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A REPLY

TO A

CRITICAL AND MONTHLY REVIEWER,

IN WHICH IS INSERTED

EULER'S DEMONSTRATION

OF THE

BINOMIAL THEOREM.

BY

ABRAM ROBERTSON, D. D. F. R. S.

SAVILIAN PROFESSOR OF GEOMETRY.

Ne damnent quæ non intelligunt. QUINT.

Εἴ τις γε τἀληθῆ κατηγοροῦντι ἑαυτῷ συνεπίϛαται, οὗτος, οἶμαι, ϗ εἰς τὸ φανερὸν διελέγχει, καὶ διευθύνει, καὶ ἀντεξετάζει τῷ λόγῳ· ὥσπερ οὐδεὶς ἂν ἐκ τοῦ προφανοῦς νικᾶν δυνάμενος, ἐνέδρᾳ ποτὲ ϗ ἀπάτῃ χρήσαιτο κατὰ τῶν πολεμίων.

ΛΟΥΚΙΑΝ. Περὶ τοῦ μὴ ῥαδίως πιϛεύειν διαβολῇ.

OXFORD,

At the Univerſity Preſs for the Author:

Sold by J. COOKE, J. PARKER, and R. BLISS, Oxford; Meſſrs. PAYNE and MACKINLAY, and F. WINGRAVE, Strand, London; and J. DEIGHTON, Cambridge.

1808.

A REPLY

TO A

CRITICAL AND MONTHLY REVIEWER,

IN WHICH IS INSERTED

Euler's Demonſtration of the Binomial Theorem.

It may be proper to inform ſome readers, who enter on a peruſal of this Reply, that in the year 1806 a paper was printed in the Philoſophical Tranſactions of the Royal Society of London, entitled, "A new Demonſtration of the "Binomial Theorem;" and that in 1807 a paper was publiſhed in the Tranſactions of the ſame Society, "On "the Preceſſion of the Equinoxes;" both by the author of this Reply.

The appearance of the firſt of theſe papers ſeems to have excited an uncommon degree of chagrin in the minds of two writers[a], one in the Critical, the other in the Monthly Review; and, as is frequently the caſe with angry people, they have vented their diſpleaſure in illiberal expreſſions, mingled with falſehoods, in what each dignifies with the title of a review of the Demonſtration, which they pretend to have examined.

[a] Perhaps this expreſſion may not be ſtrictly accurate, as I am credibly informed, that the perſon who executes the mathematical department in the Monthly Review, very frequently writes in the Critical. The two writers above alluded to may therefore be one and the ſame individual.

Al-

Although the want of veracity, in this implied aſſertion, was very evident to me on the firſt reading of their ſtrictures, I did not think it neceſſary to print any reply to their animadverſions. The ſubject being level to the comprehenſion of every one, who has made any advancement in Algebra, I was perfectly ſatisfied, in my own mind, that mathematicians would ſoon perceive the miſrepreſentations and falſehoods employed by theſe reviewers to injure the character of my paper.

With reſpect to my paper on the Preceſſion of the Equinoxes, the ſame two critics adopt a line of conduct, in one ſenſe, diametrically oppoſite to one another; the one critique being very ſhort, and the other very prolix. The following is all that is ſaid of it in the Critical Review for November 1807. "It is impoſſible to under-"ſtand this paper without the aid of diagrams." In this account there is no incivility, either expreſſed or implied, and therefore it conveys no cauſe of offence; but ſomething more might have been expected from a candid and intelligent critic on a ſubject, which has employed the attention of ſeveral of the moſt able mathematicians in Europe, and in which even the illuſtrious Newton himſelf made a miſtake. It is alſo to be obſerved, that an analyſis of the paper is given in the introduction, without the aid of a diagram. The reviewer's account ſeems ſomething like handing over one's exertions to oblivion; in which, however handſomely the conveyance may be made, there is nothing very flattering toward the author, nor any indication of attention or knowledge in the critic.

The critique on the Preceſſion, by the Monthly reviewer, is, as already intimated, of an oppoſite deſcription. Its violence of hoſtility is carried to the extreme in every ſentence alluding to my paper. Had the cenſor confined his animadverſions within the limits of diſapprobation uſually obſerved in his monthly inſtructions, I ſhould not have taken the trouble to controvert his opinions. A reviewer muſt always aſſume an air of ſuperiority

riority over the author under his confideration, however different the real ftate of their comparative merits may be. This is perfectly underftood by readers of difcernment, and they confequently perufe the decifions of the unknown judge with a due degree of caution. On this account it is feldom or ever in the power of a malignant reviewer to inflict a wound on reputation as fevere as his rancour. Attention and circumftances lay open the defign of the affaffin, and the injurious opinion which he attempts to diffeminate is rejected with fcorn by thofe, who are acquainted with the fubject, and have a due fenfe of honour. From hence it is, that writers on topics level to the comprehenfion of readers in general are foon relieved from the confequences of the unjuft difapprobation of reviewers. The difapprobation, if moderately unfair, makes but a flight impreffion, which is quickly erafed by reflection and candour, and, if it is directed with boldnefs and malignity, it recoils on the publication in which it appeared.

The cafe is different with thofe who publifh on fubjects of abftract fcience. The judges of fuch productions are comparatively very few, and they are very thinly fcattered; and therefore authors of this clafs are much at the mercy of a reviewer, if he is determined to lower or ftifle their reputation. His mifreprefentations are boldly impofed on general readers as fair criticifm, and his falfehoods, cautioufly expreffed, are advanced with an air of integrity, and with a folemn affurance that a confcientious difcharge of his duty to the public is his only motive for his ftrictures. The detraction is read by the many, and fpread far and wide: the power of detecting its want of truth is limited to the few, and cannot be propagated with any degree of precifion, but by writing.

It was not till fome time after its publication, that I read the account of my paper on the Preceffion, given in the Monthly Review for laft September. A perufal of it gave birth to the preceding reflections, and alfo to a determination of replying to the animadverfions of both reviewers; and this deſign has been carried into execution as ſoon as my profeſſional duties would permit. By continuing to ſuffer in ſilence under their aſperſions, I muſt have appeared criminally inattentive to my own character, and unworthy the friendſhip of thoſe, who have repoſed confidence in me for integrity and exertion.

OF THE

DEMONSTRATION OF THE BINOMIAL THEOREM.

AS the accounts of my demonſtration of the Binomial Theorem, given in the Critical and Monthly Reviews, are in ſubſtance nearly the ſame, moſt of the remarks which theſe productions might ſeparately deſerve, will apply with equal propriety to both. The firſt ſenſation which I felt after I had read the pages alluding to it, was that of aſtoniſhment. The form of my expreſſions in the Demonſtration was ſo much changed, their arrangement ſo much altered, their bearing on one another ſo much diſtorted, and my manner of eſtabliſhing the extended form of the theorem ſo completely concealed, that it was only by the titles of the articles in the reviews, and the occaſional mention of my own name, that I found it to be my paper which was ſo unhandſomely criticiſed. Under this deformed and mutilated aſpect the reviewers preſent my demonſtration to their readers, and then proceed to deliver opinions, which I ſhall now take the liberty of examining.

Neither of them gives any account of the foundation on which the demonſtration reſts. On the contrary, it is cautiouſly concealed, and evidently with the deſign of making it appear that the proof is indirect. The conſequence inferred by both is, that it has no claim to originality. I now proceed therefore to ſhew, that the charge thus

thus brought forward by them is not founded in truth, and cannot be ſupported but by their own groſs miſrepreſentations.

My demonſtration may be conſidered as conſiſting of two parts. In the firſt part it is proved directly, and merely by the principles on which multiplication is conducted, that when n is a whole poſitive number, $\overline{x+a}|^n = x^n + nax^{n-1} + n.\frac{n-1}{2}a^2x^{n-2} + \&c.$ and that $\overline{x-a}|^n = x^n - nax^{n-1} + n.\frac{n-1}{2}a^2x^{n-2} - \&c.$ The firſt fourteen articles are employed in proving the truth of theſe equations; and, although it is undoubtedly true, I deemed it needleſs to aſſert, that the law of continuation is made as evidently to appear, as that there may be any number of magnitudes in each of the two ranks mentioned in the twenty ſecond and twenty third Propoſitions of the fifth book of Euclid.

The firſt part being eſtabliſhed, the ſecond commences in the 15th article. In this it is proved, that $\overline{1+x}|^n \times \overline{1+x}|^m = 1 + \overline{n+m}.x + \overline{n+m}.\frac{n+m-1}{2}.x^2 + \&c.$ when n and m are any whole poſitive numbers. The proof of this, as to the law of continuation in the ſeries, reſts upon the firſt part as a foundation; for $\overline{1+x}|^n \times \overline{1+x}|^m = \overline{1+x}|^{n+m}$, by involution, and $n+m$ is a whole number. Thus far therefore the proof is ſtill direct. But $\overline{1+x}|^n = 1 + nx + n.\frac{n-1}{2}x^2 + \&c.$ and $\overline{1+x}|^m = 1 + mx + m.\frac{m-1}{2}x^2 + \&c.$ and therefore, when the firſt of theſe ſeries is multiplied by the ſecond, the product muſt be equal to the ſeries $1 + \overline{n+m}.x + \overline{n+m}.\frac{n+m-1}{2}x^2 + \&c. = \overline{1+x}|^{n+m}$, for equals being multiplied by equals, the products muſt be equal. Now if the one ſeries be actually multiplied by the other, and the products arranged as in my multiplication A, we are

 relieved,

relieved, as I have ſtated in the 16th article, from the neceſſity of conſidering n and m as whole numbers. The ſeries expreſſing the product will ſtill be $1 + \overline{n + m} \, . \, x + \overline{n + m} . \frac{n + m - 1}{2} . \, x^2 + \&c.$ For we know by what has been previouſly and directly proved, that the coefficients of the ſeveral powers of x are reducible to this regular form, and that this muſt be the law of continuation. The proof, therefore, is ſtill direct.

