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MATHEMATICAL
TRACTS

Of the late

Benjamin Robins, Esq;

Fellow of the Royal Society,

AND

Engineer General to the Honourable the East India Company.

MATHEMATICAL TRACTS

Of the late

Benjamin Robins, Esq;

Fellow of the Royal Society,

AND

Engineer General to the Honourable the East India Company.

In TWO VOLUMES.

VOL. II.

Containing his DISCOURSE ON THE METHODS OF FLUXIONS, and of PRIME AND ULTIMATE RATIOS, with other Miscellaneous Pieces.

Published by JAMES WILSON, M.D.

Patere honoris scirent ut cunctis viam,
Nec generi tribui, sed virtuti, gloriam. Phæd.

LONDON,
Printed for J. NOURSE over against Katherine Street in the Strand, MDCCLXI.

A DISCOURSE

Concerning the

NATURE and CERTAINTY

OF

Sir ISAAC NEWTON's

METHODS

OF

FLUXIONS,

AND OF

PRIME and ULTIMATE

RATIOS.

First published in 1735.

THE CONTENTS.

INTRO-

INTRODUCTION.

1. FROM many propositions dispersed through the writings of the ancient geometers, and more especially from one whole treatise *, it appears, that the process, by which they investigated the solutions of their problems, was for the most part the reverse of the method, whereby they demonstrated those solutions. But what they have delivered upon the tangents of curve lines, and the mensuration of curvilinear spaces, does not fall under this observation; for the analysis, they made use of in these cases, is no where to be met with in their works. In later times, indeed, a method for investigating such kind of problems has been introduced, by considering all curves, as composed of an infinite number of indivisible streight lines, and curvilinear spaces, as composed in the like manner of parallelograms. But this being an obscure and indistinct conception, it was obnoxious to error.

2. SIR Isaac Newton therefore, to avoid the imperfection, with which this method of indivisibles was justly charged, instituted an analysis for these problems upon other principles. Considering magnitudes not under the notion of being increased by a repeated accession of parts, but as generated by a continued motion or flux; he discovered a method to compare together the velocities, wherewith

* Apollon. de Sectione Rationis, published by Dr. Halley at Oxford in 1706.

 homoge-

homogeneous magnitudes increaſe, and thereby has taught an analyſis free from all obſcurity and indiſtinctneſs.

3. MOREOVER to facilitate the demonſtrations for theſe kinds of problems, he invented a ſynthetic form of reaſoning from the prime and ultimate ratios of the contemporaneous augments, or decrements of thoſe magnitudes, which is much more conciſe than the method of demonſtrating uſed in theſe caſes by the ancients, yet is equally diſtinct and concluſive.

4. OF this analyſis, called by Sir Iſaac Newton his method of fluxions, and of his doctrine of prime and ultimate ratios, I intend to write in the enſuing diſcourſe. For though Sir Iſaac Newton has very diſtinctly explained both theſe ſubjects, the firſt in his treatiſe on the Quadrature of curves, and the other in his Mathematical principles of natural philoſophy; yet as the author's great brevity has made a more diffuſive illuſtration not altogether unneceſſary; I have here endeavoured to conſider more at large each of theſe methods; whereby, I hope, it will appear, they have all the accuracy of the ſtricteſt mathematical demonſtration.

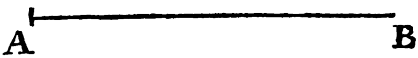
DISCOURSE

OF

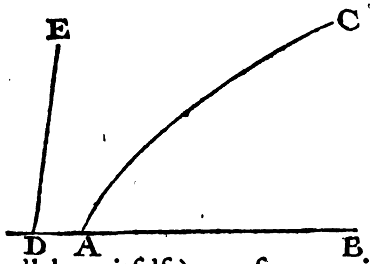
FLUXIONS, &c.

5. IN the method of fluxions geometrical magnitudes are not preſented to the mind, as compleatly formed at once, but as riſing gradually before the imagination by the motion of ſome of their extremes *.

6. Thus the line AB may be conceived to be traced out gradually by a point moving on from A to B, either with an equable motion, or with a velocity in any manner varied. And the velocity, or degree of ſwiftneſs, with which this point moves in any part of the line A B, is called the fluxion of this line at that place.



7. Again, ſuppoſe two lines A B and A C to form a ſpace unbounded towards B C; and upon AB a line DE to be erected.

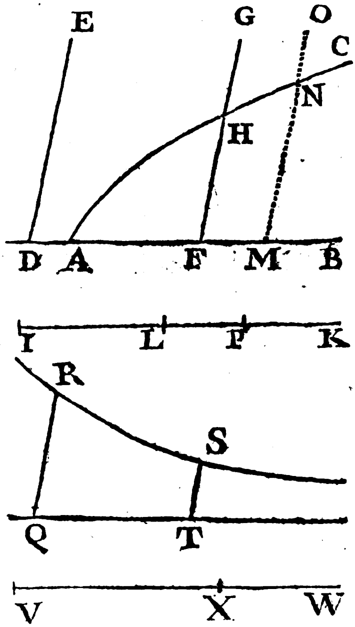


8. Now, if this line DE be put in motion (ſuppoſe ſo as to keep always parallel to itſelf,) as ſoon as it

* Newt. Introd. ad Quad. Curv.

has

has paſſed the point A, a ſpace bounded on all ſides will begin to appear between theſe three lines. For inſtance, when DE is moved into the ſituation FG, theſe three lines will include the ſpace AFH. Here it is evident, that this ſpace will increaſe faſter or ſlower, according to the degree of velocity, wherewith the line DE ſhall move. It is alſo evident, that though the line DE ſhould move with an even pace, the ſpace AFH would not for that reaſon increaſe equably; but where the line AC was fartheſt diſtant from AB, the ſpace AFH would increaſe faſteſt. Now the velocity or celerity, wherewith the ſpace AFH at all times increaſes, is called the fluxion of that ſpace.



9. Here it is obvious, that the velocity, wherewith the ſpace augments, is not to be underſtood literally the degree of ſwiftneſs, with which either the line FG, or any other line or point appertaining to the curve actually moves; but as this ſpace, while the line FG moves on uniformly, will increaſe more, in the ſame portion of time, at ſome places, than at others; the terms velocity and celerity are applied in a figurative ſenſe to denote the degree, wherewith this augmentation in every part proceeds.

10. But

10. BUT we may diveſt the conſideration of the fluxion of the ſpace from this figurative phraſe, by cauſing a point ſo to paſs over any ſtreight line IK, that the length IL meaſured out, while the line DE is moving from A to F, ſhall augment in the ſame proportion with the ſpace AFH. For this line being thus deſcribed faſter or ſlower in the ſame proportion, as the ſpace receives its augmentation; the velocity or degree of ſwiftneſs, wherewith the point deſcribing this line actually moves, will mark out the degree of celerity, wherewith the ſpace every where increaſes. And here the line IL will preſerve always the ſame analogy to the ſpace AFH; in ſo much, that, when the line DE is advanced into any other ſituation MNO, if IP be to IL in the proportion of the ſpace AMN to the ſpace AFH, the fluxion of the ſpace at MN will be to the fluxion thereof at FH, as the velocity, wherewith the point deſcribing the line IK moves at P, to the velocity of the ſame at L. And if any other ſpace QRST be deſcribed along with the former by the like motion, and at the ſame time a line VW, ſo that the portion VX ſhall always have to the length IL the ſame proportion, as the ſpace QRST bears to the ſpace AFH; the fluxion of this latter ſpace at TS will be to the fluxion of the former at FH, as the velocity, wherewith the line VW is deſcribed at X, to the velocity, wherewith IK is deſcribed at L. It will hereafter appear, that in all the applications of fluxions to geometrical problems, where ſpaces are concerned, nothing more is neceſſary, than to determine the velocity, wherewith ſuch lines as theſe are deſcribed *.

* §. 49.

11. In the ſame manner may a ſolid ſpace be conceived to augment with a continual flux, by the motion of ſome plane, whereby it is bounded; and the velocity of its augmentation (which may be eſtimated in like manner) will be the fluxion of that ſolid.

12. FLUXIONS then in general are the velocities, with which magnitudes varying by a continued motion increaſe or diminiſh; and the magnitudes themſelves are reciprocally called the fluents of thoſe fluxions *.

18. And as different fluents may be underſtood to be deſcribed together in ſuch manner, as conſtantly to preſerve ſome one known relation to each other; the doctrine of fluxions teaches, how to aſſign at all times the proportion between the velocities, wherewith homogeneous magnitudes, varying thus together, augment or diminiſh.

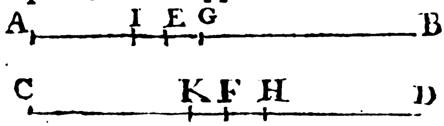
14. This doctrine alſo teaches on the other hand, how, from the relation known between the fluxions, to diſcover what relation the fluents themſelves bear to each other.

15. It is by means of this proportion only, that fluxions are applied to geometrical uſes; for this doctrine never requires any determinate degree of velocity to be aſſigned for the fluxion of any one fluent. And that the proportion between the fluxions of magnitudes is aſſignable from the relation known between the magnitudes themſelves, I now proceed to ſhew.

* Motuum vel incrementorum velocitates nominando fluxiones, & quantitates genitas nominando fluentes. Newton. Introd. ad Quadr. Curv.

16. In

16. IN the firſt place, let us ſuppoſe two lines AB and CD to be deſcribed together by two points, one ſetting out from A, and the other from C, and to move in ſuch manner, that if AE and CF are lengths deſcribed in the ſame time, CF ſhall be analogous to ſome power of AE, that is, if AE be denoted by the letter x, then CF ſhall always be denoted by $\frac{x^n}{a^{n-1}}$; where a repreſents ſome given line, and n any number whatever. Here, I ſay, the proportion between the velocity of the point moving on AB to the velocity of that moving on CD, is at all times aſſignable.

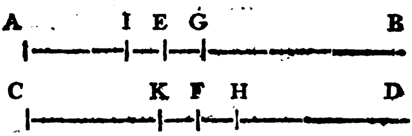


17. FOR let any other ſituations, that theſe moving points ſhall have at the ſame inſtant of time, be taken, either farther advanced from E and F, as at G and H, or ſhort of the ſame, as at I and K; then if EG be denoted by e, CH, the length paſſed over by the point moving on the line CD, while the point in the line AB has paſſed from A to G, will be expreſſed by $\frac{\overline{x+e}|^n}{a^{n-1}}$; and if EI be denoted by e, CK, the length paſſed over by the point moving on the line CD, while the point moving in AB has got only to I, will be denoted by $\frac{\overline{x-e}|^n}{a^{n-1}}$: or reducing each of theſe terms into a ſeries, CH will be denoted by $\frac{x^n}{a^{n-1}} + \frac{nx^{n-1}e}{a^{n-1}}$ $+ \frac{n\times\overline{n-1},x^{n-2}ee}{2a^{n-1}} + \frac{n\times\overline{n-1}\times\overline{n-2},x^{n-3}e^3}{6a^{n-1}} +$ &c.

&c. and CK by $\frac{x^n}{a^{n-1}} - \frac{nx^{n-1}e}{a^{n-1}} + \frac{n \times \overline{n-1},\, x^{n-2}ee}{2\, a^{n-1}}$ $- \frac{n \times \overline{n-1} \times \overline{n-2},\, x^{n-3}e^3}{6a^{n-1}}$ + &c. Hence all the terms of the former series, except the first term, *viz.* $\frac{n\, x^{n-1}e}{a^{n-1}} + \frac{n \times \overline{n-1}\; x^{n-2}ee}{2\, a^{n-1}}$ + &c. will denote FH; and all the latter series, except the first term, *viz.* $\frac{nx^{n-1}e}{a^{n-1}} - \frac{n \times \overline{n-1}\, x^{n-2}ee}{2a^{n-1}}$ + &c. will denote KF.

18. When the number n is greater than unite, while the line AB is described with a uniform motion, the point, wherewith CD is described, moves with a velocity continually accelerated; for if IE be equal to EG, FH will be greater than KF.

19. Now here, I say, that neither the proportion of FH to EG, nor the proportion of KF to IE is the proportion of the velocity, which the point moving on CD has at F, to the uniform velocity, wherewith the point moves on the line AB. For, while that point is advanced from E to G, the point moving on CD has passed from F to H, and has moved through



that space with a velocity continually accelerated; therefore, if it had moved during the same interval of time with the velocity, it has at F, uniformly continued, it would not have passed over so long a line; consequently FH bears a greater proportion to EG, than what the velocity, which the point moving on CD has at F, bears to the velocity of the point moving uniformly on AB.

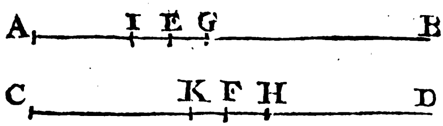
20. In

20. IN like manner KF bears to IE a leſs proportion than that, which the velocity of the point in CD has at F, to the velocity of that in A B. For as the point in CD, in moving from K to F, proceeds with a velocity continually accelerated; with the velocity, it has acquired at F, if uniformly continued, it would deſcribe in the ſame ſpace of time a line longer than KF.

21. IN the laſt place I ſay, that no line whatever, that ſhall be greater or leſs than the line repreſented by the ſecond term of the foregoing ſeries (*viz.* the term $\frac{nx^{n-1}e}{a^{n-1}}$) will bear to the line denoted by e the ſame proportion, as the velocity, wherewith the point moves at F, bears to the velocity of the point moving in the line AB; but that the velocity at F is to that at E as $\frac{nx^{n-1}e}{a^{n-1}}$ to e, or as nx^{n-1} to a^{n-1}.

22. IF poſſible let the velocity at F bear to the velocity at E a greater ratio than this, ſuppoſe the ratio of p to q.

23. IN the ſeries, whereby CH is denoted, the line e can be taken ſo ſmall, that any term propoſed in the ſeries ſhall exceed all the following terms together; ſo that the double of



that term ſhall be greater than the whole collection of that term, and all that follow. Again, by diminiſhing e,

e, the ratio of the second term in this series, to twice the third, that is, of $\frac{nx^{n-1}e}{a^{n-1}}$ to $\frac{n\times\overline{n-1}x^{n-2}ee}{a^{n-1}}$, or the ratio of x to $\overline{n-1}\times e$, may be rendered greater than any, that shall be proposed; consequently the line e may be taken so small, that twice the third term, that is $\frac{n\times\overline{n-1}x^{n-2}ee}{a^{n-1}}$, shall be greater than all the terms following the second, and also, that the ratio of $\frac{nx^{n-1}e}{a^{n-1}}+\frac{n\times\overline{n-1}x^{n-2}ee}{a^{n-1}}$ to e shall less exceed the ratio of $\frac{nx^{n-1}e}{a^{n-1}}$ to e, than any other ratio whatever, that may be proposed. Therefore let the ratio of $\frac{nx^{n-1}e}{a^{n-1}}+\frac{n\times\overline{n-1}x^{n-2}ee}{a^{n-1}}$ to e be less than the ratio of p to q; then, if $\frac{n\times\overline{n-1}x^{n-2}ee}{a^{n-1}}$ be also greater than the third and all the following terms of the series, the ratio of the series $\frac{nx^{n-1}e}{a^{n-1}}+\frac{n\times\overline{n-1}x^{n-2}ee}{2a^{n-1}}$ + &c. to e, that is, the ratio of FH to EG shall be less than the ratio of p to q, or of the velocity at F to the velocity at E, which is absurd; for it has above been shewn, that the first of these ratios is greater than the last. Therefore the velocity at F cannot bear to the velocity at E any greater proportion than that of $\frac{nx^{n-1}e}{a^{n-1}}$ to e.

24. On the other hand, if possible, let the velocity at F bear to the velocity at E a less ratio than

that of $\frac{nx^{n-1}e}{a^{n-1}}$ to e: let this leſſer ratio be that of r to s.

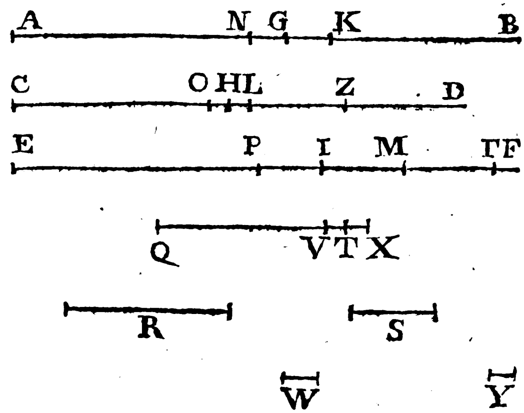
25. In the ſeries whereby CK is denoted, e may be taken ſo ſmall, that any one term propoſed ſhall exceed the whole ſum of all the following terms, when added together. Therefore let e be taken ſo ſmall, that the third term $\frac{n\times\overline{n-1}\,x^{n-2}\,ee}{2a^{n-1}}$ exceed all the following terms $\frac{n\times\overline{n-1}\times\overline{n\backsim 2}\,x^{n-3}e^3}{6a^{n-1}}$, $\frac{n\times\overline{n-1}\times\overline{n\backsim 2}\times\overline{n\backsim 3}\,x^{n-4}e^4}{24a^{n-1}}$, &c. added together. But e may alſo be rendered ſo ſmall, that the ratio of $\frac{nx^{n-1}e}{a^{n-1}}$ to $\frac{n\times\overline{n-1}\,x^{n-2}ee}{a^{n-1}}$, the double of the third term, ſhall be greater than any ratio whatever that ſhall be propoſed; and the ratio of $\frac{nx^{n-1}e}{a^{n-1}} - \frac{n\times\overline{n-1}\,x^{n-2}ee}{a^{n-1}}$ to e ſhall come leſs ſhort of the ratio of $\frac{nx^{n-1}e}{a^{n-1}}$ to e, than any other ratio, that can be named. Therefore let this ratio exceed the ratio of r to s; then the term $\frac{n\times\overline{n-1}\,x^{n-2}ee}{2a^{n-1}}$ exceeding the whole ſum of all the following terms in the ſeries denoting CK, the whole ſeries $\frac{nx^{n-1}e}{a^{n-1}} - \frac{n\times\overline{n-1}\,x^{n-2}ee}{2a^{n-1}} + \&c.$ or KF, will in

every case bear to e, or EI a greater ratio than that of r to s, or of the velocity at F to the velocity at E, which is absurd. For it has above been shewn, that the first of these ratios is less than the last.

26. If n be less than unite, the point in the line CD moves with a velocity continually decreasing; and if n be a negative number, this point moves backwards. But in all these cases the demonstration proceeds in like manner.

27. Thus have we here made appear, that from the relation between the lines AE and CF, the proportion between the velocities, wherewith they are described, is discoverable; for we have shewn, that the proportion of nx^{n-1} to a^{n-1} is the true proportion of the velocity, wherewith CF, or $\frac{x^n}{a^{n-1}}$ augments, to the velocity, wherewith AE or x is at the same time augmented.

28. AGAIN, in the three lines AB, CD, EF,



where

where the points A, C, E are given, let us suppose G, H and I to be three contemporary positions of the points, whereby the three lines AB, CD, EF are respectively described; and let the motion of the point describing the line EF be so regulated with regard to the motion of the other two points, that the rectangle under EI and some given line may be always equal to the rectangle under AG and CH. Here from the velocities, or degrees of swiftness, wherewith the points describing AB and CD move, the degree of swiftness, wherewith the point describing EF moves, may be determined.

29. THE points moving on the lines AB, CD may either move both the same way, or one forwards and the other backwards.

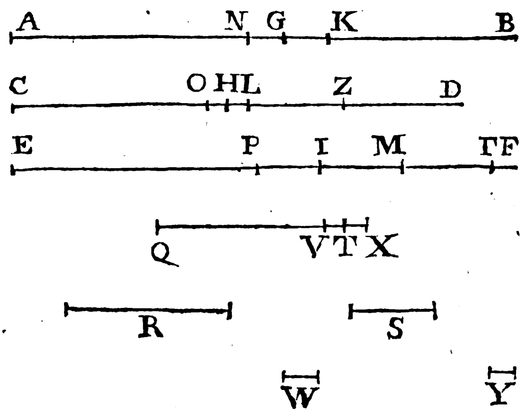
30. In the first place suppose them to move the same way, advancing forward from A and C; and since some given line forms with EI a rectangle equal to that under AG and CH, suppose $QT \times EI = AG \times CH$: then, if K, L, M are contemporary positions of the points moving on the lines AB, CD, EF, when advanced forward beyond G, H and I; and N, O, P, three other contemporary positions of the same points, before they are arrived at G, H and I; $QT \times EM$ will also be $= AK \times CL$, and $QT \times EP = AN \times CO$; therefore the rectangle under IM (the difference of the lines EI and EM) and QT will be $= AK \times HL + CH \times GK$, and $IP \times QT = AN \times HO + CH \times GN$.

31. HERE the proportion of the velocity, which the point moving on AB has at G, to that, which the point moving on CD has at H, may either keep always the same, or continually vary so, that one

 of

of these velocities (suppose that of the point moving on the line CD) shall have to the other a proportion gradually augmenting; that is, if NG and GK are equal, HL shall either be equal to OH or greater. Here, since $IM \times QT$ is $= AK \times HL + CH \times GK$, and $IP \times QT = AN \times HO + CH \times GN$, where $CH \times GK$ is $= CH \times GN$ and $AK \times HL$ in both cases greater than $AN \times HO$, IM will be greater than IP; in so much that in both these cases the velocity of the point, wherewith the line EF is described, will have to the velocity of the point moving on AB a proportion, gradually augmenting. Here therefore the line IM will bear to GK a greater proportion, than the velocity of the point moving on the line EF, when at I, bears to the velocity of the point moving on the line AB, when at G; and the line PI will have a less proportion to NG, than the velocity, which the point moving on the line EF, has at I, to the velocity, which the point moving on the line AB has at G.

32. Now let R be to S as the velocity, which the



point

point moving on AB has at G, to the velocity, which the point moving on CD has at H; then I say, that the velocity, which the point moving on EF has at I, will be to the velocity, which the point moving on AB has at G, as AG × S + CH × R to QT × R.

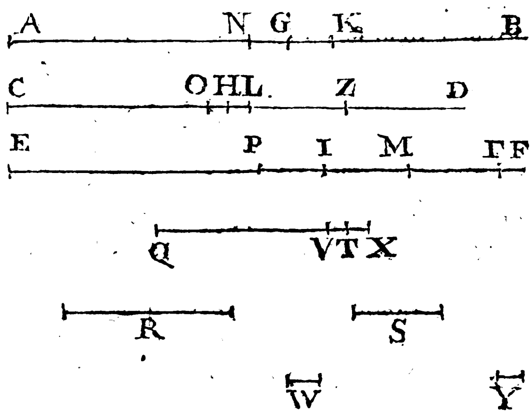
33. If possible let the velocity, which the point moving on EF has at I, be to the velocity, which the point moving on AB has at G, as AG × S + CH × R to the rectangle under R and some line QV less than QT.

34. Take W to GK in the ratio of S to R; then will AG × S + CH × R be to R × QV as AG × W + CH × GK to QV × GK. Here, because the ratio of the velocity of the point moving on the line CD to the velocity of the point moving on AB either remains constantly the same, or gradually augments, W is either equal to HL or less; but when it is less, by diminishing HL the ratio of W to HL may become greater than any ratio whatever, that may be proposed, short of the ratio of equality. The like is true of the ratio of AG to AK by the diminution of GK. Therefore let GK and HL be so diminished, that the ratio of AG × W to AK × HL shall be greater than the ratio of QV to QT; then the ratio of AG × W + CH × GK to AK × HL + CH × GK, that is, to QT × IM is greater than the ratio of QV to QT or of QV × IM to QT × IM; therefore AG × W + CH × GK is greater than QV × IM; and the ratio of AG × W + CH × GK to QV × GK is greater than the ratio of QV × IM to QV × GK, or of IM to GK; but the ratio of IM to GK is greater than that of the velocity, which the point moving on EF has at I, to the velocity, which the point moving on AB has at G; therefore the ratio of AG × W

$+ CH \times GK$ to $QV \times GK$, or that of $AG \times S + CH \times R$ to $QV \times R$, still more exceeds the ratio of the velocity at I to the velocity at G; and consequently the ratio of the velocity at I to the velocity at G is not greater than that of $AG \times S + CH \times R$ to $QT \times R$.

35. Again, if possible let the velocity, which the point moving on EF has at I, be to the velocity, which the point moving on AB has at G, as $AG \times S + CH \times R$ to the rectangle under R and some line QX greater than QT.

36. Here let Y be to NG as S to R; then will $AG \times S + CH \times R$ be to $R \times QX$ as $AG \times Y + CH \times NG$ to $QX \times NG$. But Y will be either greater than HO, or equal to it, and when greater, by diminishing HO, the ratio of Y to HO may become less than any ratio whatever, that shall be proposed, greater than the ratio of equality. The like is

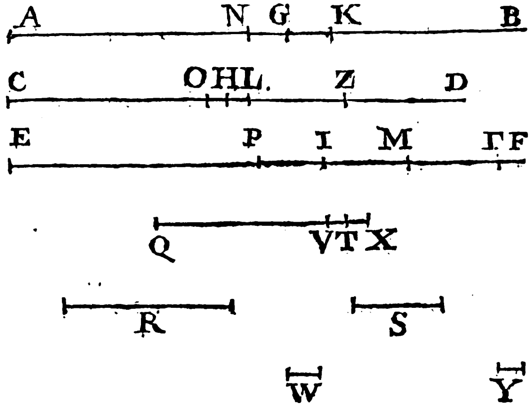


true of the ratio of AG to AN by the diminution of NG. Therefore let NG and HO be so diminished, that the ratio of $AG \times Y$ to $AN \times HO$ shall be be less than the ratio of QX to QT; then the ratio of AG×Y+CH×NG to AN×HO+CH×NG, that is, to QT×IP, is less than the ratio of QX to QT, or of QX×IP to QT×IP. Consequently AG×Y+CH×NG is less than QX×IP, and the ratio of AG×Y+CH×NG to QX×NG is less than the ratio of QX×IP to QX×NG, or of IP to NG. But the ratio of IP to NG is less than that of the velocity, which the point moving on EF has at I, to the velocity, which the point moving on AB has at G. Therefore the ratio of AG×Y+CH×NG to QX×NG, or that of AG×S+CH×R to QX×R, is also less than the ratio of the velocity at I to the velocity at G. Consequently, the ratio of the velocity at I to the velocity at G is not less than that of AG×S+CH×R to QT×R.

37. IF the points describing AB and CD move backwards together, the velocity at I will be the same, and the demonstration will proceed in like manner.

38. BUT if one of the points, as that moving on CD, recedes, while the other on AB advances forward, take in CD any fix'd point at pleasure Z; then the point on CD in respect of Z moves also forward. Again, take in the line EF, EΓ to AG as CZ to QT; then AG×CZ is = QT×EΓ; and AG×CH being = QT×EI, AG×HZ will be = QT×ΓI; and by the preceding case AG×S+ZH×R will be to QT×R as the velocity, wherewith the point moving on EF separates from Γ, when at I, to the velocity, which the point moving on AB has at G. But as AG is continually increasing, and EΓ keeps always in the same proportion to AG; the point Γ will itself be in motion, and the velocity of the point Γ will be to the velocity at G, as the line EΓ to AG, that is, as CZ to

QT, or as $CZ \times R$ to $QT \times R$; therefore the velocity, wherewith the point moving on EF, when at I, ſeparates from Γ, being to the velocity of the point moving on AB, when at G, as $AG \times S + ZH \times R$ to $QT \times R$; the abſolute velocity, which the point moving on EF has at I, will be to the abſolute velocity, which the point moving on AB has at G, as $AG \times S \backsim CH \times R$ to $QT \times R$; moving backwards,



when it ſeparates from Γ ſwifter than the point Γ itſelf moves, that is, when $AG \times S + ZH \times R$ is greater than $CZ \times R$, or $AG \times S$ greater than $CH \times R$; and when the point moving on EF, at I ſeparates from Γ with a ſlower motion, than that wherewith Γ moves, that is, when $CZ \times R$ is greater than $AG \times S + ZH \times R$, or $AG \times S$ leſs than $CH \times R$, the point moving on EF, at I advances forward.

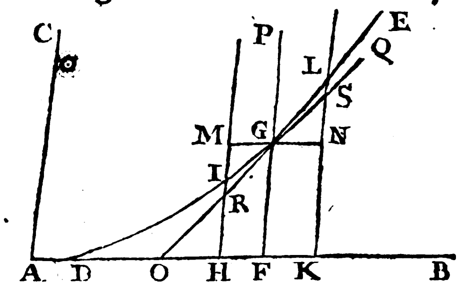
39. IN theſe demonſtrations the fluxions of lines only have been conſidered; but by theſe the fluxions of all other quantities are determined. For we have already obſerved, that the fluxions of ſpaces, whether ſuperficial or ſolid, are analogous to the velocities, wherewith lines are deſcribed, that augment in the ſame proportion with ſuch ſpaces.

40. THUS

40. THUS we have attempted to prove the truth of the rules, Sir Iſaac Newton has laid down, for finding the fluxions of quantities, by demonſtrating the two caſes, on which all the reſt depend, after a method, which from all antiquity has been allowed as genuine, and univerſally acknowledged to be free from the leaſt ſhadow of uncertainty.

41. We ſhall hereafter * endeavour to make manifeſt, that Sir Iſaac Newton's own demonſtrations are equally juſt with theſe here exhibited. But firſt we ſhall prove, that in all the applications of this doctrine to the ſolution of geometrical problems, no other conception concerning fluxions is neceſſary, than what we have here given. And for this end it will be ſufficient to ſhew, how fluxions are to be applied to the drawing of tangents to curve lines, and to the menſuration of curvilinear ſpaces.

42. IF upon the line AB be erected in any angle another ſtreight line AC, and it be put in motion upon the line AB towards B keeping always parallel to itſelf, and proceeding on with a uniform velocity: if a point alſo moves on the line AC with a velocity in any manner regulated; this point will deſcribe within the angle under CAB ſome third line DE, which will be a curve, unleſs the point moves in the line AC likewiſe with a uniform motion.



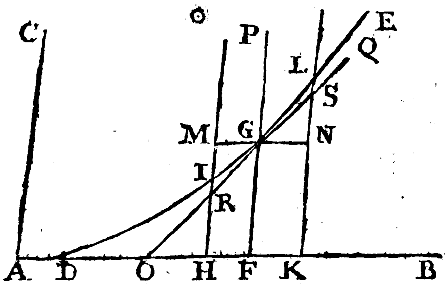
* §. 144.

43. Here,

43. Here, I say, the line AC being advanced to any situation FG, by what has already been written on the nature of fluxions, without any adventitious consideration whatever, a tangent may be assigned to the curve at the point G.

44. When the point moves on the line AC with an accelerating velocity, the curve DE will be convex to the abscisse DB. Now if two other situations HI and KL of the line AC be taken, one on each side FG, and MGN be drawn parallel to AB; while the line AC is moving from the situation HI to FG, the point in it will have moved through the length IM, and while the same line AC moves from FG to KL, the point in it will have passed over the length NL. And since the point moves with an accelerated velocity, IM will be less, and NL greater than the space, which would have been described in the same time by the velocity, the point has at G.

45. Let FO be taken to FG in the proportion of the velocity, wherewith the point F moves on the line AB, to the velocity, which the point moving on the line FP has at G, and the streight line OGQ be drawn cutting HI in R, and



KL in S; then FH will be to MR, and FK to NS in the same proportion. Therefore, from what has been said above, MR will be greater than MI, and NS less than NL; so that the line OQ, which unites

unites with the curve at the point G, lies on both sides the point G, on the same side of the curve; that is, it does not cross, or cut the curve (as geometers speak) but touches it only at the point G.

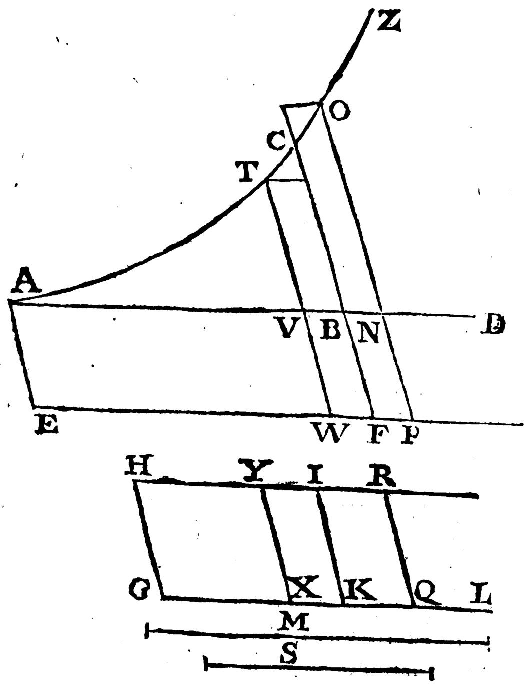
46. WHEN the point moves on the line AC with a velocity gradually decreasing, the curve will be concave towards the abscisse; but in this case the method of reasoning will be still the same.

47. IF the curve DE be the conical parabola, the latus rectum being T, and $T \times FG = DFq$, or $FG = \frac{DFq}{T}$; the fluxion of DF will be to the fluxion of $\frac{DFq}{T}$ (that is, the fluxion of FG) as T to 2 DF; therefore OF is to FG in the same proportion of T to 2 DF, or of DF to 2 FG, and OF is half DF.

48. IN like manner by the consideration of these velocities only may the mensuration of curvilinear spaces be effected.

49. SUPPOSE the curvilinear space ABC to be generated by the parallel motion of the line BC upon the line AD with a uniform velocity, within the space comprehended between the streight line AD and the curve line AZ; and let the parallelogram AEFB be generated with it by the motion of BF accompanying BC. Suppose another parallelogram GHIK to be generated at the same time by the motion of the line GH equal to AE or BF, insisting on the line GL in an angle equal to that under CBD; and let the motion of GH be so regulated, that the parallelogram GHIK be always equal to the

the curvilinear ſpace ABC. Then it is evident, by what has been ſaid above in our explanation of the nature of fluxions, that the velocity, wherewith the parallelogram EABF increaſes, is to the velocity, wherewith the parallelogram GHIK, or wherewith the curvilinear ſpace ABC increaſes; as the velocity, wherewith the point B moves, to the velocity, wherewith the point K moves.



50. Now I ſay, the velocity of the point B is to the velocity of the point K as BF to BC.

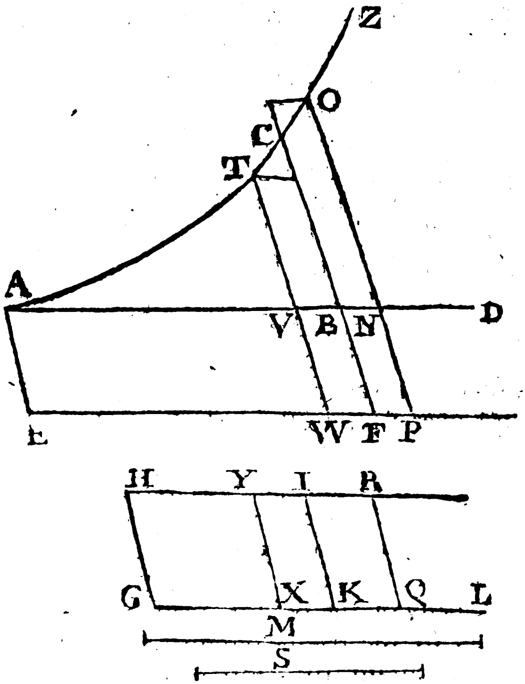
51. Suppose the curve line ACZ to recede farther and farther from AD; then it is evident, that while the parallelogram EABF augments uniformly, the curvilinear ſpace ABC will increaſe faſter and faſter;

faster; therefore in this case the point K moves with a velocity continually accelerated.

52. HERE, if possible, suppose the velocity of the point B to bear a less proportion to the velocity of the point K, than the ratio of BF to BC; that is, let the velocity of B be to the velocity of K, as BF to some line M greater than BC. Then it is possible to draw within the curve ACZ towards D a line, as ON, parallel to BC, which, though it exceed BC, shall be less than M; and the ratio of the velocity of the point B to the velocity of the point K, will be less than the ratio of BF to NO, or than the ratio of the parallelogram BP to the parallelogram BO; therefore still less than the ratio of the parallelogram BP to the space BCON. Farther let the parallelogram KIRQ be taken equal to the space BCON, then will the point K have moved from K to Q in the time, that the point B has moved from B to N. Now the parallelogram BP is to the parallelogram KR as BN to KQ, that is, as the velocity, wherewith the point B passes over BN, to the velocity, wherewith KQ would be described in the same time with a uniform motion. But as the point K moves with a velocity continually accelerated, its velocity at K is less than this uniform velocity now spoken of; therefore the velocity of the point B bears a greater proportion to the velocity of the point K than the parallelogram BP bears to the parallelogram KR; that is, than the parallelogram BP bears to the space BCON; but the first of these ratios was before found less than the last; which involves an absurdity. Therefore the velocity of B bears not to the velocity of K a less proportion than that of BF to BC.

53. AGAIN, if possible, let the velocity of B bear to the velocity of K a greater proportion than that of BF to BC, that is, the proportion of BF to some

ſome line S leſs than BC; and let the line TV be drawn parallel to CB, and greater than S, and the parallelogram TB be compleated. Here the ratio of the velocity of the point B to the velocity of the point K will be greater than the ratio of BF to TV, or than the ratio of the parallelogram BW to the parallelogram BT, therefore ſtill greater than the ratio of the parallelogram BW to the curvilinear



ſpace VTCB. Now if the parallelogram XYIK be taken equal to the ſpace VTCB, that the point deſcribing the line GL may have moved from X to K, while VT has moved to BC; ſince the parallelogram BW is to the parallelogram XI as VB to XK, that is, as the velocity, wherewith the point B has paſſed over VB, to the velocity, wherewith XK would

would be described in the same time with a uniform motion, the velocity of the point B bears a less proportion to the velocity of the point K, than the parallelogram BW bears to the parallelogram XI, because XK is described with an accelerating velocity: that is, the velocity of the point B bears a less proportion to the velocity of the point K, than the parallelogram BW bears to the space VTCB. But the first of those ratios was before found greater than the last. Therefore the velocity of B does not bear to the velocity of K a greater proportion than that of BF to BC.

54. IF the curve line ACZ were of any other form, the demonstration would still proceed in the same manner.

55. HENCE it appears, that nothing more is necessary towards the mensuration of the curvilinear space ABC, than to find a line GK so related to AB, that, while they are described together, the velocity of the point, wherewith AB is described, shall bear the same proportion at any place B to the velocity, wherewith the point describing the other line GK moves at the correspondent place K, as some given line AE bears to the ordinate BC of the curve ACZ.

