REMARKS

ON THE

OBSERVATIONS

MADE IN THE LATE

VOYAGE

TOWARDS

THE NORTH POLE,

For determining the Acceleration of the PENDULUM, in Latitude 79° 50′.

By SAMUEL HORSLEY, LL. D. Sec. R. S.

IN A LETTER TO

The Hon. CONSTANTINE JOHN PHIPPS.

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TO THE HONOURABLE

CAPTAIN PHIPPS.

DEAR SIR,

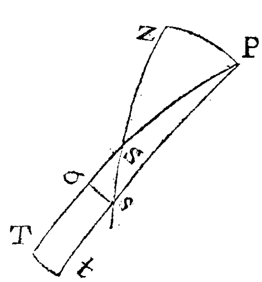
I Am favoured with your obliging preſent of your Voyage towards the North Pole, for which I return you my beſt thanks.

Mathematicians are no leſs indebted to you than mariners, for the attention which you have given to every object of ſcientific enquiry, though but remotely connected with the nautical art, which that ſingular voyage preſented. I have peruſed with particular attention the account of the obſervations of the going of the pendulum in latitude 79° 50′. and ſhall give you my remarks without apology, which it would be the higheſt injuſtice to you not to ſuppoſe unneceſſary, after the pains you have beſtowed upon the obſervation, and the minuteneſs and fidelity with which you have detailed all the circumſtances of it, as well as the ſteps of the ſubſequent calculations.

I am inclined to believe that the gain of the pendulum muſt have been very nearly what you reckon it. But the evidence of this reſts entirely upon the compariſon with the WATCH, and the ſix altitudes of the ſun taken with the aſtronomical quadrant for determining the loſs of the watch. For the exact agreement which you think you find between the gain of the pendulum as reſulting from the compariſon with the watch, and as deduced from the obſervation of the ſun's return to the vertical wire of the equatorial teleſcope, is *imaginary*. The appearance of agreement ariſes entirely from an error in the computation of the retardation of the ſun's return; and when this error is ſet right, the watch and the obſervation will be found to differ conſiderably.

The

The interval between the time when the ſun's weſtern limb touched the vertical wire on the 16th day of July, 1773, and the time of the return of the ſame limb to the vertical wire on the day following, which your computer hath reckoned $24^{h}\ 0'\ 49'',5$, could be no more than $24^{h}\ 0'\ 14''$: for, a ſmall change in the ſun's declination is to the correſponding change in the hour-angle (not, as your computer ſtates it, as the coſine of the latitude of the place of obſervation, but) as the coſine of *the ſun's declination* to the tangent of the angle contained between the circle of declination and the vertical circle: for, let Z be the obſerver's zenith, P the pole of the world, Z S the vertical circle drawn to the ſun, S the place of the ſun upon the vertical circle at the time of the firſt obſervation, *s* its place



upon the ſame vertical at the time of the ſecond obſervation. Draw the great circles Z P, P S, P *s*; round the pole P, deſcribe a ſmall circle through *s*, *s* σ, and continue the arcs P S, P *s*, till they meet the circumference of the equinoctial circle in T, *t*: now S σ is the change of declination in the interval of the two obſervations; S P *s* is the correſponding change of the hour-angle Z P S, and T *t* is the arc of the equinoctial, which meaſures the angle S P *s*. I ſay that S σ : T *t* = ſin. P S : tang. P S Z.

For

For the evanefcent triangle $S\,s\,\sigma$, is to be confidered as a right-lined triangle right-angled at σ. Therefore

$$S\,\sigma : \sigma\,s = \text{radius} : \text{tang. } s\,S\,\sigma, \text{ or } P\,S\,Z.$$

And, by the known properties of the fphere,

$$\sigma\,s : T\,t = \text{fin. } P\,\sigma : \text{radius.}$$

Ex æquo perturbatè $S\,\sigma : T\,t = \text{fin. } P\,\sigma : \text{tang. } P\,S\,Z$. That is, becaufe $P\,\sigma$ and $P\,S$ are fo nearly equal, that the fine of the one may without fenfible error be taken for that of the other, $S\,\sigma : T\,t = \text{fin. } P\,S : \text{tang. } P\,S\,Z$. Q. E. D.