From hence it alſo directly follows, that we can raiſe any binomial ſeries to any propoſed power whatever, as ſtated in the 17th article; and it immediately follows from this, and not from any new mode of proof, that we can expreſs the rth root of $1 + x$ in a binomial ſeries, as in the 18th article. The ſucceeding articles are conſequences of theſe; and a direct proof pervades the whole.

From the above conſiderations it clearly appears to me, that the opinion of the reviewers was not the reſult of an examination of my paper, when they pronounced the demonſtration to be indirect. If they had read it with any degree of attention, they muſt have perceived that the ſecond part depends on the firſt. They might indeed have ſeen this at a glance, for there is a reference to the 13th article in the 15th, and this connects the two parts. It is alſo ſaid in the introductory ſentences, "in order that "the reader may perceive the proof to be complete, a "ſucceſſive perſual of all the articles is neceſſary." I now proceed to apply theſe conſiderations to the remarks of the reviewers.

The Critical reviewer, having given what he calls a deſcription of my method, proceds to aſſert, from what he has exhibited, "that the proof is not a direct one;" and that it "has no claim to originality, for Newton "adopts the ſame method." To the firſt of theſe aſſertions I anſwer, that the method of proof which this critic preſents to his readers is certainly not a direct one, but it is not mine. By the ſecond expreſſion he evidently means to ſay, that Sir Iſaac Newton demonſtrated the theorem;

and

and to this my anſwer is, that the aſſertion is not true. Dr. Horſley thought it neceſſary to ſupply the defect by introducing a demonſtration, in page 286. Vol. I. of his edition: and the following teſtimony is ſo explicit that it cannot be miſtaken, and ſo reſpectable that it muſt be admitted. The quotations are both from Mr. Baron Maſeres's Scriptores Logarithmici.

"How Sir Iſaac diſcovered this theorem, (the binomial,) is not known; nor is any demonſtration of it, even in this eaſieſt caſe, (in which the index *m* of the power to which the binomial quantity is to be raiſed, is a whole number,) any where to be found in all his works." Vol. II. p. 167.

"This is the famous binomial theorem, invented by Sir Iſaac Newton, but of which he has no where given a demonſtration." Vol. III. p. *102.

Theſe quotations are intended for the information of general readers; for it is well known to mathematicians, that what Sir Iſaac ſaid on the ſubject was never meant by him to be conſidered as a demonſtration.

The Critical reviewer alſo ſays, "The ſame kind of proof too, if our memory does not fail us, has been adopted by the Baron Maſeres in ſome of the volumes of the Scriptores Logarithmici." I am in poſſeſſion of all the ſix volumes of that very valuable work, but I can find no ſuch proof in any one of them.

To every judicious reader the Critical reviewer muſt appear guilty of *felo de ſe*, in the flippant paragraph which concludes his critique, as it betrays a total ignorance of the neceſſity of different caſes in the demonſtration, as alſo of the different arguments requiſite to their eſtabliſhment.

The writer in the Monthly Review ſhews more ingenuity in torturing my formulæ, and is apparently more fortunate than his brother critic in diſputing the originality of my demonſtration. He ſtates in uncouth ſymbols, and in combinations which I never uſed, the eſſentials, as he thinks, of my method of proof; and after this ingenious proceeding, he infers a charge of plagiariſm againſt me, from the ſimilarity between ſome of my expreſſions and thoſe employed by Euler. That the reader may have the fulleſt evidence on which this charge is grounded, I, firſt of all, ſubmit to his peruſal a copy of Euler's demonſtration, *verbatim et literatim*, omitting his introduction, conſiſting of three articles, as not being neceſſary to the eſtabliſhment of the theorem, and not being in the leaſt connected with the accuſation advanced by the reviewer. The expreſſions printed in Italics are thoſe which the critic quotes in ſupport of his charge.

"§. 4. Ante omnia autem cum ſit $a + b = a(1 + \frac{b}{a})$ eritque quoque $(a + b)^n = a^n(1 + \frac{b}{a})^n$ ſicque totum negotium reducitur ad evolutionem hujus poteſtatis $(1 + \frac{b}{a})^n$, quæ porro ponendo $\frac{b}{a} = x$ redit ad hanc $(1 + x)^n$, quam novimus, quoties exponens n fuerit numerus integer poſitivus, æqualem fore huic ſeriei $1 + \frac{n}{1}.x + \frac{n}{1}.\frac{n-1}{2}.x^2 + \frac{n}{1}.\frac{n-1}{2}.\frac{n-2}{3}.x^3 + \frac{n}{1}.\frac{n-1}{2}.\frac{n-2}{3}.\frac{n-3}{4}.x^4$ &c. verum ſi n non fuerit numerus integer poſitivus, valorem hujus ſeriei tanquam incognitum ſpectemus, ejuſque loco hoc ſigno $[n]$ utamur, ita ut jam in genere habeamus

$$[n] = 1 + \frac{n}{1}.x + \frac{n}{1}.\frac{n-2}{2}x^2 \text{ \&c.}$$

de qua etiam nunc plus non novimus, quam caſu, quo n eſt numerus integer poſitivus, fore $[n] = (1 + x)^n$ reliquis autem caſibus quinam valores huic ſigno $[n]$ conveniant, ſequenti modo inveſtigemus: Unde demum patebit, etiam in genere fore $[n] = (1 + x)^n$ quicunque numeri pro exponente n accipiantur, quo pacto propoſito noſtro plene ſatisfecerimus.

§. 5. Ad hanc inveſtigationem inſtituendm duas hujuſmodi ſeries ſeu duo talia ſigna $[n]$ et $[m]$ in ſe invicem mul-

multiplicemus ut feriem obtineamus huic producto $[m]\,.\,[n]$ æqualem, quam facile patet hujufmodi forma expreffum iri:

$$1 + Ax + Bx^2 + Cx^3 + Dx^4 + Ex^5 \text{ etc.}$$

cujus coefficientes A, B, C, D, E etc. quemadmodum per binas literas m et n determinentur ut pateat; ipfam multiplicationem faltem inchoemus.

$$[m] = 1 + \frac{m}{1}.\,x + \frac{m}{1}.\,\frac{m-1}{2}.\,x^2 + \text{etc.}$$

$$[n] = 1 + \frac{n}{1}.\,x + \frac{n}{1}.\,\frac{n-1}{2}.\,x^2 + \text{etc.}$$

$$[m]\,.\,[n] = 1 + \frac{m}{1}.\,x + \frac{m}{1}.\,\frac{m-1}{2}.\,xx \text{ etc.}$$

$$+ \frac{n}{1}.\,x + \frac{m}{1}.\,\frac{n}{1}\,xx \text{ \&c.}$$

$$+ \frac{n}{1}.\,\frac{n-1}{2}.\,xx \text{ \&c.}$$

Quod fi jam hoc productum inchoatum cum forma affumta $1 + Ax + Bx^2 + Cx^3$ etc. qua idem productum exprimi ponimus, comparemus, ftatim intelligitur, fore $A = m + n$ et $B = \frac{m}{1}.\,\frac{m-1}{2} + \frac{m}{1}.\,\frac{n}{1} + \frac{n}{1}.\,\frac{n-1}{2}$ five $B = \frac{mm-m}{2} + mn + \frac{nn-n}{2}$ unde fit $B = \frac{m+n}{1}.\,\frac{m+n-1}{2}$.

§. 6. Quemadmodum hic duos primos coefficientes A et B, per literas m et n determinare licuit, ita manifeftum eft, fi fuperior multiplicatio ulterius continuaretur, inde etiam fequentes coefficientes C, D, E etc. per eafdem literas m et n definiri poffe, quamvis calculus mox ita fieret moleftus, ut maximum laborem requireret. Interim tamen hinc tuto concludere poffumus omnes plane coefficientes A, B, C, D, E etc; certo modo per binas literas m et n determinari debere; etiamfi ipfam rationem, qua quifque per has literas definitur, adhuc ignoremus, *hic autem imprimis obfervari convenit, hanc compofitionis rationem non ab indole literarum* m *et* n *pendere, fed perinde fe effe habituram, five hæ literæ* m *et* n *denotent numeros integros*

gros ſive alios numeros quoſcunque. Hoc ratiocinium non vulgare probe notetur, quoniam ei tota vis noſtræ demonſtrationis initiitur.

