

in two short lines; my fidelity to the king not permitting me to send you any more.

N·A LTERIUS·DVLC
DEM·CVRI·S·CRVDE

This is the size and shape of the characters. In this bit there are eight lines. There are other bits with many other words; which are all preserved in order for their publication.

There have been found likewise very lately three beautiful statues of marble, and one of them excellent: Six heads of bronze, of which there is one, that gives hopes of finding the statue it belongs to. It is a young Hercules, of a kind of work, that has no fellow in the way of metal, having the hair finished in a surprising manner. Likewise several little figures of metal; a sistrum very neat and well preserved; and there is not a day passes, in which they do not bring to me some curiosities newly found.

X *A Translation and Explanation of some Articles of the Book intituled, Theorie de la Figure de la Terre; by Mons. Clairaut, of the Royal Academy of Sciences at Paris, and F. R. S.*

Read Feb. 15, 1753. **M**R. Short, in his account of Father Frisius's *Disquisitio mathematica in causam physicam figuræ et magnitudinis telluris nostræ*, having reported that philosopher's sentiments

K

on

on my reflections upon the same matter, without taking the trouble to examine whether they were founded upon the truth or not, I find myself under the necessity of laying before the Royal Society the passages of my book, which, having been misunderstood by F. Frisius, have occasioned the misconstruction, which he has made of my sentiments, either upon the trust I give to the actual operation made for discovering the figure of the earth, or Sir Isaac Newton's theoretical inquiries about the same subject.

The expressions of Father Frisius, referr'd to by Mr. Short, are as follow :

“ Quia tamen plerique omnes hucusque, aut nihil
 “ pro figura telluris determinanda ex iis observationi-
 “ bus deduci posse cum geometra celeberrimo Rug-
 “ gero Boscovik autumârunt, aut exinde cum Ill.
 “ Clairaut, Bouguer, aliisque, contra incomparabilem
 “ virum ac prope divinum Isaacum Newton insur-
 “ gentes, admirabilem ipsius theoriam facto minus
 “ respondentem dixerunt, assignatamque in prop. 19.
 “ lib. 3. *Princip. Mathem.* terrestrium axium pro-
 “ portionem à vera absonam omnino esse, alios mihi
 “ observationibus parum, alios nimis tribuere visum
 “ est, omnes ferme oppositis erroribus peccâsse, ubi
 “ res neque aurificis lance, neque molitoris, ut aiunt,
 “ statera librandæ sunt.”

This, when compared with the propositions of my theory, which they are relative to, will appear, I hope, quite incoherent : and I cannot shew it more clearly, than by translating the last chapter of my book, to which F. Frisius refers the reader.

For

For the better understanding of that chapter, it is proper to know, that the chief results of the precedent inquiries are these theorems :

1. Supposing the earth in its former state composed of several fluids of different densities, and settled all in equilibrium by the laws of gravity and centrifugal forces, the surfaces separating the different mediums will always affect the form of a curve ; which is so near to the ellipsis, that it may be supposed so, without any error of the least moment.

2. That, in the case of the denser fluids being nearer to the center, as hydrostatics require, the spheroid will always be less flat than in the homogeneous one, and *vice versa*.

3. And as to the diminution of the gravity from the pole to the equator, it will always follow the opposite rule ; *viz.* if the spheroid be denser towards the center, the gravity will decrease in a less ratio than in the homogeneous spheroid, and *vice versa*.

4. That if δ represent the fraction found out for the difference of diameters, $\frac{1}{117}$ — δ will express the total diminution of the gravity from the pole to the equator, not only in the case of the spheroid supposed originally fluid, but in any supposition of variation for the densities and proportion of the diameters of the beds, provided they be elliptical.

These premised, let us proceed to the said last chapter of the theory of the earth's figure ; in which the principles laid down in the preceding chapters are compared with the observations.

§ LXVIII. *For the diminution of the gravity from the North to the South.*

It has been seen in the preceding chapter, that when a spheroid is not supposed homogeneous, the diminution of the gravity from the pole to the equator will be greater than in the case of homogeneity. Hence, if my theory holds in our globe, the whole decreasing of the gravity will be equal to $\frac{1}{230}$ or greater, and never less; since the ratio of 230 to 231 will (§ XXI.) express the ratio of the action of gravity at the equator and pole, when the spheroid is homogeneous.

