103.

Sir John Pringle K.
President of the Royal Society
from the Author

2338
2061

Coast & Geod. Surv.
from John M. Batchelder
July 1884.

REMARKS

ON THE

OBSERVATIONS

MADE IN THE LATE

VOYAGE

TOWARDS

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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.

LONDON:
Printed for B. WHITE, in FLEET-STREET; and P. ELMSLEY, in the STRAND.
MDCCLXXIV.
1774

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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.

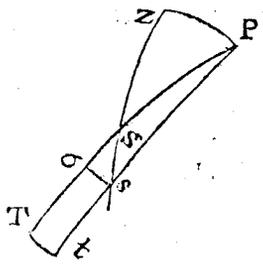
Mathematicians are no leſs indebted to you than mariners, for the attention which you have given to every object of ſcientific enquiry, though but remotely connected

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 evanescent triangle $S s \sigma$, is to be considered 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 sphere,

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

Ex æquo perturbatè $S\sigma : T t = \text{sin. } P\sigma : \text{tang. } P S Z$. That is, because $P\sigma$ and $P S$ are so nearly equal, that the sine of the one may without sensible error be taken for that of the other, $S\sigma : T t = \text{sin. } P S : \text{tang. } P S Z$. Q.E.D.

Your computer affirms, that

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

and to support this assertion he refers to Cotes *Æstimatio errorum*, *Theor.* 35.

Mr. Cotes's most elegant treatise, *De Æstimatione errorum in mixtâ Mathesi*, is published, together with the *Harmonia Mensurarum*, and some other lesser 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 scholia. I suppose, therefore, that theor. 35 is an error of the press. The theorem that ought to have been referred to, is the *thirteenth*: for, when the sun returns for the second 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 say, the side 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 σ? 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

	′	″
Which turned into time gives	0	8,3
The change of the equation was	0	5,4
Hence true interval of the two transits — —	h 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

conſidered the ſubject attentively, that do not acquieſce in the author's concluſions.

In the ſecond part of that treatiſe *, 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 ſpheroid,

$\frac{P-\Pi}{\Pi} = 2\ \varepsilon - \delta$: Therefore $\delta = 2\ \varepsilon - \frac{P-\Pi}{\Pi}$; and therefore, according to your obſervations $\delta = \frac{1}{251}$. This is the juſt concluſion from your obſervations of the pendulum, taking it for granted, that the meridians are ellipſes: which is an hypotheſis, upon which all the reaſonings of theory have hitherto proceeded. But plauſible as it may ſeem, I muſt ſay, that there is much reaſon from experiment to call it in queſtion. If it were true, the increment of the force which actuates the pendulum, as we approach the poles, ſhould be as the ſquare of the ſine of the latitude: or, which is the ſame thing, the decrement, as we approach the equator, ſhould be as the ſquare of the coſine of the latitude. But whoever takes the pains to compare together ſuch of the obſervations of the pendulum in different latitudes, as ſeem to have been made with the greateſt care, will find that the increments and decrements do by no means follow theſe proportions; and in thoſe which I have examined, I find a regularity in the deviation which little re-

* § XLIX.

ſembles

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—Π 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—Π 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—Π, according to Clairault's theorem:

			P—Π	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	$\frac{1}{331}$
The Academicians.	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.

POST-

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,

ſought, is, in this particular inſtance, very nearly the ſame with the proportion which univerſally obtains between thoſe times, viz. that of coſin. S to the radius. But had the obſervation been taken at a time when the ſun's altitude had been very different from his declination, the error of his rule would have diſcovered itſelf; of which you may eaſily ſatisfy yourſelf by trial, if you think it worth while.

In juſtice to Captain PHIPPS *I think myſelf obliged to inform the public, that the foregoing letter is publiſhed with his conſent; and that I have his authority to ſay, that the calculations, which have given occaſion to it, namely that of the retardation of the ſun's return to the vertical wire, and that of the time which the ſun's diameter ſhould take to paſs the vertical wire, were both made by Mr.* ISRAEL LYONS.

F I N I S.

104.

CONSIDERATIONS

on

M. Buache's Memoir

concerning

New-Britain and the *North Coast* of New-Guinea,

by

Dalrymple.