§. 7. Hinc facilis nobis via aperitur, veros valores omnium coefficientium A, B, C, D, E etc. inveniendi, dum ſcilicet literas m et n tanquam numeros integros ſpectamus, quandoquidem hinc eædem demonſtrationes oriuntur ac ſi quoſcunque alios numeros denotarent. Spectatis autem literis m et n ut numeris integris utique habebimus $[m] = (1 + x)^m$ et $[n] = (1 + x)^n$ unde harum formularum productum erit $[m] \cdot [n] = (1 + x)^{m+n}$ jam vero hæc poteſtas evolvitur in hanc ſeriem : $1 + \frac{m+n}{1} \cdot x + \frac{m+n}{1} \cdot \frac{m+n-1}{2} \cdot x^2 + \frac{m+n}{1} \cdot \frac{m+n-1}{2} \cdot \frac{m+n-2}{3} \cdot x^3$ nunc igitur ſi literas m et n in genere ſpectemus hanc ſeriem iſto ſigno $[m + n]$ indicari oportet, unde hanc inſignem veritatem nanciſcimur, ſemper eſſe $[m] \cdot [n] = [m + n]$ quicunque etiam numeri loco iſtarum literarum adhibeantur.

§. 8. Cum igitur binæ hujuſmodi formulæ $[m]$ et $[n]$ in ſe invicem ductæ præbeant ſimplicem formulam ejuſdem indolis, ita etiam plures ejuſmodi formulæ in ſe invicem ductæ ad ſimplicem revocabuntur, habebimus ſcilicet ſequentes reductiones

$$[m] \cdot [n] = [m + n]$$
$$[m] \cdot [n] \cdot [p] = [m + n + p]$$
$$[m] \cdot [n] \cdot [p] \cdot [q] = [m + n + p + q] \text{ etc.}$$

hinc ſi omnes iſti numeri m, n, p, q etc. inter ſe capiantur æquales ſcilicet $= m$ obtinebimus ſequentes reductiones poteſtatum

$$[m]^2 = [2m] ; [m]^3 = [3m] ; [m]^4 = [4m] ; \text{ etc.}$$

unde generaliter erit $[m]^a = [am]$; denotante a numerum quemcunque integrum.

§. 9. His prænotatis denotet litera i numerum quemcunque integrum poſitivum ac ſtatuamus primo $2m = i$ ut ſit $m = \frac{i}{2}$ ac poſtremarum formularum prima dabit

[i

$[\frac{i}{2}]^2 = [i]$ quia autem i eſt numerus integer, erit $[i] = (1 + x)^i$ (vide §. 4.) ſicque erit $[\frac{i}{2}]^2 = (1 + x)^i$ unde radicem quadratam extrahendo fit $[\frac{i}{2}] = (1 + x)^{\frac{i}{2}}$ ſicque jam tantum ſumus conſecuti, ut theorema Neutonianum etiam verum ſit caſibus, quibus exponens n eſt hujuſmodi fractio $\frac{i}{2}$.

§. 10. Simili modo ſi ponamus $3m = i$ ut ſit $m = \frac{i}{3}$, altera formularum ſuperiorum præbet $[\frac{i}{3}]^3 = [i] = (1 + x)^i$ hinc radicem extrahendo nanciſcimur $[\frac{i}{3}] = (1 + x)^{\frac{i}{3}}$ ſicque theorema noſtrum etiam verum eſt ſi exponens n fuerit hujuſmodi fractio $\frac{i}{3}$, atque hinc in genere manifeſtum fore $[\frac{i}{a}] = (1 + x)^{\frac{i}{a}}$ ita ut jam demonſtratum ſit, theorema noſtrum verum eſſe, ſi pro exponente n fractio quæcunque $\frac{i}{a}$ accipiatur, unde veritas jam eſt evicta pro omnibus numeris poſitivis loco exponentis n accipiendis.

§. 11. Supereſt igitur tantum, ut veritas quoque oſtendatur pro caſibus, quibus exponens n eſt numerus negativus. Hunc in finem in ſubſidium vocemus reductionem primo inventam $[m] . [n] = [m + n]$ ubi denotet m, numerum poſitivum ſive integrum ſive fractum ita ut ſit uti modo oſtendimus $[m] = (1 + x)^m$, deinde vero ſtatuatur $n = - m$ eritque $m + n = 0$ ideoque $[0] = (1 + x)^0 = 1$, quibus ſubſtitutis formula ſuperior ſuppeditat $(1 + x)^m . [- m] = 1$ unde colligimus $[- m] = \frac{1}{(1 + x)^m} = (1 + x)^{-m}$ ſicque etiam demonſtratum eſt theorema Neutonianum verum quoque eſſe, ſi exponens n fuerit nume-

rus

rus negativus quicunque atque adeo hoc theorema nunc quidem firmissimis rationibus est confirmatum."

The greatest part of the 15th, and the whole of the 16th articles of my demonstration, are now submitted to the reader's perusal, and the expression is printed in Italics, which the reviewer quotes in support of his charge.

"It is evident that if the series equal to $\overline{1+x}|^n$ be multiplied by the series equal to $\overline{1+x}|^m$, the product must be equal to the series which is equal to $\overline{1+x}|^{n+m}$. Now the two first mentioned series being multiplied into one another, and the parts being arranged according to the powers of x, the several products will stand as in the following representation.

$$\overline{1+x}|^n = 1 + nx + n.\frac{n-1}{2}x^2 + n.\frac{n-1}{2}.\frac{n-2}{3}x^3 \text{ \&c.}$$

$$\overline{1+x}|^m = 1 + mx + m.\frac{m-1}{2}x^2 + m.\frac{m-1}{2}.\frac{m-2}{3}x^3 \text{ \&c.}$$

$$\begin{array}{rrrr} 1 + nx + & n.\frac{n-1}{2}x^2 + & n.\frac{n-1}{2}.\frac{n-2}{3}x^3 & \text{\&c.} \\ mx + & m.nx^2 + & m.n.\frac{n-1}{2}x^3 & \text{\&c.} \\ & m.\frac{m-1}{2}x^2 + & m.\frac{m-1}{2}.nx^3 & \text{\&c.} \\ & & m.\frac{m-1}{2}.\frac{m-2}{3}x^3 & \text{\&c.} \end{array}$$

For the sake of reference hereafter let this be called multiplication A.

Now with respect to the coefficients prefixed to the several powers of x, in the foregoing multiplication, two observations are to be made, by means of which the demonstration of the theorem may be extended to fractional exponents.

In the first place, supposing n and m to be whole numbers, the sum of the coefficients prefixed to any individual power of x, in multiplication A, must be equal to the coefficient prefixed to the same power of x in the binomial

nomial series $1 + \overline{n+m}x + \overline{n+m} \,.\, \frac{n+m-1}{2} x^2 + \overline{n+m} \,.\, \frac{n+m-1}{2} \,.\, \frac{n+m-2}{3} x^3 + \overline{n+m} \,.\, \frac{n+m-1}{2} \,.\, \frac{n+m-2}{3} \,.\, \frac{n+m-3}{4} x^4 +$ &c. The certainty of this circumstance rests partly on the 13th arrticle, and partly on a plain axiom, *viz.* that equals being multiplied by equals the products are equal.

In the second place it is to be observed, that the whole coefficient of any power of x, in the products of multiplication A, may be reduced to the regular binomial form, established in the 13th article. Thus $n \,.\, \frac{n-1}{2} + mn + m \,.\, \frac{m-1}{2}$, the whole coefficient of x^2, by actual multiplication becomes $\frac{n^2 + m^2 + 2mn - n - m}{2} = \overline{n+m} \,.\, \frac{n+m-1}{2}$. Also $n \,.\, \frac{n-1}{2} \,.\, \frac{n-2}{3} + mn \,.\, \frac{n-1}{2} + m \,.\, \frac{m-1}{2} \,.\, n + m \,.\, \frac{m-1}{2} \,.\, \frac{m-2}{3}$, the whole coefficient of x^3, by actual multiplication becomes $\frac{n^3 + m^3 - 3n^2 - 3m^2 + 3n^2 m}{6} + \frac{3m^2 n - 6mn + 2n + 2m}{6} = \overline{n+m} \,.\, \frac{n+m-1}{2} \,.\, \frac{n+m-2}{3}$. And from the preceding observation it is evident, that we may in the same manner reduce the whole coefficient of any other power of x, in the products of multiplication A to the regular binomial form.

16. But in proceeding, as above, to change the form of the coefficients prefixed to any power of x, in multiplication A, into the regular binomial form, we are not under the necessity of supposing n and m to be whole numbers. *The actual multiplications will end in the same powers of* n *and* m, *the same combinations of them, and the same numerals, whether we consider* n *and* m *as whole numbers or as fractions.*

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We are therefore at liberty to ſuppoſe n and m to be any two fractions whatever, in the two ſeries multiplied into one another in multiplication A, and the ſame two fractions will take the place of n and m reſpectively in the regular binomial ſeries $1 + \overline{n+m}\,x + \overline{n+m}\,.\,\frac{n+m-1}{2}\,.\,x^2 + \overline{n+m}\,.\,\frac{n+m-1}{2}\,.\,\frac{n+m-2}{3}\,x^3 + \overline{n+m}\,.\,\frac{n+m-1}{2}\,.\,\frac{n+m-2}{3}\,.\,\frac{n+m-3}{4}\,x^4 +$ &c. which expreſſes the product of the two ſeries into one another."

