce allow it to lie otherwayes yn neare ym, but I have not had leasure to examine this matter fully nor could I ever get the observations I proposed for this triall, the heavens either proveing cloudy, or businesse interfering when they were to be made. I have corrected all their motions, Cassinis old numbers being now 8 degrees false, in the first,<sup>(11)</sup> which is nearely an hours motion. If you have occasion for them I shall get a breviat of them transcribed for you, & be glad of such an occasion to serve you & shew how much I am obliged by your kind concession of ye perusal of your papers, tho I beleive I shall not get a sight of them till our common freind Mr Hooke & the rest of the towne have bene first satisfied.<sup>(12)</sup>

I have no opinion of ye supernumerary satellits of  $\text{H}_2$ . I feare they were discovered as the former when hee was neare his station when his motion in ye Ephemerides being contrary to yt in ye heavens makes our over curious observers to make satellits of small fixed stars & when they cannot find them againe, the planet haveing left theire neighbourhood, they pretend that these dissappeare for a part of theire revolutions: this is the truth of the case in some of them for I can not find the 2 new ones with a 24 foot glasse hitherto. but I designe to trie againe ere long if perhaps I may have better successe.

I forgot to adde one particular concerneing ye orbits of ye satellits: that I make its plane inclined to ye plane of  $\text{J}_1$ s orbite  $2^\circ 40'$  and the North inter-section in  $3^{\text{rd}}$  of  $\infty$  at present; tis retrograde but the motion very slow.<sup>(13)</sup> I have not yet defined the quantity per Annum. it will be requisite to have a many more yeares observations before yt be done. if you desire to be satisfied in any further particulars, let me have your desires proposed in a line or two, you can not propose more readily then you shall be willingly answered by Sr

Your very affectionate freind & humble Servant

JOHN FLAMSTEED

P.S.

Pray when you see Mr Crompton give him my humble service & let him know I cannot send him his Watch till after the holidayes that all faults will be amended it & hee will have no cause to complaine of it hereafter. J.F.

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

## NOTES

(1) The stars marked *A* and *B* in figure of No. 253, p. 357. The proper placing of these stars is a prominent item in the ensuing correspondence, see Letters 281 and 282.

(2) The figures are given in the next few lines: later they are revised (see Letter 283, p. 429).

(3) The date 1682 was first written; then Flamsteed altered the '2' to '1', but not very clearly. Put into Newton's notation (p. 425) the particulars are:

Star *A* Longitude  $\text{8 } 26^{\circ} 41' 48''$  Latitude  $12^{\circ} 8' 20''$  N,  
 Star *B* „  $\text{8 } 28^{\circ} 40' 16''$  „  $11^{\circ} 17' 12''$  N.

'Difference of longitude  $1^{\circ} 58' 28''$ : therefore their distance  $2^{\circ}. 06'. 46''$ .'

(4) The distance  $1^{\circ} 46' 06''$  is given by Newton in No. 253, a copy of which was presumably sent to Flamsteed.

(5) This is probably a reference to Cassini's *Observations sur la Comète qui a paru au mois de Décembre 1680 et en Janvier 1681* (Paris, 1681).

(6) *Phil. Trans.* 8 (1673), 6094.

(7) The entries in the first and second columns of the subjoined table give the times of one and of five revolutions.

(8) *Phil. Trans.* 13 (1683), 404. Flamsteed gave the corresponding figures for the years 1685, 6 and 7 in subsequent communications to the *Philosophical Transactions*.

(9) It is unfortunate that we have no particulars of what Newton wrote in the letter Flamsteed received through Paget, for it appears that Newton is answering specific queries throughout the present letter. The lunar theory became a topic of great importance in their subsequent correspondence, and it would be interesting to know if its first introduction here arose spontaneously, or in answer to a remark of Newton's.

(10) See note (13), p. 299.

(11) That is, in the first satellite. To make the meaning clear, the punctuation of this sentence has been corrected by the insertion of commas and the removal of a stop after 'false'.

(12) Flamsteed seems to be alluding to the papers on motion which Paget brought to Halley. See Letter 275, note (1).

(13) See Letter 293, and note (6), p. 450.

## 274 NEWTON TO FLAMSTEED

30 DECEMBER 1684<sup>(1)</sup>

From the original in the Bodleian Library, Oxford.

In reply to Letter 273; for answer see Letter 275

Sr

I thank you heartily for your kind information about those things I desired. In my observations about ye Comet I was only carefull of ye proportions of ye distances from small stars to those between ye starrs. Afterwards I believe I

made some mistake in ye reduction to minutes & seconds, for ye observations in many cross distances fitted one another well. Your information about ye Satellits of Jupiter gives me very much satisfaction. The orbit of Saturn is defined by Kepler too little for ye sesquialterate proportion.<sup>(2)</sup> This Planet so off as he is in conjunction with Jupiter ought (by reason of Jupiters action upon him) to run beyond his orbit about one or two of ye suns semidiameters or a little more & almost all the rest of his motion to run as much or more within it. Perhaps that might be ye ground of Keplers defining it too little. But I would gladly know if you ever observed Saturn to err considerably from Keplers tables<sup>(3)</sup> about ye time of his conjunction with Jupiter. The greatest error I conceive should be either ye year before conjunction when Saturn<sup>(4)</sup> is 3 or 4 signes from ye Sun *in consequentia* or the yeare after when Saturn is as far from ye sun *in antecedentia*. You seem to insinuate as if Saturn had not yet any more Satellits that [*sic*] one discovered by Hugenius. I should be glad to know if it be so. And one more favour I would beg, yt if you have any observation<sup>(5)</sup> of the greatest elongation of any of ye Satellits of Jupiter from his center (chiefly that of ye outmost satellite) in degrees & minutes you would communicate it to me wth ye time of ye observation. For I would glad know ye proportion of ye orbits of ye Satellits to yt of Jupiter as exactly as I can. And if you have any such observation of ye greatest elongation of ye satellite of  $\text{h}_2$  I would also beg ye favour of that. Sr I am

Your much obliged Friend

to serve you

Is. NEWTON.

Trin. Coll. Dec 30  
1680.<sup>(6)</sup>

A good new yeare to you.

For Mr John Flamsteed at  
the Observatory in  
Greenwich neare  
London

NOTES

(1) The correct year of the date is determined by a comparison with the dates and contents of Letters 273 and 275. The '1680' clearly written at the foot of the letter is a slip. Above the 0 someone has put a 6, and following it Flamsteed has written 'should be 1686 or 1685'.

(2) Kepler's third planetary law.

(3) The Rudolphine Tables, see note (11) of Letter 57, vol. 1, p. 149.

(4) Except in this sentence every reference to Saturn throughout the letter has been lightly underlined, as also the opening sentence.