" La vérité qu'il convient de mettre dans tout ſon jour pour l'avancement des connoiſſances, eſt que l'opinion de M. Dalrymple ſe trouve détruite, au lieu d'être confirmée par les découvertes du Capitaine Carteret, & que ſon plan de la Nouvelle Bretagne, le plus défectueux de tous ceux que l'on connoît ſur cette partie, ne peut ſervir, ſur-tout dans un ouvrage fait pour inſtruire, qu'à perpétuer des erreurs & retarder le progrès des découvertes.

M. Buache, Eclairciſſemens Géographiques ſur la Nouvelle Bretagne & ſur les côtes ſeptentrionales de la Nouvelle Guinée.

Mem. de l'Acad. R. des Sciences, 1787. *P.* 128.

LONDON,

Printed by GEORGE BIGG, 1790.

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ADVERTISEMENT.

FOR the ready underſtanding the following MEMOIR reference muſt be had to the ſeveral *Plates*, which contain all the important information, that I recollect to have ſeen, in Maps of theſe parts; and it will be, I flatter myſelf, acceptable to the Publick to have the whole brought into one view: I have indeed thought it unneceſſary to reduce to a commodious ſize for this MEMOIR, the *Map* from the *Engliſh Tranſlation* of *Linſchoten*, 1595, and *That* from an antient French MS, wherein the *Eaſt Coaſt* of *New Holland* is laid down, now at the *Britiſh Muſeum*, as I have already publiſhed theſe *Maps* on Whole Sheets, to which the curious may refer.

The Plates properly appertaining to this MEMOIR are

1. . . The Plate originally conſtructed in 1762, and publiſhed in 1770, wherein there is a copy of *Dampier*; and a tracing of *his outline*, filled up from the *antient Maps* and *Deſcriptions*.

2. . Copy of the *Map* of *Ortelius*, in 1574.

   Dº . . . . . 1589.

   Dº . . . . *Tattonus*, 1600, from M. Buache.

   Salomon Iſlands, from *Arcano del Mare*, 1661.

3. . *Map* of *New-Guinea*, &c. from the *Arcano del Mare* by *Dudley*, Duke of Northumberland, . Edit. 1646.

   Dº . . Dº . . Dº . . Edit. 1661.

4.

4. . *Map* of the *North Coaſt* of *New-Guinea*, and the *Salomon Iſlands*, by *John Teixeira*, 1649, from M. *Thevenot*.

M. Buache's *Map* of *New-Britain*, and the *North Coaſt* of *New-Guinea*, 1787.

5. . *Map* of *New-Guinea*, &c. M. *Guillaume Sanſon*.

D° . D° . from Taſman 1642.

D° . D° . from *Schouten* and *Le Maire* 1617.

D° . D° . by M. *Robert de Vaugondy*, 1756, from the *Hiſtoire* des *Navigations* aux *Terres Auſtrales*.

D° . D° . 1774, from M. *Freville's* Tranſlation of my Collection of South-Sea Voyages.

6. . Map of *part* of *New-Britain*, &c. from a Spaniſh MS by Don Franciſco Antonio Maurelle, 1781.

D° . D° . from Capt. Carteret, 1767.

7. . *Map* of *New-Guinea*, &c. from M. Bougainville, 1768.

Early in Life I ſaw, at *Madraſs*, a *Spaniſh* MS of theſe parts; which, to the beſt of my recollection, contained the *Diſcoveries* of *Torres* in 1606 on the South of *New-Guinea*, It was amongſt the Papers of Mr. *William Roberts*, who had been a *Supra Cargo* to *Manila*, and who loſt his Life in the ſiege of Madraſs, but on my return there in 1762, after my firſt Voyage to *Sooloo*, I in vain endeavoured to trace it: I am aſſured that the Original letter of *Torres*, dated at Manila 12th July 1607, is ſtill exiſting in Spain.

5th Auguſt, 1790.

Dalrymple.

CONSIDERATIONS on *M. Buache's Memoir*, concerning NEW BRITAIN, &c.

18th March, 1790.

I did not, till this day, ſee M. *Buache's* Memoir, and his *Maps* accompanying *It*; At the ſame time that I make my acknowledgements for the courteous manner in which M. *Buache* ſpeaks of myſelf, I ſhall criticiſe *His Memoir* and *Map* with the ſame freedom he has done mine.

So diſtinguiſhed a Geographer, as M. *Buache*, muſt be ſenſible, that PRECISION is not to be expected in *Geographical Combinations*; *Surveys* and *Aſtronomical Obſervations* ONLY can give PRECISION: *all* that can be preſumed from *combinations*, is an *Aproximation* to the *truth:* and in General Geography, THAT is the *utmoſt* to be expected.