And this conclusion of my theory quite agrees with experience; for, from all the observations relating to the gravity made in several places of the globe, either by actual measures of the second pendulum, or by the difference of duration of the same pendulum's vibrations, it appears, that the gravity decreases from the north to the south in a greater ratio, than it would be if the total diminution from the pole to the equator were only $\frac{1}{231}$.

§ 69. *For the proportion of the two diameters.*

Supposing, as in the precedent chapter, the earth originally fluid, it follows, from the § LXV. that the ratio of the two diameters cannot exceed that of 230 to 231; since, § XX. 230 to 231 is the ratio in the case of the homogeneous spheroid; and as the mensurations of the gravity cannot agree with the supposition of the homogeneity, the diameters of the earth ought to be in a ratio less than 230 to 231.

Without

Without adhering to the supposition of the earth's being formally fluid, but admitting, as in the chap. 3 and 4, all generality possible in the variation of density and ratio of diameters of the beds or strata laid down from the center to the surface, there will still happen a difference of the diameters less than $\frac{1}{130}$. For, by § L. the total diminution of the gravity from the pole to the equator being subtracted from $\frac{1}{117}$, the remainder is the difference between the diameters. Now the diminution of the gravity having been found greater than $\frac{1}{210}$, the ellipticity or difference of diameters ought to be less than that fraction, and consequently the ratio of diameters less than 230 to 231.

That consequence of my theory is not so happy as the preceding; for the degree measured in the north compared to that of France give the two diameters as 177 to 178, which ratio is greater than 230 to 231, instead of being less, as the theory would require.

As the measures made in the north have been performed with great care and exactness, their result seems at first to be preferred to that of my theory. But a reflection upon the errors unavoidable in actual measures, and an examination of the limits of these errors, will shew, that, without violating the measures, they would be brought nearer the theory, and even agree with it.

For, by a convenient calculation, it will be found, that a diminution less than 60 toises, made to the difference between the degrees of Paris and Tornea, would give the diameters in the ratio of 230 to 231. And if it be considered, what is the smallness of an error of 60 toises, when divided in two operations, which
require

require so great a number of astronomical and geographical observations, it will be thought, that an error a little larger may be supposed, without disparaging either our operation, or Mr. Picard's; and thus theory and experience would agree.

Supposing, for example, that the difference between the degrees of Paris and Tornea has been found too great by 80 toises, the difference between the two diameters will come out of about $\frac{1}{260}$, which, subtracted from $\frac{1}{11}$, gives $\frac{1}{206}$ for the diminution of the gravity from the pole to the equator. And such a conclusion would agree pretty well with the observations made in France and Lapland with the excellent clock of Mr. Graham.

However, altho' the errors to be supposed in the operations, to reconcile them with my theory, be in themselves small enough, I shall abstain from asserting, that they have been committed. It is a fact not to be decided, till after the result of the observations, which are expected from Peru. For the great difference, which is to be found between the degrees of Quito and Tornea, is the only means of knowing, whether the diameters be less or greater than 230 to 231.

Were the question only to demonstrate the flatness of the earth, the measures of the degree of Paris and Tornea would be full sufficient; but to discover the true ratio of diameters, is what can be performed only by the comparison between the degrees, whose mutual distance is the greatest.

Such a ratio once fixed, if it happen to be less than 230 to 231, it will be very easy, by the preceding theory, to imagine some hypothesis for the inside of
the

the earth, which shall agree with both theory and observation, whether admitting the supposition of the original fluidity of the globe, or not.

But if the diameters were found undoubtedly in a greater ratio to one another than 230 to 231, I own, that not only the theory established in this second part of my book must be abandoned, but it would be very difficult to reconcile the measures of the pendulums with those of the degrees in Sir Isaac's system. And I dare say, that the success in that case would hardly depend upon any natural hypothesis.

The subsequent LXX article containing only a proof, that the preceding theory agrees with any ratio between $\frac{1}{23}$ and $\frac{1}{9}$, for the quantity, which expresses the excess of Jupiter's equator above its axis, there is no necessity for the translation of the arguments leading to a result so answering to the observations; and I pass to the conclusion of that article, which ends my book.