56. THE method of finding such lines is the subject of Sir Isaac Newton's Treatise upon the Quadrature of Curves.

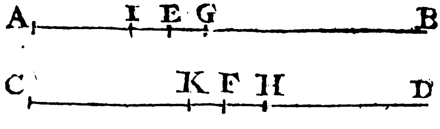
57. FOR example, if ACZ be a conical parabola as before, and $\Gamma \times BC = ABq$; taking $GK = \frac{ABc}{3\Gamma \times GH}$, the parallelogram $HK = \frac{ABc}{3\Gamma}$, $= \frac{1}{3}AB \times BC$, is equal to the space ABC; for GK being equal to $\frac{ABc}{3\Gamma \times GH}$, the fluxion of GK or the velocity

velocity, wherewith it is deſcribed at K, will be to the fluxion of AB, or the velocity, wherewith B moves, as $\frac{ABq}{\Gamma}$ or BC to GH or AE.

58. HAVING thus, as we conceive, ſufficiently explained, what relates to the proportions between the velocities wherewith magnitudes are generated; nothing now remains, before we proceed to the ſecond part of our preſent deſign, but to conſider the variations, to which theſe velocities are ſubject.

59. WHEN fluents are not augmented by a uniform velocity, it is convenient in many problems to conſider how theſe velocities vary. This variation Sir Iſaac Newton calls the fluxion of the fluxion, and alſo the ſecond fluxion of the fluent; diſtinguiſhing the fluxions, we have hitherto treated of, by the name of the firſt fluxions. The ſecond fluxions may alſo vary in different magnitudes of the fluent, and the variation of theſe is called the third fluxion of the fluent. Fourth fluxions are the changes to which the third are ſubject, and ſo on *.

60. In the two fluents AE and CF, whoſe fluxions we compared at §. 16, &c. where AE being denominated by x, CF was equal to $\frac{x^n}{a^{n-1}}$; and the fluxion of



AE bore to the fluxion of CF the proportion of a^{n-1} to nx^{n-1}. Here it is evident, that the antecedent a^{n-1} of this proportion being a fix'd quantity, and the conſequent nx^{n-1} a variable one; the fluxion

* Fluxionum (ſcilicet primarum) fluxiones ſeu mutationes magis aut minus celeres fluxiones ſecundas nominare licet, &c. Newt. Quadr. Curv. in Princip.

of

of AE does not bear to the fluxion of CF always the ſame proportion. If n be the number 2, the fluxion of AE is to the fluxion of CF as a to the variable quantity $2x$; and if n be the number 3, the fluxion of AE to that of CF will be as a^2 to $3x^2$. Therefore if AE be deſcribed with a uniform velocity, when n is any number greater than unite, CF is ſo deſcribed with a velocity continually accelerating, that when n is $= 2$, this velocity augments in the ſame proportion as CF itſelf increaſes; and when n is $= 3$, it augments in the duplicate of that proportion, &c.

61. HERE therefore we ſee, that while one quantity flows uniformly, the other is deſcribed with a varying motion; and the variation in this motion is called the ſecond fluxion of this quantity.

62. IT is evident farther, that in this inſtance, when n is $= 2$, the variation of the velocity is uniform; for the velocity keeping always in the ſame proportion to x, while x increaſes uniformly, the velocity muſt alſo increaſe after the ſame manner. But when n is $= 3$; ſince the velocity is every where as x^2, and x^2 does not increaſe uniformly; neither will the velocity augment uniformly. So that it appears by this example, that the variation in the velocity, wherewith magnitudes increaſe, may alſo vary, and this variation is called the third fluxion of the magnitude.

63. IN the ſame manner may the fluxions of the following orders be conceived; each order being the variation found in the preceding one. And the conſideration of velocities thus perpetually varying, and their variation itſelf changing, is a uſeful ſpeculation; for moſt, if not all, the bodies, we have any

acquaintance with, do actually move with velocities thus modified.

64. A STONE, for inſtance, in its direct fall towards the earth has its velocity perpetually augmented; and in Galileo's Theory of falling Bodies, when the whole deſcent is performed near the ſurface of the earth, it is ſuppoſed to receive equal augmentations of velocity in equal times. In this caſe therefore the velocity augments uniformly, and the ſecond fluxion of the line deſcribed by the falling body will in all parts of that line be the ſame; ſo that third fluxions cannot take place in this inſtance; ſince the variation of the velocity ſuffers no change, but is every where uniform.

65. BUT if the ſtone be ſuppoſed to have its gravity at the beginning of its fall leſs than at the ſurface of the earth, the variation of its velocity at firſt will then be leſs than the variation toward the end of its motion; or in other words, the ſecond fluxions in the beginning and end of its fall would be unequal; conſequently, third fluxions would here take place, ſince the variation would be ſwifter, as the body in its fall approached the earth.

66. THE ſtone in this laſt inſtance then not only moves with a velocity perpetually varying, as in the preceding example, but this variation continually changes. In the true theory of falling bodies, neither this laſt variation nor any ſubſequent one can ever be uniform; ſo that fluxions of every order do here actually exiſt.

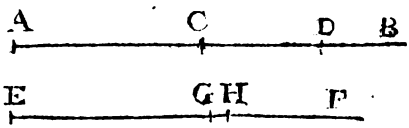
67. THE ſame is true of the motion of the planets in their elliptic orbs; of the motion of light at the confines of different mediums, and of the motion of all pendulous bodies.

68. In short, a uniform unchangeable velocity is not to be met with in any of those bodies, that fall under our cognizance; for in order to continue such a motion as this, it is necessary, that they should not be disturbed by any force whatever, either of impulse or resistance; but we know of no spaces, in which at least one of these causes of variation does not operate.

69. HAVING thus explained the general conception of second, third, and following fluxions; and having shewn, that they are applicable to the circumstances, which do really occur in all motion, we are acquainted with; we will now endeavour to declare the manner of assigning them.

70. AND in the first place second fluxions may be compared together as follows. Suppose any line to be so described by motion, that it always preserve the same analogy to the first fluxion of any magnitude; then the velocity, wherewith this line is described, that is, the fluxion of this line, will be analogous to the second fluxion of the aforesaid magnitude. For it is evident, that this line will perpetually alter in magnitude in the same proportion, as the fluxion, to which it is analogous, varies.

71. SUPPOSE AB to be a fluent described with a varying motion; the second fluxion at any one point C may be compared with the second fluxion at any other point D, by causing the line

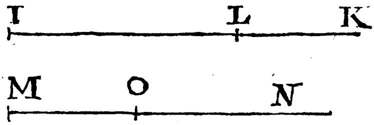


EF to be described by the motion of a point, so as to keep always the same analogy to the first fluxion of the fluent AB. Suppose EG be to EH, as the

 first

firſt fluxion at C to the firſt fluxion at D; then the ſecond fluxion at C will be to the ſecond fluxion at D, as the firſt fluxion of the line EF at G, to the firſt fluxion of the ſame at H.

72. In like manner, if another fluent IK be generated along with the former fluent AB, and alſo deſcribed with a variable motion; the ſecond fluxion of this latter fluent IK at any place L may be compared with the ſecond fluxion at any part of the former fluent AB, by deſcribing the line MN with ſuch a motion, as always to preſerve the ſame analogy to the firſt fluxion of the fluent IK, as the line EF bore to the firſt fluxion of AB. Suppoſe MO to be to EG, as the firſt fluxion of IK at L to the firſt fluxion of AB at C; then the ſecond fluxion at L will be to the ſecond fluxion at C, as the velocity, wherewith the line MN is deſcribed at O, to the velocity, wherewith the line EF is deſcribed at G.

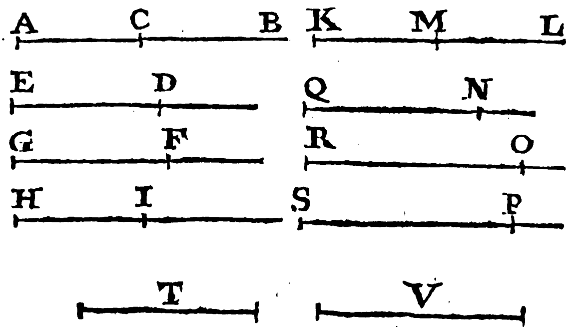


73. In the ſame manner if a line be deſcribed analogous to the ſecond fluxion of any magnitude, the fluxion of this line will expreſs the third fluxion of that magnitude, and ſo of all the other orders of fluxions.

74. In the next place the relation, in which the ſeveral orders of fluxions ſtand with regard to each other, will appear by the following propoſition.

75. Let the line AB be deſcribed by the motion of the point C moving with a varying velocity, and let a ſeries of lines be adapted to this line AB in ſuch manner, that the point D moving upon the firſt line of this ſeries at the ſame time with the point C, may ever

ever terminate a line ED analogous to the velocity of the point C; the point F at the same time terminating upon the second line of this series a line GF analogous to the velocity of the point D; and HI upon the third line being by the motion of the point I made ever analogous to the velocity of the point F; &c.



76. If now another line KL be described by the motion of the point M, and if a series of lines be adapted to this line KL in the like analogy by the motion of the points N, O, P, so that QN be to ED as the velocity of the point M to the velocity of the point C, RO to GF as the velocity of the point N to that of the point D, and SP to HI as the velocity of the point O to that of F; I say, that if the velocity of the point C has to the velocity of the point M always the same proportion at equal distances from A and K, that then the velocity of D to that of N will be in the duplicate of that proportion; the velocity of F to that of O in the triplicate of that proportion; the velocity of I to that of P in the quadruplicate of that proportion, and so on in the same order, as far as these series of lines are extended.

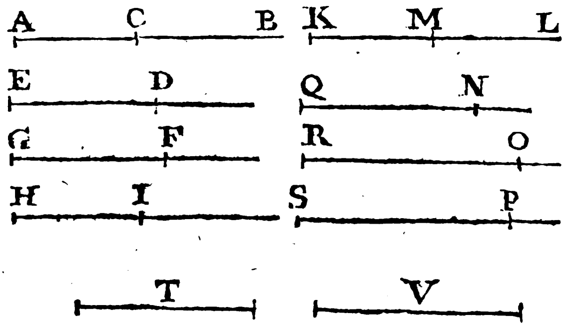
77. Suppose the velocity of the point C be always to the velocity of the point M, as the line T to the line V, when these points are at equal distances from A and K. Then, since the times, in which equal lines are described, are reciprocally as the velocities of the describing points; the time, in which AC receives any additional increment, will be to the time, in which KM shall have received an equal increment, as V to T.

78. Now ED is always to QN in the proportion of T to V. Therefore the variation, by increase or diminution, that ED shall receive to the like variation, which QN shall receive, while the lines AC, KM are augmented by equal increments, will be also as T to V. But the time, wherein ED will receive that variation, to the time, wherein QN will receive its variation, will be as V to T. Consequently, since the velocities, wherewith different lines are described, are as the lines themselves directly, and as the times of description reciprocally, the velocity of the point D to that of the point N will be in the duplicate ratio of T to V.

79. Again, the velocity of D being to the velocity of N, when AC and KM are equal, always in the same duplicate ratio of T to V, and GF being always to RO as the velocity of the point D to the velocity of the point N, the variation, by increase or diminution, of the line GF to the like variation of RO, while AC and KM receive equal augmentation, will also be as the velocity of D to the velocity of N, that is in the duplicate ratio of T to V. But the time, in which the line GF receives its variation, will be to the time, in which RO receives its variation, as V to T. Hence the velocity of the point F will

will be to the velocity of the point O in the triplicate ratio of T to V.

80. AFTER the fame manner, the velocity of the point I will appear to have to the velocity of the point P the quadruplicate of the ratio of T to V.



81. BUT from what we have faid above, it is evident, that the velocity of the point D is to the velocity of the point N, as the fecond fluxion of AC to the fecond fluxion of KM; the velocity of the point F to the velocity of the point O, as the third fluxion of AC to the third fluxion of KM; and the velocity of the point I to the velocity of the point P, as the fourth fluxion of AC to the fourth fluxion of KM. And hence appears the truth of Sir Ifaac Newton's obfervation at the end of the firft propofition of his book of Quadratures, that a fecond fluxion, and the fecond power of a firft fluxion, or the product under two firft fluxions; a third fluxion, and the third power of a firft, or the product under a firft and fecond, and fo on; are homologous terms in any equation. For, as it appears by this propofition, that if the velocity, wherewith any fluent is augmented, be in any proportion increafed; its fecond fluxion will increafe in the duplicate of that

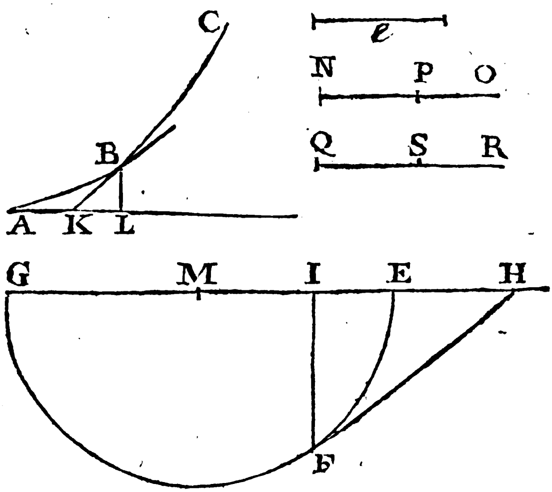
proportion, the third fluxion in the triplicate, and the fourth fluxion in the quadruplicate of that ſame proportion; it is manifeſt, that the terms in any equation, that ſhall involve a ſecond fluxion, will preſerve always the ſame proportion to the terms involving the ſecond power of a firſt fluxion, or the product of two firſt fluxions; the terms involving a third fluxion will preſerve the ſame proportion to the terms involving the third power of a firſt, or the product of a firſt and ſecond, or the product of three firſt fluxions; and the terms containing a fourth fluxion will keep the ſame proportion to the terms containing the fourth power of a firſt, the product of a ſecond and the ſecond power of a firſt, the ſecond power of a ſecond, or the product of a firſt and third; &c. however be increaſed or diminiſhed the firſt fluxion, or the velocity, wherewith the fluents augment.

82. IN the problems concerning curve lines, which relate to the degree of curvature in any point of thoſe curves, or to the variation of their curvature in different parts, theſe ſuperior orders of fluxions are uſeful; for by the inflexion of the curve, whilſt its abſciſſe flows uniformly, the fluxion of the ordinate muſt continually vary, and thereby will be attended with theſe ſuperior orders of fluxions.

83. FOR example, were it required to compare the different degrees of curvature either of different curves, or of the ſame curve in different parts, and in order thereto a circle ſhould be ſought, whoſe degree of curvature might be the ſame with that of any curve propoſed, in any point, that ſhould be aſſigned; ſuch a circle may be found by the help of ſecond fluxions. When the abſciſſes of two curves flow with equal velocity; where the ordinates have equal firſt fluxions, the tangents make equal angles

gles with their respective ordinates. If now the second fluxions of these ordinates are also equal, the curves in those points must be equally deflected from their tangents, that is, have equal degrees of curvature. Upon this principle such circles, as have here been mentioned, may be found by the following method.

84. THE curve ABC being given, let it be required to find a circle equally incurvated with this curve at the point B. Suppose EFG to be this circle, in which the tangent FH at the point F makes with the ordinate FI the same angle, as the tangent



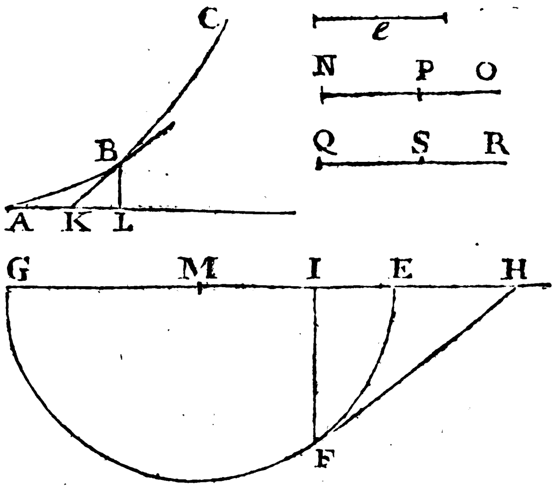
BK, drawn to the other curve ABC at the point B, makes with the ordinate BL of that curve. Now if the two abscisses AL and EI are described with equal velocities, the first fluxions of the ordinates LB and IF will be equal; and therefore, if the two curves are equally incurvated at the points B and F, the

the second fluxions of these ordinates will be also equal. If M be the center of the circle EFG, and ME be denoted by a and MI by x, IF will be $=\sqrt{aa-xx}$; and, by the rules for finding fluxions, the first fluxion of IF will be to the fluxion of MI, or of x, as $\dot{x}$ to $\sqrt{aa-xx}$.

85. Now suppose the line NO to be so described, that the fluxion of MI, or of x, shall be to the first fluxion of IF, as some given line e to NP in the line NO, then will NP be $=\frac{ex}{\sqrt{aa-xx}}$. Suppose likewise the line QR to be so described, that the fluxion of AL in the curve ABC shall be to the first fluxion of LB, as the same given line e to QS in the line QR. Here the first fluxions of IF and LB being equal, NP and QS are equal. And since the second fluxions of IF and LB are equal, the fluxions of NP and QS are also equal. But NP was $=\frac{ex}{\sqrt{aa-xx}}$, and by the rules for finding fluxions, the fluxion of NP will be to the fluxion of MI as eaa to $\overline{aa-xx}^{\frac{3}{2}}$, that is, as $e\times EMq$ to IFc. Therefore in the curve ABC the fluxion of QS to the fluxion of AL will be in the same proportion of $e\times EMq$ to IFc. Hence by finding first QS, then its fluxion, from the equation expressing the nature of the curve ABC, the proportion of $e\times EMq$ to IFc will be given. Consequently the proportion of e to IF will be also given, because the ratio of EMq to IFq is the same with the given ratio of HFq to HIq, or of KBq to KLq. And hereby the circle EFG will be given, whose curvature is equal to the curvature of the curve ABC at the point B.

86. Sup-

86. SUPPOSE the curve ABC to be the conical parabola, where ALq shall be equal to $\gamma \times LB$, γ being the latus rectum of the axis. Here e will be to QS as γ to $2AL$; for that is the ratio of the fluxion of AL to the fluxion of BL: therefore QS is $=\frac{2e}{\gamma} AL$, and consequently the fluxion of QS to the fluxion of AL (that is $e \times EMq$ to IFc) as $2e$ to γ, or as $2e \times EMq$ to $\gamma \times EMq$; in so much that IFc is $=$



$\frac{1}{2}\gamma \times EMq$, and the given ratio of IFq to EMq (namely the ratio of KLq to KBq) is the same with the ratio of $\frac{1}{2}\gamma$ to IF: that is, IF is equal to half the latus rectum appertaining to the diameter of the parabola, whose vertex is the point B.

87. THIS is all we think necessary towards giving a just and clear idea of the nature of fluxions, and for proving the certainty of the deductions made from

from them. For it muſt now be manifeſt to every reader, that mathematical quantities become the proper object of this doctrine of fluxions, whenever they are ſuppoſed to increaſe by any continued motion of prolongation, dilatation, expanſion or other kind of augmentation, provided ſuch augmentation be directed by ſome general rule, whence the meaſure of the increaſe of theſe quantities may from time to time be eſtimated. And when different homogeneous magnitudes increaſe after this manner together, one may vary faſter than another. Now the velocity of increaſe in each quantity, is the fluxion of that quantity. This is the true interpretation of Sir Iſaac Newton's appellation of fluxions, Incrementorum velocitates. For this doctrine does not ſuppoſe the fluents themſelves to have any motion. Fluxions are not the velocities, with which the fluents, or even the increments, which thoſe fluents receive, are themſelves moved; but the degrees of velocity, wherewith thoſe increments are generated. Subjects incapable of local motion, ſuch as fluxions themſelves, may alſo have their fluxions. In this we do not aſcribe to theſe fluxions any actual motion; for to aſcribe motion, or velocity to what is itſelf only a velocity, would be wholly unintelligible. The fluxion of another fluxion is only a variation in the velocity, which is that fluxion. In ſhort, light, heat, ſound, the motion of bodies, the power of gravity, and whatever elſe is capable of variation, and of having that variation aſſigned, for this reaſon comes under the preſent doctrine; nothing more being underſtood by the fluxion of any ſubject, than the degree of ſuch its variation.

88. TO aſſign the velocities of variation or increaſe in different homogeneous quantities, it is neceſſary to compare the degrees of augmentation, which

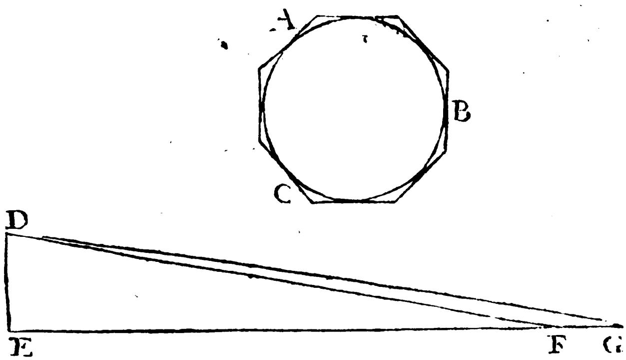
which thoſe quantities receive in equal portions of time; and in this doctrine of fluxions no farther uſe is made of ſuch increments: for the application of this doctrine to geometrical problems depends upon the knowledge of theſe velocities only. But the conſideration of the increments themſelves may be made ſubſervient to the like uſes upon other principles; the explanation of which leads us to the ſecond part of our deſign.

OF PRIME and ULTIMATE RATIOS.

89. THE primary method of comparing together the magnitudes of rectilinear ſpaces is by laying them one upon another: thus all the right lined ſpaces, which in the firſt book of Euclide are proved to be equal, are the ſum or difference of ſuch ſpaces, as would cover one another. This method cannot be applied in comparing curvilinear ſpaces with rectilinear ones; becauſe no part whatever of a curve line can be laid upon a ſtreight line, ſo as wholly to coincide with it. For this purpoſe therefore the ancient geometers made uſe of a method of reaſoning, ſince commonly called the method of exhauſtions; which conſiſts in deſcribing upon the curvilinear ſpace a rectilinear one, which though not equal to the other, yet might differ leſs from it than by any the moſt minute difference whatever, that ſhould be propoſed; and thereby proving, the two ſpaces, they would compare, could be neither greater nor leſs than each other.

90. FOR example, in order to prove the equality between the ſpace comprehended within the circumference of a circle, and a triangle, whoſe baſe ſhould be equal to the circumference of that circle, and its altitude to the ſemdiameter, Archimedes takes this method.

method. About the circle he deſcribes a polygon as ABC, and makes it appear, that by multiplying the ſides of this polygon, there may at length be deſcribed ſuch a one, as ſhall exceed the circle leſs than by any difference, that ſhall be propoſed, how minute ſoever that difference be. By this means it was eaſy to prove, that the triangle DEF, whoſe baſe EF ſhould be equal to the circumference of the

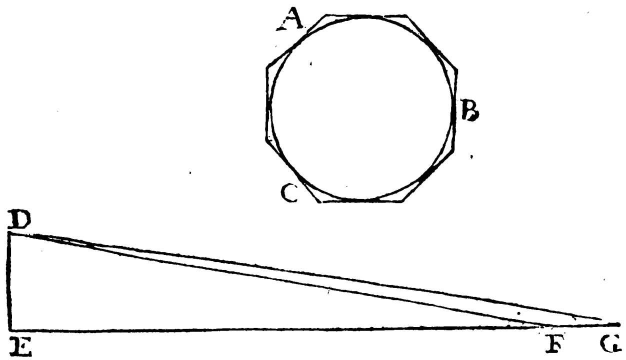


circle, and altitude ED equal to the ſemidiameter, is not greater than the circle. For were it greater, how ſmall ſoever be the exceſs, it were poſſible to deſcribe about the circle a polygon leſs than the triangle; but the circumference of the polygon is greater than the circumference of the circle, therefore the polygon can never be leſs, but muſt be always greater than the triangle; for the polygon is equal to a triangle, whoſe altitude is the ſemidiameter of the circle, and baſe equal to the circumference of the polygon. It appears therefore impoſſible for the triangle DEF to be greater than the circle.

91. THUS far Archimedes makes uſe of the polygon circumſcribing the circle and no farther: but inſcribing another within the circle he proves, by a ſimilar proceſs of reaſoning, that it is impoſſible for the

the triangle to be leſs than the circle; becauſe if it were leſs, a polygon might be inſcribed within the circle greater than this triangle, which he alſo proves to be impoſſible; whereby at length it becomes certain, that the triangle DEF is neither greater nor leſs than the circle, but equal to it.

92. However, the triangle may be proved not to be leſs than the circle by the circumſcribed polygon alſo. For were it leſs, another triangle DEG, whoſe



baſe EG is greater than EF, might be taken, which ſhould not be greater than the circle. But a polygon can be circumſcribed about the circle, the circumference of which ſhall exceed the circumference of the circle by leſs than any line, that can be named, conſequently by leſs than FG, that is, the circumference of the polygon ſhall be leſs than EF, and the polygon leſs than the triangle DEG; therefore it is impoſſible, that this triangle ſhould not exceed the circle, ſince it is greater than the polygon: conſequently the triangle DEF cannot be leſs than the circle.

93. THUS the circle and triangle may be proved to be equal by comparing them with one polygon

polygon only, and Sir Isaac Newton has instituted upon this principle a briefer method of conception and expression for demonstrating this sort of propositions, than what was used by the ancients; and his ideas are equally distinct, and adequate to the subject, with theirs, though more complex. It became the first writers to choose the most simple form of expression, and the least compounded ideas possible. But Sir Isaac Newton thought, he should oblige the mathematicians by using brevity, provided he introduced no modes of conception difficult to be comprehended by those, who are not unskilled in the ancient methods of writing.

94. THE concise form, into which Sir Isaac Newton has cast his demonstrations, may very possibly create a difficulty of apprehension in the minds of some unexercised in these subjects. But otherwise his method of demonstrating by the prime and ultimate ratios of varying magnitudes is not only just, and free from any defect in itself; but easily to be comprehended, at least by those who have made these subjects familiar to them by reading the ancients.

95. IN this method any fixed quantity, which some varying quantity, by a continual augmentation or diminution, shall perpetually approach, but never pass, is considered as the quantity, to which the varying quantity will at last or ultimately become equal; provided the varying quantity can be made in its approach to the other to differ from it by less than by any quantity how minute soever, that can be assigned *.

96. UPON this principle the equality between the fore-mentioned circle and triangle DEF is at once

* Princ. Philos. Lib. I. Lem. 1.

 dedu-

deducible. For, ſince the polygon circumſcribing the circle approaches to each according to all the conditions above ſet down, this polygon is to be conſidered as ultimately becoming equal to both, and conſequently they muſt be eſteemed equal to each other.

97. That this is a juſt concluſion, is moſt evident. For if either of theſe magnitudes be ſuppoſed leſs than the other, this polygon could not approach to the leaſt within ſome aſſignable diſtance.

98. RATIOS alſo may ſo vary, as to be confined after the ſame manner to ſome determined limit, and ſuch limit of any ratio is here conſidered as that, with which the varying ratio will ultimately coincide *.

99. From any ratio's having ſuch a limit, it does not follow, that the variable quantities exhibiting that ratio have any final magnitude, or even limit, which they cannot paſs.

100. For ſuppoſe two magnitudes, B and B+A, whoſe difference ſhall be A, are each of them perpetually increaſing by equal degrees. It is evident, that if A remains unchanged, the proportion of B+A to B is a proportion, that tends nearer and nearer to the proportion of equality, as B becomes larger; it is alſo evident, that the proportion of B+A to B may, by taking B of a ſufficient magnitude, be brought at length nearer to the proportion of equality, than to any other aſſignable proportion; and conſequently the ratio of equality is to be conſidered as the ultimate ratio of B+A to B. The ultimate proportion then of theſe quantities is

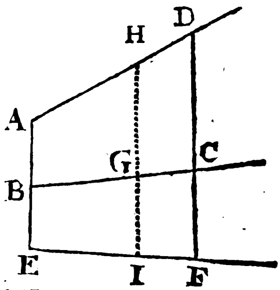
* Princ. Philoſ. Lib. I. Lem. 1.

here

here assigned, though the quantities themselves have no final magnitude.

101. THE same holds true in decreasing quantities.

102. THE quadrilateral ABCD bears to the quadrilateral EBCF the proportion of AB+DC to BE+CF, provided the two lines AE and DF are parallel. Now if the line DF be drawn nearer to AE, this proportion of AB+DC to BE+CF will not remain the same, unless the lines DA, CB, FE produced will meet in the same point; and this proportion, by diminishing the distance between DF and AE, may at last be brought nearer to the proportion of AB to BE, than to any other whatever. Therefore the proportion of AB to BE is to be considered as the ultimate proportion of AB+DC to BE+CF, or as the ultimate proportion of the quadrilateral ABCD to the quadrilateral EBCF.



103. HERE these quadrilaterals can never bear one to the other the proportion between AB and BE, nor have either of them any final magnitude, or even so much as a limit, but by the diminution of the distance between DF and AE they diminish continually without end: and the proportion between AB and BE is for this reason called the ultimate proportion of the two quadrilaterals; because it is the proportion, which those quadrilaterals can never actually have to each other, but the limit of that proportion.

104. THE quadrilaterals may be continually diminished, either by dividing BC in any known propor-

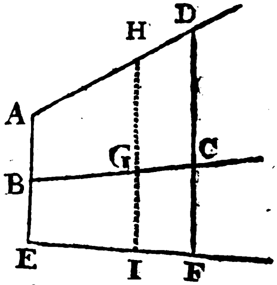
tion in G drawing HGI parallel to AE, by dividing again BG in the like manner, and by continuing this division without end; or else the line DF may be supposed to advance towards AE with an uninterrupted motion, 'till the quadrilaterals quite disappear, or vanish. And under this latter notion these quadrilaterals may very properly be called vanishing quantities, since they are now considered, as never having any stable magnitude, but decreasing by a continued motion, 'till they come to nothing. And since the ratio of the quadrilateral ABCD to the quadrilateral BEFC, while the quadrilaterals diminish, approaches to that of AB to BE in such manner, that this ratio of AB to BE is the nearest limit, that can be assigned to the other; it is by no means a forced conception to consider the ratio of AB to BE under the notion of the ratio, wherewith the quadrilaterals vanish; and this ratio may properly be called the ultimate ratio of two vanishing quantities.

105. The reader will perceive, I am endeavouring to explain Sir Isaac Newton's expression Ratio ultima quantitatum evanescentium; and I have rendered the Latin participle evanescens, by the English one vanishing, and not by the word evanescent, which having the form of a noun adjective, does not so certainly imply that motion, which ought here to be kept carefully in mind. The quadrilaterals ABCD, BEFC become vanishing quantities from the time, we first ascribe to them this perpetual diminution; that is, from that time they are quantities going to vanish. And as during their diminution they have continually different proportions to each other; so the ratio between AB and BE is not to be called merely Ratio harum quantitatum evanescentium, but Ultima ratio *.

* Vid. Princ. Philos. Lib. I. Lem. xi. in Schol.

106. Should

106. SHOULD we ſuppoſe the line DF firſt to coincide with the line AE, and then recede from it, by that means giving birth to the quadrilaterals; under this conception the ratio of AB to BE may very juſtly be conſidered as the ratio, wherewith the quadrilaterals by this motion commence; and this ratio may alſo properly be called the firſt or prime ratio of theſe quadrilaterals at their origine.



107. HERE I have attempted to explain in like manner the phraſe Ratio prima quantitatum naſcentium; but no Engliſh participle occurring to me, whereby to render the word naſcens, I have been obliged to uſe circumlocution. Under the preſent conception of the quadrilaterals they are to be called naſcentes, not only at the very inſtant of their firſt production, but according to the ſenſe, in which ſuch participles are uſed in common ſpeech, after the ſame manner, as when we ſay of a body, which has lain at reſt, that it is beginning to move, though it may have been ſome little time in motion: on this account we muſt not uſe the ſimple expreſſion Ratio quantitatum naſcentium; for by this we ſhall not ſpecify any particular ratio; but to denote the ratio between AB and BE we muſt call it Ratio prima quadrilaterum naſcentium*.

108. WE ſee here the ſame ratio may be called ſometimes the prime, at other times the ultimate ratio of the ſame varying quantities, as theſe quantities are conſidered either under the notion of vaniſhing, or

* Vid. Ibid.

of being produced before the imagination by an uninterrupted motion. The doctrine under examination receives its name from both these ways of expression.

109. THUS we have given a general idea of the manner of conception, upon which this doctrine is built. But as in the former part of this discourse we confirmed the doctrine of fluxions by demonstrations of the most circumstantial kind; so here, to remove all distrust concerning the conclusiveness of this method of reasoning, we shall draw out its first principles into a more diffusive form.

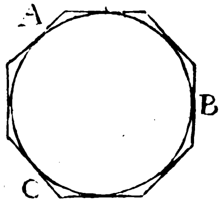
110. FOR this purpose, we shall in the first place define an ultimate magnitude to be the limit, to which a varying magnitude can approach within any degree of nearness whatever, though it can never be made absolutely equal to it.

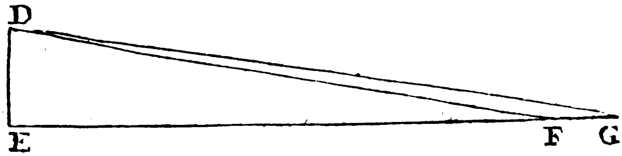
111. THUS the circle discoursed of above, according to this definition, is to be called the ultimate magnitude of the polygon circumscribing it; because this polygon, by increasing the number of its sides, can be made to differ from the circle, less than by any space, that can be proposed, how small soever; and yet the polygon can never become either equal to the circle or less.

112. IN like manner the circle will be the ultimate magnitude of the polygon inscribed, with this difference only, that as in the first case the varying magnitude is always greater, here it will be less than the ultimate magnitude, which is its limit.

113. AGAIN, if EG, the base of the triangle DEG, be supposed always equal to the circumference of the polygon, the triangle DEF will be the ultimate

mate magnitude of the triangle DEG, ſince the baſe EG will conſtantly be greater than the baſe EF, equal to the circumference of the circle only, and yet EG may be made to approach EF nearer than by any difference, that can be named.





114. UPON this definition we may ground the following propoſition; That, when varying magnitudes keep conſtantly the ſame proportion to each other, their ultimate magnitudes are in the ſame proportion.

115. LET A and B be two varying magnitudes, which keep conſtantly in the ſame proportion to each other; and let C be the ultimate magnitude of A, and D the ultimate magnitude of B. I ſay, that C is to D in the ſame proportion.

116. As A is a varying magnitude continually approaching to C, but can never become equal to it, A may be either always greater or always leſs than C. In the firſt place ſuppoſe it greater. When A is greater than C, in approaching to C it is continually diminiſhed; therefore B keeping always in the ſame proportion to A, B in approaching to its limit D is alſo continually diminiſhed.

A. B.
C. D.
E.

117. Now, if possible, let the ratio of C to D be greater than that of A to B, that is, suppose C to have to some magnitude E, greater than D, the same proportion as A has to B. Since C is the ultimate magnitude of A in the sense of the preceding definition, A can be made to approach nearer to C than by any difference, how small soever that shall be proposed, but can never become equal to it, or less. Therefore, since C is to E as A to B, B will always exceed E; consequently can never approach to D so near as by the excess of E above D: which is absurd. For since D is supposed the ultimate magnitude of B, it can be approached by B nearer than by any assigned difference.

118. After the same manner, neither can the ratio of D to C be greater than that of B to A.

119. If the varying magnitude A be less than C, it follows, in like manner, that neither the ratio of C to D can be less than that of A to B, nor the ratio of D to C less than that of B to A.

120. It is an evident corollary from this proposition, that the ultimate magnitudes of the same or equal varying magnitudes are equal.

121. NOW from this proposition the fore-mentioned equality between the circle and triangle DEF will again readily appear. For the circle being the ultimate magnitude of the polygon, and the triangle DEF the ultimate magnitude of the triangle DEG, when the polygon and the triangle DEG are equal, by this proposition the circle and triangle DEF will be also equal.

122. If the preceding proposition be admitted, as a genuine deduction from the definition, upon which it is grounded; this demonstration of the equality of the circle and triangle cannot be excepted to. For no objection can lie against the definition itself, as no inference is there deduced, but only the sense explained of the term defined in it.

123. THE other part of this method, which concerns varying ratios, may be put into the same form by defining ultimate ratios, as follows.

124. If there be two quantities, that are (one or both) continually varying, either by being continually augmented, or continually diminished; though the proportion, they bear to each other, should by this means perpetually vary, but in such a manner, that it constantly approaches nearer and nearer to some determined proportion, and can also be brought at length in its approach nearer to this determined proportion than to any other, that can be assigned, but can never pass it: this determined proportion is then called the ultimate proportion, or the ultimate ratio of those varying quantities.

125. TO this definition of the sense, in which the term ultimate ratio, or ultimate proportion is to be understood, we must subjoin the following proposition: That all the ultimate ratios of the same varying ratio are the same with each other.

126. Suppose the ratio of A to B continually varies by the variation of one or both of the terms A and B. If the ratio of C to D be the ultimate ratio of A to B, and the ratio of E to F be likewise the ultimate ratio of the same; I say, the ratio of C to D is the same with the ratio of E to F.

127. If poſſible, let the ratio of E to F differ from that of C to D. Since the ratio of C to D is the ultimate ratio of A to B, the ratio of A to B, in its approach to that of C to D, can be brought at length nearer to it, than to any other whatever. Therefore if the ratio of E to F differ from that of C to D, the ratio of A to B will either paſs that of E to F, or can never approach ſo near it, as to the ratio of C to D: in ſo much that the ratio of E to F cannot be the ultimate ratio of A to B, in the ſenſe of this definition.

128. THE two definitions here ſet down, together with the general propoſitions annexed to them, comprehend the whole foundation of this method, we are now explaining.

129. We find in former writers ſome attempts toward ſo much of this method, as depends upon the firſt definition. Lucas Valerius in a moſt excellent treatiſe on the center of gravity of ſolid bodies, has given a propoſition nothing different, but in the form of the expreſſion, from that we have ſubjoined to our firſt definition; from which he demonſtrates with brevity and elegance his propoſitions concerning the menſuration and center of gravity of the ſphere, ſpheroid, parabolical and hyperbolical conoids. This author writ before the doctrine of indiviſibles was propoſed to the world. And ſince, Andrew Tacquet, in his treatiſe on the cylindrical and annular ſolids, has made the ſame propoſition, though ſomething differently expreſſed, the baſis of his demonſtrations, at the ſame time very judiciouſly expoſing the inconcluſiveneſs of the reaſonings from indiviſibles. However, the conſideration of the limits of varying proportions, when the quantities expreſſing thoſe proportions have themſelves no limits, which

which renders this method of prime and ultimate ratios much more extenſive, we owe intirely to Sir Iſaac Newton. That this method, as thus compleated, is applicable not only to the ſubjects treated by the ancients in the method of exhauſtions, but to many others alſo of the greateſt importance, appears from our author's immortal treatiſe on the Mathematical principles of natural philoſophy.