In juſtice to a Geographer, the *period* muſt be conſidered, in which the Geographical Diſquiſition was made: The *Map* of *China*, by *Ludovico Georgio*, publiſhed in 1584 by *Ortelius*, expreſſing the *general ſituation* of the *Provinces* in *that Empire*, was a valuable acceſſion to Geography above two Centuries ago; although *That Old Map* would be diſgraced on a

 compariſon

comparison with the *Surveys* made by the Jesuit Missionaries in the *present Century*.

I have now before me the *Original* of my Map concerning the *Salomon Islands*, which is the subject of M. *Buache's* criticism; This Map is dated 7th December 1762. It was constructed by tracing *Dampier's Map*; and filling up his Outline by comparing It with *De Bry*, 1596, and *Herrera's Description*. In 1767, I published a Pamphlet on the South-Sea Discoveries, in which there was a *General Map*, wherein I first conveyed to the World the idea I entertained of the *Salomon Islands* being *New Britain*: The Memoir concerning the *Salomon Islands*, though of an earlier date, was not published 'till 1770, in the "Historical Collection of Voyages and Discoveries in the South-Pacific Ocean," in which Collection I published the Plate containing the *Copy* of *Dampier's Map*, and *That* which was the result of my having collated *Dampier* with *De Bry*, &c. This Plate I afterwards lent to Capt. *Forrest*, in whose Voyage, published in 1779, M. *Buache* seems first to have met with it.

When This Map was constructed, I could not possibly have any knowledge of the subsequent discoveries, which have been made by Capt. *Carteret*, M. *Bougainville*, M. *Surville*, &c. but at that time I had not even seen many documents that have since fallen into my hands, in the printed Maps of early times, as well as in some MSS. which I have since published.

The

The Geographers of the present Century have represented the *Salomon Islands* at a remote distance from *New Guinea*, in the *middle* of the *Pacific-Ocean*, contrary to the early Maps, which lay them down in the vicinity of *New Guinea:* This Error of Modern Geographers [a] was what my Map was chiefly meant to point out: a comparison of the *figure* of the Old Maps with *Dampier*, led me to think *his New Britain*, was, in truth, *not one*, but *several Islands*, which has been fully established by Capt. *Carteret's* Voyage, notwithstanding M. *Buache* is pleased to say, that "my Opinion is over-"turned, instead of being confirmed, by Capt. *Carteret's* discoveries." It was also my opinion, that these *several Islands* were what the antient Maps named the *Salomon Islands*.

My Hypothesis did not go so far as to insinuate, that the *Spanish* Discoveries had not extended to a greater Latitude than *Dampier's New Britain*, on the contrary, in the General Map, I had marked the *Land*, on the *South* of *New Britain*, by the name *Guadalcanal*, a name given to it in *Mendana's* Voyage to the *Salomon Islands*; This *Land*, *I*, *very erroneously*, supposed to be what

[a] M. *De Lisle* 1700 appears amongst Modern Geographers the Father of this Error, for he is the first Modern Geographer in whom I find the *Salomon Islands* placed at a remote distance from *New Guinea:* It is true, *Dudley*, in the *Arcano del Mare*, Edit. 1661, has laid them down at 800 leagues from *Peru*, but *De Lisle* does not adopt *Dudley's* position, he removes them farther Westward, where certainly there are no such Islands.

what the *Geelvink* visited in 1705: and, although Mr. *Buache* admits me to be *right* in this supposition, the *actual Map* of the *Geelvink's Voyage*, which I published in 1781, shews, that The *Geelvink's Discoveries* were much farther to the Westward, where both *Dampier* and *Bougainville* suppose there is a *Strait*, and which *Strait* is expressed in the Map, to the *English* Translation, of *Linschoten*, 1595: On the Entrance of this *Great Bay* of the *Geelvink*, lyes *Dory* where Capt. *Forrest* was.