The preceding theory agreeing with all the measures of the pendulum, and observations of Jupiter's diameters, if, besides, it happen, that the measures expected from Peru give, when compared with those of Lapland, a difference of diameters less than $\frac{1}{35}$, this theory will have all possible confirmation, and the universal gravitation so well agreeing with the motions of the planets will also agree with their figures.

Now I beg every candid reader to examine, whether, in that chapter quoted by F. Frisus, I have too much relied upon the certainty of observations, and attempted to disparage Sir Isaac Newton's discoveries.

In the first place, I will ask of Father Frisius, if, before the operations, which I depended upon, were performed, I could establish any thing against their agreeing, or not, with Sir Isaac's proposition about the same matter?

He perhaps will answer, That my remark of the LXIX art. *But if the diameters were found undoubtedly in a greater ratio to one another than 230 to 231,* imports, that I was not thoroughly convinced, that what care soever would be taken by the gentlemen sent into Peru, they never would be able to measure their degree with a sufficient exactness, to conclude, from its length, compared with that of the other degrees, whether the diameters were in a greater or less ratio than 230 to 231; and consequently he will think, that my being in suspense about it was an offence against Sir Isaac's theoretical determination. Then, I request Father Frisius to tell me, why he is so good as to commend operations so void of use as those, which tended only to discover what was demonstrated before, and needed not to be confirmed, since it could not be invalidated.

Perhaps Father Frisius, in representing me as depending too much upon the observations, relied on these expressions of the LXIX art. *As the measures of the gravity cannot agree with the supposition of the homogeneity:* and I confess, that it seems to me impossible to reconcile the great number of all the measures of that sort with the table, which follows the homogeneity. For the simplicity of the means made use of in the performance of those measures cannot admit the errors, which should be supposed to bring them to Sir Isaac's theory: but as this theory is founded

on

on the homogeneity, which is only a mere supposition; and as he has himself suspected, in his second and third edition, that the internal parts of the earth might be denser than those towards the superficies, I do not see how I oppose myself that illustrious philosopher, when I assume the same hypothesis, as he does. As I shall use all possible endeavour to understand F. Frisius's meaning, I hazard this conjecture. Seeing that I thought favourably enough of the exactness to be obtained in astronomy, when observations have been already made in great numbers, and with all possible care, to suppose them fit to let us know, whether the diameters are in a greater or a less ratio than 230 to 231; and being informed afterwards, that the operation made in Peru led those, who have made use of it, to imagine the spheroid flatter than the homogeneous, he concludes, that I cannot help thinking like them, and accordingly indulges himself in exposing, how much I over-rate the validity of observations, and how little I know the submission due to a proposition of Sir Isaac; which, I must say, by the bye, that great man has never himself given as impossible to be opposed by experience. But yet I would ask of F. Frisius, wherefore he will guess at my sentiments, whilst I have not given room to know them on that point? How can he know, whether, since the examination of all the measures, I have not found any way to reconcile them with the theory? Which I say in no manner as a hint I intend to make any corrections in those measures, but merely to shew the little foundation, which F. Frisius had to represent me as he has done.

I.

However

However difficult it may be to account for Father Frisius's expressions, I shall hazard yet another conjecture. His great zeal for Sir Isaac, for which he is certainly to be commended (if not blinded by that zeal) has hindered him from distinguishing between the different ways of opposing that great man's sentiments. Perceiving then, that my calculations (§ L. Part II.) had led me to a result quite different from Sir Isaac's assertion, (Prop. XX. lib. 3.) he was offended at my boldness to such a degree, that he was unable to examine impartially what I said; and, instead of discussing a mathematical question quite independent of any actual measure, wherein if I were mistaken, he would have forced every geometrician to condemn me, he has supposed, that I have built my argument upon an operation, which was not performed at the time when I wrote.

This conjecture would appear to me the true cause of F. Frisius's error, if it were not inconsistent with a proceeding of his towards Sir Isaac, which I will venture to relate. After F. Frisius has examined himself the 19 problem of the third book of the *Principia*, which is much less complicated than that I spoke of, the truth of which is incontestable, he finds, by his own mistake, a disagreement with the result of that proposition, and charges that illustrious author, without the least apology, with an error, which, says he, (quite from the purpose) is the sixth, that has been found in the same work, and also gives an enumeration of the five others, altho' they are not at all concerned in the question.