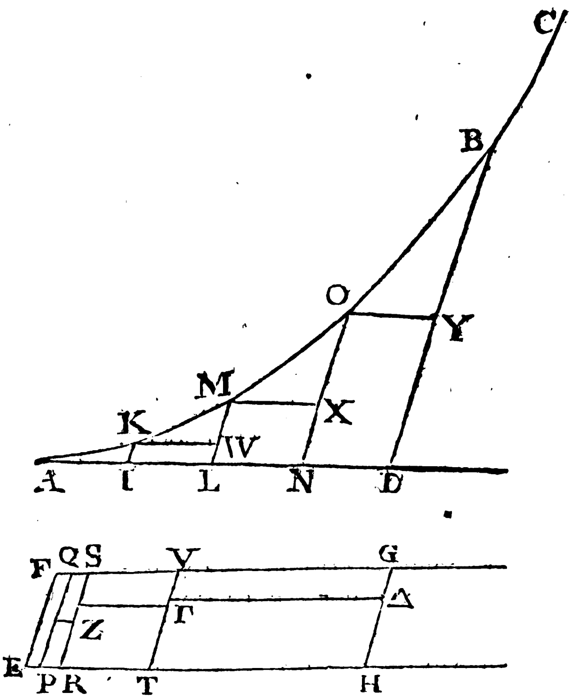
130. HOWEVER, we ſhall farther illuſtrate this doctrine in applying it to the ſame general problems as thoſe, whereby the uſe of fluxions was above exemplified.

131. We have already given one inſtance of its uſe in determining the dimenſions of curvilinear ſpaces; we ſhall now ſet forth the ſame by a more general example.

132. Let ABC be a curve line, its abſciſſe AD, and an ordinate DB. If the parallelogram EFGH, formed upon the given line EF under the ſame angle, as the ordinates of the curve make with its abſciſſe, be in all parts ſo related to the curve, that the ultimate ratio of any portion of the abſciſſe AD to the correſpondent portion of the line EH, ſhall be that of the given line EF to the ordinate of the curve at the beginning of that portion of the abſciſſe; then will the curvilinear ſpace ADB be equal to the parallelogram EG.

133. In the curve let the abſciſſe AD be divided into any number of equal parts AI, IL, LN, ND, and let the ordinates IK, LM, NO be drawn, and alſo in the parallelogram EG the correſpondent lines PQ, RS and TV. In the curve compleat the parallelograms IW, LX, NY, and in the parallelogram EG make the parallelogram PZ equal to the paral-

parallelogram IW, the parallelogram RΓ equal to LX, and the parallelogram TΔ equal to NY: then the whole figure IKWMXOYD will be equal to the whole figure PZΓΔH. But in the curve, by increasing the number and diminishing the breadth of these parallelograms, the figure IKWMXOYD



will approach nearer and nearer in magnitude to the curvilinear space ADB; in so much that their difference may be reduced to less than any space, that shall be assigned; therefore the curvilinear space ADB is the ultimate magnitude of the figure IKWMXOYD. Farther, since the parallelogram EG is in all parts so related to the curve, that the ultimate ratio of every por-

portion, as LN, of the abſciſſe AD to RT, the correſpondent portion of EH, is the ſame with the ratio of EF or RS, to LM; the ultimate ratio of the parallelogram LX, or its equal RΓ, to the parallelogram RV, is the ratio of equality. This is alſo true of all the other correſpondent parallelograms; therefore, the ultimate ratio of the figure PZΓΔH to the parallelogram PG is the ratio of equality; that is, the figure PZΓΔH, by increaſing the number of its parallelograms, can be brought nearer to the parallelogram PG than by any difference whatever, that may be propoſed. Moreover, by increaſing the number of ordinates in the curve, the reſiduary portion AI of the abſciſſe can be reduced to leſs than any magnitude, that ſhall be propoſed; whereby the parallelogram EQ, correſponding to this portion of the abſciſſe, may be alſo reduced to leſs than any magnitude, whatever propoſed; and the parallelogram PG be brought to differ leſs from EG than by any aſſigned magnitude how ſmall ſoever. Since therefore the figure PZΓΔH can be brought nearer to the parallelogram PG than by any difference, that can be aſſigned; the figure PZΓΔH can be brought alſo nearer to the parallelogram EG than by any difference that can be aſſigned. Conſequently the parallelogram EG is the ultimate magnitude of the figure PZΓΔH. Therefore the figures PZΓΔH and IKWMXOYD being equal varying magnitudes, and the ultimate magnitudes of equal varying magnitudes being equal, the curvilinear ſpace ADB is equal to the parallelogram EG.

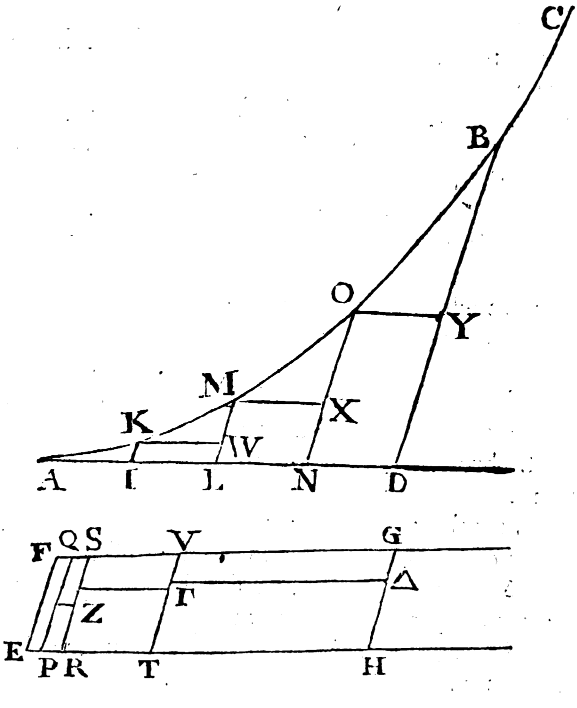
134. SUPPOSE the curve ABC were a cubical parabola convex to the abſciſſe, that is, ſuppoſe Θ a given line, and $\Theta q \times LM = AL^c$. If EH be $= \frac{ADqq}{4\Theta q \times EF}$, then the parallelogram EG will be equal to the ſpace ADB.

As

135. As EH is $= \frac{ADqq}{4\Theta q \times EF}$, ER will be $= \frac{ALqq}{4\Theta q \times EF}$ and ET $= \frac{ANqq}{4\Theta q \times EF}$, consequently RT $=$

$$\frac{ALc \times LN + \frac{3}{2} ALq \times LNq + AL \times LNc + \frac{1}{4} LNqq}{\Theta q \times EF}.$$

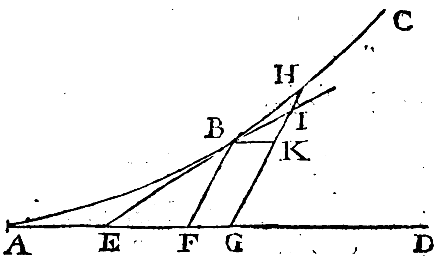
Therefore the parallelogram EG is here so related in



all parts to the curve, that LN is to RT as $\Theta q \times EF$ to $ALc + \frac{3}{2} ALq \times LN + AL \times LNq + \frac{1}{4} LNc$. Now it is evident, that the ratio of LN to RT can never be so great as the ratio of $\Theta q \times EF$ to ALc; but yet, by diminishing LN, the ratio of LN to RT may at length be brought nearer to this ratio than to any other whatever, that ſhould be propoſed. Conſequently by the preceding definition of what is to be underſtood by an ultimate ratio, the ratio of $\Theta q \times EF$ to ALc is the ultimate ratio of LN to RT. But ALc being $= \Theta q \times LM$, $\Theta q \times EF$ is to ALc as EF to LM. Therefore the ratio of EF to LM is the ultimate ratio of LN to RT. Conſequently, by the preceding general propoſition, the parallelogram EG is equal to the curvilinear ſpace ADB. And this parallelogram is equal to $\frac{1}{4}AD \times DB$.

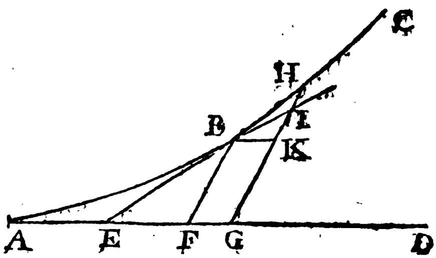
136. AGAIN, this method is equally uſeful in determining the ſituation of the tangents to curve lines.

137. IN the curve ABC, whoſe abſciſſe is AD, let EB be a tangent at the point B. Let BF be the ordinate at the ſame point B, and GH another ordinate parallel to it, which ſhall meet the tangent in I, and the line BK, parallel to the abſciſſe AD, in K. Here the ratio of HK, the difference of the ordinates, to BK can never be the ſame with the ratio of BF



to FE, unleſs by the figure of the curve the tangent chance to cut it in ſome point remote from B; this ratio of BF to FE being the ſame with that of IK to KB. But it is farther evident, that the nearer GH is to FB, the ratio of KH to KB will approach ſo much the nearer to the ratio of IK to KB; and the angle, which the curve BC makes with the tangent

tangent BI being less than any right-lined angle, it is manifest, that GH may be made to approach towards FB, 'till the ratio of HK to KB, shall at length approach nearer to the ratio of IK to KB, or of BF to FE, than to any other ratio whatever, that shall be proposed; that is, the ratio of BF to FE is the ultimate ratio of HK to KB. Therefore, if from the properties of the curve ABC the ratio of HK to KB be determined, and from thence their ultimate ratio assigned; this ratio thus assigned will be the ratio of BF to FE; because all the ultimate ratios of the same variable ratio are the same with each other.



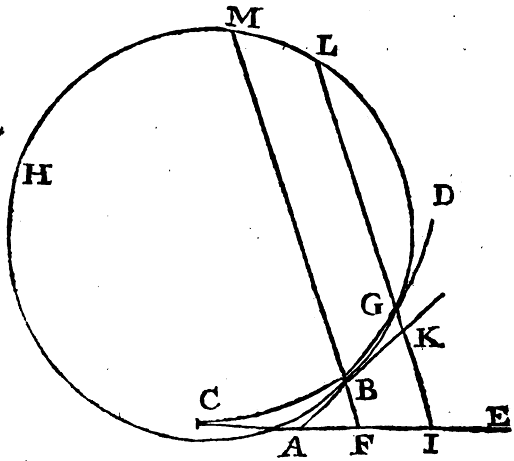
138. Suppose the curve ABC again to be a cubical parabola, where BF is $= \frac{AFc}{Zq}$, and GH $= \frac{AGc}{Zq}$. Here HK will be $= \frac{3AF \times AG \times FG + FGc}{Zq}$; therefore HK is to FG, or BK, as $3AF \times AG +$ FGq to Zq. Consequently the ratio of HK to BK can never be so small as the ratio of 3AFq to Zq; but by diminishing BK it may be brought nearer to that ratio than to any other determinate ratio whatever; that is, the ratio of 3AFq to Zq is the ultimate ratio of HK to KB. Therefore, if BF bear to FE the ratio of 3AFq to Zq, the line BE will touch the curve in B: and EF will be equal to $\frac{1}{3}$ AF.

139. AFTER the situation of the tangent has been thus determined, the magnitude of HI, the part of the ordinate intercepted between the tangent and the curve, will be known. For example, in this instance since BF is to FE, that is IK to FG, as $3\ AFq$ to Zq, IK will be $= \frac{3\ AFq \times FG}{Zq}$, and HK being $= \frac{3\ AF \times AG \times FG + FGc}{Zq}$, HI will be $= \frac{3AF \times FGq + FGc}{Zq} = \frac{FGq}{Zq} \times \overline{3AF + FG}$. Now by this line HI may the curvature of curve lines be compared.

140. Let the streight line AB touch the curve CBD in the point B; CE being the absciss of the curve, and BF the ordinate at B. Take any other point G in the curve, and through the points G,B, describe the circle BGH, that shall touch the line AB in B; lastly, draw IKGL parallel to FB. Here are two angles formed at the point B with the circle, one by the line BK, the other by the curve; and the proportion of the first of these angles to the second will be different in different distances of the point G from the point B. And by the approach of G to B the angle between the circle and curve will be diminished, even so much as at length to bear a less proportion to the angle between the circle and tangent, than any, that can be proposed. That is, by the approach of the point G to B the angle between the tangent and circle may be brought nearer to the angle between the tangent and the curve, than by any difference how minute soever homogeneous to those angles; therefore the magnitude of the circle being continually varied by the gradual approach of G to B, and the angle between the tangent and circle thereby also varied; the angle

 between

between the tangent and curve is the ultimate magnitude of these angles. That is, the ultimate of these circles determines the degree of curvature of the curve CBD at the point B. But in the circle the rectangle under LKG is equal to the square of BK. And while the magnitude of KL varies perpetually by the approach of the point G towards B; if BM, taken in FB produced, be the ultimate magnitude of KL, the circle described through M and B to touch the tangent AK in B will be the circle, by which the curvature of the curve CBD in B is to be estimated.



141. SUPPOSE the curve CBD to be the cubical parabola as before, where $Zq \times FB$ is $= CFc$, then KG will be $= \frac{FIq}{Zq} \times \overline{3CF + FI}$. Hence LK $(= \frac{BKq}{KG})$ is $= \frac{BKq}{FIq} \times \frac{Zq}{3CF + FI}$. But it is evident, that in a given situation of the tangent AB the ratio of BKq to FIq is given; therefore LK

LK will be reciprocally as $3CF + FI$, and will continually increaſe, as the point G approaches to the point B, but can never be ſo great, as to equal $\frac{BKq}{FIq} \times \frac{Zq}{3CF}$; yet by the near approach of G to B, LK may be brought nearer to this quantity than by any difference, that ſhould be propoſed. Therefore, by our former definition of ultimate magnitudes, $\frac{BKq}{FIq} \times \frac{Zq}{3CF}$ is the ultimate magnitude of LK. Conſequently, if BM be taken equal to this $\frac{BKq}{FIq} \times \frac{Zq}{3CF}$, the circle deſcribed through M is that required.

142. WE have now gone through all, we think needful for illuſtrating the doctrine of prime and ultimate ratios; and by the definitions, which have been here given of ultimate magnitudes and proportions, compared with the inſtances ſubjoined of the application of this doctrine to geometrical problems, we hope our readers cannot fail of forming ſo diſtinct a conception of this method of reaſoning, that it ſhall appear to them equally geometrical and ſcientific with the moſt unexceptionable demonſtration.

143. THEREFORE we ſhall in the next place proceed to conſider the demonſtrations, which Sir Iſaac Newton has himſelf given, upon the principles of this method, of his precepts for aſſigning the fluxions of flowing quantities.

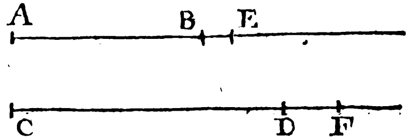
OF
Sir ISAAC NEWTON'S
METHOD
Of demonstrating his Rules for finding
FLUXIONS.

144. SIR Isaac Newton has comprehended his directions for computing the fluxions of quantities in two propositions; one in his Introduction to his treatise on the Quadrature of curves; the other is the first proposition of the book itself. In the first he assigns the fluxion of a simple power, the latter is universal for all quantities whatever.

145. FOR determining the fluxion of a simple power suppose the line AB to be denoted by x, and another line CD to be denoted by $\frac{x^n}{a^{n-1}}$, or by considering a as unite, CD will be denoted by x^n.

146. SUPPOSE the points B and D to move in equal spaces of time into two other positions E and F; then DF will be to BE in the ratio of the velocity, wherewith DF would be described with a uniform motion,

motion, to the velocity, wherewith BE will be described in the same time with a uniform motion. But if the point describing the line AB moves uniformly; the velocity, wherewith the line CD is described,



will not be uniform. Therefore the space DF is not described with a uniform velocity; in so much that the velocity, wherewith DF would be uniformly described, is never the same with the velocity at the point D. But by diminishing the magnitude of DF, the uniform velocity, wherewith DF would be described, may be made to approach at pleasure to the velocity at the point D. Therefore the velocity at the point D is the ultimate magnitude of the velocity, wherewith DF would be uniformly described. Consequently the ratio of the velocity at D to the velocity at B is the ultimate ratio of the velocity, wherewith DF would be uniformly described, to the velocity, wherewith BE is uniformly described. But DF being to BE as the velocity, wherewith DF would be uniformly described, to that, wherewith BE is uniformly described, the ultimate ratio of DF to BE is also the ultimate ratio of the first of these velocities to the last; because all the ultimate ratios of the same varying ratio are the same with each other. Therefore the ratio of the velocity at D to the velocity at B, that is, of the fluxion of CD to the fluxion of AB, is the same with the ultimate ratio of DF to BE.

147. If now the augment BE be denoted by o, the augment DF will be denoted by $nx^{n-1}o + \frac{n \times \overline{n-1}}{2} \times x^{n-2}o^2 + \frac{n \times \overline{n-1} \times \overline{n-2}}{6} x^{n-3}o^3 +$ &c. And here it is obvious, that all the terms after the firſt taken together may be made leſs than any part whatever of the firſt, that ſhall be aſſigned. Conſequently the proportion of the firſt term $nx^{n-1}o$ to the whole augment may be made to approach within any degree whatever of the proportion of equality; and therefore the ultimate proportion of $nx^{n-1}o + \frac{n \times \overline{n-1}}{2} x^{n-2}o^2 + \frac{n \times \overline{n-1} \times \overline{n-2}}{6} x^{n-3}o^3 +$ &c. to o, or of DF to BE, is that of $nx^{n-1}o$ only to o, or the proportion of nx^{n-1} to 1.

148. And it has already been proved, that the proportion of the velocity at D to the velocity at B is the ſame with the ultimate proportion of DF to BE; therefore the velocity at D is to the velocity at B, or the fluxion of x^n to the fluxion of x, as nx^{n-1} to 1.

149. IN the firſt propoſition of the treatiſe of Quadratures the author propoſes the relation betwixt three varying quantities x, y, and z to be expreſſed by this equation $x^3 - xy^2 + a^2z - b^3 = 0$. Suppoſe theſe quantities to be augmented by any contemporaneous increments great or ſmall. Let us alſo ſuppoſe ſome quantity o to be deſcribed at the ſame time by ſome known velocity, and let that velocity be denoted by m; the velocity, wherewith the augment of x would be uniformly deſcribed in that time be denoted by $\dot{x}$; the velocity, wherewith the augment of y would be uniformly deſcribed in the

the ſame time by $\dot{y}$; and laſtly the velocity, wherewith the augment of z would be uniformly deſcribed in the ſame time by $\dot{z}$. Then $\frac{o\dot{x}}{m}$, $\frac{o\dot{y}}{m}$, and $\frac{o\dot{z}}{m}$ will expreſs the contemporaneous increments of x, y, and z reſpectively. Now when x is become $x + \frac{o\dot{x}}{m}$, y is become $y + \frac{o\dot{y}}{m}$ and z become $z + \frac{o\dot{z}}{m}$; the former equation will become $x^3 + \frac{3x^2o\dot{x}}{m} + \frac{3xo^2\dot{x}\dot{x}}{m^2} + \frac{o^3\dot{x}^3}{m^3} - xy^2 - \frac{o\dot{x}y^2}{m} - \frac{2xo\dot{y}y}{m} - \frac{2\dot{x}o^2\dot{y}y}{m^2} - \frac{xo^2\dot{y}\dot{y}}{m^2} - \frac{\dot{x}o^3\dot{y}\dot{y}}{m^3} + a^2z + \frac{a^2o\dot{z}}{m} - b^3 = 0$. Here, as the firſt of theſe equations exhibits the relation between the three quantities x, y, z, as far as the ſame can be expreſſed by a ſingle equation; ſo this ſecond equation, with the aſſiſtance of the firſt, will expreſs the relation between the augments of theſe quantities. But the firſt of theſe equations may be taken out of the latter; whence will ariſe this third equation $\frac{3x^2o\dot{x}}{m} + \frac{3xo^2\dot{x}\dot{x}}{m^2} + \frac{o^3\dot{x}^3}{m^3} - \frac{o\dot{x}y^2}{m} - \frac{2xo\dot{y}y}{m} - \frac{2\dot{x}o^2\dot{y}y}{m^2} - \frac{xo^2\dot{y}\dot{y}}{m^2} - \frac{\dot{x}o^3\dot{y}\dot{y}}{m^3} + \frac{a^2o\dot{z}}{m} = 0$; which alſo expreſſes the relation between the ſeveral increments; and likewiſe if o be a given quantity, this equation will equally expreſs the relation between the velocities, wherewith theſe ſeveral increments are generated reſpectively by a uniform motion. And this equation being divided by o will be reduced to more ſimple terms, and yet will equally expreſs the relation of theſe velocities; and then the equation will become $\frac{3x^2\dot{x}}{m} + \frac{3xo\dot{x}\dot{x}}{m^2} + \frac{o^2\dot{x}^3}{m^3} - \frac{\dot{x}y^2}{m} - \frac{2x\dot{y}y}{m} - \frac{2\dot{x}o\dot{y}y}{m^2} - \frac{xo\dot{y}\dot{y}}{m^2} - \frac{\dot{x}o^2\dot{y}\dot{y}}{m^3} + \frac{a^2\dot{z}}{m} = 0.$ Now let us form an equation out of the terms of this, from which the quantity o is absent. This equation will be $\frac{3x^2\dot{x}}{m} - \frac{\dot{x}y^2}{m} - \frac{2x\dot{y}y}{m} + \frac{a^2\dot{z}}{m} = 0$; and this equation multiplied by m becomes $3x^2\dot{x} - \dot{x}y^2 - 2x\dot{y}y + a^2\dot{z} = 0$. It is evident, that this equation does not express the relation of the forementioned velocities; yet by the diminution of o this equation may come within any degree of expressing that relation. Therefore, by what has been so often inculcated, this equation will express the ultimate relation of these velocities. But the fluxions of the quantities x, y, z are the ultimate magnitudes of these velocities; so that the ultimate relation of these velocities is the relation of the fluxions of these quantities. Consequently this last equation represents the relation of the fluxions of the quantities x, y, z.

150. It is now presumed, we have removed all difficulty from the demonstrations, which Sir Isaac Newton has himself given, of his rules for finding fluxions.

151. IN the beginning of this discourse we have endeavoured at such a description of fluxions, as might not fail of giving a distinct and clear conception of them. We then confirmed the fundamental rules for comparing fluxions together by demonstrations of the most formal and unexceptionable kind. And now having justified Sir Isaac Newton's own demonstrations, we have not only shewn, that his doctrine of fluxions is an unerring guide in the solution

lution of geometrical problems, but alſo that he himſelf had fully proved the certainty of this method. For accompliſhing this laſt part of our undertaking it was neceſſary to explain at large another method of reaſoning eſtabliſhed by him, no leſs worthy conſideration; ſince as the firſt inabled him to inveſtigate the geometrical problems, whereby he was conducted in thoſe remote ſearches into nature, which have been the ſubject of univerſal admiration, ſo to the latter method is owing the ſurprizing brevity, wherewith he has demonſtrated thoſe diſcoveries.

CONCLUSION.

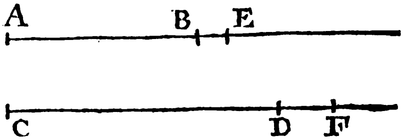
152. THUS we have at length finiſhed the whole of our deſign, and ſhall therefore put a period to this diſcourſe with the explanation of the term momentum frequently uſed by Sir Iſaac Newton, though we have yet had no occaſion to mention it.

153. AND in this I ſhall be the more particular, becauſe Sir Iſaac Newton's definition of momenta, That they are the momentaneous increments or decrements of varying quantities, may poſſibly be thought obſcure. Therefore I ſhall give a fuller delineation of them, and ſuch a one, as ſhall agree to the general ſenſe of his deſcription, and never fail to make the uſe of this term, in every propoſition, where it occurs, clearly to be underſtood.

154. IN determining the ultimate ratios between the contemporaneous differences of quantities, it is often previouſly required to conſider each of theſe differences apart, in order to diſcover, how much of thoſe differences is neceſſary for expreſſing that ultimate ratio. In this caſe Sir Iſaac Newton diſtinguiſhes, by the name of momentum, ſo much of any difference,

ence, as constitutes the term used in expressing this ultimate ratio.

155. Thus in § 147, where BE is $= o$, and DF equal to $nx^{n-1}o + \frac{n\times\overline{n-1}}{2}x^{n-2}o^2 + \frac{n\times\overline{n-1}\times\overline{n-2}}{6} \times x^{n-3}o^3$ + &c. the ultimate ratio of DF to BE being the ratio of $nx^{n-1}o$ to o, such a part only of DF as is denoted by $nx^{n-1}o$, without the addition of any of the following terms of the series, constitutes the whole of the momentum of the line CD; but the momentum of AB is the same as the whole difference BE, or o.



156. In like manner, if A and B denote varying quantities, and their contemporaneous increments be represented by a and b; the rectangle under any given line M and a is the contemporaneous increment of the rectangle under M and A, and $A\times b + B\times a + a\times b$ is the like increment of the rectangle under A, B. And here the whole increment $M\times a$ represents the momentum of the rectangle under M, A; but $A\times b + B\times a$ only, and not the whole increment $A\times b + B\times a + a\times b$, is called the momentum of the rectangle under A, B; because so much only of this latter increment is required for determining the ultimate ratio of the increment of $M\times A$ to the increment of $A\times B$, this ratio being the same with the ultimate ratio of $M\times a$ to $A\times b + B\times a$; for the ultimate ratio of $A\times b + B\times a$ to $A\times b + B\times a + a\times b$ is the ratio of equality. Consequently the ultimate ratio of $M\times a$

$M \times a$ to $A \times b + B \times a$ differs not from the ultimate ratio of $M \times a$ to $A \times b + B \times a + a \times b$.

157. THESE momenta equally relate to the decrements of quantities, as to their increments, and the ultimate ratio of increments, and of decrements at the same place is the same; therefore the momentum of any quantity may be determined, either by considering the increment, or the decrement of that quantity, or even by considering both together. And in determining the momentum of the rectangle under A and B Sir Isaac Newton has taken the last of these methods; because by this means the superfluous rectangle is sooner disengaged from the demonstration.

158. HERE it must always be remembered, that the only use, which ought ever to be made of these momenta, is to compare them one with another, and for no other purpose than to determine the ultimate or prime proportion between the several increments or decrements, from whence they are deduced *. Herein the method of prime and ultimate ratios essentially differs from that of indivisibles; for in the method of indivisibles momenta are considered absolutely as parts, whereof their respective quantities are actually composed. But though these momenta have no final magnitude, which would be necessary to make them parts capable of compounding a whole by accumulation; yet their ultimate ratios are as truly assignable as the ratios between any quantities whatever. Therefore none of the objections made against the doctrine of indivisibles are of the least weight against this method: but while we attend carefully to the observation here laid down, we shall be as secure against error, and the mind will receive as full satisfaction,

* Neque spectatur magnitudo momentorum, sed prima nascentium proportio. Newt. Princ. Phil. Lib. II. Lem. 2.

as

as in any the moſt unexceptionable demonſtration of Euclide.

159. WE ſhall make no apology for the length of this diſcourſe: for as we can ſcarce ſuſpect, after what has been above written, that our readers will be at any loſs to remove of themſelves, whatever little difficulties may have ariſen in this ſubject from the brevity of Sir Iſaac Newton's expreſſions; ſo our time cannot be thought miſemployed, if we ſhall at all have contributed, by a more diffuſive phraſe, to the eaſier underſtanding theſe extenſive, and celebrated inventions.

AN

AN

ACCOUNT

OF THE

PRECEDING DISCOURSE.

First printed in The Present State of the Republick of Letters for October 1735.

1. SOME doubts having lately arisen concerning Sir *Isaac Newton*'s doctrines of fluxions, and of prime and ultimate ratios; this treatise was written with design to give such an idea of both these subjects, as might clear them from uncertainty, without entering into the discussion of any particular objections.

2. For this end the author has been careful, not only to distinguish both these methods from that usually known by the name of indivisibles, but also from each other.

3. The manner wherein the ancients demonstrated, what relates to the mensuration of curvilinear spaces, not giving any distinct notion of the principles, upon which they built their analysis of such problems; about thirty years before Sir *Isaac Newton* invented his method of fluxions, *Cavalerius*, a mathematician of *Italy*, proposed in these problems a new form of reasoning*. He supposes, that all surfaces might be

* In Geometria Indivisibilibus promota, *Edit.* 1635.

be filled up with parallel lines, and all ſolids by parallel planes; and then lays down this fundamental propoſition, That different planes are in the ſame proportion to each other as all the lines contained in each, and different ſolids in the ſame proportion as all the planes contained in them. As this is a manner of expreſſion hardly accompanied with any ideas; ſince it is not at all intelligible to ſpeak of the collective number of lines or planes, where their number is wholly undetermined and infinite: this method was from the beginning oppoſed, as ungeometrical, and in no meaſure agreeable to that clearneſs of conception and expreſſion, for which the mathematical ſciences had ever been celebrated. But however, as by proper cautions error in the concluſions might be avoided, and this method promiſed great aſſiſtance in the analyſis of a ſubject, wherein the ancients had made the leaſt progreſs; it was warmly eſpouſed by *Torricellius*, and others of the moſt illuſtrious geometers. Our countryman Dr. *Wallis*, and many after him, thought it an improvement of this method to ſubſtitute, in the room of lines, parallelograms, whoſe breadth was to be called infinitely ſmall; and for the planes, whereby ſolids were ſuppoſed to be filled up, priſms or cylinders of an infinitely ſmall altitude: ſo that the method of indiviſibles at length ſuppoſed all geometrical magnitudes, whether lines, ſurfaces, or ſolids, to be compoſed of an infinite number of homogeneous magnitudes, each infinitely ſmall.

4. But this is a mode of expreſſion no way more intelligible than the other. Sir *Iſaac Newton* therefore inſtituted a manner of conception upon quite different principles. He obſerving (to uſe his own words) that indiviſibles have no being either in geometry or in nature*; inſtead of this infinite and in-

* Philoſ. Tranſact. No. 342. p. 205. or Commer. Epiſtolic. p. 38.

con-

conceivable ſubdiviſion of magnitudes already formed, he conſidered them as produced before the imagination by ſome motion. And thus the ſame magnitude will in ſome parts increaſe faſter than in others, and different magnitudes deſcribed together increaſe by different degrees of ſwiftneſs. Now if the proportion between the celerity of increaſe of two magnitudes produced together is in all parts known; it is evident, that the relation between the magnitudes themſelves muſt from thence be diſcoverable.

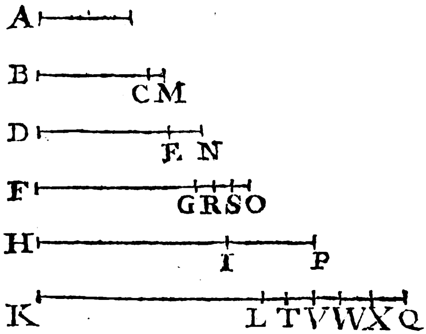
5. This is the foundation of his method of fluxions. And to form a true idea thereof, we muſt diſtinguiſh between the increaſe, which a line or figure receives in any given ſpace of time, and the velocity, wherewith that increaſe is generated: for though by the velocity, wherewith any line or figure continually augments, the quantity of its increaſe may be known; and on the contrary, from the quantity of the increaſe the velocity, wherewith that increaſe was produced; yet the quantity thus added is not the velocity, wherewith it is generated. And the method of fluxions requires the knowledge of theſe velocities of increaſe only: as Sir *Iſaac Newton*'s other method of prime and ultimate ratios proceeds entirely upon the conſideration of the increments produced.

6. In the method of fluxions are introduced no forms of expreſſion, but what convey very clear and diſtinct ideas, and ſuch as have not the leaſt affinity with the mode of conception ſchemed out in that of indiviſibles. This manner therefore of conſidering magnitudes, as they are gradually produced before the mind, is a genuine way of diſcovering the relation between ſuch magnitudes, which may afterwards be proved to bear that relation by a ſubdiviſion into parts, as practiſed by the ancient geometers; ſince it is ſhewn in this diſcourſe, that the rules for finding

finding fluxions are demonstrable according to the ancient forms.

7. The demonstration here given for the fluxion of a power, is formed upon the model of that delivered by Sir *Isaac Newton* himself; but may be otherwise performed after the following manner.

8. Let A, BC, DE, FG, HI, KL, and A, BM. DN, FO, HP, KQ be two series's of continued proportionals beginning with the same term A; then if *b* be a number denoting the distance of the terms KL, KQ from A, and *k* a number denoting the



distance of the terms FG, FO from the same; the ratio of LQ, the difference of the terms KL, KQ the most remote from A, to GO, the difference of the other terms FG, FO, is greater than the ratio of $b \times KL$ to $k \times FG$, and less than the ratio of $b \times KQ$ to $k \times FO$.

9. The ratio of DE to DN is the duplicate of the ratio of BC to BM; for the ratio of DE to A is the duplicate of that of BC to A, and the ratio of A to DN is the duplicate of that of A to BM; therefore, by equality, the ratio of DE to DN is the duplicate of the ratio of BC to BM.

10. In like manner the ratio of FG to FO is the triplicate of the ratio of BC to BM, and so of the rest. Insomuch that between any two terms in these series's, equally distant from the first term A, as many mean proportionals in the ratio of BC to BM will fall, as are the number of the intermediate terms in either series between these terms and A. Thus between FG and FO fall the mean proportionals FR, FS, and between KL, KQ, the mean proportionals KT, KV, KW, KX. Here the differences GR, RS, SO will be equal in number to *k*, by which is denoted the distance of the term FG from A, and the differences LT, TV, VW, WX, XQ will be equal in number to *b*. Now KL is to FG as LT to GR, and LT, TV, VW, WX, XQ are in the same continued proportion as GR, RS, SO: therefore KL is to FG as LW, the sum of LT, TV, VW, whose number is *k*, to GO, the sum of GR, RS, SO, whose number is likewise *k*. But the ratio of LQ to GO is compounded of the ratio of LQ to LW, and of that of LW to GO. Now the ratio of LQ to LW is greater than the ratio of *b* to *k*; therefore the ratio of LW to GO being the same with that of KL to FG, the ratio of LQ to GO is greater than that compounded of the ratio of *b* to *k*, and of that of KL to FG; that is, greater than the ratio of $b \times$ KL to $k \times$ FG.

11. Again: KQ is to FO as QX to QS; therefore QX, XW, WV, VT, TL being in the same continued proportion with OS, SR, RG; KQ is to FO as QV, the sum of QX, XW, WV whose number is *k*, to GO. But the ratio of LQ to GO is compounded of the ratio of LQ to QV, which is less than the ratio of *b* to *k*, and of the ratio of QV to GO, or of the ratio of KQ to FO. Therefore the ratio of LQ to GO is less than that of $b \times$ KQ to $k \times$ FO.

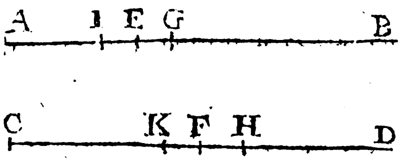
12.

12. Again, I say, that LQ and GO may be taken so small, that the ratio of LQ to GO shall be less than any ratio, that shall be proposed greater than the ratio of $b \times$ KL to $k \times$ FG; or greater than any ratio, that shall be proposed less than the ratio of $b \times$ KQ to $k \times$ FO.

13. Let DE, DN be the terms in these series's at the same distance from A, as KL is from FG; then DE will be to A as KL to FG, and $b \times$ DE to $k \times$ A as $b \times$ KL to $k \times$ FG; likewise DN to A as KQ to FO, and $b \times$ DN to $k \times$ A as $b \times$ KQ to $k \times$ FO.

14. Now in the first place take $b \times$ DN in a less ratio to $k \times$ A, than the ratio proposed greater than that of $b \times$ KL to $k \times$ FG. Then the ratio of bE KQ to $k \times$ FO will be less than the ratio proposed; but the ratio of LQ to GO is less than that of $b \times$ KQ to $k \times$ FO, and therefore will be less than the ratio proposed.

15. In the next place, let $b \times$ DE be taken to $k \times$ A in a greater ratio than the ratio proposed less than that of $b \times$ KQ to $k \times$ FO. Then will the ratio of $b \times$ KL to $k \times$ FG be greater than that now proposed. But the ratio of LQ to GO is greater than that of $b \times$ KL to $k \times$ FG, consequently greater than that now proposed.



16. Now in the figure at § 16. of this book, AE being denoted by x, let CF be denoted by $\frac{x^{\frac{m}{n}}}{a^{\frac{m-n}{n}}}$, m and n representing any two whole numbers.

Then AE and CF will be two terms in a ſeries of proportionals beginning from a, the number n denoting the place of AE, and m the place of CF. Here I ſay, the velocity, wherewith the point deſcribing CD moves at F, is to the velocity of the point moving on AB at E, as $m \times$ CF to $n \times$ AE;

that is, as $m \times \frac{x^{\frac{m}{n}}}{a^{\frac{m-n}{n}}}$ to $n \times x$, or as $\frac{m}{n} x^{\frac{m-n}{n}}$ to $a^{\frac{m-n}{n}}$.

17. In the firſt place G and H, as likewiſe I and K being other contemporary poſitions of the points moving on AB and CD; AG and CH will be terms in a ſeries of proportionals beginning from a, and likewiſe AI and CK terms in another ſeries of proportionals alſo beginning from a ſituated in like manner, as the terms AE and CF in the ſeries, to which they belong. Therefore, if m be greater than n, from what has above been written, FH bears a greater proportion to EG, and KF a leſs proportion to EI than $m \times$ CF bears to $n \times$ AE. Conſequently, if IE be equal to EG, KF will be leſs than FH; inſomuch that if the point on AB moves with a uniform velocity, the point on CD moves with a velocity continually accelerated.

18. Now, if poſſible, let the velocity at F bear to the velocity at E a greater proportion than that aſſigned, ſuppoſe the ratio of p to q.

19. Becauſe the ratio of p to q is greater than that of $m \times$ CF to $n \times$ AE, let the ratio of $m \times$ CH to $n \times$ AE be leſs than the ratio of p to q. Then, by what has been above written, the ratio of FH to EG is leſs than the ratio of $m \times$ CH to $n \times$ AE, conſequently leſs than the ratio of p to q, or of the velocity at F to the velocity at E; which is abſurd, the firſt of theſe ratios being greater than the laſt.

20. Again, ſuppoſe the velocity at F bear to the

velocity at E a leſs proportion than that aſſigned, ſuppoſe the ratio of r to s.

21. The ratio of r to s being leſs than that of $m \times CF$ to $n \times AE$, let the ratio of $m \times CK$ to $n \times AE$ be greater than the ratio of r to s. Then the ratio of KF to IE will be greater than the ratio of $m \times CK$ to $n \times AE$; conſequently greater than the ratio of r to s, or of the velocity at F to the velocity at E; which is abſurd, the firſt of theſe ratios being leſs than the laſt.

