



LXVI. Remarks on Mr. Yeates's Papers on the Lunar Theory

Mr. James Utting

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and then pressed ; the acid in the sugar saturated with chalk, and the liquid clarified with the white of an egg, skimmed, and boiled to the consistence of a syrup marked 29° in the areometer. The quantity of syrup produced was five pounds nine ounces. M. Bignami submitted one pound of this syrup to spontaneous evaporation in the open air ; but he only obtained three ounces two drachms of concrete sugar ; the syrup had doubtless not been sufficiently purified, or had experienced some degree of fermentation. A like quantity of five pounds nine ounces of the syrup of beet-root of the same degree of concentration, and the syrup of the arbutus cannot be supposed inferior to it, would have given two pounds nine ounces of pure concrete sugar. A new experiment of M. Bignami, in which the sugar had been converted with more care into the consistency of a crystallisable granulous syrup, produced four pounds two ounces and a half of sugar, which corresponds with the produce obtained in Spain, and proves that the fruit of Dalmatia is equally rich in sugar.

The sugar which was presented to the Government was white enough, very hard, and exactly similar in the grain and taste to cane sugar. The syrup was very agreeable, and might of itself form a valuable article of produce for the inhabitants of countries where the common arbutus grows spontaneously.

LXVI. *Remarks on Mr. YEATES'S Papers on the Lunar Theory.*
By Mr. JAMES UTING, of Lynn Regis.

To Mr. Tilloch.

SIR, — IN your Magazine for last month, your correspondent Mr. Yeates observes, that *my remarks* on his papers are *very curious* ; but whether they apply to the *substantial* part of his argument he leaves for others to determine. Agreed, so do I. Again he says, “My argument is a list of corresponding eclipses, which I have been at the pains to collect, and trust you will allow me the credit of having advanced my *hypothesis* on some *foundation*. It is true I have filled up the list with many computed dates, &c. But since these fill up the *steps* of the ladder in their true places, and give a *consistency* to the whole, I presume little *apology* may be required for their introduction.”

Now, sir, in the first place I deny that the argument is *substantial* ; secondly, assert that if his *hypothesis* had a foundation, he has overturned it ; and thirdly, that if a person ascends this ladder by treading on the substituted steps, he may probably be in danger of being precipitated to the bottom, and therefore an *apology* would give a *consistency* to the whole.

But

But to proceed. The Chaldean period is the most perfect and shortest of any, in which the solar and lunar motions, and the motions of their perigee and nodes, are nearly coincident. The other periods, which I before stated, show the analogy which subsists between the solar years and the lunar periods. But if the ☉ and ♃ were in conjunction at the beginning of those periods, the like circumstance would not take place in a series of consecutive returns, owing to the ♃'s perigee and nodes not being in conjunction also. The conjunction would likewise be affected by the ♃'s acceleration. In reference to the period of 39512 solar years (and not 36512 as stated at page 356), containing 488695 lunations, wanting only five seconds of the line of conjunction of the ☉ and ♃; this calculation is founded on the mean motion of the luminaries (see note, p. 14), and is given in order to show the absurdity of instituting such long periods, as the ♃'s accel. amounts to nearly 760 degrees! It is a matter of surprise to me, that Mr. Yeates could view it in any other light, it being actually so expressed. In regard to the period alluded to in Ferguson's Astronomy, as revised by me, viz. of 13700 years, it may be necessary to observe, that the lunar motions are arrived at a degree of perfection of which *Mr. Y. appears to be totally ignorant*. They did not attain their present elevation from *data* and proceedings similar to those of your correspondent, but from an accumulated mass of astronomical observations; viz. of transits, and zenith distances, &c. made during a period of many years, and the subsequent results compared with ancient observations and eclipses: so that, if the falling back of the line of the nodes can be ascertained during the space of about a century only, it is surely no great presumption to calculate their period, as is done in respect to the revolution of the earth's perigee, and the equinoctial points: some have even gone so far as to calculate when the time will arrive when the *ecliptic* will coincide with the *equator*, and a universal spring prevail all over the earth. But this idea is erroneous, as the revolutions of the former appear to be consecutive, while that of the latter is only vibratory. Mr. Yeates admits that it was by the aid of a period of 600 years, that *Hipparchus* extended his science in calculating ephemerides of the sun and moon, as he is related to have done, and that with sufficient exactness for the regulating of their calendar.—*Vide* p. 358. As I before remarked, in the time of Sir *Isaac Newton*, the lunar tables gave the ♃'s long. to within five minutes of a degree from the truth; whereas they now give it to within ten seconds, and generally much nearer. So that, embracing the lunar motions at the present day, and applying them to a period of 13700 years, the ad-

vantage in my favour is as 30 to 23, even admitting that no improvement was made in the lunar theory from the time of *Hipparchus* to that of *Newton*! I therefore contend that an entire period is completed within the limits of 760 Chaldean periods. As I before asserted, the difference produced by the \mathfrak{D} 's accel., even if it was completely neglected as far as the period affects the earth, would only amount to about one hour; and as the \mathfrak{D} 's accel. is proportional to the square of the time, it would in a complete period amount to but little more than four days only.