Your computer affirms, that

$$S\,\sigma : T\,t = \text{fin. } Z\,P : \text{tang. } P\,S\,Z;$$

and to fupport this affertion he refers to Cotes *Æftimatio errorum, Theor.* 35.

Mr. Cotes's moft elegant treatife, *De Æftimatione errorum in mixtâ Mathefi*, is publifhed, together with the *Harmonia Menfurarum*, and fome other leffer tracts, in a quarto volume, printed at Cambridge in the year 1722. It contains in all, four lemmata, three corollaries, twenty-eight theorems, and two fcholia. I fuppofe, therefore, that theor. 35 is an error of the prefs. The theorem that ought to have been referred to, is the *thirteenth*: for, when the fun returns for the fecond time to the vertical circle Z S, and comes upon it at s, when before it had been at S, the triangle Z P S is changed into Z P s: that is to fay, the fide Z S is changed into Z s, the angle Z P S

Z P S is changed into Z P *s*, and the ſide P S into P *s*, and the angle P S Z into P *s* Z; and the only two parts of the triangle which remain unchanged, are the ſide Z P, which is the complement of the latitude, and the azimuth-angle P Z S: ſo that the thing to be inveſtigated is this; when one ſide of a ſpherical triangle (Z P) and one of the two angles adjacent to that ſide (Z) remain of a certain conſtant quantity, what change muſt the other angle (P) adjacent to the unchanging ſide (Z P) undergo, in order that the ſide (P S), oppoſite to the unchanging angle (Z), may be changed in its magnitude by a given ſmall quantity $S\sigma$? This queſtion is anſwered by Mr. COTES's thirteenth theorem, and by no other propoſition in his whole tract. And in that thirteenth theorem, Mr. COTES affirms, as I do, that the arc which meaſures the variation of the angle adjacent to the unchanging ſide, is to the variation of the ſide oppoſite to the unchanging angle, as the tangent of the angle oppoſite to the unchanging ſide to the ſine of the ſide oppoſite to the unchanging angle. And his demonſtration is derived from the very ſame obvious principles.

Inſtead of the proportion therefore which your computer ſtates, make the following: As the ſine of P S, *i. e.* as the coſine of ſun's declination, to the tangent of S, ſo is the change in declination to the change in the hour-angle $2' 5'', 1$;

Which

	h	′	″
Which turned into time gives		0	8,3
The change of the equation was		0	5,4
Hence true interval of the two tranſits — —	24	0	13,7
Interval by pendulum	24	2	4,5
Therefore the gain of the pendulum ſhould be —	0	1	50,8
Gain by cold			2,72
Gain by increaſe of latitude		1	48,08

	″
Gain of pendulum upon watch	79,3(a)
Loſs of watch by mean of ſix ſolar altitudes — —	5,5
Gain of pendulum upon mean time	73,8
Gain by cold — —	2,72
Gain by increaſe of latitude —	71,08

	′	″
Gain by increaſe of latitude according to obſervation	1	48,08
Ditto by watch	1	11,08
Difference	0	37,00

Thus the obſervation gives the gain of the pendulum 37″ more than the watch. But as the watch went ſo well during the whole voyage, as its loſs in theſe twenty-four hours was aſcertained by ſix altitudes of the ſun, and as the gain now given by the watch agrees ſo nearly with the reſult of the ſubſequent compariſons at SMEERENBERG POINT, I have no doubt but that the error lies entirely upon the obſervation of the ſecond tranſit. I ſuppoſe the teleſcope, from ſome unperceived cauſe, had ſhifted its azimuth; which is the more probable, as it does not appear that any means were uſed to verify the poſition of the inſtrument. Perhaps

(*a*) The interval between the two tranſits in mean time was $24^h\ 0'\ 13'',7$. And the watch having loſt in that time $5'',5$, the interval in time of the watch was $24^h\ 0'\ 8'',2$: which is more than $23^h\ 26'\ 42'',5$ (the time in which the pendulum had gained $77'',5$ upon the watch, vid. Tab. *A*.) by $33'\ 25'',7$. But as $23^h\ 26'\ 42'',5$ to $33''\ 25'',7$ (*i. e.* as 211 to 5 very nearly) ſo is $77'',5$ to $1'',8$. Therefore $77'',5 + 1'',8$, or $79,''3$, will be the gain of pendulum upon watch in the whole interval between the two tranſits.

the

the ſituation upon the ſmall rocky iſland afforded none *(b)*.