In writing his critique the reviewer ſeems to have had three objects in view. The firſt was, to make his account of my demonſtration cover as much paper as poſſible; the ſecond was, to give of my memoir as intricate a repreſentation as poſſible; and the third was, to lower its reputation as much as poſſible. After quoting the paſſages printed in Italics, but at ſome diſtance from one another, and involved in his own confuſion, he ſays, "We think that theſe extracts prove, beyond the poſſi- "bility of a doubt, that the principle of Mr. Robertſon's "demonſtration is preciſely the ſame with that which "was adopted by Euler thirty years ago. It is not our "wiſh, on the ſameneſs of the principle, and the ſimpli- "city of the two demonſtrations, to found a charge of "plagiariſm againſt Mr. R.: but he muſt abandon all "claim to originality or priority of invention; which "muſt be awarded to Euler." Notwithſtanding the above diſavowal of any wiſh to "found a charge of pla- "giariſm againſt" me, the reviewer, ſeven months after the appearance of the critique now under conſideration, when ſpeaking of my paper on the Preceſſion of the Equinoxes, ſays of me; "In a former Review reſpecting "his demonſtration of the Binomial Theorem, we no- "ticed the circumſtance of the coincidence, that the "main principle of that demonſtration was the ſame "with that which Euler advanced and uſed fifty years "ago."

" ago." As the reviewer's dates are ſomething like Falſtaff's men in Buckram, it is proper to inform the reader, that Euler's demonſtration is contained in Novi Commentarii Academiæ Scientiarum Imperialis Petropolitanæ Tom. XIX. pro anno 1774.

After aſſuring the reader, that I did not ſee Euler's demonſtration till my own was printed, I proceed to conſider the two circumſtances on which the preceding inſinuation of plagiariſm is founded.

The firſt of theſe circumſtances is the multiplication of one binomial ſeries by another, as in my multiplication A; the ſecond is the great ſimilarity between Euler's and my expreſſions, printed in Italics, concerning the ſameneſs of the products, whether n and m be whole numbers or fractions. Now if the multiplication of one ſeries by another is to be conſidered as a proof of plagiariſm, Euler and others muſt be adjudged guilty, as authors before him ſet the example of ſuch proceſſes, in which they have been followed to the preſent times, by almoſt every writer on Algebra. The accuſation of plagiariſm, founded on the ſecond circumſtance, is frivolous in the higheſt degree. Any ſchool-boy, who underſtands a little algebra, is able to reduce, for inſtance, $n \cdot \frac{n-1}{2} + mn + m \cdot \frac{m-1}{2}$ to $\overline{n+m} \cdot \frac{n+m-1}{2}$; and he knows that the reduction " will end in the ſame powers of n and m, " the ſame combinations of them, and the ſame nume- " rals, whether we conſider n and m as whole numbers " or as fractions." It is a truth of which every one is convinced, who underſtands algebraical multiplication; and the enunciation of this truth admits of ſo little variation, that no difference could reaſonably be expected in Euler's and my appeal to the principle. It is moreover ſuch an anſwer as every mathematical teacher has given, and ſtill muſt give, to any inquiſitive pupil on the ſubject.

The reader may judge how eager the reviewer was to fix a charge of plagiariſm upon me, by his founding one upon the two circumſtances already ſtated, and by his inability to recur to any coincidence in the uſe made of them, to prove or ſtrengthen the accuſation. If my combinations of the principles had been ſuch as Euler employed in his memoir, the critic would have had good ground for his inſinuation of plagiariſm; and he would have been fully juſtified in awarding the priority of invention to Euler. But, this not being the caſe, his inſinuation and award juſtify me in aſſerting, that he has ſhewn at once his wiſh and his inability to injure the character of my paper. On the whole, I conſider his review of my demonſtration as a bungling and groſs miſrepreſentation from the beginning to the end; and his charge of plagiariſm as more becoming a boy, with a ſmattering of Algebra, than an intelligent and candid critic.

In Euler's memoir on the ſubject in queſtion, it is taken for granted that the Binomial Theorem has been previouſly demonſtrated, as far as it relates to the raiſing of integral powers. There is, therefore, nothing in it analogous to the firſt part of my paper; which, as already ſtated, I deem abſolutely neceſſary as a foundation for the ſecond part. Beyond the circumſtances already mentioned, Euler's memoir is ſo ſhort, that it is to be conſidered rather as the outlines of a proof, than as any thing like a complete demonſtration.

OF

ON THE

PRECESSION OF THE EQUINOXES.

AS the account, in the Monthly Review, of my paper on the Preceffion of the Equinoxes may have been perufed by fome who have not feen the paper itfelf, or who have feen and not examined it, I think it proper, firft of all, to fubmit the whole of the introductory part of the paper to their confideration.

"Perhaps the folution of no other problem, in natural philofophy, has fo often baffled the attempts of mathematicians, as that of determining the preceffion of the equinoxes, by the theory of gravity. The phenomenon itfelf was obferved about one hundred and fifty years before the Chriftian æra, but Sir Ifaac Newton was the firft who endeavoured to eftimate its magnitude by the true principles of motion, combined with the attractive influence of the fun and moon on the fpheroidal figure of the earth. It has always been allowed, by thofe competent to judge, that his inveftigations relating to the fubject evince the fame tranfcendent abilities as are difplayed in the other parts of his immortal work, THE MATHEMATICAL PRINCIPLES OF NATURAL PHILOSOPHY: but for more than half a century paft it has been juftly afferted, that he made a miftake in his procefs, which rendered his conclufions erroneous.

"Since the detection of this error, fome of the moft eminent mathematicians in Europe have attempted folutions of the problem. Their fuccefs has been various; but their inveftigations may be arranged under three general heads. Under the firft of thefe may be placed fuch as lead to a wrong conclufion, in confequence of a miftake committed in fome part of the proceedings. The

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ſecond head may be allotted to thoſe, in which the concluſions may be admitted as juſt, but rendered ſo by the counteraction of oppoſite errors. Such may be ranked under the third head, as are conducted without error fatal to the concluſion, and in which the reſult is as near the truth as the ſubject ſeems to admit.

"The authors of thoſe inveſtigations of each of the three deſcriptions are entitled to much praiſe. Their productions afford the moſt unqueſtionable proofs of great talents, great zeal, and great perſeverance, exerted in the cultivation of ſcience. The miſtakes committed in thoſe of the two firſt deſcriptions, and the obſcurity and perplexity with which thoſe of the third may be charged, are, in my opinion, to be attributed to the ſame cauſe, the uncultivated ſtate of that particular department of the doctrine of motion, which conſtitutes the appropriate foundation for the ſolution of the problem. The department to which I allude is that of compound rotatory motion.

"In conſequence of this perſuaſion, I have, in the firſt nine of the following articles, endeavoured to inveſtigate the primary properties of compound rotatory motion, from clear and unexceptionable principles. The diſturbing ſolar force on the ſpheroidal figure of the earth is then calculated, and the angular velocity which it produces is afterwards compared with that of the diurnal revolution, by means of the properties of rotatory motion previouſly demonſtrated. The quantity of annual preceſſion is then calculated in the uſual way, and alſo that of nutation, as far as they are produced by the diſturbing force of the ſun."

In drawing up this introduction, contained in the foregoing quotation, I intentionally omitted the names of thoſe who had written on the ſubject. As different opinions have been entertained concerning the merits of their ſolutions, I was deſirous of avoiding any appearance of partiality; and I was the more impreſſed with the

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propriety of ſuch ſilence, as miſtakes have been made in the ſubject by ſome authors, of whom, notwithſtanding, I can never ſpeak but with the higheſt reſpect. The truth is, that the ſolution of the problem requires ſuch a nice adjuſtment of the parts eſſential to its completion, that even very able mathematicians, till within theſe laſt ſeven years, found a great difficulty in ſeparating what had been accurately written on the ſubject, from that which is objectionable. Even the late Dr. Horſley, whoſe very great ſtrength of mind and keenneſs of penetration were never queſtioned, after commenting on the ſteps by which the illuſtrious Newton attempted the ſolution, concludes with the following paſſage, alluding to one part of his proceſs: "Si hoc vere dictum ſit, neſcio "qui fieri poſſit, ut alius ſit punctorum æquinoctialium "motus, a vi ſolis oriundus, quam calculi Newtoniani "ſuadent. Quem tamen longe alium invenerunt viri per-"magni Eulerus et Simpſonus noſtras; quos velim Lec-"tor conſulas. Ipſe nil definio."—My critic, in the Monthly Review, was not troubled with any ſuch diffidence; for, as will appear hereafter, not being acquainted with the ſubject, he was not aware of any cauſe of diſmay.