22. If the point on the line AB ſhould move from I to G, not with a uniform velocity, but with a velocity continually increaſing; ſince, when IE is equal to EG, KF is leſs than FH, the point on CD will move with a velocity more accelerated; and if the point moving from I to G proceed with a decreaſing velocity, the motion from K to H will at leaſt decreaſe ſlower: inſomuch that in theſe caſes alſo the proportion of FH to EG will be greater, and that of KF to IE leſs than that of the velocity at F to the velocity at E. Therefore the demonſtration will here proceed in the ſame manner as before.

23. If m be leſs than n, KF will be greater than FH, and the point on the line CD move with a decreaſing velocity, when the motion from I to G is uniform; but the demonſtration will here alſo proceed after the like manner. Nor will it be different when one of the numbers m or n is negative.

24. The doctrine of fluxions, as delivered by Sir *Iſaac Newton*, conſiſts of two parts; the form of conception we have above deſcribed, and the method of applying it for the ſolution of mathematical problems. The ſurprizing improvements Sir *Iſaac Newton* has made in the analytical part of geometry by theſe principles, his immortal treatiſe on the quadrature of curves abundantly ſets forth. But the author of this diſcourſe deſigning only to conſider the mode of conception propoſed in this doctrine, he

has avoided any particular explanation of the forms of calculation, that having been largely performed by others. But as the introduction to this subject, the most read, has accommodated these calculations to the system of indivisibles*, it has occasioned the true mode of conception, so necessary to give this doctrine a place in geometry, to be very much neglected. The author therefore has shewn at large, how we may carry along with us the genuine form of conception in the application of this doctrine to the mensuration of curvilinear spaces, the drawing of tangents, and other problems, to which these principles are to be applied.

25. Fluxions not affording the most convenient means for synthetic demonstration, Sir *Isaac Newton*, who in all his writings has shewn the strongest desire of using brevity, invented still another form of reasoning, from what he calls the prime and ultimate ratios of the increments or decrements of varying quantities, whereby to avoid the length of the ancient demonstrations by exhaustions.

26. This method is as essentially different from that of indivisibles, as the former; but however, requires somewhat greater attention to avoid falling into that faulty manner of conception. It depends on the first lemma of his mathematical principles of natural philosophy, the genuine meaning of which is, That those quantities are to be esteemed ultimately equal, and those ratios ultimately the same, which are perpetually approaching each other in such a manner, that any difference how minute soever being given, a finite time may be assigned, before the end of which the difference of those quantities or ratios shall become less than that given difference.

* Analyse des infiniment petits. Par le Marquis de l'*Hôpital*.

27. What Sir *Isaac Newton* intends we should understand by the ultimate equality of magnitudes, and the ultimate identity of ratios proposed in this lemma, will be best known from the demonstration annexed to it. By that it appears, Sir *Isaac Newton* did not mean, that any point of time was assignable, wherein these varying magnitudes would become actually equal, or the ratios really the same; but only that no difference whatever could be named, which they should not pass. The ordinate of any diameter of an hyperbola is always less than the same continued to the asymptote; yet the demonstration of this lemma can be applied, without changing a single word, to prove their ultimate equality. The same is evident from the lemma immediately following, where parallelograms are inscribed, and others circumscribed to a curvilinear space. Here the first lemma is applied to prove, that by multiplying the number and diminishing the breadth of these parallelograms *in infinitum*, that is, perpetually and without end, the inscribed and circumscribed figures become ultimately equal to the curvilinear space, and to each other; whereas it is evident, that no point of time can be assigned, wherein they are actually equal; to suppose this were to assert, that the variation ascribed to these figures, though endless, could be brought to a period, and be perfectly accomplished; and thus we should return to the unintelligible language of indivisibles. The excellence of this method consists in making the same advantage of this endless approximation towards equality, as by the use of indivisibles, without being involved in the absurdities of that doctrine. In short, the difference between these two may be thus explained.

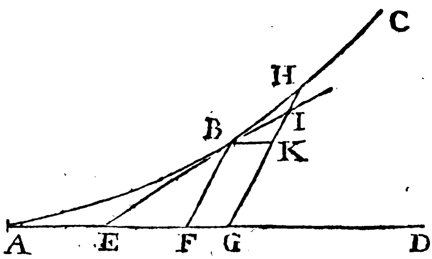
28. There are but three ways in nature of comparing spaces: one is by shewing them to consist of such, as by imposition on each other will appear to occupy the same place: another is by shewing their

proportion to fome third; and this method can only be directly applied to the like fpaces as the former, for this proportion muft be finally determined by fhewing, when the multiples of fuch fpaces are equal, and when they differ: the third method, to be ufed where thefe other two fail, is by defcribing upon the fpaces in queftion fuch figures, as may be compared by the former methods, and thence deducing the relation between thofe fpaces by that indirect manner of proof commonly called *deductio ad abfurdum*; and this is as conclufive a demonftration, as any other, it being indubitable, that thofe things are equal, which have no difference. Thus *Euclide* and *Archimedes* demonftrate all they have writ concerning the comparifon and menfuration of curvilinear fpaces. The method advanced by Sir *Ifaac Newton* for the fame purpofes differs from theirs, only by applying this indirect form of proof to fome general propofitions, and from thence deducing the reft by a direct form of reafoning. Whoever compares the fourth of Sir *Ifaac Newton*'s lemmas with the firft, will fee, that the proof of the curvilinear fpaces there confidered having the proportion named depends wholly upon this, that if otherwife the figure infcribed within one of them could not approach by fome certain diftance to the magnitude of that fpace: and this is precifely the form of reafoning, whereby *Euclide* proves the proportion between different circles. As this method of reafoning is very diffufely fet out in the writings of the ancients, and Sir *Ifaac Newton* has here expreffed himfelf with that brevity, that the turn of his argument may poffibly efcape the unwary; the author has recommended the reading the ancients, as the beft introduction to the knowledge of this method. The impoffible attempt of comparing curvilinear fpaces without having any recourfe to the forementioned indirect method of arguing produced the abfurdity of indivifibles.

29. As the magnitudes called in this lemma ultimately equal may never abſolutely exiſt under that equality; ſo the varying magnitudes holding to each other the variable ratios here conſidered may never exiſt under that, which is here called their ultimate ratio. Of this Sir *Iſaac Newton* gives an inſtance, which the author of this treatiſe has repeated after him, from lines increaſing together by equal additions, and having from the firſt a given difference. For the ultimate ratio of theſe lines in the ſenſe of this lemma, as Sir *Iſaac Newton* himſelf obſerves, will be the ratio of equality, though theſe lines can never have this ratio; ſince no point of time can be aſſigned, when one does not exceed the other.

30. In like manner the quantities, called by Sir *Iſaac Newton* vaniſhing, may never ſubſiſt under that proportion, here eſteemed their ultimate.

31. In § 137. of this treatiſe, where BF bears the ſame proportion to the ſubtangent FE, as that wherewith the lines HK, KB vaniſh, theſe lines muſt not be conceived, by the name of evaneſcent or any other appellation, ever to ſubſiſt under that proportion;



for ſhould we conceive theſe lines in any manner to ſubſiſt under this proportion, though at the inſtant of their vaniſhing, we ſhall fall into the unintelligible notion of indiviſibles, by endeavouring to repreſent to the imagination ſome inconceivable kind of exiſtence of theſe lines between their having a real mag-

magnitude, and becoming absolutely nothing. Sir *Isaac Newton* was himself apprehensive, that this mistake might be made; for as he thought fit (in compliance with the bad taste, which then prevailed) to continue the use of some loose and indistinct expressions resembling those of indivisibles, for which he has himself apologised, he expresly cautions us against misinterpreting him in this manner, when he says: *Si quando dixero quantitates quam minimas, vel evanescentes, vel ultimas, cave intelligas quantitates magnitudine determinatas, sed cogita semper diminuendas sine limite* *. Thus expresly has he declared to us, that vanishing quantities, or whatever other less accurate appellation he names them by, are to be considered as indeterminate quantities bearing to each other under their different magnitudes different proportions; and that the limit of these proportions, which the quantities themselves can never obtain, is that, for the sake of which, these quantities are considered: insomuch, that since these quantities have different proportions, while they obtain the name of vanishing quantities, the author of this treatise has justly observed the term ultimate to be necessarily added to denote that proportion, which is the limit of an endless number of varying ones. The like remark is necessary, when these quantities are considered in the other light as arising before the imagination: for then the proportion intended must be specified by calling it the first or prime proportion of these quantities. And as this additional epithet is necessary to express the proportion intended, so it is absurd to apply it to the quantities themselves; as Sir *Isaac Newton* says, there are *rationes primæ quantitatum nascentium*, but not *quantitates primæ nascentes* †.

32. The author of this treatise thought, the readiest method to guard against all errors of this kind

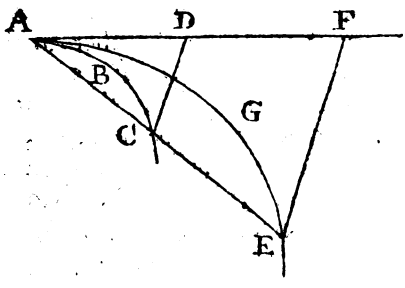
* Princip. Phil. Lem. xi. in schol.

† Philosophical Transact. No. 342. pag. 205. or Commer. Epistolic. p. 38.

was

was to represent the principles of prime and ultimate ratios, and their application to geometrical subjects under such a form of expression, as might be so totally inconsistent with indivisibles, as not to be capable by any misinterpretation of being accommodated to that erroneous manner of conception. But at the same time he took care, that his phrase should not differ essentially from Sir *Isaac Newton*'s; as will appear by comparing the two modes of expression in the following instance, being the seventh of Sir *Isaac Newton*'s lemmas concerning prime and ultimate ratios, which is to this effect.

33. If any arch ABC be subtended by a chord AC, and at A, where the curvature is understood to be uninterrupted, it be touched by the streight line AD; if the point C be supposed to approach towards A, till those points coincide, the ultimate ratio of the chord, arch, and tangent will be the ratio of equality; provided the tangent AD be terminated



by some line DC drawn from C, the extremity of the arch, so as always to make some angle with the tangent AD.

34. This Sir *Isaac Newton* thus demonstrates.

35. While the point C approaches to the point A, suppose AC and AD always to be produced to the distant points E and F, and EF to be drawn parallel to CD; and let the arch AGE be always similar to the

the arch ABC. Now when the points A and C coalesce, the rectilinear angle under EAF must vanish; therefore the right lines AE, AF, which are always of a finite magnitude, and also the intermediate arch AGE must coincide, and consequently become equal. Therefore the ultimate ratios of the streight lines AC, AD, and the curve ABC, all which vanish, when the point C coincides with A, will be the ratio of equality.

36. Now in the phrase of this book, it must be said, that the arch AGE can never be equal to the chord AE; nor the chord AE equal to the tangent AF, unless when the angles under AEF and under AFE chance to be equal. But, by causing the point C to approach the point A, the ratios of these three lines to each other may at last be brought nearer to the ratio of equality than to any other whatever. Therefore, according to the definition in § 124. of this treatise, the ultimate ratio of any one of these three lines to either of the others is the ratio of equality. And again, since the streight lines AC, AD, and the arch ABC are always to each other in the same proportion with the lines AE, AF, and the arch AGE; the ultimate ratios of the three lines AC, AD, ABC will be the same with the ultimate ratios between AE, AF, AGE, by the proposition subjoined to that definition: therefore the ultimate ratio between any two of these lines AC, AD, ABC is the ratio of equality.

37. By this instance it is manifest, that the style, under which the author has treated this subject, is only an interpretation of Sir *Isaac Newton*'s; and such an interpretation he thought alone sufficient to answer the purpose of his writing. No objection had been made against the truth of the conclusions drawn from this method of reasoning. Indeed all error of that kind may be avoided by proper circumspection even in the use of indivisibles. But as the

only ſuſpicion lay againſt the propriety in the conception and expreſſion advanced in this doctrine; if his interpretation involves in it no perplexed or imperfect ideas, which the author flatters himſelf will be allowed, it is a full juſtification of this method.

38. The author believes himſelf as deſirous, as any one can be, to preſerve propriety of expreſſion and perſpicuity of conception in mathematical ſubjects. He therefore freely acknowledges, that he has not vindicated this doctrine, unleſs he ſhall be found to have accommodated to it a clear and unexceptionable mode of expreſſion, nor freed the inventor from cenſure, unleſs he has alſo ſhewn, that the turn of phraſe recommended by Sir *Iſaac Newton*, without any forcible conſtruction, is adapted to convey the ſame ideas. The author has often lamented the negligence of geometrical writers in regard to their ſtyle and diction. The introduction of the terms of arithmetic into geometry by *Des Cartes*, and the favourable reception of the unintelligible jargon of indiviſibles have overwhelmed the mathematical ſciences with ſuch a profuſion of intricate and inconceivable forms of ſpeaking, that they began to be no longer that guide to ſound reaſoning, which they had hitherto been thought. To reſtore in ſome meaſure geometry from this corruption was the deſign of Sir *Iſaac Newton* in advancing the doctrine of prime and ultimate ratios; and how far the author of this treatiſe has proved him ſucceſsful, muſt be ſubmitted to the judgment of the publick.

39. Sir *Iſaac Newton* has made uſe of prime and ultimate ratios chiefly for ſynthetic demonſtrations; yet as they furniſh a direct manner of proof, it is manifeſt they may be alſo applied to the analyſis of problems.

40. To compleat the deſign of this treatiſe, it was neceſſary to explain Sir *Iſaac Newton*'s own demonſtrations of his rules for finding fluxions; for which

purpoſe

purpose nothing more was thought necessary than to dilate Sir *Isaac Newton*'s words by a small paraphrase. For though Sir *Isaac Newton* demonstrates these rules upon the principles of prime and ultimate ratios, yet after what had been written, it seemed scarce possible any longer to confound these two methods together. Indeed when the author considers, how expressly Sir *Isaac Newton* himself has distinguished them, he owns himself surprised, that this mistake should ever have been made.

41. The treatise concludes with an explication of what is to be understood by the momentum of quantities, which is a term appertaining to the doctrine of prime and ultimate ratios only*.

42. The term momentum being of no other use than to give the expression in particular cases greater brevity, the truth of this doctrine has no dependance on the sense of this term; therefore it was not necessary to be taken notice of in this general account; but as it has been conceived to contain something very abstruse, if not unintelligible, the author has explained it at large.

43. And here the author confesses, he met with the greatest difficulty; for it must be acknowledged, that Sir *Isaac Newton*'s description is capable of an interpretation too much resembling the language of indivisibles. But were we to allow Sir *Isaac Newton*'s definition of momenta to be founded entirely upon that erroneous doctrine, the utmost, that will follow from so large a concession, is only this, that though he invented the doctrines of fluxions, and of prime and ultimate ratios; yet he has demonstrated some of the propositions in his mathematical prin-

* These symbols $\dot{o}$ and $\dot{x}$ are put for things of a different kind: the one is a moment, the other a fluxion or velocity.

Mr. *Leibnitz* hath no symbols of fluxions in his method. He used the symbols of moments or differences *dx*, *dy*, *dz*, *Philos. Trans.* N. 342. p. 204. and Commer. Epistolic. p. 37, 38.

ciples

ciples of natural philosophy by the means of indivisibles. It cannot hence be inferred, that his doctrine of prime and ultimate ratios has any connexion with indivisibles, or was insufficient for these cases. The author of this treatise has certainly freed the doctrine from this latter imputation by shewing, that such a sense may be put upon the word momentum, as will render those very demonstrations of Sir *Isaac Newton*, where this word is used, as just as any other upon the principles of prime and ultimate ratios. For this purpose the author, without confining himself to the express words of Sir *Isaac Newton*, has given such a definition of this term, as he thought most suitable to the principles of that method.

44. The author of this treatise undertook not to prove, that Sir *Isaac Newton* has never deviated from the utmost propriety of expression, nor that he never demonstrated any proposition upon the principles of indivisibles. He knows Sir *Isaac Newton* did sometimes make use of that method of reasoning; but this he contends for, that the methods under consideration are absolutely different from that doctrine.

45. When Sir *Isaac Newton* first invented his method of fluxions, he demonstrated the rules of that method by indivisibles, as he acknowledges himself*, That in his calculus he made use of the character or symbol *o* to denote an infinitely small quantity. This he did both in his treatise of series and fluxions written in the year 1671, and also in his treatise on quadratures as he at first writ it, from which we have a

* Philos. Trans. No. 342. p. 205. or Com. Epist. p. 38. In his (*Newton's*) calculus there is but but one infinitely little quantity represented by a symbol, the symbol *o*.

Mr. *Newton* used the letter *o* in his analysis, and in his book of quadratures, and in his *principia philosophiæ*, and still uses it in the very same sense as at first. Philos. Trans. Ibid. p. 204. or Com. Epistol. p. 37.

transcript in Dr. *Wallis** not as he afterwards corrected it in his own edition of that treatise. He has likewise made use of the same in one proposition of his Principles of natural philosophy†. And he informs us, that though in demonstrating any proposition he chose to use the letter *o* for a finite moment of time, *&c.* and perform the whole calculation by the geometry of the ancients in finite figures or schemes without any approximation, and when the calculation was at an end, and the equation reduced, to suppose the moment *o* to decrease *in infinitum* and vanish, yet in the investigation, when it would make dispatch, he would suppose the moment *o* to be infinitely little, using all manner of approximation, which he conceived would produce no error in the conclusion‖. Accordingly we find in his treatise of quadratures, he freed his demonstrations from this defect, under which they first laboured; and the proposition of his principles of philosophy, where he continued the use of indivisibles, is only the analysis of a problem.

46. Thus it appears, that Sir *Isaac Newton* did sometimes allow himself the use of indivisibles; but it also appears, that he always had a dislike to that method, as we learn from his own words, when he says, Since we have no ideas of infinitely little quantities, he introduced fluxions, that he might proceed by finite quantities as much as possible§. But as the brevity, wherewith he chose to write, obliged him still to have recourse to indivisibles for demonstrating the rules of this new method, he at length invented his other method of prime and ultimate ratios, and thereby entirely got over that difficulty.

* Oper. Vol. II. p. 392.
† Lib II. prop. 10.
‖ Philos. Trans. No. 342. p. 179. or Com. Epistol. p. 9.
§ Philos. Trans. Ibid. p. 205. or Com. Epistol. p. 38.

47.

47. We may likewiſe hence learn, how it came to paſs, that his definition of momenta ſhould contain expreſſions bearing ſome analogy to thoſe of indiviſibles: for he informs us, that originally he uſed the word moment in a ſenſe agreeable to that doctrine; telling us, That from the moments of time he gave the name of moments to the momentaneous increaſes, or infinitely ſmall parts generated in moments of time*; though in his principles of philoſophy he directs us to interpret his meaning according to the doctrine of prime and ultimate ratios, where he ſays, *neque ſpectatur magnitudo momentorum ſed prima naſcentium proportio*†; and the author of the preſent treatiſe has accommodated his deſcription of momenta to this, which he conceives to be Sir *Iſaac Newton*'s intention.

48. As the proportion between the increments of magnitudes is in this doctrine conſidered only for diſcovering, what is here called their ultimate ratio; when the real proportion of theſe increments is not to be expreſſed, but by terms too complex, it is convenient, by neglecting ſome ſuperfluous part of the increments, or by a proper addition to them, to form new quantities, which ſhall not only bear to each other a more ſimple proportion, but the ultimate ratio alſo of each quantity, thus formed, to the increment, whence it is deduced, ſhall be the ratio of equality: for by the proportion of ſuch quantities the ultimate ratios of the increments are more readily aſſignable. Theſe are the quantities called momenta. For example; if the increment of any line denoted by x be repreſented by o, the increment of the line denoted by any power x^n will be $nx^{n-1}o + n \times \frac{n-1}{2} x^{n-2}oo +$ &c. Here as the ultimate ratio of the

* Phil. Tranſ. Ibid. p. 178. or Com. Epiſt. p. 7.
† Lib. II. Lem. 2.

first of these increments to the last is that of o to $nx^{n-1}o$, the line denoted by this term $nx^{n-1}o$ only is sufficient to express that ultimate proportion, and therefore may be assumed for the momentum of x^n; and then, to preserve a similitude of phrase, the entire increment of x is also to be called the momentum of that line.

49. Although it is a great mistake to suppose the validity either of the doctrine of fluxions, or of that of prime and ultimate ratios, to depend upon what Sir *Isaac Newton* has demonstrated concerning the momenta of quantities; yet since his demonstration of the momentum of a rectangle had been controverted, the author has given a brief account of the principles, upon which that demonstration proceeds. And this may be represented more at large as follows.

50. To give this demonstration its utmost extent, suppose some third variable line Z, to which A and B, the sides of the rectangle in question, are in any manner related; and let a and b not be the real increments of A and B, but bear to the increment of Z the most simple relation, whereby they can express the ultimate ratio of the increments of A and B to the correspondent increment of Z; then may a and b be called the momenta of A and B respectively. And since, at the same magnitudes of Z, A, and B the ultimate ratios between their decrements are the same with those between their increments, by the same magnitudes a and b may be also expressed the ultimate ratios of the decrements of A and B to the correspondent decrement of Z.

51. In like manner the ultimate ratio of the increment of the rectangle under A, B to the correspondent increment of any other rectangle under A and some given line M will be the same with the ultimate ratio of the decrement of the rectangle $A \times B$ to

to the correſpondent decrement of the rectangle A × M at the ſame magnitudes of A and B. Therefore the ultimate ratio of the increment of A × B to the correſpondent increment of A × M, will be the ſame with the ſum of ſuch increment and decrement of A × B to the ſum of the correſpondent increment and decrement of A × M.

52. Farther, the ultimate ratio of the increment of A × B to the correſpondent increment of A × M will be the ſame with the ultimate ratio of that augmentation, which the rectangle under A, B will receive by increaſing the ſides, either by their reſpective momenta, or by analogous parts of thoſe momenta, to the augmentation, which the rectangle under A, M will receive from the moment of A, or a ſimilar part thereof. Therefore the ultimate ratio of the increment of A × B to the correſpondent increment of A × M will be the ſame with the ultimate ratio of that augmentation, which the rectangle A × B will receive from increaſing its ſides A and B by half their momenta *a* and *b*, to the augmentation, which A × M will receive from increaſing A by half its momentum *a*. In like manner the ultimate ratio of the decrement of A × B to the correſpondent decrement of A × M will be the ſame with the ultimate ratio of that diminution, which the rectangle A × B will receive by taking from each of its ſides half its momentum, to the diminution, which the rectangle A × M will receive from half the momentum of A.

53. Hence it follows, that the ultimate ratio of the increment of A × B to the correſpondent increment of A × M is the ſame with the ultimate ratio of the ſum of the augmentation and diminution, which the rectangle A × B will receive from half the momenta of its ſides, to the ſum of the augmentation and diminution, which the rectangle A × M will receive from half the mo-

mentum of A; that is, the same with the ultimate ratio of $\frac{1}{2}a \times B + \frac{1}{2}b \times A + \frac{1}{4}ab$ and $\frac{1}{2}a \times B + \frac{1}{2}b \times A - \frac{1}{4}ab$ together, or of $a \times B + b \times A$, to $a \times M$.

54. As the square is comprehended under this general proposition for all rectangles, so by a similar artifice we may demonstrate the momentum of any other power. For instance, in the cube of any variable quantity A, whose increment or moment is o, if we divide that increment into two parts p and q, that the ratio of p to q may be subduplicate of the ratio of $3A - q$ to $3A + p$; $3A \times pp + p^3$ will be equal to $3A \times qq - q^3$, whereby the cube of $A + p$, or $A^3 + 3A^2p + 3Ap^2 + p^3$, will exceed the cube of $A - q$, or $A^3 - 3A^2q + 3Aq^2 - q^3$, by $3A^2 \times \overline{p + q}$, or $3A^2 \times o$, the momentum of A^3.

55. Here dividing the increment o into two equal parts will not answer the purpose intended; for by deducting the cube of $A - \frac{1}{2}o$, or $A^3 - \frac{3}{2}A^2o + \frac{3}{4}Ao^2 - \frac{1}{8}o^3$, from the cube of $A + \frac{1}{2}o$, or $A^3 + \frac{3}{2}A^2o + \frac{3}{4}Ao^2 + \frac{1}{8}o^3$, the residue will be $3A^2o + \frac{1}{4}o^3$, exhibiting more than is necessary for the momentum of the cube of A; for the momentum should be the simplest term, whereby the intended ultimate ratio can be expressed.

56. In other compound quantities the demonstration may be conducted upon the same model by such a division, as each particular case shall require, of the moments of the original quantities, whereof those under consideration are compounded; but when such division is of too perplex a kind, another method of demonstrating is to be preferred.

57. This is abundantly sufficient for explaining the demonstration in question. And as the author of this discourse presumes, he has given throughout a genuine representation of Sir *Isaac Newton's*

real and ſole intention: ſo he hopes it will appear, that however leſs exact in the choice of his expreſſions that great man may have been at any other time; yet when he purpoſely deſcribes theſe methods, and explains their principles, he is not only perfectly conſiſtent with himſelf, but has alſo delivered his meaning with ſuch perſpicuity, that we need not have recourſe to any deference for his authority to be fully ſatisfied of the truth of theſe doctrines.

A REVIEW OF

Some of the principal Objections that have been made to the Doctrines of Fluxions and Ultimate Proportions; with some Remarks on the different Methods, that have been taken to obviate them.

First published in The Present State of the Republick of Letters *for* December 1735.

1. THE objections, that have been made to the conception and nature of fluxions, have principally arisen, either from confounding this doctrine with the method of indivisibles, and the differential calculus of foreigners, or from supposing (as fluxions are said to be velocities) that the fluxion of a quantity, and the velocity of a quantity, were synonimous terms; forgetting that it is not to the quantities themselves, but to their degree of increase or decrease, that this velocity intended by the fluxion is ascribed. But as these mistakes can be no longer made without the greatest negligence or disingenuity; it may be reasonably supposed, that no exception of this kind will for the future be insisted on. We shall there-

therefore at this time confine ourſelves to the objections of another kind; ſuch as have been urged againſt thoſe operations, by which the proportion of the fluxions of different flowing quantities are determined.

2. Theſe objections have been particularly levelled at that expreſſion of Sir *Iſaac Newton*, *Fluxiones ſunt in prima ratione augmentorum naſcentium*, — or *in ultima ratione partium evaneſcentium* *. Which being uſually thus tranſlated, that *fluxions are in the prime ratio of the naſcent augments*, — or *in the ultimate ratio of the evaneſcent parts* †; it has been from hence aſked, What theſe naſcent or evaneſcent parts, augments or decrements are? If of any magnitude, then it will be confeſſed by the eſpouſers of this doctrine, that their ratio is not the ſame with the ratio of the fluxions. If it is anſwered, that they are of no magnitude; it is then ſaid, that to talk of the ratio of nothings, is ſuch a ſtrain of language, as it is ſuppoſed, the warmeſt followers of the inventor will ſcarce undertake to defend.

3. To obviate this objection, two explanations have been given of this quotation.

4. The firſt endeavours to ſhew, how this imagined difficulty may be avoided, not by conſidering theſe naſcent augments and evaneſcent decrements as being actually vaniſhed, in which caſe they can have no proportion, nor yet as being of any real magnitude, when their proportion cannot be the ſame with the proportion of the fluxions; but by ſuppoſing that there can be repreſented to the mind ſome intermediate ſtate of theſe augments or decrements at the very inſtant, in which they vaniſh.

5. Another writer, in his diſcourſe concerning the nature and certainty of Sir *Iſaac Newton*'s me-

* *Introduct. ad Quadrat. Curv.*

† *Harris*'s Lexicon Technicum, Vol. II. in the word Quadrature.

thods of fluxions and of prime and ultimate ratios, has endeavoured to ſhew, that this objection is founded on an erroneous hypotheſis; for that by the ultimate proportion of varying quantities was only meant the limit of their varying proportions, and not a proportion, that theſe varying quantities could ever exiſt under during their variation; and conſequently that the true explication of this paſſage ſhould be, fluxions are in that proportion, which is the ultimate to all thoſe varying proportions, that the decrements bear to each other, whilſt they are vaniſhing or diminiſhing; that is, the limit of the proportions, that the decrements bear to each other, as they diminiſh, is the true proportion of the fluxions. By this interpretation, which is ſupported by Sir *Iſaac Newton*'s own words, the above-mentioned objection immediately falls to the ground; ſince it is altogether founded on the ſuppoſition, that the decrements in their imagined evaneſcent ſtate did really bear to each other the proportion of the fluxions; whereas this paſſage, when truly underſtood, does not ſuppoſe, that the decrements can, in any circumſtance whatever, bear to each other that proportion; but aſſerts on the contrary, that the proportion of the fluxions is only a proportion limiting all the varying proportions, that theſe decrements have to each other in their various degrees of diminution.

6. At the ſame time that this objection was raiſed againſt the doctrine of fluxions, the method of prime and ultimate ratios was excepted to: in particular it was urged, that the quantities or ratios, aſſerted in this method to be ultimately equal, were frequently ſuch, as could never abſolutely coincide. As for inſtance, the parallelograms inſcribed within the curve in the ſecond *Lemma* of the firſt book of Sir *Iſaac Newton*'s *Principia*, cannot by any diviſion be made equal to the curvilinear ſpace they inſcribed; whereas in that *Lemma* it is aſſerted, that they are ultimately equal to that ſpace.

7. And

7. And here again two different methods of explanation have been given. The first, supposing that by ultimate equality a real assignable coincidence is intended, asserts, that these parallelograms and the curvilinear space do become actually, perfectly, and absolutely equal to each other. But the author of the above-mentioned treatise has given such an interpretation of, this method, as did no ways require any such coincidence.

8. In his explication of this doctrine of prime and ultimate ratios he defines the ultimate magnitude of any varying quantity to be the limit of that varying quantity, which it can approach within any degree of nearness, and yet can never pass. And in like manner the ultimate ratio of any varying ratio is the limit of that varying ratio. These definitions being premised, he demonstrates, that when varying magnitudes keep always in the same proportion, then their ultimate magnitudes will be in that same proportion; and that all the ultimate ratios of any particular varying ratio is the same. From these propositions thus established, all, that has at any time been demonstrated by the ancient method of exhaustions, may be most easily and elegantly deduced; and that by a method not yielding in brevity to the artless inconclusive process by indivisibles.

9. It is evident, that no coincidence of the varying quantity and its limit is at all supposed necessary in this method; since the ultimate magnitude of a varying one is not to be denominated from any such coincidence of the varying one with it, but from its being that magnitude, which the varying one can approach within any degree of nearness.

10. It has been supposed, that the accuracy of the demonstrations founded on this doctrine did in reality depend on this coincidence; but this mistake has arisen from forgetting, that the demonstrations deduced from this method are applied to the limits of varying

varying magnitudes and proportions, and not to the varying quantities or proportions themſelves.

11. Thus, if by means of a polygon deſcribed about a circle, we were to demonſtrate the equality of that circle to a triangle, having for its baſe the circumference of that circle, and the ſemidiameter for its altitude, the proof would not be founded on the real coincidence of the polygon and circle, ſince this could not be effected by any diminution of the ſides of the polygon; but its demonſtration would altogether proceed by ſhewing, that as the circumſcribed polygon could approach both the circle and triangle within any degree of nearneſs, and yet could paſs neither of them; therefore the circle and triangle, thus ſhewn to be the limits or ultimate magnitudes of the ſame varying magnitude, cannot differ from each other.

12. In like manner in demonſtrating the proportion that the fluxions of two flowing quantities bear to each other, the demonſtration is not founded on the coincidence of the proportion of the decrements with that proportion, which is given for the proportion of the fluxions; for the coincidence of theſe proportions cannot by any diminution of the decrements be ever effected: but the proof depends upon this, that, ſince by diminiſhing the decrements the proportion of thoſe decrements can be brought within any degree of nearneſs to that given proportion, and alſo to the proportion of the fluxions, and yet can never paſs either of them; therefore that given proportion, and the proportion of the fluxions, cannot differ from each other, they being thus ſhewn to be each of them the limit or ultimate proportion of the ſame varying proportion.

13. From hence it appears, that the coincidence of the variable quantity and its limit, could it be always proved, would yet bring no addition to the accuracy of theſe demonſtrations; and ſince by the diviſion

division of magnitudes no such coincidence can ever take place, why to the natural difficulty of these subjects should the obscurity of so strained a conception be added? certainly neither brevity, perspicuity, nor exactness can be at all promoted, by supposing in these demonstrations that circumstance to be ever necessary, which in numberless instances is not possible; and which by its taking place or not, can no ways affect the justness of the conclusion.

14. But it has been urged against this explication, that Sir *Isaac Newton* does in his first *Lemma* of his first book assert such a coincidence; and therefore, though the method of managing prime and ultimate proportions here described may be conclusive, yet it is not a true interpretation of Sir *Isaac Newton*.

15. What foundation, there is for this charge, will best appear by considering the *Lemma*; and that this may be done with more convenience, we will insert a literal translation of it.

16. *Quantities, and the ratios of quantities, that during any finite time constantly approach each other, and before the end of that time approach nearer than any given difference, are ultimately equal.*

17. In order that the coincidence between the variable quantity and its limit should be intended in this *Lemma*, it is necessary, that the phrase of *given difference* should mean a difference, that may be taken at pleasure, after the celerity or degree of approach of these quantities or ratios is in every part determined.

18. But if, according to the most usual and authentic signification of this phrase, there is meant by the *given difference*, in this Lemma, a difference first assigned, according to which the degree of approach of these quantities may be afterwards regulated; then variable quantities or ratios, and their limits, though they do never actually coincide, will come within the description of this *Lemma*; since the differ-

ence

ence being once aſſigned, the approach of theſe quantities may be ſo accelerated, that in leſs than any given time the variable quantity, and its limit, ſhall differ by leſs than the aſſigned difference.

19. Now that the latter ſenſe is the true interpretation, will appear from the demonſtration and application of the *Lemma*.

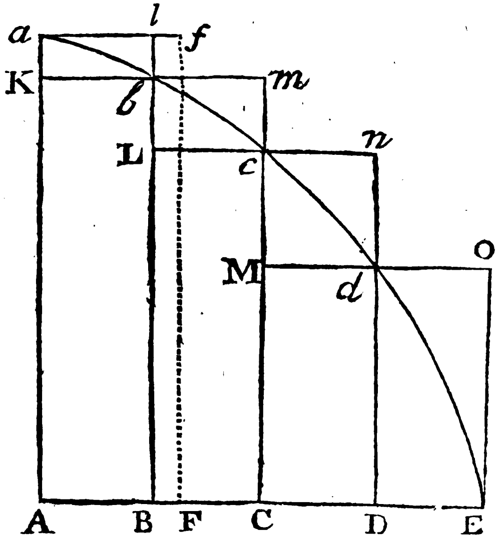
20. In the firſt place, the demonſtration of this Lemma may, without the change of a ſingle word, be applied to prove, that the ultimate ratio of the ordinate of the hyperbola to the ſame ordinate continued to the aſymptote is the ratio of equality; and yet it is confeſſed, that in this caſe there can never be an actual coincidence.

21. In the next place, the quantities in many of the ſucceeding *Lemmas*, to which the firſt is applied, are ſuch, where the approach is determined by a ſubdiviſion into parts; but by this method of proceeding it is obvious, that no coincidence can ever be obtained.

22. However it is ſaid, that by motion this coincidence may be actually made to take place even in theſe quantities; as, ſuppoſe in the ſecond Lemma a point E to deſcribe the line EA with a continued motion in the ſpace of an hour, and let it be conceived, that in every point of time during that hour, a rectangle, as AB*l*, is raiſed upon AB, that point of the line EA, which at that point of time is yet undeſcribed; alſo upon every other part of the line equal to AB let other rectangles be erected, as in the figure, at the ſame point of time. It is ſaid that by this means, at the end of the hour, when the point E arrives at A, the curvilinear ſpace and the inſcribed figure will actually coincide.

23. To this it may be replied, that ſuppoſing the coincidence could by this means take place, it would prove, that no ſuch coincidence was ever intended by Sir *Iſaac Newton*; ſince had he regarded it as a neceſſary

necessary circumstance, he would certainly have applied to this *Lemma* a method of inscribing the figure,

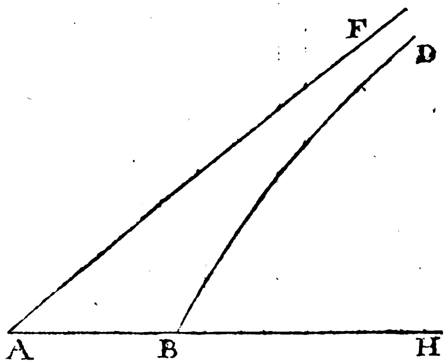


by which such a coincidence might be shewn; whereas by describing the parallelograms by a continual division, and making their bases constantly equal, and always some *aliquot* part of the whole, he has necessarily excluded the description of them by motion, by which means only it is supposed, that this coincidence can be brought about.

24. But farther, this supposed demonstration, that an actual coincidence of the inscribed figure may be effected by the forementioned motion, is really inconclusive; since from a like method of proceeding may be deduced this absurd conclusion, that hyperbolas coincide with their asymptotes.

25. Suppose an hyperbola BD, its diameter AH, and its asymptote AF. Now let the line AH revolve about the center A with an angular motion, till it coincides with the line AF; then, since it is demonstrated

ſtrated by geometers, that every right line drawn within the angle FAH will, if produced, meet with the hyperbola; it is evident, that the line AH will meet with it in every part of its motion through the angle FAH. Moreover, as the line approaches the aſymptote, the interſection thereof with the curve will continually become leſs and leſs diſtant from the aſymptote; inſomuch that this line may be made to approach ſo near the aſymptote, that this interſection ſhall be leſs remote from it, than by any diſtance, how minute ſoever, that can be named. Now let us



ſuppoſe the line AH to employ any given ſpace of time, as an hour, in paſſing over the angle HAF; then does the interſection of the revolving line with the hyperbola continually approach to the aſymptote during the ſpace of this hour, and before the end of the hour this point in the hyperbola will approach nearer to the aſymptote than by any difference, that can be propoſed; conſequently by the method of reaſoning above made uſe of, we muſt conclude, that at the end of the hour the hyperbola actually coincides with the aſymptote.

26. If it be examined, wherein lies the fallacy of theſe concluſions, it will be found, that though the

the meeting of the hyperbola and its aſymptote, and the coincidence of the inſcribed figure and the curvilinear ſpace, ſeem to be pointed out and determined by this form of reaſoning; yet to continue the hyperbola and aſymptote till they actually meet, requires the delineation of a line longer than any line, that can be aſſigned; and to deſcribe a figure within the curve under the ſuppoſed circumſtance of coincidence, requires the delineation of a line leſs than any line, that can be aſſigned: both which operations are equally impoſſible.