You finiſh your article relating to the pendulum with ſaying, "that theſe obſervations give a figure of the earth nearer to Sir Iſaac Newton's computation, than any others which have hitherto been made;" and then you ſtate the ſeveral figures given, as you imagine, by former obſervations, and by your own. Now it is very true, that *if* the meridians be ellipſes, or, *if* the figure of the earth be that of a ſpheroid generated by the revolution of an ellipſis, turning on its ſhorter axis, the particular figure, or the ellipticity of the generating ellipſis, which your obſervations give, is nearer to what Sir ISAAC NEWTON ſaith it ſhould be, if the globe were homogeneous, than any that can be derived from former obſervations. But yet it is not what you imagine. Taking the gain of the pendulum in latitude 79° 50′ exactly as you ſtate it, the difference between the equatorial and the polar diameter, is about as much leſs than the Newtonian computation makes it, and the hypotheſis of homogeneity would require, as you reckon it to be greater. The proportion of 212 to 211 ſhould indeed, according to your obſervations, be the pro-

(b) Captain Phipps, in a letter to me of the 15th of September, ſays, "You were right in ſuppoſing that the ſituation of the teleſcope did not admit of any means of verifying its poſition."

portion of the force that acts upon the pendulum at the poles, to the force acting upon it at the equator. But this is by no means the same with the proportion of the equatorial diameter to the polar. If the globe were homogeneous, the equatorial diameter would exceed the polar by $\frac{1}{230}$ of the length of the latter: and the polar force would also exceed the equatorial by the like part. But if the difference between the polar and equatorial force be greater than $\frac{1}{230}$, (which may be the case in an heterogeneous globe, and seems to be the case in ours,) then the difference of the diameters should, according to theory, be less than $\frac{1}{230}$, and *vice versa*.

I confess this is by no means obvious at first sight; so far otherwise, that the mistake, which you have fallen into, was once very general. Many of the best mathematicians were misled by too implicit a reliance upon the authority of NEWTON, who had certainly confined his investigations to the homogeneous spheroid, and had thought about the heterogeneous only in a loose and general way. The late Mr. CLAIRAULT was the first who set the matter right, in his elegant and subtle treatise on the figure of the earth. That work hath now been many years in the hands of mathematicians, among whom I imagine there are none, who have considered confidered the fubject attentively, that do not acquiefce in the author's conclufions.

In the fecond part of that treatife*, it is proved, that putting P for the polar force, Π for the equatorial, δ for the true ellipticity of the earth's figure, and ε for the ellipticity of the homogeneous fpheroid,

$\frac{P-\Pi}{\Pi} = 2\,\varepsilon - \delta$: Therefore $\delta = 2\,\varepsilon - \frac{P-\Pi}{\Pi}$; and therefore, according to your obfervations $\delta = \frac{1}{251}$. This is the juft conclufion from your obfervations of the pendulum, taking it for granted, that the meridians are ellipfes: which is an hypothefis, upon which all the reafonings of theory have hitherto proceeded. But plaufible as it may feem, I muft fay, that there is much reafon from experiment to call it in queftion. If it were true, the increment of the force which actuates the pendulum, as we approach the poles, fhould be as the fquare of the fine of the latitude: or, which is the fame thing, the decrement, as we approach the equator, fhould be as the fquare of the cofine of the latitude. But whoever takes the pains to compare together fuch of the obfervations of the pendulum in different latitudes, as feem to have been made with the greateft care, will find that the increments and decrements do by no means follow thefe proportions; and in thofe which I have examined, I find a regularity in the deviation which little re-

* § XLIX.

fembles

ſembles the mere error of obſervation *(b)*. The unavoidable concluſion is, that the true figure of the meridians is not elliptical. If the meridians are not ellipſes, the difference of the diameters may indeed, or it may not, be proportional to the difference between the polar and the equatorial force; but it is quite an uncertainty, what relation ſubſiſts between the one quantity and the other; our whole