M. *Buache* has therefore supposed me to be *right* where I was certainly *wrong:* M. *Buache* is also mistaken in supposing that I had "without farther examination "adopted one of the Maps made for the *Histoire des* "*Navigations aux Terres Australes*," [a] for, although, in laying dawn my *erroneous supposition* of the *Geelvink's* Discovery, I followed the circumstances, recited from *Nicolas Struyk*, in the *Histoire des Navigations aux Terres* "*Australes*,

[a] "Mais M. Dalrymple, en dressant ce plan, n'avoit pris en considération "que la partie qui forme la Nouvelle Bretagne, & dans laquelle il prétendoit "retrouver les îles de Salomon; pour tout le reste, c'est-à-dire, pour la "côte de la Nouvelle Guinée, il avoit également adopté, sans autre examen, "une des cartes dressées pour l'Histoire des Navigations aux Terres Australes, "sur laquelle on avoit fait usage des anciennes découvertes. S'il eût été "dans le cas de faire des recherches particulières sur cette côte, avec l'attention "& les connoissances dont il a donné tant des preuves, il eût aisément "reconnu les erreurs de la carte qu'il adoptoit; il eût vu en même temps "le peu de fondement de son systême sur les îles de Salomon, & il l'eût "abandonné sans peine." *M. Buache Eclaircissemens, &c.* P. 129.

Auſtrales, applying thoſe circumſtances to a wrong place; I did not follow *any* of the *Maps* in that *Work*, as muſt be ſufficiently obvious to any one who compares my Map with them: At the ſame time, I by no means intend to depreciate the labours of M. *Robert de Vaugondy*, the Geographer who conſtructed the Map of *New Guinea* in the *Hiſtoire des Navigations aux Terres Auſtrales*; Although I do not, in all things, concur with him, I muſt do that Geographer the juſtice to ſay, he has the merit of reſtoring, from the early Maps, what other modern Geographers had improperly expunged.

I readily admit, that I have not yet, even to this day, ſeen any documents, by which a tolerable repreſentation can be made of *Mendana's* Diſcoveries, in 1567, during his *firſt Voyage*, but the queſtion in iſſue between M. *Buache* and me, is *not*, "whether *Mendana's* Diſcoveries "be *truly* repreſented?" becauſe that I have not pretended to do; but the queſtion is *ſimply* "Whether "the Iſlands called *Salomon Iſlands*, in the old Maps, be "what *Dampier* names *New-Britain* or *not?*"

M. *Buache* ſuppoſes that Iſlands on the North of *New-Guinea*, which are marked in ſome of the old Maps under the name of *Barbada*, &c. are *Dampier's New-Britain*, which the *Arcano del Mare* has without reaſon removed farther Eaſtward: M. *Buache* has given a Map which he alledges to be a copy of one in the *Arcano del Mare*, 1647. I conceive he has made a miſtake in quoting the *Edition*; for the Map he has

 engraved

engraved appears evidently to be taken from the Edition 1661, and not from that of 1646: for the clear elucidation of this matter, and to enable the Public to judge, how far *Dudley* is a competent Authority to rely upon, I have been induced to copy his Maps from both Editions, so far as concern the present subject: It appears from the Map in his 1st Edition that he had some idea of *Mendana's last* Voyage, 1595, for the *Marquesas* are laid down between the Long. 241° and 245° in which *Lima* is in 311°½. This places *Magdalena* in 66° W from *Lima*, which being in Long. 76° 49′

	66. 0.	
gives . . .	142. 49.	W.
By *Cook* it is in	138. 49.	
or only .	4. 0.	more to the East.

But the *Marquesas* are omitted in the Edition 1661,[a] which have a cluster of *Islands*, under the name of the *Salomon Islands*, from about the Lat. 8° to 11°½ S, and Long. 263. to 266 or about 123° W, a part of the Pacific Ocean which has not been traversed in any of the late Voyages; I do not mean to insinuate that I think *Dudley* right in this position of the *Salomon Islands*; and he is certainly wrong in the date, 1580, which he assigns for *Mendana's* Discovery of them: Yet I think it

[a] It should be here taken notice, that the 1st Volume of the Edition 1661 contains the *Charts* that were in the Edition 1646, but the 2d Volume contains the Charts *peculiar* to this Edition 1661.