27. It may perhaps be worth while to examine, how it happens, that the meeting of the hyperbola and its aſymptote ſhould be acknowledged impoſſible, and yet the coincidence of the inſcribed figure and curvilinear ſpace ſo ſtrongly contended for, when they each of them require a conſtruction equally inconceivable and unattainable. The reaſon, I ſuppoſe, for this extraordinary partiality is, that as a quantity in augmenting without limit did moſt obviouſly paſs beyond the utmoſt ſtretch of imagination, it was without difficulty granted, that the delineation or conception of any ſuch magnitude was impoſſible. Whereas, when a quantity diminiſhed without limit, the imagination could trace it during the whole time of its diminution; and conſequently the conception of a quantity leſs than any whatever, has been thought poſſible by ſome, who allow the abſurdity of pretending to conceive a quantity greater than any whatever.

28. If it be ſaid, that though the hyperbola and its aſymptote cannot be deſcribed under the circumſtance of meeting each other; yet the inſcribed figure and the curvilinear ſpace can be deſcribed under the circumſtance of coincidence; ſo that the curvilinear ſpace itſelf is the laſt form of its inſcribed figure.

29. I anſwer, this is not true; for the ſuppoſed laſt form of the inſcribed figure muſt eſſentially differ from

from the curvilinear ſpace, the perimeter of the inſcribed figure contiguous to the curve being in every deſcription, and conſequently in this imagined laſt equal to the ſum of the lines *a*A, AE: whereas if the curvilinear ſpace was really the laſt form of the inſcribed figure, their perimeters could not differ. Since then the curvilinear ſpace is not the laſt form of the inſcribed figure, and ſince the laſt form of this figure cannot be deſcribed, but by the delineation or conception of a line leſs than any line, that can be aſſigned; it is evident, that the coincidence in this caſe does equally, with the meeting of the hyperbola and its aſymptote, involve an impoſſibility.

30. But the ſtrongeſt proof, that Sir *Iſaac Newton* does not always conſider this coincidence of the variable quantity, or ratio and its ultimate, as neceſſary in his method, is, that he himſelf tells us, that if two lines increaſing without limit have always a given difference, then their ultimate ratio will be the ratio of equality. Now the phraſe of ultimate ratio is peculiar to him and to his method, and cannot poſſibly be ſuppoſed in this place to have a ſignification different from what, it had in the firſt and ſubſequent *Lemmas*; conſequently the ultimate ratio is, by his own expreſs deſcription, a ratio, that the variable one, it is aſcribed to, cannot always coincide with.

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DISSERTATION

SHEWING,

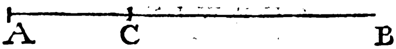
That the account of the doctrines of Fluxions, and of prime and ultimate Ratios, delivered in a treatiſe, entitled, A diſcourſe concerning the nature and certainty of Sir *Iſaac Newton*'s methods of fluxions, and of prime and ultimate ratios, *is agreeable to the real ſenſe and meaning of their great inventor.*

Firſt publiſhed in The Preſent State of the Republick of Letters *for* April 1736.

1. THE principal purſuit of the geometers of the laſt century was in ſearch after general methods for diſcovering the nature and properties of curved figures. Sir *Iſaac Newton* ſucceeded ſo happily in this attempt, as to eſtabliſh a very extenſive method of computation for theſe purpoſes. And being diſſatisfied with the doctrine of indiviſibles, or of infinitely ſmall quantities, which had been hither-

to employed in theſe enquiries, he introduced his doctrines of fluxions, and of prime and ultimate ratios, as juſter principles, whereon to found this method of computation. The book, we are now going to conſider, is confined to the explanation of theſe principles.

2. The ſubſtance of what we have there ſaid with relation to the conception and nature of fluxions, is as follows.



3. Let the line AB be ſuppoſed to be tracing out by the motion of a point ſetting forward from A; then the velocity of that point in any part of its motion, as at C, will be the fluxion of the line AC at that time deſcribed.

4. And as the velocity of this point in different parts of its motion may be ſuppoſed to be any how increaſed or diminiſhed, the degree of this increaſe or diminution at C is the ſecond fluxion of the line AC.

5. Again, ſince this increaſe or diminution may be of different degrees at different places, it may itſelf alſo have a change, which at different places will be greater or leſs. And the degree of this change at the place C is the third fluxion of the line AC.

6. The fluxion of other quantities is not the velocity of the motion, whereby they are increaſed, but the rate of that increaſe. And here the terms velocity, celerity, and degree of ſwiftneſs, which originally belong to actual motion, being applied to this rate of increaſe in a ſenſe ſomewhat figurative, Sir *Iſaac Newton* in lines choſe to call the actual motion, wherewith they are deſcribed, the fluxion of thoſe lines, as being an idea more obvious than the rate of their increaſe, which otherwiſe might have been aſ-

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signed for the fluxion of lines, as well as of all other quantities *.

7. The case of lines being thus the simplest in the doctrine of fluxions, we have shewn in our discourse, how that of all other flowing quantities may be reduced to this by causing a point so to pass over any streight line, that its length measured out, while the other flowing quantity is describing, shall augment in the same proportion with such flowing quantity. So that the fluxion or velocity of increase of this fluent, will be ever proportional to the actual velocity of the point describing the line. And from this consideration we demonstrated in particular, that when a curvilinear space is supposed to be described by the uniform and parallel motion of an ordinate, the fluxion of that space will be every where as the length of the ordinate.

8. That our account of fluxions is the same with that given by Sir *Isaac Newton*, will appear from his own words. In the introduction to his treatise of Quadratures he says, *Quantitates mathematicas, non ut ex partibus quam minimis constantes, sed ut motu continuo descriptas hic considero. Lineæ describuntur, ac describendo generantur, non per appositionem partium, sed per motum continuum punctorum, superficies per motum continuum linearum, &c. Considerando igitur, quod quantitates æqualibus temporibus crescentes, & crescendo genitæ, pro velocitate majori vel minori qua crescunt ac generantur, evadunt majores vel minores; methodum quærebam determinandi quantitates ex velocitatibus motuum vel incrementorum, quibus generantur;*

* As is done in Dr. *Wallis*'s Works, Vol. II. p. 391. where it is said of him, *Per* fluentes quantitates *intelligit indeterminatas, id est quæ in generatione curvarum per motum localem perpetuo augentur vel diminuuntur, et per earum* fluxionem *intelligit celeritatem incrementi vel decrementi.* And also in the quotation §. 8. from the Quadratures, Sir *Isaac Newton*, speaking of the fluxions of fluents in general, says, *Et earum fluxiones seu celeritates crescendi.*

& has motuum vel incrementorum velocitates nominando fluxiones, & quantitates genitas nominando fluentes, &c. Again, in the book itself he says, *Quantitates indeterminatas, ut motu perpetuo crescentes vel decrescentes, id est, ut fluentes vel defluentes, in sequentibus considero, designoque literis* z, y, x, v, *& earum fluxiones, seu celeritates crescendi noto iisdem literis punctatis. Sunt & harum fluxionum fluxiones, sive mutationes magis aut minus celeres, quas ipsarum* z, y, x, v *fluxiones secundas nominare licet, &c.* Moreover, in some observations he made on a letter of *Leibnitz*, which were printed at the end of *Raphson*'s *Historia Fluxionum* *, he declares, That in a paper written by him so long ago as 1665, the direct method of fluxions was set down in these words: *An equation being given expressing the relation of two or more lines* x, y, z, *&c. described in the same time by two or more moving bodies,* A, B, C, *&c. to find the relation of their velocities,* p, q, r, *&c.* And in the Philosophical Transactions, N° 342, p. 190 †, he says, *When he considers lines as fluents described by points, whose velocities increase or decrease, the velocities are the first fluxions, and their increase the second.* And these last have particular regard to the first part of his description of fluxions, where he calls them *velocitates motuum.*

9. Here it most manifestly appears, that our description of fluxions is the very same, Sir *Isaac Newton* has himself delivered; and as he has never attempted to represent them in any other light, we cannot sufficiently admire, how he came to be charged by a late writer with having given so various and inconsistent accounts of them ||. And this notion of fluxions and their different orders is evidently free not only from any impossible, but even obscure sup-

* P. 116, or Recueil de diverses Pieces sur la Philosophie, &c. à Amsterd. 1720, Tom. II. p. 89.

† Or Commercium Epistolic p. 10.

|| Defense of Free-Thinking in Mathematicks, p. 41.

positions.

positions. Insomuch that this writer, for the support of his objections against this doctrine, found it necessary to represent the idea of fluxions as inseparably connected with the doctrine of prime and ultimate ratios, intermixing this plain and simple description of fluxions with the terms used in that other doctrine, to which the idea of fluxions has no relation: and at the same time by confounding this latter doctrine with the method of *Leibnitz* and the foreigners, has proved himself totally unskilled in both.

10. These two methods of Sir *Isaac Newton* are so absolutely distinct, that their author had formed his idea of fluxions before his other method was invented, and that method is no otherwise made use of in the doctrine of fluxions, than for demonstrating the proportion between different fluxions. For, in Sir *Isaac Newton*'s words*, as the fluxions of quantities are nearly proportional to the contemporaneous increments generated in very small portions of time, so they are exactly in the first ratio of the *augmenta nascentia* of their fluents. With regard to this passage the writer of the Analyst has made a twofold mistake. First, he charges Sir *Isaac Newton*, as saying these fluxions are very nearly as the increments of the flowing quantity generated in the least equal particles of time. Again, he always represents these *augmenta nascentia*, not as finite indeterminate quantities, the nearest limit of whose continually varying proportions are here called their first ratio, but as quantities just starting out from non-existence, and yet not arrived at any magnitude, like the infinitesimals of the differential calculus. But this is contrary to the express words of Sir *Isaac Newton*, who after he had shewn how to assign by his method of

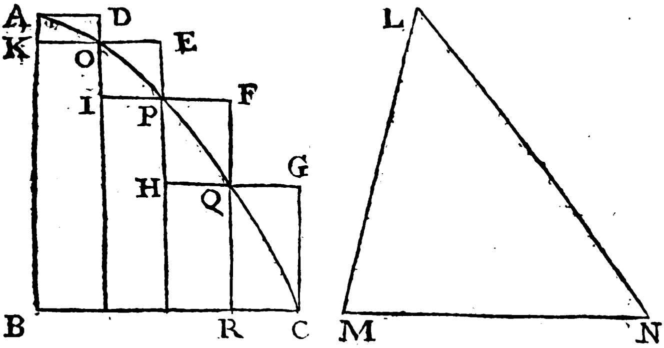
* Fluxiones sunt quam proxime ut fluentium augmenta æqualibus temporis particulis quam minimis genita, &, ut accurate loquar, sunt in prima ratione augmentorum nascentium. *Newton. Introd. ad Quad. Curv.*

prime and ultimate ratios the proportion, that different fluxions have to one another, he thus concludes. *In finitis autem quantitatibus Analysin sic instituere, & finitarum nascentium vel evanescentium rationes primas vel ultimas investigare consonum est geometriæ veterum: & volui ostendere, quod in methodo fluxionum non opus sit figuras infinite parvas in geometriam introducere.*

11. And this leads us to consider Sir *Isaac Newton*'s doctrine of prime and ultimate ratios, which is of the greatest use in demonstrations relating to curves. It is no other than an abbreviation and improvement of the form of demonstrating used by the ancients on the like occasions. For this reason we premised to our explanation of prime and ultimate ratios a short description of that method, which we shall now consider more at large.

12. As no two different curves can be so laid on each other as to coincide either in whole or in part, it is evident, that the spaces bounded by such curve lines cannot be immediately compared either with each other, or with right-lined figures; nor for the same reason can such spaces be the sums or differences of others, that are capable of being thus compared. In examining then the dimensions and proportions of these curvilinear spaces, some other method must be made use of, than those that are required in the comparison of right-lined figures.

13. Suppose a space bounded by the curve AC, and the right lines AB, BC. Let the base BC be divided into any number of parts, and on those divisions let parallelograms be drawn forming the figures ADOEPFQGCB and KOIPHQRB, the first circumscribing, and the last inscribing the given space ACB. It is now obvious, that by diminishing the breadth of these parallelograms, these figures may be made to differ from the space ACB, and from each other by less than any space, how minute soever, that shall be named; that is, the circumscribed



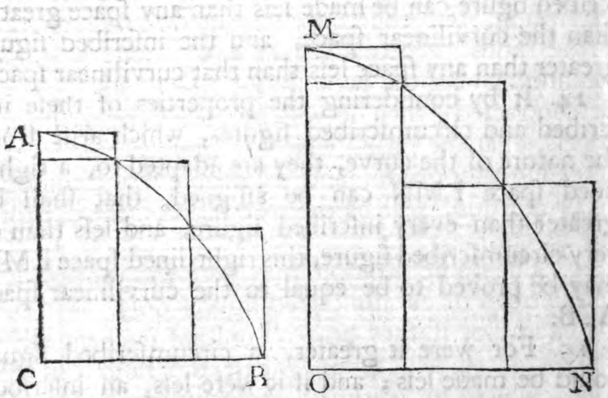
scribed figure can be made less than any space greater than the curvilinear space, and the inscribed figure greater than any space less than that curvilinear space.

14. If by considering the properties of these inscribed and circumscribed figures, which arise from the nature of the curve, they are adapted to, a right-lined space LMN can be assigned, that shall be greater than every inscribed figure, and less than every circumscribed figure, this right-lined space LMN may be proved to be equal to the curvilinear space ACB.

15. For were it greater, a circumscribed figure could be made less; and if it were less, an inscribed figure could be made greater.

16. Instead of both inscribed and circumscribed figures, we might have made use of one of them only, suppose of the inscribed, by proving the space MLN to be greater than every inscribed figure, and also capable of being approached by such a figure within less than any given difference. For thus the space MLN can neither be less nor greater than the curvilinear space. If it were less, an inscribed figure would exceed it; and if it were greater, no inscribed figure could approach it so near as its excess above the curve.

17. Again, ſuppoſe there are two curvilinear ſpaces, ACB, and MON. If parallelograms, whoſe breadth may be any how diminiſhed, are drawn inſcribing and circumſcribing theſe curves; and if they are deſcribed in ſuch a manner, that the circumſcribed figure of one curve to the circumſcribed figure of the other, and the inſcribed to the inſcribed, has one and the ſame conſtant proportion in every deſcription: I ſay, that the curvilinear ſpace ACB is to the curvilinear ſpace MON in that proportion, which the inſcribed and circumſcribed figures conſtantly bear to each other.



18. For no ſpace greater than ACB can have to MON this proportion; ſince if it could, a figure might be circumſcribed about ACB leſs than this ſuppoſed greater ſpace, and this circumſcribed figure to its correſpondent figure circumſcribing MON would be in the ſame proportion, as the ſuppoſed greater ſpace to the curvilinear ſpace MON; that is, four quantities being in the ſame proportion, the firſt would be leſs than the third, and the ſecond greater than the fourth. Nor can any ſpace leſs than ACB have to MON the conſtant proportion of the figures in

in one curve to the figures in the other. For if it could, a figure might be inſcribed within ACB, which would be greater than this ſuppoſed leſſer ſpace; and this inſcribed figure to its correſpondent figure inſcribing MON would be in the ſame proportion, as this imagined leſſer ſpace to the curvilinear ſpace MON; that is, four quantities being in the ſame proportion, the firſt would be greater than the third, and the ſecond leſs than the fourth. Thus no ſpace but ACB can be to MON in the conſtant proportion of the circumſcribed and inſcribed figures.

19. If the proportion of the circumſcribed figures, or of the inſcribed is not the ſame in every deſcription, but conſtantly changing (ſuppoſe one of the ratios perpetually increaſing, and the other diminiſhing) as the breadths of the parallelograms are contracted; then it will be ſhewn by a ſimilar proceſs, that the ratio, which is greater than every increaſing proportion, and leſs than every diminiſhing proportion, will be the true ratio of the curvilinear ſpaces.

20. The demonſtration may here too proceed by the inſcribed or circumſcribed figures only, as in the firſt example, if they bear a conſtant unchangeable ratio in every deſcription, or if the ratio can be found, which the ratio of thoſe figures does perpetually approach, and to which by diminiſhing the parallelograms it will at laſt come nearer, than to any other ratio, that ſhall be given.

21. Though we have here made uſe of parallelograms, yet any other method of deſcribing the circumſcribed and inſcribed figures, may equally take place; provided the figures ariſing from ſuch deſcription can be made to differ from the curvilinear ſpace by leſs than any difference whatever, that ſhall be propoſed. And in the deſcription of theſe figures lies the great artifice of theſe demonſtrations; for they ought to be ſo drawn, that the right lined ſpace in our firſt in-

ſtance,

ſtance, greater than the inſcribed, and leſs than the circumſcribed figure, may from the conſideration of theſe figures be moſt eaſily determined; or that the proportions of theſe figures in the following inſtances, may in every deſcription be eaſily aſſignable.

22. In the manner here deſcribed did the ancient geometers demonſtrate, whatever they diſcovered relating to the dimenſions or proportions of curve lines, curvilinear ſpaces, and ſolids bounded by curved ſurfaces. And this is the form of demonſtration, which is now called the method of exhauſtions. But as theſe demonſtrations, by determining diſtinctly all the ſeveral magnitudes and proportions of theſe inſcribed and circumſcribed figures, did frequently extend to very great lengths, other methods of demonſtrating had been contrived, whereby to avoid theſe circumſtantial deductions. The firſt attempt of this kind, known to us, is that we mentioned to have been made by *Lucas Valerius*. But afterwards *Cavalerius*, an *Italian*, about the year 1635, advanced his method of indiviſibles, in which he propoſes not only to abbreviate the ancient demonſtrations, but to remove the indirect form of reaſoning uſed by them of proving the equality or proportion between lines and ſpaces from the impoſſibility of their having any different relation; and to apply to theſe curved magnitudes the ſame direct kind of proof, that was before applied to right lined quantities.

23. This method of comparing magnitudes invented by *Cavalerius*, ſuppoſes lines to be compounded of points, ſurfaces of lines, and ſolids of planes; or, to make uſe of his own deſcription, ſurfaces are conſidered as cloth conſiſting of parallel threads, and ſolids are conſidered as formed of parallel planes, as a book is compoſed of its leaves, with this reſtriction, that the threads or lines, of which ſurfaces are compounded, are not to be of any conceivable breadth, nor the leaves or planes of ſolids of

any

any thickness. He then forms these propositions, that surfaces are to each other as all the lines in one to all the lines in the other, and solids in like manner in the proportion of all their planes.

24. But this method of indivisibles, as here explained, is manifestly founded on inconsistent and impossible suppositions. For while the lines, of which surfaces are supposed to be made up, are real lines of no breadth; it is obvious, that no number of them can form the least imaginable surface: if they are supposed to be of some sensible breadth, in order to be capable of filling up spaces, that is, in reality to be parallelograms, how minute soever be their altitude, the surfaces may not be to each other in the proportion of all such lines in one to all the like lines in the other; for surfaces are not always in the same proportion to each other with the parallelograms inscribing them.

25. The same contradictory suppositions do obviously attend the composition of solids by parallel planes, or of lines by such imaginary points.

26. This heterogeneous composition of quantity, and confusion of its species, so different from that distinctness, for which the mathematicks were ever famous, was opposed at its first appearance by several eminent geometers, particularly by *Guldinus* and *Tacquet*, who not only excepted to the first principles of this method, but tax the conclusions formed upon it, as erroneous. But as *Cavalerius* took care, that the threads or lines, of which the surfaces to be compared together were formed, should have the same breadth in each (as he himself expresses it) the conclusions deduced by his method might generally be verified by sounder geometry; since the comparison of these lines was in effect the comparing together the inscribed figures.

27. As in the application of this method, error by proper caution might be avoided, the assistance it

it ſeemed to promiſe in the analytical part of geometry, made it eagerly followed by thoſe, who were more deſirous to diſcover new propoſitions, than ſollicitous about the elegance or propriety of their demonſtrations. Yet ſo ſtrange did the contradictory conception appear of compoſing ſurfaces out of lines, and ſolids out of planes, that in a ſhort time it was new modelled into that form, which it ſtill retains, and which now univerſally prevails amongſt the foreign mathematicians.

28. In this reformed notion of indiviſibles, ſurfaces are now ſuppoſed as compoſed not of lines, but of parallelograms, having infinitely little breadths and ſolids in like manner as formed of priſms having infinitely little altitudes. By this alteration it was imagined, that the heterogeneous compoſition of *Cavalerius* was ſufficiently evaded, and all the advantages of his method retained.

29. But here again the ſame abſurdity occurs, as before. For if by the infinitely little breadth of theſe parallelograms we are to underſtand, what theſe words literally import, that is, no breadth at all; then they cannot, any more than the lines of *Cavalerius*, compound a ſurface; and if they have any breadth, the right lines bounding them cannot coincide with a ſurface bounded by a curve line.

30. The followers of this new method grew bolder than the followers of *Cavalerius*; and having transformed his points, lines, and planes, into infinitely little lines, ſurfaces and ſolids, they pretended, they no longer compared together heterogeneous quantities, and inſiſted on their principles being now become genuine: but the miſtakes, they frequently fell into, were a ſufficient confutation of their boaſts. For notwithſtanding this new model, the ſame limitations and cautions were ſtill neceſſary. For inſtance, this agreement between the inſcribing figures and the curved ſpaces, to which they are adapted, is

only

only partial, and in applying their principles to propositions already determined by a juſter method of reaſoning, they eaſily perceived this defect: both in ſurfaces and ſolids it was evident at the firſt view, that the perimeters diſagreed. But as no one inſtance can be given, where theſe indiviſible or infinitely little parts do ſo compleatly coincide with the quantities, they are ſuppoſed to compound, as in every circumſtance to be taken for them without producing erroneous concluſions; we find where a ſurer guide to correct their reaſoning was wanting or diſregarded, theſe figures were often imagined to agree, where they ought to have been ſuppoſed to differ.

31. We might produce numerous inſtances of ſuch errors from the writings of modern computiſts. The moſt celebrated of theſe, *Leibnitz* and *John Bernoulli*, will furniſh us with ſufficient examples, from the ſingle error of ſuppoſing infinitely ſmall arches abſolutely to coincide with their chords.

32. The firſt, in two diſſertations, one on the reſiſtance of fluids, and another on the motion of the heavenly bodies, has on this principle reaſoned falſly concerning the lines intercepted between curves and their tangents, as has been obſerved by Sir *Iſaac Newton* himſelf*. And *Bernoulli*, in a diſſertation likewiſe on the reſiſtance of fluids, has made the ſame miſtake, and even inſulted over Sir *Iſaac Newton*, where he had by means of his juſter method of reaſoning avoided that error; and *Bernoulli* upon the ſame principles had erred formerly in a pretended ſolution of the problem concerning iſoperimetrical curves †.

33. But the proceeding of Monſ. *Parent* is ſo very extraordinary, that it deſerves alſo to be mentioned. He has had the raſhneſs to oppoſe erroneous deduc-

* Philoſoph. Tranſ. N° 342. p. 208. Or Comm. Epiſt. p. 42.
† Acta Erudit. An. 1706.

tions

tions from this abſurd principle to the moſt indubitable demonſtrations of the great *Huygens*. It having been ſhewn by *Galileo*, that any heavy body will deſcend through the chord of a circle terminating at its loweſt point in the ſame time, as it will fall through its perpendicular diameter; from this principle of indiviſibles, that the arch and the chord do at laſt coincide, it was falſly concluded, that the time of the fall through the ſmalleſt arches muſt be equal to the time of the fall through the diameter. This conteſt between demonſtration and error has been thought of ſuch importance, that ſeveral elaborate diſſertations have been publiſhed to ſhew by what means, the idolized doctrine of infinitely ſmall quantities could produce fallacious concluſions *.

34. Thus it appears, that the doctrine of indiviſibles contains an erroneous method of reaſoning, and in conſequence thereof, in every new ſubject, to which it ſhall be applied, is liable to freſh errors.

35. It is alſo manifeſt, that the great brevity it gave to demonſtrations, aroſe entirely from the abſurd attempt of comparing curvilinear ſpaces in the ſame direct manner, as right lined figures can be compared; for in order to conclude directly the equality or proportion of ſuch ſpaces no ſcruple was made of ſuppoſing, contrary to truth, that rectilinear figures capable of ſuch direct compariſon could adequately fill up the ſpaces in queſtion; whereas the doctrine of exhauſtions does not attempt from the equality or proportion of the inſcribing or circumſcribing figures to conclude directly the like proportions of theſe ſpaces, becauſe thoſe figures can never in reality be made equal to the ſpaces, they are adapted to. But as theſe figures may be made to differ from the ſpaces, to which they are adapted, by leſs than any ſpace propoſed, how minute ſoever; it ſhews, by a juſt

* See Memoires de l'Acad. des Sciences, 1722.

though

though indirect deduction, from these inscribing and circumscribing figures, that the spaces, whose equality is to be proved, can have no difference, and that the spaces, whose proportion is to be shewn, cannot have a different proportion from that assigned them.

36. But Sir *Isaac Newton*, by his doctrine of prime and ultimate ratios, has found out the proper medium, whereby to avoid the impossible notion of indivisibles on the one hand, and the length of exhaustions on the other. As it was Sir *Isaac Newton's* express design absolutely to free his method of reasoning from every part of the obscurity and inconsistent notions of indivisibles, and as all the objections raised against him have entirely been grounded on a suspicion, that he has not fully succeeded in that design; we thought the readiest method of vindicating this doctrine was to cast it into a form as remote as possible from any appearance of agreement with that absurd system.

37. For this purpose we defined the ultimate magnitude of any varying magnitude to be a fixt quantity, which the varying one can approach within any degree of nearness, and yet can never pass: And the ultimate ratio of any varying ratio to be a fixt ratio, which the varying one can approach with any degree of nearness, but yet can never pass.

38. These definitions thus premised, we demonstrated the two following propositions.

I. That when the varying magnitudes continue always in the same ratio, their ultimate magnitudes will be also in the same ratio.

II. That all the ultimate ratios of the same varying ratio are the same to each other.

39. In this form it was easy to explain the doctrine of prime and ultimate ratios without the use of any of those expressions, which had been misunderstood to have reference to the method of indivisibles.

But

But to make appear the identity of this doctrine, as by us expounded, with what Sir *Isaac Newton* has himself delivered; we premised a short representation of the true sense, in which we apprehended, his phraseology ought to be understood. As he founded his doctrine on the first lemma in the first section of his *Principia Philosophiæ*, in my discourse I have thus represented his meaning in that lemma, That in this method any fixt quantity or ratio, which some varying quantity or ratio by a continual augmentation or diminution shall perpetually approach, but never pass, is considered as the quantity or ratio, to which the varying one will at last or ultimately become equal; provided the varying quantity or ratio can be made in its approach to the other to differ from it less than by any difference how minute soever, that can be assigned. Consonant to this representation of Sir *Isaac Newton*'s meaning, in the account given of my book, this lemma was thus interpreted; those quantities are to be esteemed ultimately equal, and those ratios ultimately the same, which are perpetually approaching each other in such a manner, that any difference how minute soever being given, a finite time may be assigned, before the end of which the difference of those quantities or ratios shall become less than that given difference.

40. And to this interpretation the following remark was subjoined, That this lemma did not mean, or necessarily imply, that any point of time was assignable, wherein these varying magnitudes would become actually equal, or the ratios really the same; but only that no difference whatever could be named, which they should not pass.

41. This interpretation of Sir *Isaac Newton* is evidently conformable to our definitions.

42. A learned gentleman, who, concealed under the name of *Philalethes Cantabrigiensis*, had expressly entered into controversy with the author of the

Analyst

Analyst, though he allows the truth of our method of reasoning to be unqueſtionable; yet he thinks, we have in ſome meaſure deviated from the exact intention of Sir *Iſaac Newton*. He has interpreted the forementioned lemma after a manner ſomething different. But his interpretation does not aſcribe to the word *given*, uſed by Sir *Iſaac Newton* in this lemma, the true ſenſe of that word in geometry, but ſuppoſes it to ſtand for *aſſignable*; whereas it properly ſignifies only, what is actually aſſigned. *Philalethes* inſinuates, that by our interpretation, and the forementioned remark upon it, Sir *Iſaac Newton* is rendered obnoxious to the charge of firſt ſuppoſing, what he would prove, and with proving only what he has before ſuppoſed. But our interpretation cannot poſſibly mean leſs than this, that thoſe quantities and ratios will have no laſt difference, which are perpetually approaching each other in ſuch a manner, that any difference how minute ſoever being given, a finite time may be aſſigned, before the end of which the difference of thoſe quantities or ratios ſhall become leſs than that given difference. This is certainly no identical propoſition, though its truth be very obvious; and Sir *Iſaac Newton*'s demonſtration of it is accordingly very ſhort.

43. However I acknowledge, had Sir *Iſaac Newton* here no other intention, but ſimply to prove ſo obvious a truth, a diſtinct propoſition for this purpoſe only might perhaps have well been ſpared. But we muſt conſider this propoſition in another light. There are two ways, whereby good writers explain the uſe of terms they introduce: one is by expreſsly defining them; another, when, to avoid that formality, they convey the ſenſe of ſuch terms by their manner of uſing them. And to make appear, that Sir *Iſaac Newton*, by the demonſtration annexed to this lemma, has ſufficiently evinced, in what ſenſe the lemma itſelf muſt be underſtood, and at the

ſame time to prove what that ſenſe is, it was ſhewn, that this demonſtration is no leſs applicable to quantities, which only approach without limit to the ratio of equality, than it will agree to ſuch quantities, as at laſt become actually equal. For this purpoſe this demonſtration was applied to the ordinate of an hyperbola, compared with the ſame continued to the aſymptote, which do approach without limit to the ratio of equality, though they never become actually equal. But as *Philalethes* has taken exception to this inſtance, not conceiving how to regulate this approach, ſo as to bound it within a finite time; without enquiring how far that limitation was neceſſary to our purpoſe, we ſhewed a method of adding this circumſtance by cauſing a line to turn upon the center of the hyperbola, and paſs with an equable motion from the diameter to the aſymptote: for by ſuppoſing the forementioned ordinate continually to accompany the interſection of this line with the hyperbola, the whole motion here required will be brought within ſuch a finite ſpace of time, as he imagined neceſſary. And in this view it is equally manifeſt, that the ordinate, and its continuation, can never become equal, till they are both extended to infinity, and all idea of them is loſt *.

44. By this we think it very evident, that Sir *Iſaac Newton* has neither demonſtrated the actual equality of all quantities capable of being brought under this lemma, nor that he intended ſo to do.

45. When-

* *Philalethes* not perceiving, how the demonſtration of Sir *Iſaac Newton's* firſt *lemma* could be applied to this caſe, Mr. *Robins* thus ſhewed it.

Here the hypotheſis aſſumed is a property of the hyperbola commonly known, That the curve continually approaches its aſymptote, as it is farther extended, ſo that by removing any ordinate farther and farther from the vertex, it will approach nearer and nearer in magnitude to the ſame continued to the aſymptote, without limit. Now to prove the ultimate equality of

45. Whenever the quantities or ratios compared in this lemma are capable of an actual equality, they must really become so. But when they are incapable of such equality, the phrase of ultimately equal must of necessity be interpreted in a somewhat laxer sense; that is, as Sir *Isaac Newton* in the 71 proposition of the first book of his *Principia* expresses it, *pro aequalibus habeantur*, *are to be esteemed equal* *, and means only, that such quantities or ratios approach without limit. Accordingly we find, that immediately after this lemma he uses the expressions *ultimo in ratione aequalitatis*, and *ultimo aequales*, as synonymous terms. However, as in every subject of this lemma all ultimate difference is excluded, the consequences drawn

of these two lines, let us say thus. If you deny it, let them be ultimately unequal, and let their difference be D; therefore they cannot approach to equality nearer than by the given difference D, contrary to the hypothesis. Thus is the demonstration of this *lemma*, without changing a single word, applied to the present case †. *Present State of the Republick of Letters for* August 1736, p. 100.

* *Philalethes* objected to this translation, *are to be esteemed equal*, and contended, that it ought to have been rendered, *let them be taken for equal*, or, *let them be esteemed equal*. To which Mr. *Robins* replied thus.

But why is *habeantur* the imperative mood? *Philalethes* was deceived by the preceding words, which direct certain constructions; but here, where a consequence is concluded from these constructions, the potential mood is required, in which, to use the learned *Linacer*'s words, *indicatio est potentiae, debitive, aliquando voluntatis*. How then is, *are to be esteemed equal*, a false translation? Is not that *pro aequalibus haberi debent?* Just as *non expectes, ut statim gratias agat, qui sanatur invitus*, is expounded by this great grammarian *non debes expectare*. See his excellent treatise, *De emendata structura latini sermonis, lib.* 1. *De Modo*.

I have quoted *Linacer*, because he was the first, who gave the name of potential to this mood, when it bears any of the three significations here mentioned: but this form of the verb having all these senses is a point agreed among grammarians. See *Alvarez* and *Vossius*. Present State of the Republick of Letters for *August* 1736, p. 102.

† Review, § 20.

from it are equally juſt and perſpicuous, whether the quantities do or do not become actually equal, and the ratios actually coincident. And this reſtriction of the ſenſe of this lemma is abſolutely neceſſary to be attended to in this doctrine; becauſe Sir *Iſaac Newton* himſelf has applied it to quantities and ratios incapable of an actual equality or agreement.

46. In the account of our Diſcourſe, the lemma immediately following, where parallelograms are inſcribed, and others circumſcribed to a curvilinear ſpace, was produced as an example of this. It was there alſo obſerved, that vaniſhing quantities may never actually have that proportion, which, according to this lemma, is ſaid ultimately to belong to them.

47. In this ſecond lemma Sir *Iſaac Newton* directs, that the number of theſe parallelograms ſhould be augmented *in infinitum*. This muſt not be interpreted, till the number become infinitely great: for this is the expreſs language of indiviſibles. We render the words *in infinitum*, *endleſsly*, and perform, what is here directed, by that ſimple and obvious method practiſed by the ancient geometers, of continually ſubdividing the baſe of the curve *. And it

* *Philalethes* thus animadverted on this place.

I apprehend our preſent enquiry is, not how Mr. *Robins* performs what is here directed; but how Sir *Iſaac Newton* intended it ſhould be performed.

There are two ways, which we may conceive the baſe of the curve to be continually ſubdivided. One is, the method practiſed by *Euclid* and the other *ancient Geometers*, which conſiſts in continually repeating the operation directed in the tenth propoſition of the firſt book, or in the tenth propoſition of the ſixth book of the Elements.

To which animadverſion Mr. *Robins* made this reply.

Mr. *Robins* thinks himſelf directed by the words of Sir *Iſaac Newton* to make the ſubdiviſion in the manner here propoſed. *Philalethes*, in not underſtanding this place, confirms Mr. *Robins* in his opinion, how needful a knowledge in the ancients is to qualify it is manifest, that such subdivision can never be actually finished. *Philalethes* on the other hand endeavours to describe a complex kind of motion, whereby he apprehends this multiplication of the parallelograms can be brought to a period. But as the inscription and circumscription of these figures require, that the bases of those parallelograms be constantly equal, and each some aliquot part of the whole base, any such description by continued motion is necessarily excluded, as has been already observed *. *Philalethes* charges this with being too hasty an assertion: because in another lemma, namely the third, parallelograms are supposed to be described to a curve, whose bases should not be equal. But it was not asserted, that Sir *Isaac Newton* had supposed this equality in all the propositions, wherein he may have had occasion to consider parallelograms described to curves; but that it was constantly and always to take place in this second lemma; which being a distinct and separate proposition, must have a demonstration compleat within itself.

48. However, should we so far depart from Sir *Isaac Newton*, as to admit of this complex kind of motion, what idea can we form of the inscribed or circumscribed figure, to which we are at last

qualify a person for understanding either Sir *Isaac Newton* or himself. But however *Philalethes* has endeavoured to shew his knowledge in the ancients by quoting two propositions from *Euclide*'s *Elements*; one of which teaches, how to divide a line in the same proportion as some other line is divided, the other shews, that by taking from any quantity more than half, and from the remainder more than half continually, the residue may be reduced within any degree of smallness. How much these propositions are to the purpose of inscribing and circumscribing parallelograms to a curvilinear space, let *Philalethes* shew. When *Philalethes* has gone further in the ancients than the *Elements* of *Euclide*, he will be better able to comprehend Mr. *Robins*'s meaning, and judge upon the point in question. *Present State of the Republick of Letters for* August 1736, p. 107.

* Review, § 23.

actually to arrive, which with any propriety of ſpeech is to be ſtiled equal to the curve?

49. Sir *Iſaac Newton* in the corollaries annexed to the third lemma expreſsly declares, there is no ultimate ſum of theſe parallelograms, nor no ultimate figure compounded of them, diſtinct from the very curve itſelf. *Philalethes* himſelf acknowledges this. Though how far this conceſſion agrees with the reſt of his opinion, will be beſt underſtood hereafter. But at preſent we ſhall examine more particularly Sir *Iſaac Newton*'s meaning in the corollaries now mentioned.

50. The treatiſe of Sir *Iſaac Newton*, in which this doctrine is delivered, is written throughout with that degree of brevity, as occaſions a general complaint of the difficulties attending the ſtudy of it. And this conciſeneſs is no where perhaps more remarkable than in the ſection now under conſideration; inſomuch, that it often requires careful attention to diſcover the exact meaning, and full force of the expreſſions. More than once Sir *Iſaac Newton*, to convey his intention the eaſier to thoſe, who had been accuſtomed to the method of indiviſibles, has introduced ſome expreſſions analogous to the phraſeology of that doctrine, when the brevity, he had preſcribed to himſelf, occaſioned his not giving expreſs notice of it. Of this kind we muſt reckon the concluſion of the following paſſage. *Ultimæ rationes illæ, quibuſcum quantitates evaneſcunt, revera non ſunt rationes quantitatum ultimarum, ſed limites ad quos quantitatum ſine limite decreſcentium rationes ſemper appropinquant; et quas propius aſſequi poſſunt quam pro data quavis differentia, nunquam vero tranſgredi, neque prius attingere quam quantitates diminuuntur in infinitum* *. The laſt words of this paſſage mean in reality no more, than that the quantities will never have the

* Newtoni Princip. Lib. 1. Lem. xi. in Schol.

ratio

ratio mentioned; and the only reaſon to be aſſigned, why Sir *Iſaac Newton* expreſſed this in the manner he has done, is, that he addreſſes himſelf to thoſe, who had been accuſtomed to indiviſibles, and accommodates himſelf to their language. The expreſſion in the firſt of the corollaries before-mentioned is evidently adapted to the ſame deſign. The *ultima ſumma* there mentioned, in ſtrict propriety of ſpeech, has no kind of meaning, for it is really infinite. His intention could only be here to ſignify, that what in the language of indiviſibles might be called the laſt ſum of theſe parallelograms, or the figures ſuppoſed in that method to be compoſed of ſuch infinite number of parallelograms, or other right-lined figures, is really nothing diſtinct from the curve itſelf.