It is ſurely proper for a critic to examine the ſeveral parts of a production, in the ſame order as they are delivered by the author; and it is a duty incumbent upon him thus to proceed, when the order of the parts is diſtinctly ſtated in the proceſs which he is conſidering, and mention is made of their bearing on one another, ſo as to produce the intended effect. It may therefore be fairly ſuppoſed, if the reviewer had been candid, he would have examined, firſt of all, my articles on compound rotatory motion; then thoſe in which the ſun's diſturbing force is calculated; and afterwards, the combination of theſe two parts, according to what is ſtated in the laſt paragraph of my introduction. By ſuch a proceeding he would ſo far have been able to preſerve the appearance of candour, and it would alfo have enabled him to prefent to his readers a diftinct character of my paper.

The tafk which he had undertaken required fuch a character; but judging from his effort, he has, either from prejudice or inability, completely failed in drawing it. He has given a lift of writers on the fubject, but he has not enlightened his readers with any detail of their refpective merits. He has prefented us with feveral formulæ, but he has forgotten to mention the fteps by which they were eftablifhed; and he has even ventured to point out a plan, as he thinks, for the folution of the problem under confideration, but he has no where even mentioned the only very difficult part, which is abfolutely requifite for its completion. He has, moreover, in his review of my paper, paffed over in filence what for feveral years has been confidered as the only very difficult part in the problem. From this he has diverted the attention of his readers to matter which has been common to authors fince the time of Sir Ifaac Newton, and ever muft be common; and in this part, becaufe I adopted the figures and manner of proceeding of a particular author, he has endeavoured to fix upon me a charge of grofs plagiarifm. This he has done, in one part, without any allufion to my general acknowledgement in the introduction, in which it is faid, that "the quantity of an-"nual preceffion is calculated in the *ufual* way, and alfo "that of nutation, as far as they are produced by the "difturbing force of the fun."

As the reviewer, by his arrangement, has contrived to give a reprefentation of my paper, as confufed and perplexed as perhaps can well be produced, I take the liberty, in examining his critical pretenfions and the extent of his veracity, of proceeding according to the order pointed out in my introduction. His remarks on my articles concerning compound rotatory motion are therefore firft of all to be confidered.

In page 12 of the Review the critic fays, "In the "con-

" construction of his articles, we imagine that Mr. Ro-
" bertson wished to demonstrate every thing *more Geome-*
" *trico*, since otherwise that which is diffused over five
" pages, might have been comprised in two. We are
" not averse to the consumption of ink and paper, when
" perspicuity and distinctness are to be the result: but, as
" Mr. Robertson was evidently writing to mathemati-
" cians of tolerable growth and manhood, who must, for
" the comprehension of the latter part of his paper, be
" well acquainted with the ordinary fluxionary processes,
" to such undoubtedly he would have been more intelli-
" gible if he had been more succinct. Why, in treating
" of angular velocities, were not the fluxionary symbols
" used, since they are not in a subsequent part avoided?
" Is it necessary to re-state, in a formal article, that an
" angle varies as the arc directly and radius inversely? If
" ϑ be an angle, z its corresponding arc, and r, c the ra-
" dius and cosine, then $\dot{\vartheta} \propto \frac{\dot{z}}{r} \propto \frac{\dot{c}}{\sqrt{(r^2-c^2)}} \propto \dot{c}$, if
" $\sqrt{r^2-c^2}$ the sine be given. Would not this have suited
" the acquirements and comprehension of mathematicians,
" as well as the three geometrical articles of Mr. Robert-
" son? for we must repeat, that the convenience of those
" who must necessarily be his readers ought to be con-
" sulted by an author. Mr. R. was not writing to the
" students of Oxford, who have scarcely wetted their feet
" in the waters of the elements; his readers are the
" members of the learned society in whose Transactions
" his memoir is inserted."

The full flow of pertness, which runs through the preceding quotation, can only be perceived by a mathematician; but any one of common sense may judge of the merits of the canon of criticism which it contains. The plain interpretation of it is this: even when the subject under consideration is a geometrical figure, it is perfectly needless to use a diagram; it is only necessary to express the result of the consideration in algebraical symbols. His reasons for this rule appear to be two; and the first

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is, that it will ſave the conſumption of ink and paper. The ſecond is, that it will be ſuitable to the acquirements and comprehenſion of mathematicians.

It muſt be admitted, that the firſt of theſe reaſons is good, as omitting an inveſtigation, and putting down the reſult only, will aſſuredly ſave ink and paper. Thus far I yield to the profound ſagacity of the critic, and acknowledge my failing in economy. His ſecond reaſon does not reſt upon ſo firm a foundation. He himſelf may be one who can graſp a ſyſtem by intuition, or who from the ſlighteſt hint relating to the ſmalleſt part can immediately ſpring at and ſeize the whole; but in condeſcenſion to the generality of readers he ſhould conſider, that ſuch mental agility and gigantic powers are very ſeldom to be found.

Every attentive reader of the paper will perceive my reaſons for proceeding geometrically. The three firſt articles, of which the reviewer complains, may be peruſed in five minutes, and the manner in which they and the fourth article are demonſtrated, relieves the doctrine of compound rotatory motion from much perplexity. Thoſe who have treated the ſubject moſt fully, as far as I know, are Landen, and the late Profeſſor Robiſon of Edinburgh; the firſt in his 11th Memoir, and the latter in the 3d edition of the Encyclopædia Britannica, article 117, &c. under the word *Rotation*. I have all the reſpect for the memory of theſe authors, which a very high opinion of their abilities and works can produce; but I am ſo far from fearing a compariſon of my articles with theirs, that I entreat the reader of this to enter upon it. If he does, he will be able to judge for himſelf of what I have written on compound rotatory motion, and even thoſe of very moderate mathematical attainments may make a general compariſon in half an hour.

In anſwer to the reviewer's queſtion, whether his ſymbolic expreſſion would not have ſuited the acquirements and comprehenſion of mathematicians, as well as the three firſt geometrical articles in my paper, I reply without

out hefitation in the negative. By doing as he propofes he undoubtedly means to fay, as he expreffes himfelf in another part of his review, that he would illuminate mathematical proceffes, and enlighten his readers. I am of a different opinion : by fo doing I think he would keep them in the dark. When he wrote the canon, he muft have forgotten thefe words of Horace,

> " Brevis effe laboro,
> Obfcurus fio."

It is deemed needlefs to detain the reader with any remarks on the reviewer's obfervations, beginning with the laft paragraph in page 12, and ending with line 7 from the bottom in page 13, as they do not in the leaft affect the character of my memoir. By introducing them he certainly wafted ink and paper; but if a fenfe of this want of economy give him a moment's vexation, in the next he may be cheered by the recollection, that his recompence is proportional to the extent and not to the quality of his writing, and that in collecting the paffages alluded to from different authors, he did not fubject himfelf to any mental exertion.

I now proceed to confider thofe articles in my paper, in which the fun's difturbing force is calculated.

In the 66th Prop. and its Cors. Lib. I. Princip. Sir Ifaac Newton pointed out methods of afcertaining the fun's difturbing force, and his firft commentators fo fully explained them, that the proceedings of fubfequent writers can only be confidered as fo many modifications of the fame inveftigations. From a knowledge of this circumftance I was perfectly aware, that, as to matter, there was neither room nor occafion for originality in this part of my folution. It therefore remained for me to determine, whether it would be moft advifable to proceed by calculating the fun's difturbing force on the whole mafs of the earth, confidered as a fpheroid, or by calculating the fame force on the matter of the earth, without the greateft infcribed fphere. Sir Ifaac Newton fet the example of eſtimating the ſun's diſturbing force by the latter method. Some of the moſt able mathematicians, who have given or attempted ſolutions of the problem, adopted his figure, and, ſo far in ſubſtance, followed his proceſs; but I have never heard of their being accuſed of plagiariſm. The reviewers of their productions, perhaps, underſtood where the difficulty in the ſubject lay, and, from their ſenſe of common honeſty, they adhered to truth, or did not venture to expoſe their ignorance of the ſubject.

As almoſt all the doubts and miſtakes in the ſubject originated in the ſecond of the above-mentioned two methods of eſtimating the diſturbing force, I preferred the firſt, viz. that uſed by Simpſon; and becauſe I did ſo, and ſo far in ſubſtance uſed Simpſon's method, the reviewer accuſes me of plagiariſm. Thus, ſpeaking of my paper, he ſays, "Concerning the articles after the 9th, very little remains to "be ſaid: but we think that we are juſtified in ſtating, that "they are borrowed from Thomas Simpſon. Thus, art. "11. is the ſame as problem 1. of Miſcellaneous Tracts, "p. 11, differing only in $\frac{M}{SC^2}$, being put F the ſun's ab-"ſolute force on a particle at C. Articles 13, 14, are the "ſame as the 3d lemma of Simpſon, page 7. Article "15th is the ſame as corollary 1. of the 3d lemma. Ar-"ticle 16. is the ſame as corollary 2. of the 3d lemma; "and article 18. is the ſame as lemma 1. page 2."