I do not contend that the knowledge of these long periods is a matter of any great importance; but they evidently convey an idea of the sublimity and grandeur of the celestial motions. Mr. Yeates (at p. 88) has given us a statement of the mean synodic revolutions of the \mathfrak{D} as stated by different astronomers: the mean of the last ten of them, or from the time of *Hipparchus*, amounts to 29 days 12 hours 44 minutes 2 seconds 35 thirds, which differs from the respective results of Messrs. Vince, Gregory, Woodhouse, Squire, Young, Lalande, Laplace, Delambre, Biot, Burg, and Burckhardt, by less than one-fourth of a second only! Mr. Yeates on the opposite page has given four results adduced from his own calculations, the mean of which is 29 days 12 hours 44 minutes 28 seconds 9 thirds; the extremes of the above are those of Ptolemy and Whiston, 45 thirds in excess, and 2 minutes 35 thirds in deficiency respectively. Mr. Y.'s mean exceeds the mean of the ten results above referred to, by 25 seconds 34 thirds; his difference is therefore 34 times greater than that of Ptolemy in excess; and Whiston's difference from the mean result is only about one-tenth part in deficiency of what Mr. Yeates's is in excess! There must surely be some *subtilty*, or something *superlatively grand* and *exquisite*, in the calculations of Mr. Yeates, as he differs 34 times in excess, from that of any of those above mentioned, all of them most celebrated astronomers which have existed within nearly the last two thousand years!! This result is indeed *very curious*. Had Mr. Y. confined himself to giving a list only of the observed eclipses, divested of those which are inserted from calculations, much verbosity would have been spared, and he might have acquitted himself with credit. The inferences to be drawn from what Mr. Yeates has done, will in no wise refute my assertions; and in respect to the period of 912 years, I defy him to establish it, or to controvert the contents of my former letter. At page 355 he says, "I presume, sir, there is no occasion here to introduce anomalistic calculations of the sun and moon: the mass of evidence already produced in the corresponding eclipses at 912 years distance, and the eclipses recorded to have happened, show most

most evidently that the true motions of the sun and moon's apogee and node must agree at and after such an interval, or such phenomena could not take place.

It appears, sir, to this gentleman, of little consequence, whether the D is distant from her node ten degrees on the one side, or ten degrees on the other; or whether the D is within 90 degrees of her perigee, it appears to be a circumstance which he has not given himself the trouble to ascertain. Where eclipses are observed in various parts of the globe, it is necessary to reduce the time to one meridian; and also to apply the equation of time before the interval between the eclipses can be truly ascertained. The difference of longitude of the \odot and D must also be taken into the account; the D must likewise be divested of the equations which regulate the inequalities in her motion, and the correction for the acceleration applied before her mean motion in longitude, or her mean synodic revolution, can be correctly ascertained.

The acceleration of the D is a periodical equation (as indeed are all the equations which regulate her motion); its period is very long, and is equal in length to that of the variation of the eccentricity of the earth's orbit, on which it depends; its period includes millions of years! It will be accelerated and retarded by the same quantity; and therefore if the mean motion be taken for the whole time of the acceleration, or retardation, it will be found never to vary: the mean motions of the perigee and nodes of the lunar orbit are also subject to secular equations, being always proportional to that of the D 's longitude. In consequence of the duration of this period being at present unknown, and also the time when the acceleration will attain its maximum, we are not enabled to apply this equation to its corresponding epoch. This equation, as given by Laplace, in its present form will for ever increase, which cannot be the case. But it may be extended back to the most ancient observations of the D , and probably for many centuries to come, without any sensible error.

That Dr. Maskelyne ever ventured to obtrude such *romantic speculations* on the public, is not disputed; the duties of his office as Astronomer Royal, and his superintendence of the calculations of the Nautical Almanack, were sufficient to occupy his time; and which have immortalised the memory of that illustrious astronomer: but, although he did not launch into such speculations, the elements on which they are founded were strictly applied under his directions by the computers of the above work. To render the theory of the moon perfect, researches as extensive as those which have already been made are required. Observations made at remote periods, in conjunction with theory, are

requisite to elicit the magnitude of all the equations, which regulate the inequalities in the lunar motions.

If the revisers of the lunar theory had not been more successful in their efforts than Mr. Yeates towards *cultivating more perfectly the lunar astronomy*, the science would have been extremely low in the scale of improvement, compared with its present elevation.

I remain, sir, yours truly,

Norfolk-street, Lynn Regis,
Dec. 4, 1820.

JAMES UTTING.

P. S. With the *Editor's* permission, I beg leave to express my sentiments of esteem, being principally indebted to the works of *Dr. Hutton* for that information which I have acquired in the sciences, and by my own application only. I sincerely congratulate this gentleman on the receipt of the very respectful letter from the Marquis De Laplace, confirming the truth and originality of the very laborious and intricate Calculations of the mean Density of the Earth, and confirming beyond all doubt *the universal attraction of matter !!!* J. U.

LXVII. *On the Specific Gravities of the GASES, and the different Musical SOUNDS which they occasion in the same ORGAN-PIPE.*
By Mr. JOHN FAREY Sen.

To Mr. Tilloch.

SIR, — I HAVE on two occasions* endeavoured to call the attention of any Experimentalists, who might have the opportunities which I myself do not possess, and who might be so inclined, to the trying experimentally, of the truth of that theory, which assigns an *Interval between the Sounds produced in a given Pipe, by two different Gases, which is measured by the inverse Subduplicate Ratio of the specific Gravities of these Gases.*

At the periods to which I allude, sufficient precision had not been given to the experimental determinations of the *specific Gravities*, of many of the Gases, but which important *data*, have now lately been supplied, by our eminent and indefatigable chemist, Dr. Thomson, of Glasgow; who has taken the utmost care in procuring 26 different Gases, unmixed, and in a state of purity, and in weighing these Gases, and calculating their specific Gravities, in which latter operation, he has availed himself (in all

* P. M. vol. xxxvii. p. 3, and Edin. Ency. vol. x. p. 120.

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