51. To aſſert that any collection of theſe inſcribed or circumſcribed parallelograms can ever become actually equal to the curve, is certainly an impropriety of ſpeech; for equality can properly ſubſiſt only between figures diſtinct from each other. Such expreſſions therefore are ſo far from giving any additional advantage to this method of reaſoning, that they can only tend to confound this method with indiviſibles. For we have already ſhewn, that the eſſence of indiviſibles conſiſts in endeavouring to repreſent to the mind ſuch inſcribed or circumſcribed figure, as actually ſubſiſting, equal to the curve. That method does not merely depend on a particular ſet of exceptionable expreſſions; but however the phraſeology be varied, yet while the ſame mode of thinking is attempted, we are ſtill involved in that erroneous doctrine. Whatever ſtate of the inſcribed or circumſcribed figure is ſuppoſed previous to this equality, or by whatever changes it is conceived to degenerate into its imagined laſt form: yet as long as we fancy ourſelves capable of ſeeing directly in this laſt form thoſe properties, which theſe figures had before, we are ſtill immerged in

the doctrine of indivisibles. It is certainly therefore an advantage to this method, to seclude from it any expressions leading towards such faulty conceptions.

52. But *Philalethes* seems apprehensive of weakning the force of Sir *Isaac Newton*'s demonstrations by this means, when he tells us, that Sir *Isaac Newton contents not himself with any the most near approximations, but carries his demonstrations to the utmost accuracy and geometric rigor; accordingly every one of the examples he has given in the lemmata of the first section, are of such quantities and ratios, as do actually arrive at their respective limits.* Does *Philalethes* here suppose the truth of Sir *Isaac Newton*'s demonstrations to depend on this actual equality of the variable quantity and its limit? He confesses our demonstrations to be just, which do not suppose this actual equality.

53. He also says, that *the supposing this actual equality seems greatly to exceed the method of the ancients in perspicuity as well as in the conciseness of its demonstrations.*

54. That this method should be more perspicuous is impossible, the method of the ancients being perfect in that respect. Certainly there are not in their method, what *Philalethes* (though I think without reason) insinuates of this, any demonstrated truths, that must be owned, though we do not perfectly see every step, by which the thing is brought about. That it exceeds the method of the ancients in conciseness is true; but that is not occasioned by this supposed actual equality of the variable quantity and its ultimate; since, as we have shewn this to be at the best but a superfluous circumstance, the supposing it necessary is so far from promoting conciseness, that it adds to the length of this doctrine, by obliging us to labour in every case after some idea of motion however intricate, whereby to represent to our minds this actual equality. In the case we have been considering, the motion proposed for forming these parallelograms is sufficiently intricate. If it were conveni-

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ent to make use of a polygon described within or without a curve, as in the first proposition of the second section of the first book of the *Principia*, another kind of motion must be accommodated to that case. And so for every variation of these inscriptions and circumscriptions we must strain our imagination for some involved and perplexed kind of motion applicable to each *.

55. The sum of what, we have been saying, amounts to this, that in our interpretation of Sir *Isaac Newton*, no subtle inquiry after means to bring about an actual equality between the curve and the inscribed and circumscribed figures is at all necessary; which is affixing intricate circumstances to this method, no way necessary to the truth or clearness of

* On this passage *Philalethes* thus observes.

However, since Mr. *Robins* is pleased to talk so much about *straining our imagination for some involved and perplexed kind of motion*, let us see, if we cannot find some plain and easy way, of representing to the imagination, that actual equality, at which the inscribed and circumscribed figures will arrive with each other, and with the curvilineal figure, at the expiration of the finite time.

Accordingly *Philalethes* attempted to represent to the imagination, what he calls the actual equality, at which the inscribed and circumscribed figures will arrive with each other, and with the curvilinear figure, at the expiration of the finite time.

To this Mr. *Robins* answered, Is this producing any continued motion for the purpose, Mr. *Robins* speaks of?

However, *Philalethes*, in executing his own design imagined two curves to be described, whose ordinates might express continually the proportion between the inscribed and circumscribed parallelograms in question; by the intersection or concurrence of which curves it might be found, when the inscribed and circumscribed figures become equal. But though each of these lines are drawn by *Philalethes* in his figure, as simple curves; yet in reality they are each compounded of an endless number of portions of as many different curves combined together. This, adds Mr. *Robins*, is a specimen of *Philalethes*'s skill in the common algebra of curve lines, to give us an equation expressing the nature of a single curve, one which in reality includes an infinite series. *Appendix to the Present State of the Republick of Letters for* September 1736, p. 1.

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the demonstrations. For this actual equality can scarce be represented to the mind, but in such complex, not to say confused ideas, that will bring us upon the very borders of indivisibles, and render us perpetually obnoxious to the absurdities of that doctrine. It is therefore without question an advantageous representation of this method, to free it from every perplexity of this kind. And we have shewn our interpretation, which thus removes this doctrine quite beyond the reach of every objection, that has hitherto been levelled against it, to be conformable to the most proper signification of Sir *Isaac Newton*'s words, both in his second lemma, where those figures are considered, and also in the first, where the principles of this method are established.

56. And our interpretation of this first lemma is still more abundantly necessary in applying this lemma to what, Sir *Isaac Newton* calls vanishing quantities. For as these quantities are supposed continually to diminish, and by that means to have their proportions varied; nothing is more evident, than that their dimunition will never bring them actually to bear that ratio to one another, with which in this lemma the ratio of these quantities is said to become ultimately the same.

57. This will most evidently appear by Sir *Isaac Newton*'s words, *Ultimæ rationes illæ, quibuscum quantitates evanescunt, revera non sunt rationes quantitatum ultimarum, sed limites, ad quos quantitatum sine limite decrescentium rationes semper appropinquant; & quas proprius assequi possunt quam pro data quavis differentia, nunquam vero transgredi, neque prius attingere, quam quantitates diminuuntur in infinitum. Res clarius intelligetur in infinite magnis. Si quantitates duæ, quarum data est differentia, augeantur in infinitum, dabitur harum ultima ratio, nimirum ratio æqualitatis; nec tamen ideo dabuntur quantitates ultimæ, seu maximæ, quarum ista est ratio. In sequentibus igitur, si quando facili rerum*

rerum conceptui consulens, dixero quantitates quam minimas, vel evanescentes, vel ultimas; cave intelligas quantitates magnitudine determinatas, sed cogita semper diminuendas sine limite *.

58. Here it is expresly declared, that these *ultimæ rationes, quibuscum quantitates evanescunt*, are not *rationes quantitatum ultimarum*; but only limits, to which the ratios of these quantities, which themselves decrease without any limit, continually approach; and to which these ratios can come within any difference, that may be given, but never pass, nor even reach those limits, before the quantities are diminished to nothing. To explain this more distinctly, and to prevent, as much as possible, his reader from seeking after any state or condition, at which these quantities can actually arrive, wherein to be the subjects of this proportion; he draws a parallel between this case, and the case of quantities supposed to augment without end. For says he, such quantities may have an ultimate ratio, though here will not be any last quantities as the subjects of that ratio.

59. *Philalethes* is very unwilling to allow this intended for an exact parallel. But Sir *Isaac Newton* expresly affirms, his intention of introducing it was to render more clear the thing, he had been immediately speaking of, that is, the nature of the ultimate ratios of quantities decreasing without limit.

60. *Philalethes* indeed asserts a difference between the two cases. In vanishing quantities he asserts their ratio actually to come up to *(attingere)* their limit; but in the other not. It is true the quantities in one case may be reduced absolutely to nothing, but in the other can never be extended to an infinite magnitude. Yet in both cases the quantities are equally incapable of being converted by the variation ascribed to them into any condition, wherein they will be the

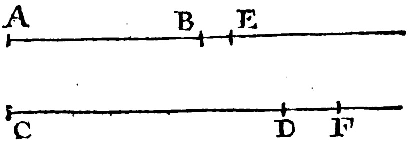
* Newtoni Princip. Philos. Lib. 1. Lem. xi. in Schol.

subjects

subjects of that ratio, which is called their ultimate. Nay, Sir *Isaac Newton* has been so particularly careful, lest any of his readers, from brief expressions, which they would afterwards find in this book, should imagine, he in the least favoured the attempt of indivisibles, to pursue such quantities to a diminution actually infinite, here further adds this express caution, that by whatever name he might hereafter denote these vanishing quantities, they were never to be considered as determinate, but as variable ones, diminishing without limit; consonant to what he said before, *Nolim indivisibilia, sed evanescentia divisibilia intelligi* *.

61. This interpretation of Sir *Isaac Newton*, so expressly conformable to his own words, at once dissipates all the objections, the author of the *Analyst* has raised against the demonstrations, whereby Sir *Isaac Newton* proves the operations in his method of fluxions, though condemned with so much freedom for fallacious and inconclusive.

62. The form of these demonstrations may be represented thus.



63. Let the two points B and D, one tracing out the line AB, and the other the line CD, be supposed to set forward together from A and C, and to arrive in the same time at B and D. Now it is required, from the given relation between these described lines AB and CD, in all the correspondent magnitudes of those lines, to determine the proportion of the velocities of the points at B and D; that is, to determine

* Newton, Princip. Phil. ibid.

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the proportion of the fluxions of thoſe lines AB and CD from the known relation of the flowing lines.

64. Suppoſe theſe points advance to E and F in the ſame time. Then if their velocities are always in the ſame proportion, the augments BE and DF will be in the proportion of thoſe velocities. But if one of the points, ſuppoſe D, is more accelerated than the other B; then the ratio of DF to BE will be a proportion greater than that of the velocities: but the ſmaller BE and DF are taken, the nearer will the proportion of theſe ſpaces DF and BE approach to the proportion of the velocities at D and B; and the difference between theſe two proportions may be diminiſhed in any degree whatever by a ſufficient diminution of the augments DF and BE. Then the ultimate proportion of the decreaſing quantities DF and BE will be the true proportion of the velocities, or of the fluxions at D and B.

65. This is expreſsly the method, by which Sir *Iſaac Newton* determines the proportion of the fluxions of different magnitudes in all caſes. Having firſt compared ſuch contemporaneous augments as have a finite, that is, a real magnitude; then he ſuppoſes theſe auguments continually to diminiſh, and having determined the neareſt proportion, to which they conſtantly tend during their diminution, he aſſigns this as the true proportion of the velocities or fluxions.

66. Sir *Iſaac Newton* deſcribes the proceſs, we have here explained, by theſe words: † *Evaneſcant jam augmenta illa & eorum ultima ratio erit*, &c.

67. Now this paſſage is thus tranſlated and commented on by the author of the *Analyſt*, *Let theſe increments vaniſh*, i. e. *let the increments be nothing*; and from hence he takes great pains to ſhew the abſurdity of comparing together, and aſſigning the pro-

† Introd. ad Quad. Curv.

portion

portion of quantities, after they are ſuppoſed to be deſtroyed.

68. But here he commits a twofold error: firſt in imagining, that the operation, which the increments are by Sir *Iſaac Newton* ſuppoſed to undergo in order to have their ultimate proportion aſſigned, and which he deſcribes by the verb *evaneſcant*, is confined to that point of time only, at which the increments are actually gone and aboliſhed; and, ſecondly, in imagining, that by the ultimate ratio of varying quantities is meant a ratio, that thoſe quantities do at ſome time or other exiſt under.

69. As to the firſt ſuppoſition, when the points E and F are conceived to move backwards, till they arrive at B and D, the diminiſhing of the augments BE and DF, as well as their abolition at laſt in the points B and D effected by this means, is by Sir *Iſaac Newton* comprehended under the general deſcription of *evaneſcant*, let them vaniſh; as is moſt evident, not only by the paſſage above quoted from Sir *Iſaac Newton*'s introduction to his treatiſe of Quadratures, where he ſays his analyſis inveſtigated *finitarum naſcentium vel evaneſcentium rationes primas vel ultimas*, but alſo by the words ſince produced, that *the vaniſhing quantities by him conſidered are diviſible, and not determinate, but continually diminiſhing*.

70. Since therefore theſe vaniſhing quantities are expreſsly declared by Sir *Iſaac Newton* to be finite and variable; his expreſſion in this place muſt be underſtood to relate to the whole time they are vaniſhing. And his words are free from any impropriety; for the term vaniſhing is daily applied to objects during the time of their diſappearing, before they are actually out of ſight, abſolutely ſignifying no more than going to vaniſh. Juſt as we ſay that the ſun is ſetting in the moſt limited ſignification of that word, as ſoon as its under limb touches the horizon; and as ſoon as ever the ſun is quite out of ſight, it is no longer

longer setting, but actually set: so these quantities, being of a finite, that is, of a real magnitude, do not vanish instantaneously, but with the utmost propriety may be said to be vanishing all the time they are undergoing the diminution ascribed to them.

71. The second error of the author of the *Analyst*, that of supposing the ultimate ratio of varying quantities to be a ratio, which these quantities must some time or other exist under, we have fully shewn to be contrary to Sir *Isaac Newton*'s express declaration.

72. Upon this mistake is grounded the charge of indivisibles being unavoidably supposed in this doctrine, when he says, no quantity can be admitted as a medium between a finite quantity and nothing, without admitting infinitesimals. By what has been above said, it appears, that *Philalethes* had no necessity, for avoiding the consequence here charged upon the doctrine, to have recourse to that definition of a *nascent increment*, which follows. *A nascent increment is an increment just beginning to exist from nothing, or just beginning to be generated, but not yet arrived at any assignable magnitude, how small soever.*

73. Here a nascent augment seems to be represented, as a quantity neither of any finite magnitude, nor yet as absolutely nothing, and can scarce be conceived of otherwise, than as in an intermediate *state* between both. I apprehend *Philalethes* was induced to frame this definition from the terms *nascent* and *evanescent*, by which those, who had composed demonstrations, or writ upon this subject in our language, had rendered Sir *Isaac Newton*'s words *nascentes* and *evanescentes*. And these *English* words bearing the form of nouns adjective, they too frequently neglected the addition of prime and ultimate, necessary to render the sense compleat in expressing the ratio, which is the limit to the ratios of these quantities. We find Sir *Isaac Newton* so cautious in this particular, that in the account of the *Commer-*

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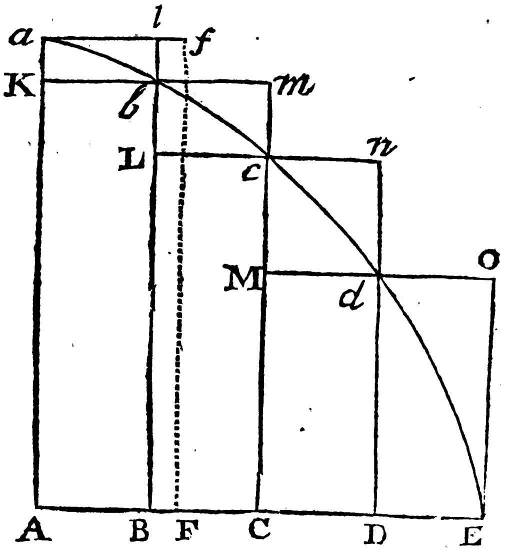
tium Epistolicum, publiſhed in the *Philoſophical Tranſactions*, though written by him in *Engliſh*, he retains the *Latin* expreſſions. Since Sir *Iſaac Newton* intended by *quantitates naſcentes* and *evaneſcentes* finite and indeterminate quantities capable of bearing different proportions, the term prime or ultimate is abſolutely neceſſary to expreſs that ratio, which is the limit of thoſe different ones.

74. From this inadvertent uſe of the words *naſcent* and *evaneſcent* we expreſſed a diſlike to them in our diſcourſe.

75. But now, to ſum up the whole of what we have ſaid upon this head, ſince Sir *Iſaac Newton* has expreſsly told us, that the quantities, he calls *naſcentes* and *evaneſcentes*, are by him always conſidered



as finite quantities; that ratio called their prime or ultimate cannot be the ratio, which thoſe quantities themſelves at any time muſt actually have. Our interpretation

terpretation therefore of the lemma so often mentioned is absolutely necessary in applying it to these quantities. And as we have shewn no different sense to be required in any other subject of this doctrine; so our representation of Sir *Isaac Newton*'s mind has not only been proved to be conformable to the genuine meaning of his words, but is also perfectly consistent with itself: whereas we must confess ourselves at a loss to reconcile *Philalethes* with himself in the acknowledgment he makes, that the ultimate form even of the perimeter of the inscribed figure in Sir *Isaac Newton*'s second and third lemmas is no other than the curve itself; that is, in each triangle *a*K*b*, *b*L*c*, *c*M*d*, *d*DE, the rectilinear sides *a*K, K*b*, *b*L, L*c*, *c*M, M*d*, *d*D, DE, vanish into the curve itself: if at the same time he supposes evanescent quantities subsisting at each point of the curve, which can be the subjects of the proportions, between the ordinate, tangent, and subtangent.

76. We shall now proceed to consider, what Sir *Isaac Newton* has called the *momenta* of quantities. This term was used very early by him. In or before the year 1669*, he drew up a short discourse *de Analysi per æquationes numero terminorum infinitas*. Here the word moment frequently occurs. He has told us *this tract teaches how to resolve finite equations into infinite ones, and how by the method of moments to apply equations both finite and infinite to the solution of problems* †. He says, that *he there called the moment of a line a point in the sense of* Cavalerius, *and the moment of an area a line in the same sense* ‡. The passage in the book, to which this relates, is as follows. *Nec vereor loqui de unitate in punctis, sive lineis infinite*

* Philos. Transf. No. 342. p. 204. Or Comm. Epist. p. 36, 37.
† Philos. Transf. Ibid. p. 176. Or Comm. Epist. p. 5.
‡ Philos. Transf. Ibid. p. 178. Or Comm. Epist. p. 7.

parvis, siquidem proportiones ibi jam contemplantur geometræ, dum utuntur methodis indivisibilium ||. And he has told us, *from the moments of time he gave the name of moments to the momentaneous increases, or infinitely small parts of the abscissa and area generated in moments of time* **. He says, *Leibnitz hath no symbols of fluxions in his method, but used the symbols of moments or differences* dx, dy, dz ††. All this is suitable to the doctrine of indivisibles. He likewise tells us, *because we have no ideas of infinitely little quantities, he introduced fluxions into his method, that it might proceed by finite quantities as much as possible* ||||. Hence it appears, he had not at the first discovered his doctrine of prime and ultimate ratios, which entirely rejects indivisibles, or infinitely little quantities; but at length falling upon it, *he founded his method* [of fluxions] *on the* primæ quantitatum nascentium rationes, *which have a being in geometry, whilst indivisibles, upon which the differential method is founded, have no being either in geometry, or in nature* *. Accordingly he tells us, *When he is demonstrating any proposition, he uses the letter* o *for a finite moment of time, or of its exponent, or of any quantity flowing uniformly, and performs the whole calculation by the geometry of the ancients in finite figures or schemes without any approximation: and so soon as the calculation is at an end, and the equation is reduced, he supposes, that the moment* o *decreases* in infinitum, *and vanishes. But when he is not demonstrating, but only investigating a proposition, for making dispatch he supposes the moment* o *to be infinitely little, and forbears to write it down, and uses all manner of approximations, which he*

|| Comm. Epist. p. 85.
** Philos. Trans. Ibid. p. 178. Or Comm. Epist. p. 7.
†† Philos. Trans. Ibid. p. 205. Or Comm. Epist. p. 37, 38.
|||| Philos. Trans. Ibid. p. 205. Or Comm. Epist. p. 38.
* Philos. Trans. Ibid. p. 205. Or Comm. Epist. p. 38.

conceives

conceives will produce no error in the conclusion *. Here Sir *Isaac Newton* declares, he was wont to use the word moment in two senses; examples of both which he then mentions. And it is observable in his rule for finding the relation of fluxions, as published out of his old papers by Dr. *Wallis* in 1693, the word moment is used in the sense of indivisibles; but when he came to give that rule himself in his book of *Quadratures* first printed in 1704, he used that word in the other sense †.

77. Before he had published any thing on these subjects, he thought fit for the sake of brevity to in-

* Philos. Trans. Ibid. p. 179. Or Comm. Epist. p. 9.

† But *Philalethes* contended, that by the expression *infinite parvis*, once only used in Sir *Isaac Newton*'s *Analysis*. he did not mean indivisibles; because in that work the expressions, *in infinitum diminui et evanescere, sive esse nihil*; *continuo diminuatur donec tandem evanescent*; *diminuetur donec evanescent*; *continuo decrescit donec tandem penitus evanescent*: were all conformable to Sir *Isaac Newton*'s constant doctrine, and peculiarly his own, and never used by any writers upon indivisibles. To which weak argument Mr. *Robins* made the following reply.

Here *Philalethes* is too hasty; for such expressions are to be found in Dr. *Wallis*. Sir *Isaac Newton*'s doctrine of prime and ultimate ratios does not depend merely upon these words It would be tedious to refer to all the places of Dr. *Wallis*, where such expressions as these occur. It is sufficient to name the two following. In the *Arithmetick of Infinites*, *propp.* 20, 40, are these words, *excessus ille, si in infinitum procedatur, prorsus evaniturus est*. *Huygens* in his illustration of *Fermat*'s method of drawing tangents, though he proceeds undoubtedly upon the principles of indivisibles, yet has made use of the word *evanescens* in the following passage, *Nam termini, &c. quantitates infinite parvas, sive omnino evanescentes continebant* **. It is certainly very easy to see, what Sir *Isaac Newton* meant by the words *infinite parvis* in the *Analysis*; because he tells us expresly, that he used them in imitation of those, who used indivisibles. *Nec vereor loqui de unitate in punctis, sive lineis infinite parvis, siquidem proportiones ibi jam contemplantur geometræ, dum utuntur methodis indivisibilium.* Appendix to the Present State of the Republick of Letters for *September* 1736, p. 17.

** Divers Ouvrages de Mathematique et de Physique par Mess. de l'Academie Royale des Sciences, à Paris 1693, p. 332.

troduce this term *moment* in the ſecond book of his *Principia Philoſophiæ*. As the geometers of his time had been much accuſtomed to indiviſibles, he did not ſcruple there to deſcribe moments according to the ſenſe of that doctrine, as he had done formerly, to be *incrementa vel decrementa momentanea* ‡. As in another place of that treatiſe he acknowledges his uſing ſeveral expreſſions favouring indiviſibles, but at the ſame time ſhews how that idea may be corrected, when ſuch expreſſions occur ‖; ſo likewiſe here he does the like: he ſhews how to correct the idea ariſing from this deſcription of moments. He ſays, *you muſt never conſider their magnitudes, but their prime ratio.* He adds, *it would come to the ſame thing, if inſtead of theſe moments you uſed the velocities of increaſe or decreaſe of quantities, which he is wont to call fluxions, or if you uſed any finite quantities proportional to theſe fluxions.*

78. Notwithſtanding all this caution of Sir *Iſaac Newton*, he has not eſcaped being cenſured. I therefore endeavoured in my diſcourſe to clear up this affair of the moment, not thereby to vindicate the genuineneſs of Sir *Iſaac Newton*'s methods of fluxions, and of prime and ultimate ratios, which I had before ſufficiently ſhewn to be accurate, and did not in the leaſt depend on the interpretation of the word moment; but to make appear, as on this head there had been raiſed great clamor and boaſting, that it was without any manner of foundation. To this end in my diſcourſe I gave a deſcription of moments ſuitable to the doctrine of prime and ultimate ratios; and ſince Sir *Iſaac Newton*'s demonſtration of the moment of a rectangle had been attempted to be exploded, though it is moſt accurate as well as brief, this is likewiſe explained; and is alſo more fully enlarged upon in the account of my book.

‡ Lemm. II.

‖ Lib. I. Lemm. XI. in Schol.

79. The

79. The miſtakes, that have here ariſen, were occaſioned by not ſufficiently attending to Sir *Iſaac Newton*'s laſt mentioned caution. From thence it will appear, in calling $\frac{1}{2}a$ and $\frac{1}{2}b$ the halves of the moments of A and B, by *a* and *b* he meant finite quantities in the prime or ultimate ratio of the correſpondent increments or decrements of A and B.

80. Upon this principle, if the ſides of a rectangle, which are denoted by A and B, be augmented and diminiſhed by half ſuch lines expreſſed by *a* and *b*, as ſhall be in the ultimate ratio of the increments or decrements of the ſides A and B, generated in equal portions of time; the difference ($aB+bA$) of ſuch rectangles, as are contained by the ſides A and B thus augmented and diminiſhed, will expreſs the momentum of the original rectangle.

81. The exception to the demonſtration, *Philalethes* has given, of the method for finding the *momentum* of a rectangle is, that as it demonſtrates too much, it muſt of neceſſity be inconcluſive.

82. He has endeavoured to prove, that the moment of the rectangle is an arithmetick mean proportional between the contemporaneous increment and decrement of the ſame rectangle.

83. But it has been ſhewn, that this is only true, when the ſides augment in the ſame conſtant proportion.

84. Conſequently the ſuppoſed demonſtration of *Philalethes* muſt be defective; for there is no part of it, but the concluſion, that contains any reſtriction to this particular caſe.

85. *Philalethes* ſays, that *Sir* Iſaac Newton *never admitted of indiviſibles, nor of quantities infinitely ſmall, conceived as actually exiſting in a fixed determinate and invariable ſtate.*

86. That Sir *Iſaac Newton* has made uſe of indiviſibles in the very ſenſe of *Cavalerius*, and that the

doctrine of moments was originally founded on them, we have already proved from his own words.

87. We are alſo told, *that Sir* Iſaac Newton *in the lemma, where he determines the moments, amongſt other methods of conceiving them conſiders theſe moments as the differences of* Leibnitz, or as *Philalethes* afterwards explains it, *as finite quantities exceedingly ſmall.* But this is directly contrary to Sir *Iſaac Newton*'s deſcription, who, ſpeaking of theſe *momenta*, as *incrementa momentanea*, in the ſenſe of indiviſibles, ſays, *particulæ finitæ non ſunt momenta.* Afterwards indeed he adds a caution, whereby we may underſtand the ſignification of the word moment in the true ſenſe of his method of prime and ultimate ratios; that is, that theſe *momenta* may be expreſſed by finite quantities, not confined to be exceedingly ſmall, but of any magnitude, provided they were in the prime or ultimate ratios of their correſponding increments or decrements.

88. And it is afterwards ſaid, that *the courſe taken by Sir* Iſaac Newton *to find the finite difference of variable quantities, though not rigorouſly geometrical in the higher caſes, yet approaches nearer to geometric rigour than the method of* Leibnitz.

89. Now I ſay, that were theſe differences in this ſenſe conſidered as finite ſmall quantities; however, Sir *Iſaac Newton*'s computation might come nearer the value of any quantity ſought after, than that of *Leibnitz*; yet conſidered as a medium of demonſtration to determine the abſolute value of ſuch quantity, both will be totally, and therefore equally, void of geometric rigour.

90. It is ſaid, that *in the firſt caſe of this lemma Sir* Iſaac Newton *is naturally to be underſtood as conſidering the ſides of the rectangle to flow either uniformly or proportionally.*

91. I ſay this cannot be the natural interpretation of that caſe, becauſe it is immediately quoted to prove

prove the ſecond caſe, where the augmentation is confeſſedly different. Nor can there be any reaſon aſſigned to ſhew, why it ſhould be thus underſtood: for Sir *Iſaac Newton* has computed the moment of the rectangle, not by ſuppoſing the ſides increaſed and diminiſhed by their reſpective increments and decrements generated in equal portions of time, but by finite lines expreſſing half their correſpondent moments, as we explained it above; ſo that his determination is by this means general, and according to the utmoſt geometric rigour.

92. I hope, I have here not only ſhewn, that the account, I lately publiſhed, of Sir *Iſaac Newton*'s doctrines of fluxions, and of prime and ultimate ratios is entirely conformable to the ſenſe of that great man; but have alſo placed them in ſuch a light, as the objections, that have been raiſed againſt them, will at once appear to proceed from miſconceptions and miſrepreſentations of the ſubjects. Sir *Iſaac Newton* has been charged with having given various and inconſiſtent accounts of his methods, and been repreſented as ſtruggling with inſuperable difficulties, and impoſing on his followers. How little reaſon there is for all theſe imputations will be manifeſt from the following conſiderations.

93. Sir *Iſaac Newton* being very young at the beginning of his mathematical ſtudies, diſcovers a very extenſive and compendious method of calculation, which he readily applied to the finding the *maxima* and *minima*, drawing tangents, determining the curvature of curves, ſquaring curvilinear ſurfaces, and to other problems of the like ſort. About the year 1665, becauſe, as he ſays, we have no ideas of indiviſibles, or infinitely little quantities, he introduced fluxions into his calculations, that he might proceed without indiviſibles, as much as poſſible. But in determining the proportions of theſe fluxions he ſtill allowed himſelf ſome uſe of infinitely little

quantities. No doubt, but upon reading the ancients he from thence would have been enabled to have demonſtrated the proportions of fluxions according to their accurate methods; for he did much more, by finding out one of his own, more compendious than theirs, and equally geometrical. This ſerved not only to demonſtrate the proportions of fluxions, but was applicable to the ſynthetic demonſtration of all propoſitions relating to curves. When he diſcovered this method of prime and ultimate ratios, we cannot certainly know. We are ſure he had part of it in 1669, on account of a demonſtration added at the end of his *Analyſis per æquationes, &c.* which was ſent at that time by Dr. *Barrow* to Mr. *Collins*. But moſt probably he had not then compleated this method, ſince in the *Lectures* he read the ſame year at *Cambridge* on his admirable diſcoveries in opticks, he uſed indiviſibles in his demonſtrations*.

94. It was in 1686 he firſt diſcloſed his doctrine of prime and ultimate ratios in his immortal work the *Mathematical Principles of Natural Philoſophy*. It

* *Philalethes* affirming Sir *Iſaac Newton* never uſed indiviſibles in his demonſtrations, Mr. *Robins* thus replied.

Whoever has read Sir *Iſaac Newton*'s *Lectiones Opticæ*, and will deny, that he has at any time made uſe of indiviſibles, muſt be very much a ſtranger to that doctrine, and to the ſtyle of thoſe writers, who followed it; but I ſhall ſet down two paſſages, where Sir *Iſaac Newton* owns, that he uſed the phraſes of indiviſibles in the ſenſe then generally underſtood. In page 98. *Concipias itaque arcum* bc *in æquales et indefinite multas partes dividi, et ejuſmodi tot ſumi, quæ minus quam una parte (hoc eſt, indefinite parum) differunt ab arcu* cd, *atque adeo ipſi pro more conſueto cenſeantur æquales*, &c. Again in page 127, *Age* NZ *occurrentem* CI *in* g, *et, ut mos eſt, concipe infinite parvum arcum* BN *æqualem eſſe*, &c. Here the words *pro more conſueto*, and *ut mos eſt*, plainly ſhew, that by the phraſes *indefinite multas*, *indefinite parum*, and *infinite parvum* he meant the ſame as other writers had done. *Appendix to the preſent State of the Republick of Letters for* Sept. 1736. pag. 19.

is ſurpriſing with what modeſty, and as it were fearfulneſs to offend ſuch, as had been admirers of indiviſibles, he introduced ſo excellent and truly geometrical method, by cenſuring the other in the ſofteſt manner. Though in anſwering the objections, that might be ſtarted againſt his own method, he evidently proves, he was fully appriſed of the real imperfections of indiviſibles, at the ſame time ſhewing a way to avoid them; yet he ſcarce condemns them himſelf, and frequently makes uſe of expreſſions peculiar to them, thinking it ſufficient once for all to inform thoſe, who did not approve of indiviſibles, how to correct ſuch expreſſions, and render them conformable to his method of prime and ultimate ratios.

95. In this treatiſe he but once mentions his doctrine of fluxions, and though what he ſays of them is ſhort, yet it is very juſt. It ſeems, as if he took notice of them chiefly, that a cypher, he thought fit to explain relating to them, might be underſtood. He here indeed demonſtrates ſynthetically, and very accurately the foundation of his method of calculation, which is common to the methods of fluxions, of prime and ultimate ratios, of moments, and of differentials.

96. In the year 1704. he publiſhed his book of Quadratures, a work worthy his profound genius. He had now ſufficiently ſeen the abuſes, that had been made of the doctrine of infinitely ſmall quantities, in what was called the differential calculus. In the introduction to this book he delivers a very diſtinct account of his method of fluxions, and teaches how to find out their proportions by his method of prime and ultimate ratios; in order, as he ſays, to ſhew there was no occaſion in the uſe of fluxions to introduce infinitely little quantities into geometry; but ſtill with his uſual modeſty ſaying errors might be avoided in the other method, if we proceeded

proceeded cautiously. In all this plain narrative of matter of fact, there appears no inconsistency in Sir *Isaac Newton*'s accounts of his methods, or the least shadow of having been ever puzzled or confounded in his ideas about them.

97. Hence it is very manifest, that as Sir *Isaac Newton* used at first indivisibles, so he soon corrected those faulty notions by his doctrine of fluxions, and afterwards by that of prime and ultimate ratios. And all the absurdity of expression, and all the inconsistency with himself charged on him by the author of the *Analyst*, arises wholly from misrepresentation. For example, it has been asserted, that there is as little sense in the phrase, a fluxion of a fluxion, as to speak of the velocity of a velocity *. This objection supposes, that the simple word velocity can always be substituted in the room of the word fluxion. But by Sir *Isaac Newton*'s description of the fluxions of magnitudes, it is evident, that the single words can never be used promiscuously: for the fluxion of any quantity is not the velocity of that quantity, but the velocity, wherewith it at all times augments or diminishes; for instance, the fluxion of a line is not the velocity, wherewith that line moves, but the velocity of the point, by whose motion the line is described. Therefore, first fluxions not being the velocities of their fluents, but the velocities, with which the fluents increase or diminish, the fluxion of a fluxion is not the velocity of the first fluxion, but the velocity or degree of swiftness, with which the first fluxion increases or diminishes. Again, because Sir *Isaac Newton* has said fluxions are in the first ratio of the *augmenta nascentia*; therefore fluxions, and what some are pleased to call nascent augments, are so absurdly confounded together, that the expressions of a fluxion of a fluxion and the nascent augment of

* Defense of Free-thinking in mathematicks, *p.* 24.

a nascent augment are represented as synonymous. Lastly, the description, Sir *Isaac Newton* has given, of moments for their use, who had been accustomed to the method of indivisibles, is set up as a standard to interpret his doctrines of fluxions and of prime and ultimate ratios; and the cautions he gave, in order that the term moment might be understood suitable to those doctrines, have been either neglected or misunderstood, and himself represented as imposing on his followers. And as Sir *Isaac Newton* from the demonstration, he has given, of the *momentum* of a rectangle, and of other more compound quantities deduces the *momentum* of a power, which *momentum* may be applied to determine the fluxion of such powers: he is thence charged with being unsatisfied with the truth of his demonstration, nay of exerting the utmost subtility and skill, the greatness of his genius was capable of, in struggling with a difficulty imagined insuperable; for no other reason, than because he has given another more direct demonstration of the fluxion of powers in the introduction to his treatise of Quadratures, which differing a little in form from the other, is represented as grounded upon different principles; whereas in the demonstration of the *momentum* of a rectangle the moments of the sides are taken instead of their compleat increments, upon the very same foundation as the superfluous terms are rejected out of the increment of the power in the other demonstration. And this author thinks it very reasonable to suspect, that Sir *Isaac Newton* might not be fully persuaded of the truth of what, he has undertaken expresly to demonstrate; because he has happened to declare himself so cautious upon a certain point, as to decline the attempt of demonstrating it, though he was strongly persuaded of its truth *.

* Analyst, *p.* 27.

98. In

98. In the account of his book of Quadratures given in the *Acta Eruditorum* in 1708, there is an insinuation reflecting on the candour and honour of this most excellent person.

99. This occasioned old papers to be examined in order to vindicate his reputation; the result of which was published in a treatise entituled *Commercium Epistolicum D. J. Collins, &c.* This made it plainly appear, that he was the real inventor of these methods, we have been describing, and proved his rival *Leibnitz* in several particulars a most egregious plagiary.

100. In an account of this book written by Sir *Isaac Newton* himself, and often referred to in this dissertation, he without reserve discovered his dislike to indivisibles or infinitely little quantities, and being here obliged to compare his methods with that called *Leibnitz*'s differential calculus; he absolutely denied this latter capable of demonstrating a proposition, because it was founded on indivisibles; which clearly proves, he lookt on no demonstration as valid, that was built on those absurd principles.

101. To conclude, I am sorry, that in any particulars relating to Sir *Isaac Newton*'s doctrine of prime and ultimate ratios I should differ in sentiment from the learned *Philalethes*, and perhaps from several other excellent persons. As I learnt this doctrine solely from Sir *Isaac Newton*'s Works; so the account, I have published of it, is agreeable to the opinion, I had ever entertained concerning it. Notwithstanding the terms of indivisibles, and other figurative expressions, that frequently occur in his writings, I always thought his true design very manifest. About many of these forms of speech he has given us a caution in order to prevent mistakes, declaring he used them to assist the imagination. But there are others of the like sort not expresly taken notice of, he perhaps believing no body liable to be deceived

deceived by their uſe. The ultimate ratio of vaniſhing quantities, the ratio with which quantities vaniſh, are in ſtrict propriety of ſpeech figurative expreſſions: nay, the laſt form of a figure, and the form, wherewith a figure vaniſhes, might be interpreted upon the foot of indiviſibles. But here theſe phraſes only ſignify the limits, to which the ratios of the vaniſhing quantities, and the forms of the changing figures approach within any degree of nearneſs without being ever able to arrive at them. As in Opticks, though the focus of rays after refraction is underſtood to denote the place, where they meet; yet the point on the axis of any ſpherical glaſs, aſſigned by optical writers for ſuch focus, is the point, which only limits all the interſections of a pencil of rays with the axis after their refraction, and is ſo far from being the point, where any number of rays actually unite, that no individual ray, excepting that which paſſes in the axis itſelf, is ſuppoſed to paſs through it. Thus the laſt form of a vaniſhing figure is not the form, which that changing figure will ever arrive at, or ſubſiſt under; but a form, to which it will approach within any degree of nearneſs poſſible to be aſſigned. To aſſert that a vaniſhing triangle, for inſtance, would ever exiſt under that form, which is called its laſt, would be, as the author of the *Analyſt* expreſſes it, to imagine a triangle in a point. But this is not Sir *Iſaac Newton*'s meaning. Theſe ultimate ratios, and laſt forms are the limits only of the continually varying ratios, and changing forms of the vaniſhing quantities and figures. And it is the whole purpoſe of this method, from theſe varying ratios and changing forms to find the fixt ratio and ſtable form, which is ſaid to be the laſt of theſe varying ones.