I cannot forbear saying, that the manner, in which I have proposed my remarks upon the 20th proposition

of Sir Isaac, has nothing of that slight way of treating so great a man; and as my utmost wish is to be judged on that account by the Royal Society, I shall relate what were my objections; which I cannot effect in a more concise and clear method, than by giving the translation of the article, which contains it.

§ LI. of the second part of the theory, &c.
 “ In which is seen what had induced Sir H. Newton
 “ to think, that the planets, when denser at the cen-
 “ ter than at the surface, ought to be flatter than in
 “ case of homogeneity.

“ Some years ago I gave, in the *Philos. Transf.*
 “ N^o 449, the theorem of the precedent article;
 “ and on this occasion I mentioned a passage of Sir
 “ Isaac contrary to it. Not having at that time
 “ looked into the second edition of his *Principia*, I
 “ could not know what had engaged that illustrious
 “ philosopher to think so; and far from suspecting
 “ any mistake in his proposition, I was contented to
 “ think, that the difference between our conclusions
 “ arose from a different way of conceiving the inside
 “ of the earth; and I imagined, that he had happen’d
 “ to fall upon such a disposition of parts, as would
 “ answer to his assertion. I then follow’d only his
 “ commentators, and especially Dr. Gregory, shew-
 “ ing, that his explanation of Sir Isaac’s conclusion
 “ was wrong, as grounded upon a proposition, which
 “ did not hold in the present case. For that pro-
 “ position (which is, that the gravity at any point
 “ of the earth is inversely as the distance from the
 “ center) has only room, when the earth is homo-
 “ geneous; and, consequently, ought not to be made

“ use of, when the density is greater towards the
 “ center than at the superficies.

“ Since I have discover'd, that the theorem, the
 “ demonstration of which I had given in the *Philos.*
 “ *Transf.* for the case of beds supposed of the same
 “ ellipticity, has room in an infinity of other suppo-
 “ sitions, I have taken greater care to discover what
 “ could have induced Sir Isaac to think, that the
 “ earth is flatter, as the gravity is more decreasing
 “ from the pole towards the equator; and I believe
 “ I have found it out in the second edition of the
 “ *Principia*, and it is, for having built upon the
 “ same argument as Dr. Gregory.

“ In p. 386, after having observed, that the ex-
 “ periments gave a diminution of two lines to the
 “ second pendulum from the pole to the equator, he
 “ argues thus: Since, says he, the case of homoge-
 “ neity afforded only $1 \frac{87}{1000} 4$, the difference $7\frac{1}{6}$ miles
 “ between the two diameters (which follow'd from
 “ the same hypothesis) is to be magnified in the ratio
 “ of $1 \frac{87}{1000}$ to 2, and it will come out $31 \frac{7}{12}$ miles
 “ for the real difference. For, pursues he, the re-
 “ tardation of the pendulum at the equator denotes
 “ the diminution of the gravity in that place; and the
 “ lighter the matter is there, the higher will it rise
 “ to be equiponderant with that of the pole.

“ Further, p. 387, examining the measures of the
 “ degrees of latitude made in France by M. Cassini,
 “ by which the earth is higher at the pole than
 “ at the equator by about 95 miles, he pretends,
 “ that accordingly the pendulum should be longer at
 “ the equator than at the pole by about half an inch.
 “ And all that, methinks, shews the opinion, which

“ Sir

“ Sir Isaac was of, that, in any case whatsoever, the
 “ equilibrium requires a gravity inversely propor-
 “ tional to the length of the columns, which pro-
 “ portion, as I hope to have evinced, is only demon-
 “ strated in the case of homogeneity, and is not true
 “ in general. Thus, what I argued in the *Phil.*
 “ *Transf.* against Dr. Gregory, has also room against
 “ Sir Isaac.”

By all that I have said, every body may judge, whether differing from Sir Isaac's sentiments on a point, which I had for so long a time examined, I did not express my disagreement with him in as decent a manner, as any one should, when speaking of so great a man. And in case the Royal Society thought some alterations were to be made in the form of my remarks, I declare, that I shall execute it, as may be prescribed to me by that illustrious company. But I cannot help thinking, that, unless those, who would examine my demonstrations, find some error in them, no alteration is requisite to be made in my expressions. I desire then either F. Frisius, or any geometrician, who thinks the question worth his examination, to take the trouble of reviewing my calculations, and to believe me ready to acknowledge my error, when shewn to me by a candid and impartial examiner.