it not improbable there may have been Iſlands diſcovered thereabout in 1580, as Sir *Richard Hawkins* mentions having ſeen a MS of the Voyage, where Iſlands were diſcovered nearly in that ſituation, viz. 750 leagues from *Santa*. In the 1ſt Edition of *Dudley's Arcano del Mare*, in the vicinity of *New-Guinea*, are ſome Iſlands; ſome whereof are ſimilar, in name, to what the Edition of 1661 calls the *Salomon Iſlands*, but theſe adjacent to *New-Guinea*, he expreſſly ſays are *not* the *Salomon Iſlands*: he places the Eaſternmoſt Iſland which he calls *C. della Guada* in about 192° that is in 163° 40′ E from *Greenwich*. Theſe are unqueſtionably, I think, the *Santa Cruz Iſlands*, (the Eaſt part of which *C. Carteret* places in about 166° E. confounded with *Mendana's firſt* Diſcoveries. The *Iſlands* of the old Maps, which M. *Buache* ſuppoſes to be *Dampier's New-Britain*, appear to me to be thoſe which *Dampier* paſſed, after he got through the *Strait* that bears his name: This, I think, is very evident by inſpecting all the old Maps (*Dudley* excepted) all of them taking notice of the *Volcanos*; whereas there is no indication, in either Maps or Journals, of any *Volcano* to the *South* of *New Britain*.

I readily grant that the *Salomon Iſlands* in the Old Maps[a] are carried into a higher Latitude, than *Dampier's New-Britain*:

[a] I have never ſeen the Publication of *Tattonus*, from which M. *Buache's* Map is copied; I wiſh M. *Buache* had been more explicit in its deſcription; but I have ſome MS. Charts by *Gabriel Tatton*.

Britain: [a] and I will farther grant, that if we had an *Original* Chart of *Mendana's Discoveries*, assigning such a Latitude to the *Salomon Islands* as the Maps do, It would be a presumptive argument, that my supposition had been groundless, but instead of an *Original Chart* of *Mendana's* Voyage, we have nothing but the discordant representations of Geographers, who have confounded the various Discoveries of *Mendana*, and who, I conceive, have endeavoured to adapt some Plan, that had fallen into their hands, to the Relations of his Discoveries ill-understood: Besides, it is obvious that the Latitudes in the different Editions of *Ortelius* have been falsified: for example Cape *Mendocino*, which is in 40° N Latitude is carried afterwards beyond 50° N., and therefore *Even Latitudes*, unless we have the *Original* and *faithful Documents*, are not implicitly to be relied on.

The earliest authorities are the proper authorities to consult on this subject: The First Map in which I find the *Salomon Islands*, by name, is the Map of the World by *Ortelius*, 1587, but, as He is more circumstantial in the Map of the *South-Sea*, 1589, in the Memoir to which, he expressly mentions them, as the recent discovery of *Alvaro de Mendana*, I have copied that Map; at the same time I have copied his Map of *New-Guinea*, 1574, when he was ignorant of the *Salomon Islands*, in which ignorance

[a] The Map published by *Thevenot* makes the South part in 8° S. which *Dampier* places in 7° S. So that the difference is there less considerable.

ignorance he continued in 1584; having made no alteration, in the Edition of that year, to his Map of 1574. In the Map of *Ortelius*, containing the *Salomon Iſlands*, they are marked in Latitude from 1° to about 12° S, and retain much ſimilitude to *New-Britain* of *Dampier*; The Latitude in the Map to the Engliſh Edition of *Linſchoten*, 1595, has carried them farther South, but ſtill the reſemblance is preſerved: I muſt therefore ſtill retain my opinion, that the *Salomon Iſlands* of *Mendana*, are what *Dampier* has named *New-Britain*; though M. *Buache* thinks, the leaſt attention would have convinced me there is no foundation for that opinion: Certainly, if I thought myſelf miſtaken, I would at once, without difficulty or heſitation, give up the conteſt! If I could even think, with Mr. *Buache*, that my opinion would "retard the progreſs of diſcoveries," I would bury it in my own boſom! But how my Map of the *Salomon Iſlands* could ever tend to "retard the Progreſs of "Diſcovery?" is beyond my comprehenſion! for ſuppoſing a Navigator on the ſpot, with *this Plate* before him, in which I have given a *ſimple Copy* of *Dampier's Map*, and *another*, aſſuming *Dampier's Outline*, and expreſſing my ideas of the antient diſcoveries; would it not, inſtead of *retarding*, much rather tend to *promote Diſcovery*, by inducing ſuch Navigator to inveſtigate, whether *Dampier's continued line* of *Coaſt*, or my idea of *Iſlands*, was *true?* Had I ſubſtituted an *Outline* of *my own*, in the place of *Dampier's*, I might then be charged with retarding the progreſs of diſcovery, by ſubſtituting *imagination* for *fact*. The Experience of