102. This at preſent is my real opinion of this doctrine. I do not deny, but ſome expreſſions may be found in Sir *Iſaac Newton*'s works conſiſtent with another

another ſenſe; and I am alſo very ſenſible, how frequently he accommodated himſelf to the way of writing of the mathematicians of his time, partly for aſſiſting their imagination, and partly, perhaps, for fear he might ſeem to affect innovation. But I make no ſcruple of interpreting theſe expreſſions ſuitably to my repreſentation of this doctrine; for otherwiſe I acknowledge myſelf totally incapable of reconciling this method of prime and ultimate ratios with the character, the author himſelf has given of it.

103. He prefers it to indiviſibles, becauſe, he ſays, they have no place in geometry, or in nature; and he often inſiſts, that his method is conformable to the geometry of the ancients. But how can we purſue the variable and fleeting forms of the inſcribing and circumſcribing figures *in infinitum*, ſo that, when they ſhould become equal to the curve, they may not totally withdraw themſelves from the imagination, and all idea about them be loſt? It is certain ſuch refined metaphyſical notions are not in the leaſt analogous to that ſimplicity and clearneſs in the ideas, to which the ancient geometers ever confined themſelves. However, the conception of vaniſhing quantities ever actually arriving at their ultimate proportion, has above been ſhewn to be unqueſtionably inconſiſtent with nature. Nay farther, I will not pretend on any other principles, than thoſe I have ſet forth in my diſcourſe, to defend Sir *Iſaac Newton* from the conſequences of the miſtakes, he has been charged with: for the aſſerting, that the varying ratios of vaniſhing quantities, and the changing forms of vaniſhing figures would ever attain thoſe ultimate ratios, and laſt forms, ſeems to me directly to impinge upon thoſe principles, he has expreſsly declared to be abſurd.

ADDITION

BY THE

PUBLISHER.

104. THE foregoing tracts of Mr. *Robins* not only fully explain Sir *Isaac Newton*'s doctrines of fluxions and of prime and ultimate ratios; but also vindicate them as well against the misrepresentations of the author of the *Analyst* as those of *Philalethes Cantabrigiensis*, insomuch that the former became silent; however the latter replied, and as in so doing he passed the bounds of good manners, Mr. *Robins* only made cursory remarks on the considerations *Philalethes* published. These remarks were printed in the Present State of the Republick of Letters for *August* 1736, and in an Appendix to that for *September* of the same year.

105. Some of these remarks I have already given as notes to the preceding dissertation: I shall here farther give the conclusions of them, as it contains a summary account of *Philalethes*'s capital mistakes.

106. " This is all we think necessary to remark " upon these *considerations* of *Philalethes*, and we " trust our unprejudiced reader will not find many " of his exceptions to Mr. *Robins* unanswered; and " we further hope, that we have not allowed our- " selves in any warmth or freedom of expression, " which the present intemperate manner of *Phila-*

"*lethes*'s writing will not ſufficiently excuſe. We "ſhall not here draw the uſual inference from this "change of behaviour in *Philalethes*, that he is be-"come doubtful of his own cauſe; for Mr. *Robins* "rather deſires his readers to compare impartially, "without any bias the writings of *Philalethes* with "his own, and to give the preference, where they "find the greateſt weight of unmixed and undiſ-"guiſed argument. But to remove a difficulty, "which naturally lies in the way, I ſhall conclude "this paper with examining, how it has come to "paſs, that *Philalethes* and Mr. *Robins* ſhould both "carefully have ſtudied Sir *Iſaac Newton*, with a "ſincere intention of underſtanding him, and yet "differ ſo much from one another.

107. "I think it evidently appears from the paper "of *Philalethes*, we have been conſidering, that his "reading in the mathematicks has been very much "confined. Had he been acquainted with the an-"cient writers, he could have been at no loſs to un-"derſtand, what Mr. *Robins* meant by ſaying, that "he deſcribed the parallelograms of the ſecond *lemma* "after the manner of the ancients by ſubdividing "the baſe of the curve. He could never have "thought, that Mr. *Robins* had there reference to "two propoſitions of *Euclide*; in one of which "*Euclide* had no view at all to demonſtrations by "exhauſtions, and the other, though uſed by *Euclide* "in one or two ſuch demonſtrations, is very unfit to "be applied to the ſubject Mr. *Robins* mentions *.

108. "Had *Philalethes* been verſed in the ancients, "and in the latter writers, who have imitated them, "he could have been at no loſs about the true ſenſe "of *data quavis differentia* uſed by Sir *Iſaac Newton* "in his firſt *lemma*. For this expreſſion is borrowed "from the writers, that made uſe of exhauſtions †.

* Diſſertation, §. 47. † Diſſertation, §. 36.

109. The

109. "The first proposition of the tenth book "of the *Elements*, which is applied by *Euclide* both "in his comparing of circles, of pyramids of "equal altitudes, and in one or two propositions "more, is thus expressed. Two unequal magni- "tudes being proposed, if from the greater be "taken more than half, and from the residue more "than half, and so on, there will be left at length "a magnitude less than the lesser of the proposed "magnitudes. This is directly, as Mr. *Robins* has "represented it, first assigning a difference, accord- "ing to which the degree of approach is afterwards "to be regulated.

110. "*Archimedes* in his treatise on the *Sphere and* "*Cylinder* proposes to shew, when a circle and ano- "ther space are given, that it is possible to circum- "scribe a polygon, so that the excess of the poly- "gon above the circle shall be less than the space "given. In his book of *Conoides and Spheroides* it "is shewn in Prop. xxi, that any segment of a "conoide being given cut off by a plane perpendi- "cular to the axis, or any segment of a spheroide "not greater than half the spheroide cut off in like "manner, it is possible to inscribe a solid figure, "and to circumscribe another consisting of cylin- "ders, so that the circumscribed shall exceed the "inscribed by less than any solid magnitude, that "shall be given. And this he performs by a con- "tinual bisection of the axis, 'till a cylinder is found "less than the space, that should be given, by which "cylinder the inscribed and circumscribed figures "differ from each other. From this proposition "*Philalethes* may know, what Mr. *Robins* means, "when he speaks of describing the parallelograms in "Sir *Isaac Newton*'s second *lemma* by continually "subdividing the base of the curve.

111. "After the same manner the following ex- "cellent writers express themselves.

112. "*Fed. Commandinus* de Centro Gravitatis soli-
" dorum, Prop. xi —*ita ut circumscripta superet in-*
" *scriptam magnitudine, quæ solida magnitudine sit*
" *minor.* Ibid. Prop. xxviii —*ita ut recta linea,*
" *quæ inter centrum gravitatis portionis et figuræ in-*
" *scriptæ, vel circumscriptæ interjicitur, sit minor qua-*
" *libet recta linea proposita.*

113. "*Lucas Valerius* de Centro Gr. solid. Lib. I.
" Prop. vi.—*ita ut circumscripta superet inscriptam*
" *minori spatio quantacunque magnitudine proposita.*

114. "*Joannes della Faille* de Centro Gr. Par-
" tium circuli et ellipsis, Prop. viii.—*superet latus*
" *sectoris intervallo, quod minus sit quolibet dato.* Id.
" Prop. xxvii.—*minus distet a centro gravitatis sec-*
" *toris, quolibet intervallo dato.* Id. Prop. xlii.—*sit*
" *quacunque linea recta data minor.*

115. "*Huygens* de Quadratura Hyperbolæ, Ellip-
" sis et Circuli, ex dato portionum gravit. centro,
" Theor. i.—*quæ portionem excedat spatio, quod mi-*
" *nus sit quovis dato.* Id. Theor. ii.—*sit minor spatio*
" *quovis dato.*

116. "*James Gregory* in Vera Circul. et Hyper-
" bol. Quadratur. in Schol. Prop. vi. *Continuando*
" *subduplam polygonorum descriptionem inveniri possunt*
" *duo polygona complicata, quorum differentia sit minor*
" *qualibet exhibita quantitate.*

117. "*Simson* Sect. Conic. Lib. v. Prop. xlviii.
" *Portioni parabolæ, vel portionis dimidio circumscribi*
" *potest figura, aliaque in ipsa inscribi ex parallelo-*
" *grammis æqualem latitudinem habentibus, quarum*
" *quæ circumscribitur portionem excedat, quæ vero*
" *inscribitur ab eadem deficiat spatio, quod minus sit*
" *spatio quovis dato.*

118. " And in all these propositions they always
" supposed a quantity first named, and then shew,
" how to make the approach expressed in the pro-
" position. I have set down this large number of
" quotations, because the diversity of phrase, where-
" by

"by these authors express the same sense, renders it "impossible to be mistook. Though, I confess, "this long disquisition in so evident a matter is much "more than necessary; for had any of these writers "been familiar to *Philalethes*, he could not have had "the least doubt of Sir *Isaac Newton*'s meaning. "He might indeed have discovered it even from "Sir *Isaac Newton* himself, who in the eleventh "*lemma* has thrice interpreted the phrase *pro data* "*quavis differentia* by these words, *pro differentia* "*quavis assignata*: so that by *data* he could not "mean assignable, unless *assignata* and *assignabilis* "were synonymous words.

119. "Again had *Philalethes* been at all ac-"quainted with the writers of indivisibles, he could "not have attempted at that vain distinction be-"tween the sense, in which those writers use the "phrase infinitely small, and the sense, he imagines "Sir *Isaac Newton* had assigned to it; that by "infinitely small quantities, they meant a quan-"tity fixed, determinate, invariable, but Sir *Isaac* "*Newton* meant thereby a variable quantity; where-"as it appears, that several writers in indivisibles "on set purpose avoided expressions, that im-"plied any thing fixed, determinate, or invariable. "Monsieur *Pascal*, though he never imagined, "that he did not follow the method of indivisibles, "on the contrary he particularly at page 10 of his "*Letters* published under the name of *Dettonville*, "defends himself for so doing; yet is so cautious, "as to avoid the word infinite in expressing their "number, but constantly calls it indefinite; and "certainly, when their number is considered as inde-"finite, it cannot be pretended, that their magnitude "was supposed fixed, determinate, and invariable.

120. "Again Dr. *Barrow*, who has declared "himself very expresly in favour of indivisibles, "and defends his using of them, almost constantly

"applies the term indefinite both to the number, "and the magnitude of these particles. In his first "Lecture on *Archimedes* he says, *Ponatur circulum* "*esse figuram regularem habentem latera indefinite* "*multa et parva*, &c. In his second Lecture, "Prob. ii. *Supponatur cylindrum esse prisma quod-* "*dam super polygonam basem, latera habentem in-* "*definite parva et multa*; and Prob. ix. *Posito igitur* "VM *esse infinite (vel indefinite) parvum*. Again "in his fifth geometrical Lecture, Art. 6. *arcus* "MN *indefinite parvus ponatur*. In his Differential "Method of Tangents (as Sir *Isaac Newton* calls "it in the Philosophical Transactions, N° 342, "pag. 197; or Comm. Epistolic. pag. 29.) at the "end of his tenth Lecture, he says, *curvæ arcum* "MN *indefinite parvum statuo*. In Lecture xi. "Art. 1. *æquisecetur recta* VD *indefinite punctis* A, "B,C; and a little after *cb indefinitam sectionem cur-* "*vula* GH *pro recta haberi potest*. In Lecture xii. "at the beginning, *arcum* MN *indefinite parvum* "*esse*; and at Art. 1. *Spatium vero αβδ minime* "*differt ab indefinite multis rectangulis, qualia* μϑ; "&c. Again in his ninth Optical Lecture, Art. 12. "*arcus* NR, PS *ex hypothesi sunt indefinite parvi* "*(seu minimi)*. Lect. 13. Art. 24. *ob sumptam arcuum* "*indefinitam parvitatem*.

121. "Thus it appears, that Dr. *Barrow* was so "far from considering these infinitely small quan- "tities as fixed, determinate, invariable, that he "has purposely chosen the most loose and indeter- "minate expressions, he could contrive, to denomi- "nate them by. If *Philalethes* had been in the least "acquainted with the writings of this most excel- "lent geometer, surely it would have been impossi- "ble for him to have asserted, that in all Sir *Isaac* "*Newton*'s works there is no passage, no expression "to be found, that should reasonably make him so "much as suspected of using indivisibles. Has he

"not

"not throughout his *Lectiones Opticæ* upon all occa-
"sions used either the same, or even less guarded
"expressions than these of Dr. *Barrow*?

122. "Sir *Isaac Newton*'s doctrine of prime and "ultimate ratios is not to be defended from the ac- "cusation of resembling indivisibles by any minute "variations, that may be found in his expressions, "from what are used by the writers, who followed "that other doctrine; for those writers are by no "means exactly uniform with one another, but "some have expressed themselves with more and "some with less caution. What separates the "doctrine of prime and ultimate ratios from in- "divisibles is the declaration made in the *Scholium* "to the first Section of the *Principia*, that Sir *Isaac* "*Newton* understood by the ultimate sums and "ratios of magnitudes no more than the limits of "varying magnitudes and ratios, and he puts the "defence of his method upon this, that the deter- "mining any of these limits is the subject of a pro- "blem truly geometrical. To insist, that the vari- "able magnitudes and ratios do actually attain, "and exist under these limits, is the very essence of "indivisibles. For in supposing this, we pretend to "see directly, as Mr. *Robins* has expressed it, in "these last forms or limits the properties, which "the variable figures had before; and under this "notion these limits must be allowed capable of "being compared together by a direct form of "demonstration. The impossibility only of this "actual coincidence obliged Sir *Isaac Newton* to "build his demonstrations concerning these limits "upon the negative form by *deductio ad absurdum*.

123. "If a choice of expression only were suffi- "cient to distinguish between the two methods, "*Heuraet* and Dr. *Wallis* may both be supposed to "have avoided indivisibles, though they make no "such pretensions. I here allude to this passage

" of *Heuraet* at the end of the first vol. of *Carte*'s " *Geometry*. *Unde cum illud* [*nempe quod supra de-* " *monstraverat*] *verum sit, quotcunque rectangula atque* " *tangentes extiterint; et figura ex parallelogrammis* " *constans, si eorum numerus in infinitum augeatur desi-* " *nat in superficiem* AGHIKLF, *ac tangentes similiter* " *in lineam curvam* ABCDE, *liquet superficiem* " AGHIKLF *aequalem esse rectangulo sub* Σ *et recta* " *aequali curvae* ABCDE. Again Dr. *Wallis* in his " treatise of the Cycloide, &c. (Oper. Tom. i. " p. 563.) expresses himself thus,—*rectas* oi, oc " *angulo contactus subtensas pro diminutione* oc, oi, " *tangentium ita minui, ut illae ad has rationem tandem* " *subeant data quavis minorem; ideoque evanescentibus* " oi, oc *tangenti et curvae interjectis, coincident tum* " oc, *tum* oi, *tangentis particulae, particulis cur-* " *vae* oo.

124. " Again *Philalethes* is but imperfectly in- " structed in the precepts of common algebra; else " he could not have imagined an incurvated line, " which, he saw, would meet a right line in an " infinite number of points, to be one and the same " curve, and expressible by a finite equation; " whereas the number of intersections of a right line " with every algebraick curve is limited, the num- " ber of such intersections determining the order of " the curve *.

125. " Farther, it was only owing to his little " exercise in geometrical subjects, that made him " unapprised of Mr. *Robins*'s meaning, when he " spoke in general of vanishing quantities, as " if they might be capable sometimes of bear- " ing the ratio, which he calls their ultimate; " for had *Philalethes* been as well versed in the " writings of Sir *Isaac Newton*, as might have " been expected in one, who has appeared in his

* Dissertation, § 54, in the note.

" defense,

" defence, he never could have imagined, that because Mr. *Robins* admits, that some quantities capable of an actual equality might be brought under the *lemma* so often mentioned, therefore he began to think himself obliged to allow an actual equality, where *Philalethes* contends for it. But above all it is most astonishing, that *Philalethes* should have taken so little care to understand the person he is writing against, as must be supposed, if we are to think him indeed sincere in his accusation, that Mr. *Robins* has given no less than four different interpretations of this *lemma*.

126. " With these specimens of *Philalethes*'s imperfect knowledge in the mathematicks, it would have been more becoming him to have been something less free of his censures, and not so hastily to have charged with gross errors, false reasoning, and self-contradiction a person, who at least seems to have used better endeavours to be well instructed than himself; nor should he be at all surprised, if he has missed in any measure Sir *Isaac Newton*'s meaning, who has wrote in a stile, which supposes his reader thoroughly conversant in geometrical subjects."

127. Thus Mr. *Robins* concluded his Remarks on the Considerations, which *Philalethes Cantabrigiensis* thought fit to publish on the foregoing Dissertation.

A DEMONSTRATION OF

The Eleventh Proposition of Sir Isaac Newton's *Treatise of* Quadratures. *First printed Anno* 1727, *in the* Philosophical Transactions, N° 397; *and afterwards somewhat altered in the* Abridgment *made by Mess.* Reid *and* Gray, *Anno* 1733.

THIS Proposition consists of two parts: the first is as follows.

Let there be any curve ADI, whose abscisse AB shall be denoted by z, and its ordinate BD by y; which may be related in any manner to the abscisse. And calling this the first curve, let other curves AEK, AFL, AGM, AHN, *&c.* be formed to the common abscisse AB or z, by making the ordinate BE of the second curve always equal to the area ABD of the first divided by unity; the ordinate BF of the third equal to the area ABE of the second divided by unity; the ordinate BG of the fourth equal to the area ABF of the third divided by unity; and so on continually.

Suppose now, that other curves AOS, APT, AQV, ARW, be described to the same common abscisse AB or z; in which curves the ordinate BO of the curve AOS shall be equal to zy, the ordinate

BP

BP of the curve APT equal to z^2y, the ordinate BQ of the curve AQV equal to z^3y, the ordinate BR of the curve ARW equal to z^4y, *&c.* And let the whole area ACI be denoted by A, the area ACS by B, the area ACT by C, the area ACV by D, the area ACW by E, *&c.* Then the series of curves ADI, AEK, AFL, AGM, AHN, are thus measured.

The area of the first curve ADI is $= A$

——— of the second AEK is $= zA - B$

——— of the third AFL $= \frac{zzA - 2zB + C}{2}$

——— of the fourth AGM $= \frac{z^3A - 3z^2B + 3zC + D}{6}$

—— of the fifth AHN $= \frac{z^4A - 4z^3B + 6z^2C - 4zD + E}{24}$

and so on perpetually.

Here in all the curves following the first, the index of the highest power of z is always the number, which expresses the distance of the curve from the first, and afterwards decreases regularly by unity; the first term is multiplied into A, the second into B, the third into C, the fourth into D, and so on; the coefficients are the same as in a binomial raised to the highest power of z, and the divisor is so many terms of this progression $1 \times 2 \times 3 \times 4 \times 5 \times 6$ *&c.* as is expressed by a number equal to the highest index of z.

Otherwise supposing n to represent the distance of the curve to be measured from the first; then the area sought will be found by extending $\overline{z-1}|^n$ into a series, and multiplying the first term by A, the second by B, the third by C, the fourth by D, *&c.* and dividing the whole by $n \times \overline{n-1} \times \overline{n-2}$, *&c.* continued to unity.

SE-

SECOND PART.

Supposing the first, second, third, &c. curves to be the same as before. Let t denote the whole abscisse AC, and put x for BC. Then describe the curves CXA, CYA, CZA, CΓA, where BX shall be equal to xy, BY $= x^2y$, BZ $= x^3y$, BΓ $= x^4y$, &c. This being done, and in the series of curves CIDA, CXA, CYA, CZA, CΓA, &c. the first area CIDA being put equal to P, the second CXA equal to Q, the third CYA $=$ R, the fourth CZA $=$ S, the fifth CΓA $=$ T, &c. the whole areas of the aforesaid series of curves are also determined, as follows.

The first AIC $=$ P
The second AKC $=$ Q
The third ALC $= \frac{1}{2}$R
The fourth AMC $= \frac{1}{6}$S
The fifth ANC $= \frac{1}{24}$T.

Here the areas P, Q, R, S, T are divided by numbers produced by multiplying as many terms of this series $1 \times 2 \times 3 \times 4 \times 5$ &c. together, as in the former case.

Demonstration of the First Part.

Since each of the curves AEK, AFL, &c. are formed on the common abscisse z, by every where taking for ordinates the area of the preceding curve, we are to demonstrate, that in the expressions A, zA — B, &c. each following expression is the area of the curve, that has for its ordinate that expression, which preceded it, and z for its abscisse.

Suppose $m = n - 1$;

Then $\dfrac{z^m A - \frac{m}{1} z^{m-1} B + \frac{m}{1} \times \frac{m-1}{2} z^{m-2} C \text{ \&c.}}{m \times \overline{m-1} \times \overline{m-2} \text{ \&c.}}$

And $\dfrac{z^n A - n z^{n-1} B + \frac{n}{1} \times \frac{n-1}{2} z^{n-2} C \text{ \&c.}}{n \times \overline{n-1} \times \overline{n-2} \text{ \&c.}}$

are

are by the proposition two expressions following each other, we are to shew, that the second of them is equal to the area of a curve, which has the first for its ordinate, and z for its abscisse. The fluxion of this last mentioned area is $\dot z$ into the first of these expressions; we are to shew, that the second of these expressions is the fluent of that fluxion, or which is the same, that the fluxion of this second expression is equal to $\dot z$ into the first expression.

The fluxion of the second is

$$n z^{n-1}\dot z A - \overline{n-1}\times\tfrac{n}{1} z^{n-2}\dot z B + \overline{n-2}\times\tfrac{n}{1}\times\tfrac{n-1}{2} z^{n-3}\dot z C \ \&c.$$
$$+ z^{n}\dot A - \tfrac{n}{1} z^{n-1}\dot B + \tfrac{n}{1}\times\tfrac{n-1}{2} z^{n-2}\dot C \ \&c.$$

the whole divided by $n\times\overline{n-1}\times\overline{n-2}$ &c. Here the first line consists of the fluxions of the powers of z into A, B, C, &c. and the second line is the fluxions of A, B, C, &c. into the powers of z. But each of the terms $z^n\dot A$, $z^{n-1}\dot B$, $z^{n-2}\dot C$ &c. in the second line are equal to $z^n\dot z y$, since by the formation of the curves that A, B, C, &c. are the areas of, $\dot A = \dot z y$, $\dot B = \dot z z y$, $\dot C = \dot z z z y$. The second line of this fluxion is therefore equal to $\overline{1 - \tfrac{n}{1} + \tfrac{n}{1}\times\tfrac{n-1}{2}\ \&c.}$ $\times z^n\dot z y$, that is, equal to $\overline{1-n}|^{n}\times z^n\dot z y = 0$, the whole fluxion by this means becomes

$$\frac{n z^{n-1}\dot z A - \overline{n-1}\times\tfrac{n}{1} z^{n-2}\dot z B + \overline{n-2}\times\tfrac{n}{1}\times\tfrac{n-1}{2} z^{n-3}\dot z C \ \&c.}{n\times\overline{n-1}\times\overline{n-2}\ \&c.}$$

Then striking out the common multiple n from the top and bottom, and dividing by $\dot z$, we have the same quantity equal to

$$\frac{\dot z\times z^{n-1}A - \tfrac{n-1}{1} z^{n-2}B + \tfrac{n-1}{1}\times\tfrac{n-2}{2} z^{n-3}C \ \&c.}{\overline{n-1}\times\overline{n-2}\times\overline{n-3}\ \&c.}$$

and substituting m, $m-1$, $m-2$, &c. for n, $n-1$, $n-2$, $n-3$, &c. the same becomes

$\dot z\times$

$$\frac{\dot{z} \times z^m A - \frac{m}{1} z^{m-1} B + \frac{m}{1} \times \frac{m-1}{2} z^{m-2} C \ \&c.}{m \times \overline{m-1} \times \overline{m-2} \ \&c.}$$

This then, which is the fluxion of the second expression, is shewn to be equal to $\dot{z}$ into the first expression; consequently each following term is the area of a curve, that has the preceding term for its ordinate, and z for its absciss.

Demonstration of the Second Part.

Suppose any curve whose distance from the first is denoted by n; we are to prove, that the curve, whose absciss is BC or x, and its ordinate $x^n y$ divided by $n \times \overline{n-1} \times \overline{n-2} \times \overline{n-3}$ &c. continued to unity, will be equal to it, when x is equal to AC or t.

It is evident, that when the areas ABD, ABO, ABP, ABQ, ABR, &c. decrease, the areas BCID, BCSO, BCTP, BCVQ, BCWR increase respectively; and consequently the decrements of the areas ABD, ABO, ABP, &c. or their fluxions with a negative sign, are the increments or fluxions of the areas BCID, BCSO, BCTP, &c. that is, calling the areas ABD, a; ABO, b; ABP, c; ABQ, d; and BCID, α; BCSO, β; BCTP, γ; BCVQ, δ; BCWR, ε; then $\dot{\alpha} = -\dot{a}$, $\dot{\beta} = -\dot{b}$, $\dot{\gamma} = -\dot{c}$, $\dot{\delta} = -\dot{d}$, $\dot{\varepsilon} = -\dot{e}$.

Now the fluxion of the curve, whose absciss is $= x$ or BC, and its ordinate $= x^n y$, is $\dot{x} x^n y$; that is, equal to $\dot{x} y \times \overline{t-z}|^n$; n being $= t - z$; or since the increment of x, or $\dot{x}$ is equal to the decrement of z, or $-\dot{z}$, the fluxion of the same curve is equal to $-\dot{z} y \times \overline{t-z}|^n = -\dot{z} y$ in $t^n - n \times t^{n-1} z + n \times \frac{n-1}{2} t^{n-2} \times z^2$ &c. $= -t^n \dot{z} y + n t^{n-1} \dot{z} z y - n \times \frac{n-1}{2} t^{n-2} \dot{z} z^2 y$, &c. that is $= t^n \times$

$-\dot{a} - nt^{n-1} \times - \dot{b} + n \times \frac{n-1}{2} t^{n-2} \times - \dot{c}$, &c.

or $= t^n\dot{\alpha} - nt^{n-1}\dot{\beta} + n \times \frac{n-1}{2} t^{n-2}\dot{\gamma}$, &c. and taking the fluents, the area of the curve, whose abscisse is x, or BC, and ordinate $x^n y$, is equal to $t^n\alpha - nt^{n-1}\beta + n \times \frac{n-1}{2} t^{n-2}\gamma$, &c. But when x is equal to AC, then α, β, γ, &c. will be equal to A, B, C, &c. as is very evident; consequently the area of the curve, whose abscisse is x and ordinate $x^n y$, when x is equal to AC, is $t^n A - nt^{n-1}B + n \times \frac{n-1}{2} \times t^{n-2}C$, &c. that is, equal to $\overline{t-1}|^n$ thrown into a series, and the first term multiplied by A, the second by B, the third by C, &c. But $\overline{t-1}|^n$ thrown into a series, and the first term multiplied by A, the second by B, the third by C, &c. and then the whole divided by $n \times \overline{n-1} \times \overline{n-2}$, &c. continued to unity, is equal to the area of the curve, whose place in the series is denoted by n. Therefore the area of the curve, whose abscisse is equal to x, and its ordinate to $x^n y$, taken when x is equal to AC, and divided by $n \times \overline{n-1} \times \overline{n-2} \times \overline{n-3}$, &c. continued to unity, is equal to the area of a curve, whose place in the series is denoted by n; that is, Q, which is the area of a curve, whose abscisse is x, and ordinate xy taken when x is equal to AC, is equal to the second curve AKC; half R, which is the area to the abscisse x and ordinate $x^2 y$, taken in the same manner, is equal to the third curve ALC; $\frac{1}{6}$S, which is a like area to x and $x^3 y$, is equal to the fourth curve AMC; $\frac{1}{24}$T, the area to x and $x^4 y$, x being equal to AC, is equal to the fifth curve ANC; and so on perpetually. Q. E. D.

REMARKS

REMARKS

ON

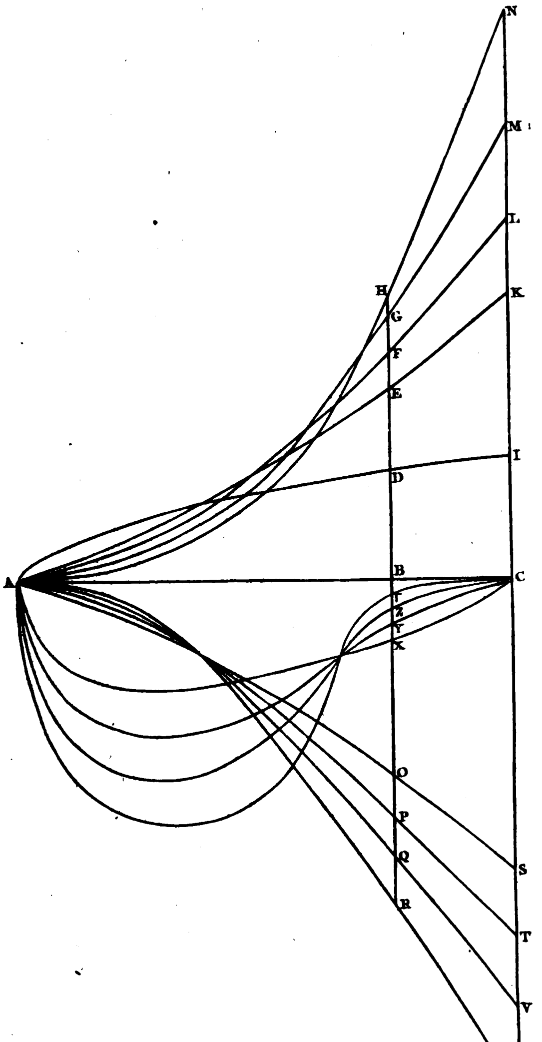
A Treatise lately printed at Paris, *and entitled,* Discours sur les Loix de la communication du Mouvement, par Monſ. Bernoulli.

First publiſhed in The Preſent State of the Republick of Letters *for* May 1728.

THE Royal Academy of Sciences at *Paris* having propoſed, in the years 1724 and 1726, two queſtions, one relating to the laws of motion and percuſſion, the other concerning the cauſe of elaſticity; Mr. *John Bernoulli*, profeſſor of the mathematicks at *Baſle*, ſent them ſome papers of his, wherein he endeavoured to prove the opinion of Mr. *Leibnitz*, in relation to the laws of motion. Theſe papers have ſince been publiſhed at *Paris*, and I intend to make ſome remarks on them.

Mr. *Bernoulli*, chap. i. § 5. in order to prove that there are no bodies perfectly hard or inflexible, lays it down as an immutable law of nature, that no body can paſs from motion to reſt inſtantaneouſly, or without having its velocity gradually diminiſhed. That

Page 174. vol. 2.



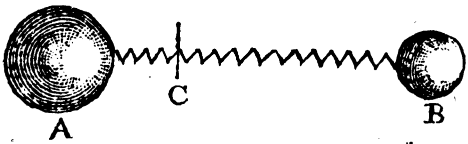
That this is a law of nature, Mr. *Bernoulli* thinks is evident from that principle, *Natura non operatur per saltum*, and from good ſenſe. But how good ſenſe can of itſelf, without experiments, determine any of the laws of nature, is to me very aſtoniſhing. Indeed from any thing Mr. *Bernoulli* has ſaid, it would have been altogether as concluſive to have begun at the other end, and have diſputed, that no body can paſs inſtantaneouſly from motion to reſt; becauſe it is an immutable law of nature, that all bodies ſhall be flexible.

In §. 18, Mr. *Bernoulli* defines perfect elaſticity to be a power in a body, by which it reſtores itſelf to the ſame figure after compreſſion, that it had before, independent of the readineſs with which it reſtores itſelf; and a ſpring or body perfectly elaſtic in this ſenſe will, he aſſures us (chap. ii. §. 2), produce the ſame velocity in a contiguous body by reſtoring itſelf, that it deſtroyed in being compreſſed. But this is contrary to experience, ſince there are great numbers of bodies perfectly elaſtic according to this definition, or ſuch as perfectly reſtore themſelves; but none are known, that will communicate the ſame velocity by their reſtitution, that they deſtroyed by their compreſſion.

Mr. *Bernoulli* affirms chap. ii. §. 3, that a ſpring muſt communicate the ſame velocity to a contiguous body in reſtoring itſelf, which it deſtroyed when compreſſed; for in this, ſays he, conſiſts the equality of action and re-action. But philoſophers underſtand no more by the equality of action and re-action, than that when one body acts on another, that other re-acts as much on the firſt at the ſame inſtant; yet Mr. *Bernoulli* here affirms, that the ſpring, when it reſtores itſelf, will act on the body as much, as the body acted on the ſpring before, while the ſpring was bending; which is a propoſition altogether different from the former.

In

In chap. iii. §. 4. Mr. *Bernoulli* obſerves, that if the ſpring AB bent between the two bodies A and B is divided in C in a reciprocal proportion to thoſe bodies, then the point C will reſt, while the whole ſpring by expanding itſelf communicates velocities to thoſe bodies: whence he would conclude, that the ſprings between A and C only act on the body A, and thoſe between C and B on the body B, whereas all the ſprings moſt certainly act againſt each body; for the part CB could not act at all againſt the body B, if the other part CA did not ſupport the point C; and indeed of this his ſixth chapter, if rightly conſidered, will be found to contain an ample proof.



And here I would obſerve, that the forces communicated to theſe two bodies are communicated by an equal preſſure acting an equal time on each, as Mr. *Bernoulli* allows. Now how theſe two bodies can receive by this means different forces, much leſs forces, that may be in any proportion aſſignable, is to me very wonderful; and yet is what really will happen, if Mr. *Bernoulli*'s hypotheſis is true, ſince thoſe bodies receive in this caſe velocities reciprocally as their maſſes, as he himſelf allows.

Mr. *Bernoulli* in his fifth chapter, takes a great deal of pains to ſhew the difference between what, he calls force *morte*, and force *vive*; or, as we ſhall tranſlate it, the *inactive* and *vivid* force of bodies; that is, between preſſure, and the force of a body in motion; and amongſt the reſt of his diſtinctions, he ſays §. 6, that the acting of any ſpring or preſſure upon a body ſupported by an immoveable obſtacle does not at all diminiſh the force of that ſpring or preſſure, as the force of air, condenſed in a recipient is not at all diminiſhed by the force, it exerts against the ſides of the recipient; but that no velocity or vivid force can be communicated to any body without the loſs of ſo much force in the preſſure, that produced it; as the elaſticity of the condenſed air would be diminiſhed, if it acted againſt any body placed in the mouth of the receiver, and communicated any degree of velocity to it.

But here is a miſtake, ſince the elaſticity is not diminiſhed, becauſe the air has ſpent any of its force by acting againſt the body, and by communicating a degree of velocity to it; but becauſe by that means it comes to poſſeſs a ſpace greater than before, whereby its expanſive force decreaſes. Now if any fluid had the ſame elaſtic force in different degrees of denſity, ſo as not to be weakened by expanding, ſuch a fluid would not loſe any of its force by putting a body in motion.

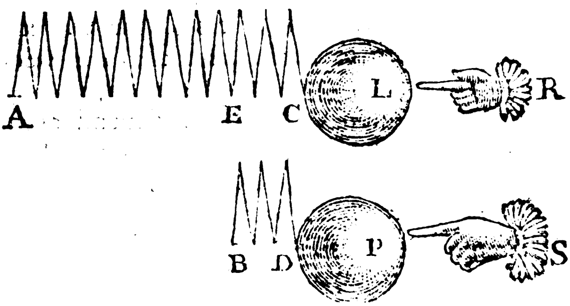
Beſides Mr. *Bernoulli* here contradicts himſelf in aſſerting, that the production of any degree of vivid force requires the loſs of an equal degree of force in the ſpring or preſſure, that produces it; when he has been arguing throughout the preceding part of this chapter, that the force of a ſpring or preſſure is entirely different in nature from what, he calls vivid force; and therefore to ſpeak of the force of a ſpring or preſſure, as equal to any degree of vivid force, is on his own principles an abſurdity; and equally abſurd is the ſeventh and eighth ſections of this chapter, and the ninth ſection of the tenth chapter.

Mr. *Bernoulli*, in chap. v. §. 2, tells us, that Mr. *Leibnitz* was the firſt, who diſcovered the force of a body in motion to be as the ſquare of the velocity, it is moved with.

Mr. *Leibnitz* was indeed the firſt, who mentioned it, but in ſuch a manner as was little to his honour, giving it by miſtake for the common opinion; for in a tract in the *Acta Eruditorum*, A. D. 1686, where he endeavours to prove the error of *Cartes* in asserting the same quantity of motion to be always preserved in the world, he says, that it is agreed on by the *Cartesians*, and all other philosophers and mathematicians, that there is the same force requisite to raise a body of one pound to the height of four yards, as to raise a body of four pounds to the height of one yard; but being shewn, how widely he was mistaken in taking that for the common opinion, which would, if allowed, prove the force of the body to be as the square of the velocity it moved with, he afterwards, rather than own himself capable of such a mistake, endeavoured to defend it as true; since he found it was the necessary consequence of what, he had once asserted; and maintained, that the force of a body in motion was proportional to the height from which, it must fall to acquire that velocity. Which assertion alone contains, in effect, all the arguments, that *Leibnitz* and his followers have hitherto chiefly relied on.

Mr. *Bernoulli* in §. 13 observes, that when Mr. *Leibnitz*'s adversaries contended for taking into consideration the time of the body's rising or falling, they were not aware, that a body may be made to rise to different heights in the same time by means of an inverted cycloid. But here Mr. *Bernoulli* himself is overseen; and does not consider, that bodies, which rise to different heights in a cycloid in the same space of time, are not stopped by the same force; for the power of gravity acts upon these bodies but with a part of its whole force, and the action upon each is proportional to the space through which, it moves. This therefore is as great a paralogism, as it would have been to have computed the forces of any body moving with different velocities from the heights, it fell to acquire these velocities, though sometimes it fell in air, and in other cases in quicksilver.

In order to consider the sixth and seventh chapters, it will be necessary to explain a scheme, that he constantly refers to. AC is a range of springs, one ex-



tremity of which is fixed at the point A; each of these springs is kept bent to a certain angle by the power R, which presses the body L against the other extremity C. BD is another range containing a less number of springs, which are fixed at B, and bent to the same angles by the power S, which presses the body P against D.

Now Mr. *Bernoulli* says, that supposing the springs AC equal in strength to the springs BD, when bent to the same angle; or, which is the same thing, that the powers R and S, and the bodies L and P are equal; then the velocity communicated to the body L by the springs AC in expanding themselves to any larger angles (the power R being taken away) will be to the velocity communicated to the body P by the springs BD in expanding themselves to the same angles (the power S being taken away) in the subduplicate proportion of the number of springs AC to the number of springs BD.

This he demonstrates by shewing the time, at which AC acts against L in any given inflexion, to be in that proportion to the time, that BD acts against P in the same inflexion, when their forces are equal. See §. 1. and 2. chap. vii.

 And

And he having aſſerted, §. 9. chap. vi. that the force communicated to L in this caſe, is to the force communicated to P in the ſimple proportion of the number of ſprings AC to the number of ſprings BD, he concludes, that the forces of the two equal bodies L and P are as the ſquares of their velocities.