I conſider this preciſion of reference as fair in the critic, and, according to the concluding ſentence in his review, I avail myſelf of the very firſt opportunity to preſent to the reader a ſpecimen of his *juſtice*. He ſays my 11th article is the ſame as prob. 1. in Simpſon. This 11th article is my firſt relating to the ſun's diſturbing force, and is to be conſidered as the foundation of the ſubſequent articles on the ſame ſubject. As to both matter and method of proceeding, it is much the ſame with the moſt early comments on the 6th cor. to Prop. 66. Lib. I. Principia, and alſo much the ſame with what is to be found in the following authors:

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Silvabelle, Prob. I. Philoſophical Tranſactions for 1755.
Walmeſley, Lemma 1. Philoſophical Tranſactions for 1756.
Friſi de Gravitate, page 120. &c.
Friſi, Coſmographiæ, page 102. &c.
La Lande, Aſtronomy, article 3695. &c.
Mr. Vince, 4to. Aſtronomy, article 1015. &c.

With what appearance of juſtice, then, can the reviewer aſſert, that my 11th article is borrowed from Simpſon, when not only in ſubſtance, but alſo in manner, it is to be found in every writer on phyſical aſtronomy? I did not borrow it from Simpſon: as others before me, I obtained my information on the ſubject from that copious ſource, the Principia, and I drew up the article in ſuch a way as was likely to be moſt ſubſervient to the object under conſideration. It is to be obſerved beſides, that my 11th article is in length more than double Simpſon's Prob. I. nor is there a ſingle line in mine which I would conſent to ſtrike out. Surely the critic's ideas of borrowing and ſameneſs are very ſingular, and, on the preſent occaſion, conſiderably twiſted from the meaning affixed to the words by men of common ſenſe and fair motives.

With reſpect to the other articles mentioned in the laſt quotation, the aſſertion of the reviewer is equally diſtant from truth. No one of them was borrowed from Simpſon. The affinity is ſuch as I have already ſtated, and is ſuch as muſt occur in the ſolution of every problem of much extent and intricacy, which has been frequently conſidered. A candid critic would have noticed this, but at the ſame time perhaps he would have remarked, that the matter common to writers on the ſubject was put together with great care, ſo as to form an eaſy union with the other parts, and that throughout there were no marks of borrowing, ſameneſs, or ſervile imitation.

The reviewer's expreſſions, immediately after the laſt quotation, oblige me to notice ſome of his previous animadverſions,

madverſions. When I drew up my paper, I was perfectly aware of the following truth, which the critic begins to ſtate near the bottom of page 6. " In a ſeparate " treatiſe, D'Alembert ſolved the preceſſion of the equi- " noxes : but it was in the 5th volume of his Opuſcules, " publiſhed in 1768, that this great mathematician made " ſome ſtrictures on the ſolution of Thomas Simpſon, " and pointed out errors, (in our opinion, real errors,) " which have never been amended, although they re- " quire correction, ſince the reſult of his calculations " agrees with reſults obtained on certain grounds, and " by more perſpicuous proceſſes."

If by this laſt remark the reviewer means to ſay, that Simpſon's errors have never been diſtinctly mentioned, and the neceſſary corrections pointed out, he is very imperfectly informed on the ſubject, as will ſoon appear to every intelligent reader of this Reply.—The extent of four pages in the Review, after the laſt quoted paſſage, conſiſts, in my opinion, partly of nonſenſe, and partly of vain oſtentatious obſervations, entirely foreign to the character of my paper.

In the ſeventh volume of the Tranſactions of the Royal Iriſh Academy, there is a memoir on the Preceſſion of the Equinoxes, by the Rev. Matthew Young, D. D. S. F. T. C. D and M. R. I. A. Of this memoir the following account was given in the Monthly Review for September 1801.

" It is known to mathematicians, that Newton, calculating the effects of the ſun's force in producing the preceſſion of the equinoxes, fell into an error, and made it leſs by one half than the truth. Since his time, the problem has been rigorouſly ſolved, firſt by D'Alembert, to whom the theory of gravitation is indebted for one of its ſtrongeſt confirmations. That great mathematician determined analytically the motions of the earth's axis, on the hypotheſis that the ſtrata of the terreſtrial ſpheroid had any form and denſity whatever ; and he not only found his reſults agreeable to obſervation, but he aſcertained

tained the true dimenfions of the fmall ellipfe defcribed by the pole of the earth.

"The principal object of the prefent memoir is to difcover diftinctly in what the fallacy of Newton's reafoning confifts; and to this end, the learned author examines the three lemmas which Newton prefixes to his 39th Propofition, and then the feveral corrections propofed to be made by Simpfon, Frifi, Milner, and Landen, in order to obtain an accurate refult. This examination is performed with much perfpicuity; and Dr. Young properly acquiefces in the correction made by Landen. This acute and excellent mathematician, in the thirteenth of his Mathematical Memoirs, Vol. II. p. 50. has fhewn, that the principal miftake in Newton's procefs arofe from not confidering the centrifugal force of the particles of the revolving ring of moons, acting in oppofition to the folar force, while fuch a ring has a tendency to revolve about a momentary axis, in confequence of the compound motion which it muft neceffarily have on becoming rigid, agreeably to the fuppofition."

Dr. Young's memoir deferved the character of perfpicuity, and much more extended praife than what is given in the Monthly Review. I perufed it with great attention, before I drew up my own, and from the inveftigations which it contains, and previous reading in Phyfical Aftronomy, I faw clearly where Simpfon's miftakes lay. I did not permit his great and juftly acquired fame to lead me into a hafty decifion. The miftakes alluded to are ftated by Dr. Young in the following paffage.

"Mr. Simpfon has pointed out the miftakes in the folutions of this problem, propofed by M. Silvabelle and Walmefley; but neither is his own calculation entirely faultlefs; and his conclufion appears to be correct, only becaufe the errors in the premifes compenfate each other. Thus he fuppofes, that the whole motive force, acting on a detached rigid ring, revolving with a two-fold motion, one round its centre, the other round a diameter, is equal to the efficient force by which the plane of the ring is moved round its diameter; whereas the former is to the latter as two to one, half the whole motive force being counteracted and rendered inefficient by the centrifugal force. 2dly. He fuppofes, that the whole efficient motive force, acting on a detached rigid annulus, revolving in the fame manner as before, is equal to the whole efficient motive force acting on an annulus, attached to and connected with a fphere, which is alfo falfe in the ratio of one to two; the centrifugal force in the cafe of an attached annulus vanifhing; and therefore no part of the whole motive force is rendered ineffectual; and confequently half the motive force in the latter cafe will produce an equal effect as the whole in the former, half of the force in the former cafe not contributing in any degree to the motion of the annulus round its diameter, but being totally employed in counteracting the tendency of the ring to revolve round a momentary axis."

The affertions contained in the preceding paffage are not to be confidered as opinions: they are the refults of demonftrations, and by them the faulty and the correct parts of Simpfon's folution are diftinctly feparated from one another. The inaccuracies ftated by Dr. Young are contained in Simpfon's 2d lemma, and its corollaries, nor have I heretofore heard of any other miftakes being attributed to him.

There are two reafons, as it appears to me, for fuppofing that the reviewer had not feen Dr. Young's memoir, or did not recollect it, when he difplayed his learning and penetration on my paper. The firft is, that a knowledge of it would have faved him from floundering about in the dark, in queft of Simpfon's miftakes. The fecond is, that a knowledge of it would have fecured him from an expofure of his grofs ignorance of the fubject. The truth of this laft obfervation will be manifeft from the following reprefentation of Dr. Young's affertions, drawn from actual demonftration, and of the reviewer's opinions, refting entirely on his own felf-conceit.

Dr.

Dr. Young.	*Reviewer.*
"Mr. Milner's and Friſi's calculations become likewiſe correct in the reſult, in the ſame manner as Simpſon's, by the mutual counteraction of equal and contrary errors. Thus they both hold, that the preceſſion of a rigid annulus is double that of a ſolitary moon, whereas they are equal, as we have already demonſtrated, by which the preceſſion would come out twice greater than the truth; but they likewiſe are of opinion, that the preceſſion of an attached and ſolitary annulus are equal, whereas the former is double that of the latter: this error, therefore, counterbalances the former."	"The plan of demonſtration, which we have here traced out, is nearly followed by Mr. Milner, in the Philoſophical Tranſactions of 1779; and that gentleman's ſolution appears to us by many degrees the moſt preciſe and perſpicuous of all ſimilar proceſſes. "Mr. Milner's demonſtration is entitled to notice and diſtinction, ſince it is not perplexed by terms of art, nor encumbered with the properties and formulæ of theories, connected perhaps with the ſubject of diſcuſſion, but not neceſſary to that diſcuſſion."

I now return to the reviewer's charge of plagiariſm. In page 14, ſpeaking of my paper, he ſays; "In article "19, the author, between the two forces to turn the "ſpheroid, makes the ſame compariſon which Simpſon "draws towards the concluſion of his ſecond problem, "p. 14. and conſequently the criticiſm, which we have "already made on the juſtneſs of that compariſon, is here "to be applied."