(b) This will beſt appear from the following table, which ſhews the different reſults of obſervations made in different latitudes. The three firſt columns contain the names of the ſeveral obſervers, the places of obſervation, and the latitude of each. The fourth ſhews the quantity of $P-\Pi$ in ſuch parts as Π is 100000, as deduced from comparing the length of the pendulum at each place of obſervation, with the length of the equatorial pendulum as determined by M. Bouguer, upon the ſuppoſition that the increments and decrements of force, as the latitude is increaſed or lowered, obſerve the proportion which theory aſſigns. Only the ſecond and the laſt value of $P-\Pi$ are concluded from compariſons with the pendulum at Greenwich and at London, not at the equator. The fifth column ſhews the value of δ correſponding to every value of $P-\Pi$, according to Clairault's theorem:

		° ′	$P-\Pi$	δ
Bouguer.	Equator.			
Bouguer.	Porto Bello.	9—34	741,8	$\frac{1}{784}$
Green.	Otaheitee.	17—29	563,2	$\frac{1}{326}$
Bouguer.	San Domingo.	18—27	591,0	$\frac{1}{358}$
Abbé de la Caille.	Cape of Good Hope.	33—55	731,5	
	Paris.	48—50	585,1	$\frac{1}{351}$
TheAcademicians.	Pello.	66—48	565,9	$\frac{1}{329}$
Captain Phipps.		79—50	471,2	$\frac{1}{251}$

By this table it appears, that the obſervations in the middle parts of the globe, ſetting aſide the ſingle one at the Cape, are as conſiſtent as could reaſonably be expected; and they repreſent the ellipticity of the earth as about $\frac{1}{340}$. But when we come within ten degrees of the equator, it ſhould ſeem that the force of gravity ſuddenly becomes much leſs, and within the like diſtance of the poles much greater than it could be in ſuch a ſpheroid.

theory,

theory, except ſo far as it relates to the homogeneous ſpheroid, is built upon falſe aſſumptions, and there is no ſaying, what figure of the earth any obſervations of the pendulum give.

I flatter myſelf that you will take theſe ſtrictures in good part; as the only motive which induces me to trouble you with them, is one which I am perſuaded is a ruling principle with yourſelf, a regard to truth. Had your account been leſs circumſtantial, the miſtakes which have crept into the calculations might not have been ſo eaſily detected.

I have the honour to be, Sir, with the greateſt reſpect and eſteem,

Your moſt obedient and

moſt humble Servant,

SAMUEL HORSLEY.

POSTSCRIPT.

The time that the ſun's diameter takes to croſs the vertical wire, hath no connection with the principal object of your enquiry: the rule, however, which is given in the note, p. 161, is not a true one. The true rule might be deduced from Cotes Theorem 21. But it may be come at in a much eaſier way, and is this:

As the coſine of the angle (S), contained by the vertical circle and circle of declination, to the radius, ſo is the ſun's diameter in time to the time ſought. You will find this rule, and a very ſimple demonſtration of it in Mr. De La Lande's Aſtronomy, article 596, firſt edition. The ſun's diameter in time on the 16th July, 1773, was 135″,2; whence the time of its paſſing your vertical wire muſt have been 137″,6. The reaſon that your computer, though working by a falſe rule, hath come in this point very near the truth, is, that at the time of your obſervation the ſun's altitude was very nearly equal to his declination, and the coſine of altitude very nearly equal to the coſine of declination; and conſequently the proportion of *coſin decl.* × *coſin S* to *radius* × *coſin alt*, which your computer takes for the proportion between the ſun's diameter in time and the time

ſought,

fought, is, in this particular inftance, very nearly the fame with the proportion which univerfally obtains between thofe times, viz. that of cofin. S to the radius. But had the obfervation been taken at a time when the fun's altitude had been very different from his declination, the error of his rule would have difcovered itfelf; of which you may eafily fatisfy yourfelf by trial, if you think it worth while.

In juftice to Captain PHIPPS *I think myfelf obliged to inform the public, that the foregoing letter is publifhed with his confent; and that I have his authority to fay, that the calculations, which have given occafion to it, namely that of the retardation of the fun's return to the vertical wire, and that of the time which the fun's diameter fhould take to pafs the vertical wire, were both made by Mr.* ISRAEL LYONS.

F I N I S.