Capt. Carteret's Voyage has ſhewn I was right in ſuppoſing a Channel through *New-Britain*, where he found *one*: future Experience muſt confirm, or refute, the other partitions I have alledged: They, who judge from Geographical Analogy, will ſcarcely believe ſuch a *ſlip* of *Mountainous Land*, as Capt. *Carteret's New-Ireland*, is really *one Iſland*. I myſelf have no doubt that what C. *Carteret's* printed Map calls *Sandwich* Island, is the NE part of *Iſabella*; although to invalidate this probability, it is alledged, that *Taſman*, after doubling the North point of what is now called, *New-Ireland*, in ſtanding *South*, ſaw *no Land* to the *Eaſt*:[a] but here M. *Buache* has miſquoted *Taſman*, I do not mean to inſinuate *intentionally*; but *Taſman's* Journal ſays, in this Place, from Lat. 5° 27′ S, they *ſaw Land Eaſt* and NE.

Whenever an *erroneous* Map is *engraved*, it unqueſtionably tends to *perpetuate errors!* It, at the ſame time, tends to bring thoſe *Errors* to *light*, and to a *fair diſcuſſion*; but whether my Plan of *New-Britain*, giving, on the ſame Plate, a Copy of *Dampier's* Map, and my own application, of the antient Diſcoveries to *Dampier's* Outline, made in 1762, and publiſhed in 1770, can be ſtiled the *moſt defective of any known?* may admit a queſtion? when a compariſon is made of it, with Mr. *Buache's* Plan publiſhed in 1787. For according to *His Syſtem*, *New-Britain*, of *Dampier*, is to be found

[a] *Taſman*, après avoir doublé la pointe occidentale de ce qui forme aujourd'hui la nouvelle Irlande; fit route directement au ſud, juſqu'à ce qu'il rencontrât la côte de la nouvelle Guinée, & il ne vit aucune terre du côté de l'eſt. P. 142.

found in the Islands, called in the Old Maps, *La Barbada*, &c. though he would be a little puzzled to torture *them*, into any thing like the form of *Dampier's New Britain*; or, to be consistent, he ought to cut *New-Britain* into shreds to denote those several Islands: But if to adopt any part of the old Discoveries is to "retard the progress of modern discovery," why does M. *Buache* separate *New-Britain* into *two Islands?* by the Channel which he makes at *Port Montague*; for there is no indication in Modern Voyages of such a Channel: As I cannot perceive any *similitude in form* to the *Salomon Islands*, of the old Maps, in the Lands seen by M. *Surville*, &c. I must leave to M. *Buache* to explain *his own System* concerning those Lands; which, I have no doubt, comprehend *Guadalcanal* of the early *Spanish* Discoveries: I do not indeed know whether M. *Buache* has published any Memoir on that Subject.

I cannot conclude without expressing my regret, that there is not a more ready communication amongst Geographers, in the various Countries of *Europe*; whereby none could be ignorant, for a considerable time after the Publication, of the Geographical Publications of any Country; such a ready communication would tend more to promote the Science of Geography, than *Memoirs*, drawn up in ignorance of what has been many years published in other Countries.

P. S.

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Before I quit this Subject a few words may be expected concerning a *Memoir* of M. *de la Borde*, meant to prove that the *Land seen* by Lieut. *Shortland* in 1788, is the same seen by M. *Bougainville* in 1768, and by M. *Surville* in 1769: This Fact is *incontestable*: and Lieut. *Shortland* can have *no pretensions* to the *Discovery* of that *Land*; It was indeed seen by Capt. *Carteret*, in 1767, before any part of it was seen by M. *Bougainville*, and called by him *Carteret* and *Simson's Islands*; but it obviously appears to have been seen by *Roggewein's Squadron* in 1722, so that even C. *Carteret* has no pretensions to the *discovery*, much less M. *Bougainville* or M. *Surville*, who saw *it after him:* I have no doubt it was seen in former times by the *Spaniards*; so that the *dispute*, amongst the Moderns, about the Honour of the *Discovery*, is a Dispute about nothing: though Lieut. *Shortland* is inexcusable for calling it a *Discovery*, since, before the Publication of his Chart, He had seen the *printed Map*, in which the *North Coast* was laid down from M. *Surville*.

Dalrymple.

21st July, 1799.