Perhaps the reader of this Reply will be aſtoniſhed, when he is aſſured, that the reviewer, after miſtaking a ſound part of Simpſon's ſolution for a faulty one, has carped at it, (and conſequently without making any impreſſion,)

preſſion,) and that this carping is the criticiſm alluded to in the laſt quotation. As to the ſameneſs of compariſon, made in my 19th article, with that which Simpſon draws towards the concluſion of his ſecond problem, p. 14. I think the reviewer has made a miſtake. I prefer this ſuppoſition to the aſſertion of his being guilty of telling an evident lie.

The reviewer, after quoting the whole of my 19th article, makes the following obſervation, which, from ſome previous expreſſions, he evidently wiſhes to be conſidered as a continuation of what he calls criticiſm, but what I have called carping.—" Now, according to this explan-" ation, it is only at the equinoxes that *the other circum-" ſtances* are the ſame, (and in this point the effect of the " diſturbing forces is nothing) : in other poſitions, *the " other circumſtances* are not the ſame ; and conſequently " there is no reaſon why the forces ſhould be equal."

In the beginning of my 19th article it is ſaid, " As the " librating force $\frac{d}{a} \times \frac{mn}{5} \times \overline{a^2-e^2} \times Z$, aſcertained in " article 16, and the force $\frac{av}{5} Z$, obtained in the laſt arti-" cle, are calculated on the ſame hypotheſis, viz. that " the force of a particle is as its diſtance from the plane " D C F in Fig. 9, or the plane D M E S in Fig. 10, if they " produce equal angular velocities, the ſpheroids in the " two figures being equal in every reſpect, and all other " circumſtances being the ſame, the forces themſelves " muſt be equal." As the truth of this aſſertion does not admit of a doubt, the only meaning which I can affix to the ambiguous expreſſions in the preceding quotation from the review, is, that as the effect of the diſturbing force at the equinoxes is nothing, in this ſituation there can be no equation. Now as the two ſides of the equation, with which the reviewer quarrels, expreſs in fact the ratio of the forces, if his objection be admitted, it would follow, that at the equinoxes no proportion could exiſt between the forces, as the diſturbing force

force is then equal to nothing. His criticifm, therefore, amounts to nothing more than an indirect cavil againft the doctrine of prime and ultimate ratios; a doctrine, to which, as far as I have heard or feen, no author of reputation has objected for more than half a century. The remark, however, contained in the laft quotation of the ftrictures, IS THE ONLY ONE IN HIS REVIEW OF MY PAPER, WHICH HAS THE APPEARANCE OF FAIR CRITICISM APPLICABLE TO MY MEMOIR, ALTHOUGH HE HAS CONTRIVED TO MAKE HIS CRITIQUE EXTEND THROUGH TEN PAGES OF THE MONTHLY REVIEW.

Simpfon in his Preface, when fpeaking of his Tract on the Preceffion of the Equinoxes, juftly obferves, that the only very difficult part is the determination of the momentary alteration of the pofition of the earth's axis. My tenth article was introduced as preparatory to this part, and in the 20th and 21ft the preceding articles concerning the fun's difturbing force were fupported by thofe on compound rotatory motion, for this important purpofe.

It was therefore neceffary in thefe laft-mentioned articles to combine the fun's difturbing force on the whole mafs of the earth, the fun's centripetal force on the earth in its orbit, and the centripetal force of the earth on a body fuppofed to revolve at the equator in the fpace of a diurnal revolution, in order to obtain an expreffion for the force caufing preceffion. Perhaps to fome it may appear very remarkable, that the critic paffes over this part of my paper in filence, as the greateft nicety in any folution muft be found in the determination to which I have alluded. For my own part, when I confider how far his ftrictures are indicative of knowledge, I reckon his forbearance on this occafion as a fingular mark of his prudence.

As the articles already mentioned, and the manner in which they are combined, conftitute all the peculiarity in my paper, there was no abfolute neceffity, after the 21ft, for any addition. It appeared to me, however, to

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be highly proper to add the four laft articles, in order to render the whole complete, as far as the fun's difturbing force is productive of preceffion. The reviewer, without intending it I fuppofe, exempts one of thefe from the charge of plagiarifm, when he fays, "Articles 22, 24, 25, "&c. are the fame with problem 4th and its corollaries "by Simpfon, p. 15. &c." Now the truth is, that they are the fame, both in fubftance and manner, with what are to be found in feveral writers; but in drawing them up, I followed no one of thofe writers in particular. Such deviations and fuch additions were made, as appeared productive of perfpicuity; and I thought myfelf perfectly fecure from the charge of plagiarifm, when in allufion to thefe articles it was ftated in the introduction, that "the quantity of annual preceffion is calculated in the "ufual way, and alfo that of nutation, as far as they are "produced by the difturbing force of the fun." Surely this acknowledgment was fufficiently full. Making no allufion to it, and at the fame time proclaiming an accufation of borrowing, conftitute, as far as I know, a new cafe in the art of reviewing. When the reader has reflected on thefe circumftances, perhaps he may be at a lofs to determine, whether injuftice or folly predominates in the critic's charge of plagiarifm.

The complaints of the critic, in his peroration, are very pathetic, and I fincerely condole with him as to his lofs of time, as I verily believe that there is good ground for his lamentations. But it was not fair in his fit of ill humour to lay the whole blame upon me. After reviewing his own exertions upon the fubject, he ought to have faid to himfelf, "From all I now fee, I alone am to "blame. In the examination of this paper, I fet out as "a critic before I underftood the fubject, and as a confe-"quence of not underftanding the doctrine of prime and "ultimate ratios, I find I am now in the fame ftate as "when I began. What I have written muft therefore "be fuppreffed. My infufficiency muft not give birth to "three crimes; falfehood towards the author, impofition

"towards

"towards my employer, and a violation of duty towards "the public."

According to appearances, neither modefty nor confcience fuggefted any thing like fuch a foliloquy to the critic. With the moft complete felf-affurance, and the moft confident reliance on his own attainments and ability, he deals out his canons of criticifm, his cenfures, and his praifes. But on examining the fayings of the unknown judge, how different are our opinions from thofe, which he feems to have formed in his own mighty mind! His canons of criticifm are found to indicate a deplorable deficiency in judgment; we afcertain that his cenfures are founded in his own mifconceptions; and it is abfolutely proved, that his praifes are beftowed on miftakes.

"Errores, non infultandi, fed veritatis ftudio, notamus."

From the preceding ftatements it is alfo evident, that his charges of plagiarifm are unfounded and frivolous. His advancing them only furnifhes an additional proof of his own ignorance in the fubject. If he had really read the writings on the preceffion, to which he alludes, he would have found a confiderable portion of them, both in fubftance and manner, to be common; and if he had deliberately confidered the nature of the problem, he would have been convinced, that, in fome parts of the folution, variations in the proceedings were neither to be expected nor defired. From a knowledge of thefe circumftances, I perufed his charges with perfect compofure.

"Idem fi clamet furem, ——
Mordear opprobriis falfis, mutemque colores?"

HORACE.

The critiques in the Monthly Review, to which the preceding obfervations apply, are not fingular either in their quality or defign. On examining that periodical publication for fome years back, I find it has been the

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general practice of the mathematical writer, or writers, to give as favourable accounts as possible of the productions of Cambridge authors; and as generally unfavourable accounts of other authors of this country.

It is not to be supposed for a moment, that this observation is intended as a reflection on that University. As a corporate body it is accountable for its public measures only, and not for the private proceedings of one or a few individuals. It cannot check the zeal of those who write in the Monthly Review, however indiscreetly and needlessly their zeal may be exerted in its favour; nor can it correct this zeal, even when it recurs to misrepresentation and falsehood. The authors only of such productions are to be blamed; but, being unknown, their imprudence chiefly affects the publication in which it appears. When a man of real attainments in science perceives that deliberate, judicious, and candid criticism pervades a review, he will listen with respect to the opinions of the critic; but the same reader will turn away, with strong disapprobation, and even disgust, from any publication, in which the tide of local prejudice is evidently so violent, as to bear down before it these guardians of society, candour, manners, and truth. Critiques written with such prejudice not only defeat their own purposes; they convey besides a very sorry compliment to the place in whose favour they are published. That reviewer must be very vain indeed, who thinks that his praises are requisite to raise or support the character of the University of Cambridge; and he must also be very foolish and unjust, when he endeavours to do this by criticising works which he does not *understand*.

The attention of many was fixed on the local prejudice above mentioned, by the following glaring instance of partiality and misrepresentation. About six years and a half ago, Dr. Hutton, of the Royal Military Academy at Woolwich, printed a second edition of his pamphlet on the Principles of Bridges; and about the same time, the late Mr. George Atwood published a Dissertation on the Con-

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ſtruction and Properties of Arches[a]. The laſt-mentioned gentleman was educated at Cambridge; the firſt was not. The appearance of the two publications, on the ſame ſubject, and nearly at the ſame time, ſeems to have induced the reviewer to form the ridiculous and unjuſt deſign of exalting the mathematical character of Mr. Atwood, and depreſſing that of Dr. Hutton. Critiques of this tendency, but written without a knowledge of the ſubject, were accordingly inſerted in the Review; and falſehoods, ſufficiently extenſive, were cautiouſly introduced, in order to form a pretence for praiſe and detraction. It appears from the correſpondence, in the numbers for May and June of 1802, that Dr. Hutton remonſtrated againſt theſe miſrepreſentations; but no ſatisfactory explanation could be obtained. He therefore addreſſed a letter, of conſiderable length, to the Editor of the Monthly Magazine, and it was inſerted in the number for Auguſt, 1802, of that publication. The following is the firſt paragraph of Dr. Hutton's letter.

To the Editor of the Monthly Magazine.

SIR,

"AMONG the various periodical publications that have been projected in the courſe of the laſt half century, reviews of new books have held a diſtinguiſhed place. The idea was plauſible and popular; the encouragement prompt and ample. The benefits to literature, and to the public, expected from ſuch regiſters, were too obvious and important not to meet the hearty good wiſhes of moſt readers. How pleaſing and convenient, to have a regular regiſter of all new publications, and an account of all that is moſt important in them! How uſeful thus, not only to be appriſed of whatever new books come forth

[a] A very able and candid review of Mr. Atwood's Publication appeared in the Britiſh Critic for January 1804. and it was accompanied with diagrams. Let no one attribute this commendation to partiality, as I do not know who wrote that critique, nor do I know who write the mathematical articles in the Britiſh Critic.

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on the fubjects we are moft interefted in, but alfo to be informed of their particular merits and contents, for directing our choice or rejection, and to be amufed and inftructed by abftracts judicioufly made from them, and, in fhort, a condenfed epitome of the whole works!—With fuch flattering ideas, we pleafed and congratulated ourfelves, eagerly encouraging fuch obliging critics and reviewers, without dreaming of any adverfe confequences refulting from them. How could we fufpect any thing difaftrous from fuch obliging, good, and able men? They could have no end in view but the public good. Who could harbour a thought of their ever being actuated by the mean paffions of jealoufy, envy, malice, and all uncharitablenefs? of turning their privilege of concealed reviewers to all the purpofes of a job, to miflead the public mind—to abufe its confidence—to puff off the very trafh of their own connections and their friends—to mifreprefent and condemn the moft valuable works of others; thus concealedly and implicitly deceiving the public by falfe notions, to the great difcouragement of true and genuine literature?"

After expofing in detail the reviewer's partiality, mifreprefentations, and infidious expreffions, Dr. Hutton proceeds to obferve, that "It is impoffible to avoid per-" ceiving, that there exifts fome fecret and myfterious " motive for fo much offenfive matter, very different from " what ought to actuate the mind and influence the con-" duct of a fair and judicious reviewer. Whatever it " may be, it certainly ought not to appear in the writ-" ings of a reviewer; whether it be fome pique againft " any man in particular, or whether it arifes from a dif-" pofition in the gentlemen of Cambridge to difcourage " the productions of others; a difpofition with which " they have fometimes been charged, though perhaps " unjuftly and illiberally. I here mentioned Cambridge, " becaufe it is faid the mathematical writer for the " Monthly Review is at prefent an ingenious young gen-" tleman, refiding in that Univerfity, of the name of

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" Woodhouſe: if I am miſtaken in this, I aſk his pardon " for the mention of his name, and he will correct me by " diſclaiming the concern. Till then, in common with " many other perſons, he will excuſe me for the belief, " that he is the reviewer, whoſe conduct in the alleged " inſtance is ſo much complained of. Whatever be the " concealed cauſe, Sir, whether one or both of thoſe juſt " alluded to, or whatever elſe, the fact appears moſt cer- " tain and evident."

A letter from Dr. Hutton, on the ſame ſubject, is alſo inſerted in the Monthly Magazine for October 1802. The following is the firſt ſentence of that letter: " A " writer in your Magazine (Sup. No. 89. p. 645.) has in- " advertently, or in following too implicitly the Monthly " reviewer, (Mr. Woodhouſe,) miſrepreſented ſome of the " principles employed in my Treatiſe on Bridges; and I will " thank you to allow room for two or three lines to cor- " rect the miſtake."—It appears from this laſt letter of Dr. Hutton's, that Mr. W. had not in the interval, between the publication of the two, owned the reviews attributed to him, nor have I ever heard that he has made any declaration on the ſubject. From this circumſtance perhaps ſome may ſuppoſe, that Mr. W. was not the author of the critiques, of which Dr. H. complains; but from particulars about to be mentioned we may fairly infer, that the reviewer was a friend of Mr. W.'s, and from a temporary fear that he could not favour him in the Monthly Review, he quitted his poſt in a moſt cowardly manner, in conſequence of Dr. Hutton's letters. The truth of this ſurmiſe reſts on the following conſideration.

In the year 1803. Mr. Woodhouſe publiſhed a quarto volume, entitled, " The Principles of Analytical Calcula- " tion." In this work the author very kindly undertakes to inform mathematicians, what is really meant by the ſign of equality, how they are to underſtand the negative ſign, what the preciſe object of the binomial theorem is, &c. &c.

Being deſirous to know what his friend the critic ſaid

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of all this inſtruction, I turned to the Monthly Review, number after number, and, ſtrange to tell, I found that in this publication he deſerted Mr. W. on this occaſion, although on others he uniformly approves of his writings, and adopts his opinions[a]: the Monthly Review is ſilent as to the merits of the above-mentioned performance. But in the developement of ſecret proceedings it frequently happens, that the diſcovery of one circumſtance enables the inquirer to follow his object; and this was the caſe in the preſent inſtance. In conſequence of the information, which I ſtated in the note in page 1. of this Reply, I proceeded to examine the internal evidence in ſupport of the aſſertion, that the ſame perſon frequently wrote in the Critical Review; and on turning to that publication for June 1803, I found the ſame learned critic holding forth with all his eloquence on the tranſcendent merits of his friend's publication. Like a ſkilful general, he had only moved out of the reach of Dr. Hutton's galling fire. His friendſhip towards Mr. W. was unabated; and it was exerted, as it appears from dates, with remarkable promptitude. The order of the Syndics of the Preſs, for fixing the price of the book, is dated the 12th of February, 1803. There muſt have been ſome interval between this time and the publication of the work, ſo that the critic muſt have been very quick in reading and comprehending his author, and very aſſiduous in drawing up a condenſed but minute account of its excellence, before June. The volume, it is true, is thin; but we are told by the critic, that "it will exerciſe the talents of the "higher mathematicians."

The ſame friend to Mr. W. appears in the Critical Review for July 1805, and, in contemplating his merits, the critic ſeems to be overwhelmed with admiration. "His "ſcience and ſkill," the reviewer obſerves, "can be ap- "preciated only by the higher mathematicians. A com-

[a] See the Monthly Review for January and December 1802, and for May 1805. See alſo the Critical Review for June 1805, from page 147 to 155.

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"parifon between the mathematicians of this country "and France, during the laft century, would be highly "worthy of his pen; for yet, notwithftanding the high "encomiums paid to the French, and the voluminous "works iffuing from the Parifian prefs, we are inclined "to think, that they have rather increafed the forms, "than added much to the ftock of fcience. Our author "will enable us to fee this matter in the cleareft point of "view, as he is one of the few mathematicians of Cam-"bridge, and when we fay Cambridge, we cannot add "many for the reft of England [a], who have ftudied with "diligence and attention the late French writers on the "differential calculus, or what we more properly call "fluxions."—*Rifum teneatis amici?* for this reviewer knows every corner of the united kingdom, and the mathematical purfuits and attainments of every individual in it. It is therefore not to be wondered at, that he knows Mr. W.'s private ftudies, and that he can compare his acquifitions with thofe of all the mathematicians in the country. As numbers labour in vain to attract public attention, and to obtain the approbation of critics, this author muft be efteemed very fortunate indeed in having an advocate, who can fo readily comprehend his moft intricate refearches, and who can, without delay, announce their comparative excellence to the world. The critic

[a] It is very eafy to account for the appearance of more mathematical publications from Cambridge than from any other place in this kingdom. The members of that Univerfity, who perfevere in the ftudy of fcience, and who are confcious of their ability either to elucidate its truths or enlarge its boundaries, can look with confidence to the Syndics of the Prefs for affiftance, to render their writings profitable to themfelves, and ufeful to the public. All apprehenfion as to the pecuniary rifk of printing is removed, and thofe who write with a view to publifh are at once ftimulated by the laudable defire of fame, and animated with the hope of a fair and honourable recompence for their labour, in the fale of their productions. In other places, mathematicians may be equally fuccefsful in their attainments, and authors of treatifes as worthy of public attention; but a wifh to prefent them to the world is checked by the deterring confideration of an immediate and ferious expence, and an uncertain and flow indemnification.

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alſo ſeems very happy when he delivers theſe eulogiums, as on theſe occaſions he prolongs his taſk with apparent pleaſure.

On other occaſions he is very frequently in a different humour. In reviewing the beſt mathematical productions, he ſo generally conceals facts on which the inveſtigations reſt, ſo groſsly miſrepreſents others, and ſo very often forms unfair combinations of circumſtances, merely with a view to blame, that the following line may be affixed as an appropriate motto to his ſtrictures:

> "Nemo Mathematicus genium indemnatus habebit."
>
> JUVENAL.

FINIS.