But this concluſion will entirely depend on the truth of what is aſſerted in that ninth ſection. Let us therefore ſee, what Mr. *Bernoulli* has advanced to make this probable.

Mr. *Bernoulli* tells us, as ſoon as the powers R and S are taken away, every ſpring exerting its force, and none of it periſhing unuſefully, the force of every one of theſe ſprings muſt of neceſſity be employed to produce its effect; which effect can be nothing elſe but moving the bodies. Therefore he ſays, the motion of each body will be ſuch, that its vivid force ſhall be preciſely equal to the total effect of that, which the ſprings taken together ſhall have contributed to it; but as each of theſe ſprings dilates itſelf equally from one given angle to another, every one of them contributes equally to produce that force; therefore the vivid forces produced in the bodies L and P, will be as the number of ſprings, that have contributed to their production.

I ſhall paſs by the equality between preſſure and vivid force, which Mr. *Bernoulli* ſeems to inſinuate in this ſection, (ſee remark on chap. v.) and ſhall only take notice, that Mr. *Bernoulli* aſſerts, that each of the ſprings in each range contributes equally to the production of the force in the impelled bodies.

But how is this to be proved? Mr. *Bernoulli* has taken a great deal of pains in this ſame chapter to ſhew, that the preſſure in each range is equal, when all the ſprings in both are bent to the ſame angle;

how

how then does it appear, that the forces produced in these bodies will be anywise different, since produced by equal pressures? Mr. *Bernoulli* endeavours to solve this by saying in his sixth section, that if the point E in the longer range was fixed, so that the number of springs included between E and C be the same with those between B and D; then the springs between E and C would produce the same acceleration in the body L, as the springs BD equal in number would produce in an equal body P; and therefore the point E not being fixed, the remaining springs between A and E by dilating themselves will act on the point E, and produce a greater acceleration in the body L than the springs EC alone, or than the springs BD produce in P.

But this is overthrowing all the principles, that he has been establishing in the foregoing part of this chapter; for if it means any thing, it must mean, that the springs AE and EC together will act with greater force on the body L, than the springs EC alone, contrary to the whole tenor of the second and third sections.

If Mr. *Bernoulli* shall at any time give the true reason, why the springs AC and BD will produce different forces in the bodies L and P, which without doubt is the different times of their acting on those bodies, this will at once destroy all his hypothesis; for then the force of the body L will not be to the force of the body P, in the proportion of the number of springs AC to the number of springs BD, or in the duplicate proportion of their velocities, but in the direct proportion of the velocities, or the subduplicate of the number of springs, seeing the forces estimated from the pressures, and times of their acting conjointly, must be in the same proportion with the velocities, because Mr. *Bernoulli* himself in §. 1. and 2. chap. vii. has estimated the velocities from the very same principles.

Mr. *Bernoulli*, chap. viii. §. 2. says, that this opinion has been confirmed by experiments made of the cavities formed in soft substances, such as clay, &c. by bodies falling with different velocities; it being always found, that the cavities formed by the same body were as the squares of the velocity it fell with; whence it has been concluded, that the force of the same body is as the square of the velocity, it moves with.

But all, that can be inferred from these experiments, is, that the resistance of such substances is equable or proportional to the time only, that they act against an immerging body; for, if the body, that forms the cavity, is a cylinder, which moves in the direction of its axis, then, since *Galileo* has demonstrated, that the height any body will rise to, when retarded by an equable resistance, like gravity, will be as the square of its velocity, it is evident, that the depth, such a cylinder will sink in a substance resisting equably, will be in the same proportion, and the cavity will be as that depth, since the cylinder moves in the direction of its axis. Moreover, the cavity formed by any other body, however irregular, will be equal to the cavity formed by a cylinder of the same weight moving with the same velocity; for whenever the cylinder, and other body should begin to form equal augmentations of their respective cavities with equal degrees of velocity, the resistance of the cylinder to the resistance of the other body, would be compounded of the proportion of the base of the cylinder to the surface directly opposed to the motion of the other body, and the proportion of the time of the cylinder's forming this augmentation in its cavity to the time, in which an equal augmentation would be made by the other body, provided the base of the cavity of that other body remained the same during the time of this augmentation, and these times would be as the

the depths of the augmentations of cavity, that is, reciprocally as the bafes of the cavities; fo that the refiftance in the cylinder would be to the refiftance againft the other body in the ratio of equality, and the two bodies lofe equal degrees of velocity, while their cavities fhould be equally augmented. Hence, by the doctrine of prime and ultimate ratios, though the bafe of the cavity formed by the irregular body is perpetually changing, yet ftill equal degrees of velocity will be loft during equal augmentations of the cavities; therefore if they enter the yielding fubftance with equal degrees of velocity, they will both, in lofing their entire velocities, form equal cavities.

What Mr. *Bernoulli* has faid, §. 4. is only a confequence of what, *Huygens* has demonftrated from the other opinion, and fhall be confidered in another place. See remarks on chap. x. §. 1.

Mr. *Bernoulli* intitles his ninth chapter, A general and geometrical demonftration, that the quantity of the force of a body is proportional to its mafs, drawn into the fquare of its velocity; which, he fays (without infifting longer on the validity of the precedent argument) is fo general, and fo very much beyond all exception, that he believes it capable of convincing the moft obftinate adherers to the vulgar opinion.

This demonftration, when ftript of what is not effential to it, is thus. Suppofe the motion of any body moving in the direction AB to be reprefented by CD, and fuppofe the triangles CED, EFD and FGD to be right-angled at the points E, F and G, and the fides CE, EF, FG and GD, equal to each other; then, as the fum of all their fquares is equal to the fquare of CD, each of thofe lines will be half CD; now, by the compofition of motion, the motion of the body in the direction AB with the velocity proportional to CD, is equivalent to the motion

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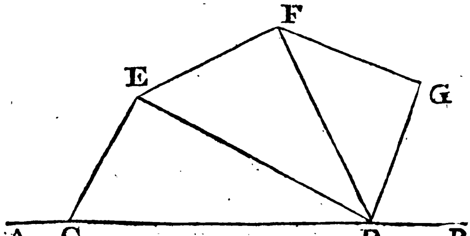


tion of the ſame body in the directions, and with velocities proportional to CE, ED, or to the motion in the directions CE, EF, FD, or in the directions CE, EF, FG, GD, with velocities, proportional to theſe reſpective lines; but each of thoſe lines is half the line CD, therefore the motion in CD with any velocity, is equivalent to the motion in CE, EF, FG, GD, with half the velocity in CD; hence Mr. *Bernoulli* concludes, that the force of a body moving with any velocity, is equal to four times the force of the ſame body moving with half that velocity, or that the force of the ſame body is as the ſquare of the velocity, it moves with.

But this propoſition, if it proves any thing, proves, that the forces of the ſame body moving with different velocities, are not only as the ſquares of the velocities, but in any aſſignable proportion to them; for as this propoſition will hold good, whether the angles CED, EFD, *&c.* are right or not, you may by varying thoſe angles find any number of equal lines CE, EF, FG, *&c.* the motions in all which ſhall compound the motion in CD, a line of any given length.

Mr. *Bernoulli* in his tenth chapter, §. 1. ſays, that the ſum of the forces of any elaſtic bodies remains the ſame after the ſtroke, that it was before. This he endeavours to prove by ſaying, that the augmentation of the force of the one is always the immediate effect of the diminution of the force of the

the other; but this is manifestly false. For, if two elastic bodies meet each other with contrary velocities, then on their first compression it is evident, that both their forces are diminished at the same time; and it may happen, that in restoring themselves, both their forces may be increased at the same time. As this principle is false, his deductions from it must of course be so too; and therefore his proving, that the sum of the squares of the velocities of any two elastic bodies, is (when multiplied into those bodies) the same before and after the stroke, is so far from being a confirmation of his method of estimating the force of bodies, that it is a contradiction to it, and not only so, but a direct consequence of the other opinion. For it is evident from the equality of action and re-action, that the loss or increase of the force of one body in one direction, is equal to the loss or increase of the force of the other body in a contrary direction: Now if you estimate the forces so lost, by the sum or difference of the velocities of the bodies before and after the stroke drawn into the bodies themselves, according to the common opinion, then you will find, that on these principles the sum of the squares of their velocities multiplied into their masses, will be the same both before and after the stroke, as *Huygens* has demonstrated.

From this equality of the sums of the squares, &c. before and after the stroke, Mr. *Bernoulli* also asserts, that the same degree of motion will always be preserved in the world; but he did not consider, that this equality holds in bodies perfectly elastic only, and there are none such in nature; therefore this hypothesis is false, allowing all his own principles. See chap. x. §. 5. chap. v. §. 9, 10, &c.

In the eleventh chapter Mr. *Bernoulli* has solved a particular case of the general problem relating to the stroke of three elastic bodies; which is, when one body in motion strikes at the same time two other

other equal bodies at rest, which are equally distant from the line of the direction of its center on each side; and in his twelfth chapter he extends it to the striking of one body, at the same time, against any number of pairs of bodies at rest, each pair being supposed equal, and equally distant on each side from the line of its direction. Mr. *Bernoulli*'s method is first to solve the problem for two pair, and so by degrees extend the solution to any number. In two pair he supposes the moving body to be divided into two parts, one part of which strikes one pair, and the other part strikes the other pair; now by computing the velocity of each of these parts after the stroke, and making an equation between them, he determines the proportion, that the body must be divided in, so that each part after the stroke shall move with the same velocity; which common velocity, he concludes, will be the same with that of the entire body. But this will be true, only when each part takes up the same time in communicating motion to the bodies it strikes against; for, if one part communicates the whole motion to the bodies, it strikes against, sooner than the other, then the two parts would be separated, if the body was actually divided, and consequently the velocity of the body, when entire, will be different from the common velocity of the parts.

Now all elastic bodies, that we have any knowledge of, will take up different times to separate in, according to the different velocities they separate with; since it is found by experience, that their resistance to compression is uniform. However, to give that for a general solution of a problem, which may be false in infinite circumstances, and can be true but in one, is a manifest paralogism; especially when those circumstances are neglected, as no ways necessary to the solution.

In

In §. 2. of the ſame chapter he ſays, that by means of this theory, the reſiſtance of a body moving in a fluid compoſed of elaſtic particles may be eaſily determined, and alſo the quantity of its motion, that it will loſe in moving through any given ſpace; a new ſubject for a ſearch, as uſeful as curious, and ſo much more worthy to be examined, as no body hitherto has undertaken it. But I am ſurprized, that Mr. *Bernoulli* ſhould mention this as ſomething yet to be done; ſeeing Sir *Iſaac Newton* has ſo amply treated this and all other parts of the doctrine of the reſiſtance of fluids in the ſecond book of his Principia.

Mr. *Bernoulli* in his fourteenth chapter endeavours to give the ſame rule for finding the center of oſcillation from his theory of forces, that *Huygens*, Dr. *Taylor*, and even himſelf, have given from the common opinion. In order to this, in §. 4. he ſuppoſes, that the force of any body will be always the ſame, when it has fallen from the ſame height, whether it falls freely, or whether any obſtacle retards its fall. If this is true, then the ſame body may have the ſame force with different velocities; ſince by increaſing the obſtacle, that hinders its deſcent, you may diminiſh its velocity in any degree aſſignable, ſtill preſerving the ſame height.

His hypotheſis of the cauſe of elaſticity is ſo full of inconſiſtancies and abſurdities, that I am deterred from making any remarks on it, for I ſhould be obliged to take notice of almoſt every paſſage. I ſhall therefore only mention one thing, which, I think, may eaſily excuſe an ampler confutation, and that is, that Mr. *Bernoulli* endeavours to account for elaſticity by the motion of particles, that are themſelves elaſtic.

And now, I think, I have proved, that nothing Mr. *Bernoulli* has urged in defence of Mr. *Leibnitz*'s opinion, is any way concluſive; that many parts of his

his discourse are contradictory; and that all his determinations of the laws of motion are wrong, since they are by him applied to bodies, which only perfectly restore themselves; whereas they are true in none but such, as restore themselves in the same time, they were compressed. See remark on chap. 1. §. 18.

And for a conclusion, I would desire Mr. *Bernoulli* to give us a true solution of the problem proposed in his twelfth chapter; for it will very much surprize the world, that so eminent a geometer should so unaccountably mistake (as I have shewn in my remarks on that chapter) in the solution of a problem, the only important one in his book, that had not been solved before.

AN

AN EXAMINATION

OF THE

NOTE

Concerning the Sun's parallax, published at the end of a book entitled Matho; five Cosmotheoria puerilis dialogus, in Quarto, Edinburgi 1738.

First printed in The History of the Works of the Learned *for* October 1738.

IN this Note the author endeavours to shew, that either the moon revolves about the earth in a manner altogether inconsistent with the general law of gravity, or that the quantity of matter in the sun and its parallax are very different, from what they are at present deemed to be.

For supposing the parallax of the sun to be about ten seconds (its usually assigned magnitude) he computes, that the gravitation of the moon to the sun would then be greater than her gravitation to the earth, by which she is supposed to be kept in her orbit; and thence he concludes, that, as in her conjunction she is placed between the earth and sun, her greater gravitation to the sun would at least in that

that ſituation draw her out of her orbit, and carry her intirely away from the earth.

The gravitation of the moon to the ſun is indeed greater than her gravitation to the earth; but the deductions of our author from this fact are altogether groundleſs.

There is no propoſition in the whole theory of motion more inconteſtable and obvious, than that the relative motions of any ſyſtem of bodies will be no ways affected by equal accelerating forces applied in parallel directions to each of the bodies, that compoſe it. Therefore, if to the earth and moon equal degrees of gravitation towards the ſun were applied in parallel directions, no change in the relative motion of the moon about the earth would be thence produced; even though the gravitation to the ſun were in each of them a thouſand times greater than the moon's gravitation to the earth.

Conſequently, from the abſolute quantity of the moon's gravitation to the ſun no inequalities will ariſe in her motion, unleſs that force be different, or differently directed from the force of the ſun on the earth. But the diameter of the moon's orbit is ſo ſmall, compared with the diſtance of the ſun, that lines drawn from any points of that orbit to the ſun, differ but little from being parallel to the line joining the earth and ſun, and her gravitation to the ſun in the ſeveral parts of her orbit is but little different from the gravitation of the earth to the ſun.

It is by theſe differences only, and not by the whole gravitation of the moon to the ſun, that the relative motion of the moon about the earth can be diſturbed; and from them ariſe the inequalities in that motion obſerved by aſtronomers.

REMARKS

REMARKS

ON

Mr. EULER's Treatise of *Motion*,

Dr. SMITH's compleat Syſtem of *Opticks*,

AND

Dr. JURIN's Eſſay upon Diſtinct and Indiſtinct *Viſion*.

Hæc eo animo accipi velim, quo ego accipiam, quoties acciderit, ut aliquis mihi errores meos indicet.———Boni autem viri munus eſſe puto, non aliorum peccata diſſimulare; ſed potius omnes homines, ſi fieri poſſet, ab inſcitiæ tenebris in lucem veritatis aſſerere.

Petr. Nonius de Errat. Orontii Finæi. In præfat.

First publiſhed in 1739.

PREFACE.

1. *IT has not been uncustomary amongst mathematicians, without any impeachment of their candour, by express treatises to correct important mistakes of their contemporaries. Thus the errors of Cardinal Cusanus were shewn by Regiomontanus, those of Orontius Finæus by Petrus Nonius, of Joseph Scaliger by Vieta and Clavius, of Longomontanus by Dr. Pell, of Gregory St. Vincent by Huygens, of Hobbes and Du Laurens by Dr. Wallis, and of Sinclair by James Gregory. And I shall here take leave after these examples to make some remarks upon two treatises, which have lately come abroad; Mr. Euler's discourse entitled Mechanica sive motus Scientia analytice exposita; and the Compleat System of Opticks by Dr. Smith.*

2. *FROM the time that Des Cartes advanced the calculations of algebra into the place of geometrical demonstrations, not only the works of the ancients, but at length the greatest part of mathematical writings have been neglected; and at present the mathematical sciences are generally considered as attainable by as little study as the operations of arithmetick. So much of the elements as can be learnt from some crude collection out of Euclide, with a few of the primary properties of the conic sections joined to some exercise in algebraical computations being thought at present, a stock sufficient to constitute a considerable geometer; and if to this be added a knowledge of the method of calculating used, where the doctrine of fluxions is required, without any true apprehension of the principles of that doctrine; the artist is supposed to have penetrated into the utmost depths of*

this ſcience. Though by this means not only all elegance and taſte are loſt, but many direct errors have been committed by perſons, compleatly adorned with theſe modern accompliſhments, in their writings on a ſubject, of which they had not read enough to render it familiar to them, or their conceptions concerning it clear and diſtinct. Whoever could in the leaſt contribute to put a check to this inundation of barbariſm, would certainly perform no inconſiderable ſervice to a branch of learning, once eſteemed the firſt of the ſciences, but at preſent reduced to a kind of mechanical practice, wherein the reaſoning faculties are little concerned.

3. *IN the firſt of the treatiſes I deſign to examine, the author has unfortunately followed the principles of his calculus with ſo little caution, as even to contradict Euclide himſelf* *; *and confides in them ſo entirely as to perſuade himſelf, againſt the voice of common reaſon, that a body may be drawn to a center by centripetal forces, where though the velocity is perpetually increaſing, yet at the center the body ſhall at once by ſome unknown cauſe be entirely ſtopt* †, *or, which is more ſtrange, annihilated* ||, *and in other caſes be turned directly back again* **; *that a reſiſting medium may be ſuppoſed endued with no other power, than that of retarding the body's motion, yet ſhall cauſe a body projected directly upward to deflect into a curve line* ††. *We find the author of the ſecond propoſing as diſcoveries, what has been the received opinion of many ages* ||||; *reviving maxims ſo deſervedly exploded, that no man, who reflects, can heſitate about their falſhood, whenever his eyes are open* ***; *deterred from attempting the ſolution of a cubic equation* †‡†; *exhibiting incon-*

* Pag. 8. 23.
† Pag. 11.
|| Pag. 23.
** Pag. 11. 21.
†† Pag. 28.
|||| Pag. 56.
*** Pag. 42.
†‡† Pag. 55.

concluſive

conclusive demonstrations ||, *and even unapprized of the meaning of so familiar a term as the similarity of figures* *.

4. *BUT how far these discourses are examples of the preceding reflection, I shall not presume to determine; nor yet what claim the writer on distinct and indistinct vision may have to mathematical knowledge of any kind, since he has not given any public specimen of great pretensions that way. Independent of these considerations as the first is intended for a very general explanation of motion, and the other promises no less than a compleat system of so extensive and interesting a subject as opticks; I hope these animadversions upon the failings, which I imagine myself to have found in them, will not be disapproved of. Since such general writings are usually had recourse to by those, who are first inquiring into any subject; it is not an unuseful labour to obviate the misinformation, to which learners by the errors in works of this nature must be exposed.*

5. *I shall also add some Remarks on the Essay upon distinct and indistinct vision subjoined to the compleat system of opticks, that the profuse praises bestowed on it by the editor may not occasion any one to be misled by the explanation there attempted of some of the principal points in Sir Isaac Newton's doctrine of light and colours, in which that great man's sense appears wholly unknown to this writer.*

6. *I have chose to deliver myself every where with great plainness, and without those apologies and excuses, with which writings of this kind do so often abound; as such affected complements upon these occasions have always given me offence in others. In relation to Mr. Euler, who is no otherwise known to me, than from his*

|| Pag. 67. 72. 75. 77. * Pag. 70.

 works,

works, though I have explained my thoughts without reserve; yet I hope free from any undue severity. In what concerns the other two gentlemen I have also made it my endeavour to avoid all excessive exaggerations of the faults, I have censured. But if any exceptionable expressions may have ever chanced to escape me, I trust, I shall easily be excused by them, who very well know the uncommon liberty, which has of late been taken with myself. To be more explicit, as the writer on distinct and distinct vision is the reputed author of the late dissertations under the name of Philalethes Cantabrigiensis, and the other gentleman is not only suspected of being his associate, but merited, no doubt, by singular services that high strain of complement, with which Mr. Faber is in one of those papers addressed; I hold myself under no farther restraint, where I have been used with such gross ill manners, than what a just deference for truth requires. I am thus free, not only because I think the treatment, I have received without the least provocation on my part, calls for some resentment; but also that I may do these gentlemen the justice to give them an opportunity of acquitting themselves, if they are falsely accused.

REMARKS

REMARKS

ON

Mr. LEONARD EULER's

TREATISE

ENTITLED

MECHANICA.

1. THE author of the firſt of the two books, I here intend to examine, was a ſcholar of the famous Mr. John Bernoulli, and has formerly publiſhed ſeveral pieces, which have met with ſuch applauſe, that he has been invited to the new Academy of Sciences erected at Peterſburg, where he now enjoys the honourable title of Profeſſor of the Sublime Mathematicks. As this performance is compoſed on a very noble ſubject, which has ſo juſtly acquired to our great Sir Iſaac Newton an unrivalled reputation; its merit has been celebrated in the various litterary Journals of different countries with high encomiums.

2. THIS treatise after stating the general properties and affections of motion considers at large the effects both of impulsive forces, and of resisting mediums on moving bodies.

3. THE first proposition is, "That every body, "which either by an absolute or relative motion is "transferred into another place, must pass through "all the intermediate places." This proposition is, I think, merely identical; the idea of passing through these intermediate spaces being necessarily and obviously included in the idea of motion.

4. THE third proposition, "That in any unequal motion the least elements of the space described may be conceived to be passed over with "a uniform motion," is not universally true.

5. IN many cases indeed the deductions founded on this position will be just, if we consider it as a figurative expression, meaning no more, than that the ultimate proportion of two contiguous spaces described in equal times by any variable motion is the proportion of equality. But where-ever this interpretation cannot be applied, there this proposition will not only be faulty in its method of conception, but all the conclusions founded on it will also be erroneous. This will most remarkably be the case, when those spaces are compared together, which a body accelerated by any force describes in the beginning of its motion; for the ultimate proportion of the first of two contiguous spaces thus described in equal times to the second is not that of equality, but the ratio of 1 to 3, as is well known to every one acquainted with the common theory of falling bodies. This proposition will also fail, when applied to the estimating the increments of velocity produced in any moving body by any accelerating force; for if the velocity receives by the action of a uniform force equal additions in two equal contiguous portions of time, the additions to the spaces described

ſcribed in conſequence of theſe increaſes of velocity will be as 1 to 3, inſtead of being to each other in the ratio of equality, as they ought to be, if this propoſition were true.

6. MR. EULER in the annexed ſcholium has ſome obſcure ſuſpicions of the fallacy of this propoſition: but as he applies it without ſcruple through the remaining part of his book, even in thoſe inſtances, in which he here ſeems doubtful; it is evident, that the genuine principles, from whence alone all deductions of this kind receive their accuracy and rigour, were unknown to him; eſpecially ſince ſome of his moſt capital errors ariſe from an unwarranted adherence to this propoſition, as we ſhall more diſtinctly ſhew hereafter.

7. THE 7th, 8th, and 9th propoſitions relating to the continuation of a body in its ſtate either of reſt, or of equable motion in a right line, unleſs diſturbed by ſome external force, ought to be propoſed, as facts only; it being from experiments, and not from any metaphyſical notions about bodies and motion, that the truth of theſe principles can be evinced. No wonder therefore, if Mr. Euler's pretended demonſtrations of theſe propoſitions be altogether inconcluſive and inconſiſtent.

8. IN the 11th propoſition he is ſo little apprized of the relation, which a demonſtration ought to have to a propoſition, that here, though the propoſition be general, the demonſtration regards one particular caſe only, and the general proof is poſtponed to a ſcholium, in which he has ſo forgot his propoſition, that he thinks it neceſſary to prove in form, that the ratio of *a* to *b* is given, which is the very thing, the propoſition ſuppoſes. Nay more, in order to make this proof he has aſſumed a triangle to be given in ſpecies, merely becauſe one angle is given. In order to prove that NL or BM will have to AL (fig. 9. Tab. I.) a given proportion, it is aſſerted, that the

triangle ALN is given in species; but this is not true, unless this proportion of BM to AL is first supposed to be given, nothing being determined in that triangle independent of that supposition, but the angle ALN.

9. Mr. EULER's 14th proposition, very indistinctly proposed, contains in the solution there given, and the corollaries annexed no less than four capital errors.

10. First it is said at the end of the solution, that the right line AD (fig. 1. Tab. II.) may be taken for the path described by the body, when acted on by a power in the direction AC. But this is false even on the confused principles of indivisibles. For were it true, the effect of the power in inflecting the body from its rectilinear motion, if estimated between D and A, would not be in the duplicate proportion of the times of its action, as it ought, but in the simple proportion of those times. The truth is, that every part of the motion, however minute, must be considered as a curve touching AB, and not as a right line intersecting it.

11. Secondly we are told in coroll. 2. that dc is equal to $\frac{Db}{dt}$, that is, to speak sense we are to conceive, that in the same time, in which the body with the velocity, it has at A, would describe the space AB, or the equal space Ab, it would with the additional velocity, it is augmented by at D, describe the space Db. But this is not true; for this increase of velocity is not acquired immediately in the point A, but during the whole time of the motion from A to B; and therefore, the ultimate relations being considered, Db is but half the space, that would be described during the passage from A to D by the increase of velocity at D; that is, in the infinitesimal jargon, dc is not equal to $\frac{Db}{dt}$, but

to $\frac{2Db}{dt}$. Mr. Euler has been led into this error by relying upon his 3d proposition quoted by him in the preceding corollary, which we have above observed to be a false one.

12. THIRDLY in the 4th corollary the same error occurs again, but in another form, being compounded with the cosine of the angle BAC.

13. FOURTHLY in the 5th corollary the angle of inflection of the direction of the body at D from the direction AB is determined to be the angle BAD, that is, the path described by the body is supposed to be the right line DA; but this is not true, as we have shewn above. Considering then the path described to be a curve touching AB in A and passing through the point D, as it ought, we must measure the inflection by the angle, that the tangent to this curve in the point D makes with the tangent AB, and this angle, ultimately considered, will be double BAD, that is, its sine in Mr. Euler's phrase is not $\frac{Bb}{BA}$, but $\frac{2Bb}{BA}$.

14. THESE errors now mentioned are variously compounded with each other in the 6th, 7th, 8th, and 9th corollaries; so that not only the general solution of this problem, but 7 of the annexed corollaries are altogether erroneous, and this even on Mr. Euler's own principles.

15. THE solution of the next proposition (prop. 15.) deserves particular notice. Here AB represents the space, which a body with the motion, it has in A, would describe in the direction AE (fig. 2. Tab. II.) during any small interval of time, and Bb the space, which the same body would have described from rest in the same interval by the power acting in the same direction AE; that is, Ab represents the whole space described in some small interval by the uniform velocity of the body in A, and by the action

action of a power in the same direction. Now we are told, that in another equal interval by the velocity acquired in b uniformly continued it would describe another space equal to Ab. But this is false. For since the velocity of the body is perpetually changing from A to b, the space Ab cannot be conceived, as described by the whole velocity acquired at b, but by some velocity intermediate betwixt that and the velocity at A; the space then, that would be described by the velocity at b in the next equal interval, will be greater than Ab, and if the action of the power was uniform from A to b, (as Mr. Euler by styling the interval of time an infinitely little one supposes) it will be equal to $Ab + Bb$; instead therefore of the equations in Mr. Euler $Ab = AB + ao$, $bc = AB + 2ao$, $cd = AB + 3ao$, $de = AB + 4ao$, there ought to be substituted $Ab = AB + ao$, $bc = AB + 3ao$, $cd = AB + 5ao$, $de = AB + 7ao$. All this has been so often shewn by every writer on the common theory of falling bodies, that I am surprized at Mr. Euler's mistaking so plain a matter. But these are not all the errors of this proposition. For Mr. Euler having by the application of his 3d proposition concluded, that the celerity of the body A is increased in the same proportion, as the increments of the spaces described by it, we may shew from the equations, we have rectified above, that taking this principle for granted, the increments of the celerity can never be proportional to the times, in which they are produced; for if the times are taken in the proportion of the numbers 1, 2, 3, 4, 5, &c. the increments of the spaces, and consequently of the velocities, if Mr. Euler is to be believed, will be in the proportion of 1, 3, 5, 7, 9, &c. a proportion not only different from that of the times, but, if the extreams are compared, perpetually receding from it. Another principal error then of this proposition is, that notwithstanding these erroneous

roneous suppositions the conclusion, "that the increments of the celerities are ultimately as the "times, they are produced in," is indisputable, as has been frequently shewn by other writers. Now it can be only by a complication of errors, that true deductions are ever derived from false principles.

16. THE process annexed to the 18th proposition is absolutely inconclusive, and unworthy the name of a solution.

17. AS Mr. Euler tells us in the 2d corollary of the 19th proposition, that he has been particularly solicitous about the rigid demonstrations of these last propositions; I have no design to charge this author with haste or negligence on account of these errors; but I consider them solely, as the effect of that inaccuracy in conception, to which the differential calculus is disposed to betray its admirers.

18. IN the solution of the 21st proposition and its 1st corollary all the errors of the 14th are continued; and consequently all the equations there given are on that account false. But in the 2d corollary Mr. Euler has fallen into an error of still a stranger nature, even following the principles of his differential calculus to an express contradiction of a proposition of Euclide's elements, which shews, that the angle at the circumference of the circle subtended by the line AD (fig. 6. Tab. II.) not the angle at the center, is equal to the angle BAD; and consequently it is not the triangle AOD, OD being supposed to be drawn, but the triangle formed on AD by lines drawn from that point, where AO continued meets the circumference beyond the center, that is similar to the triangle BA*b*; and therefore B*b* is not to AB as AD to AO, but as AD to 2AO. It is not then the radius AO of the circle touching the line AB and passing through the point D, but the diameter of that circle, which Mr. Euler has here assigned, that

is,

is, $2AO$ is $= \frac{AB \times AD}{Bb}$, not, as he makes it, $AO = \frac{AB \times AD}{Bb}$.

19. This determination of the radius of curvature in the present corollary is, as it were, the basis, on which the future examination of all curvilinear motion does immediately depend; and were not this error compensated by those, we have taken notice of already, and others, that we shall hereafter remark, there could not be one proposition in all this volume, where the body is supposed to move in a curvilinear track, but what would be erroneous. This proposition is indeed of such necessary use in the methods of proceeding followed in these volumes, that it occurs perpetually; but its falshood no where more eminently appears, than in the demonstration of the 1st proposition of the second volume, where it is more obvious, and more express, if possible, than in the present instance.

20. The value of r then assigned in the 2d corollary, and used in the succeeding ones of this 21st proposition, is double, what it ought to be.

21. But in the 10th corollary this erroneous value of r is compounded with a value of dc, which we have shewn above to be but one half of what it ought, and by this means the equation resulting in this corollary is rectified; the two errors ballancing each other.

22. THE sense of the 22d and 23d propositions may be briefly exhibited thus.

23. Let a, b, c, d, be parts of the body O considered as a point, and OA, OB, OC, OD, (fig. 8. Tab. II.) a like number of forces applied to that point, and let OG be the force compounded of all these. Then, if the parts a, b, c, d, are supposed to be separately accelerated by the respective forces OA, OB, OC, OD, I say, that the motion of the common

common center of gravity of these parts *a*, *b*, *c*, *d*, will be in the direction OG, and with the same acceleration, as would be that of the entire body O, if accelerated by the force OG.

24. THIS is the true and genuine form, under which, I think, this proposition should have been expressed: the principle of a restoring force, which Mr. Euler has introduced, serving only to perplex the reader's imagination, and render the proposition unintelligible.

25. THE demonstration is equally inaccurate; for in the first place it is no demonstration of Mr. Euler's proposition (of which indeed no demonstration can be made, as it contains no assertion, that is the proper object of demonstration,) and it is in the next place excessively artless and tedious, containing above 20 equations, when the whole may be easily dispatched by the application of a well-known proposition in statics, without any computation.

26. IN the beginning of the third chapter, which treats of right-lined motion, Mr. Euler has given Galileo's theory of falling bodies, in its own nature no difficult subject; but it is here so compounded with differential computations, that this subject may be much better learned from what has been writ in a more simple manner by others.

27. IN the remaining part of this chapter he treats in general of the direct ascent or descent of bodies accelerated by any forces referred to a given point. And in his 1st scholium to his 32d proposition he is much perplexed how to determine the motion of the body, when it has passed the point C (Tab. II. fig. 13.) He apprehends no more necessary than making *y* negative in the expression of the solution given in the proposition; and thence concludes, that if the quantity resulting is affirmative, the body will really pass the center C, but if it be negative, it is an indication, that the body will never pass the

the point C. He however owns, that in ſome caſes this method of proſecuting the motion includes a contradiction.

28. But though he has concluded, that in certain inſtances this proceeding will miſlead us, becauſe the reſult in thoſe caſes is different from what, he knows from other principles, it ought to be; yet inſtead of making the true uſe of this diſcovery, and ſuſpecting the whole proceſs, he ſtill ſuppoſes the ſolution to be right in thoſe caſes, where he has not the ſame means of comparing the reſult with known principles.

29. Notwithstanding he diſcovers by the abſurdity of the concluſion, that the method of ſolution, he has given, will in numberleſs inſtances fail in determining the motion of the body, when it has paſſed the center, he nevertheleſs in other inſtances, where the ſame abſurdity occurs, ſuppoſes the motion of the body to be rightly exhibited by theſe fallacious operations; having advanced in the 1ſt and 2d ſcholium, and 5th and 6th corollaries the three following wonderful poſitions.

1ſt, That in many caſes neither the motion nor direction of the body after it arrives in C, can be determined.

2d, That in other caſes the body, after it arrives in C, can paſs no farther.

3d, That in another caſe there mentioned the body, when it arrives in C, will inſtead of proceeding forwards fly back again in the ſame direction, in which it fell.

30. It is true, our author ſpeaks of all theſe determinations with ſome diffidence and uncertainty, and ſeems to think them very myſterious; but he ſatisfies himſelf with a blind ſubmiſſion to his computation, "quicquid autem ſit hic calculo potius "quam noſtro judicio eſt fidendum." Nay in the laſt concluſion he ſeems even to have ſatisfied his judgment;

judgment; for in the passage, he refers to in the following part of his book, he has copied Sir Isaac Newton, as he conceives, and not understanding his author seems to suppose himself sheltered under his authority.

31. Now the whole mystery, which has thus embarrassed our author, is this; When y, the distance of the body from the center, is made negative, the terms of the problem are sometimes changed with it. The centripetal force being as some power of the distance expressed by y^n, where n may be any number affirmative, or negative, whole number or fraction; if, when y is supposed negative, y^n be still affirmative, the solution gives the velocity of the body in its subsequent ascent from the center; but if y^n by this supposition becomes also negative, the solution exhibits the velocity, after the body has passed the center, upon condition, that the centripetal force become centrifugal; and when on this supposition y^n becomes impossible, this determination of the velocity beyond the center comes out impossible, the condition being so.

32. IN the corollaries and scholium annexed to the 48th proposition, which considers the ascent of bodies from a center, he meets with the same difficulties, and is at great pains to account, how the solution, he has there given, should fail him in certain cases; and to satisfy himself he recurs to infinitely small, infinitely great, and other the like ambiguous phrases, the usual modern palliatives of error. But so little does he seem convinced with his own reasoning on this head, that in the 96th section of his second volume, he has in a similar case supposed the computation to be accurate; disregarding those objections, he here makes use of to prove it fallacious.

33. THAT the computation in this proposition is unskilfully applied, cannot be doubted of at first sight;

sight; since, when n is any affirmative number, the force in C absolutely vanishes; and consequently the body will in these cases remain for ever at rest in the point C, if once placed there. Whatever computation therefore pretends under these circumstances to exhibit both the time of passing from the very center through any given space, and the velocity by that means acquired, must undoubtedly contain under it some fallacy. The truth is, the time determined by this method is the limit only of that time, in which the same given space would be described, supposing the body not to begin its motion from the very point C, but to be placed at first any where short of it. Indeed whatever be the centripetal force, if it diffuse itself every way equally from the center, no body can be moved out of the center by it.

34. THE problem proposed by Mr. Euler in his 43d proposition is from the relation of the times of description to the spaces described to determine the law of the impulsive force.

35. HERE he is entangled in difficulties from not understanding, that the conditions of a problem often put a limit upon algebraical expressions. This Sir Isaac Newton has very well illustrated in his treatise of algebra by the following easy instance *. If it were required to inscribe any chord given in magnitude within a given semicircle; x denoting the versed sine of the arch, which will be subtended by the given chord, a the diameter of the circle, and b the chord, x will be $= \frac{bb}{a}$: therefore whatever be the given magnitudes of b and a, there may be always found a value of x, which will satisfy the algebraic equation: yet it is manifest that if b be greater than a, the value of x derived from the equation will be of no use for solving the problem, which in

* P. 245.

that

that case is impossible. In like manner Mr. Euler proposes a case of his problem, (in the scholium) where the time of the body's motion shall be expressed by $\sqrt{2ax-xx}$, x denoting the space, through which the body has moved, and a some given length. Here he perceives, that this expression cannot in all magnitudes of x express the time of the body's motion; for when x is greater than a, the quantity $\sqrt{2ax-xx}$ diminishes, whereas the time already passed cannot be recalled.

36. But what he gathers from thence, is absolutely absurd; that when the body is arrived at C, it must ever remain in that point, which he owns himself is past all conception; nay more, that the velocity of the body, if it passed farther ought to be negative, which how to account for, he confesses himself unable. Indeed all, that can be performed in this problem, is to find such an impulsive force, wherein the given expression shall for some space denote the time. And when this is discovered, the limits, to which this expression must be subject, will be known. Here it comes out, that the impulsive force will be reciprocally as the cube of the distance of the body from a point at a distance equal to a from the place, whence the body set out. After this is known, it is easy so to express the time, that the expression shall agree to the body's whole motion. The time of the body's passing between the center now found, and any other place, will always be denoted by $a-\sqrt{2ax-xx}$.

37. In the following chapter Mr. Euler treats of right-lined motion in a resisting medium.

38. Here in the scholium annexed to the 20th definition we are told, that all other laws of resistance, except that which observes the duplicate proportion of the velocity, are merely imaginary; which is also repeated in the 1st corol. of prop. 50. This is so great a mistake, that no medium is yet known, in

which the whole resistance is accurately in that proportion. Our author had not attended to what, Sir Isaac Newton expresly takes notice of, that in all fluids some share of their resistance, though much the least, is owing to some degree of tenacity between their parts, and that this resistance is the same in all degrees of velocity.

39. IN the 4th corollary of the 50th proposition our author tells us, that when $m > 1$, the body, before it arrived at A (Tab. IV. fig. 2.), had somewhere, as in C, an infinite velocity; the meaning of which strange assertion is no more, than that a body, which should set out from the point C, determined in the manner there laid down, with any velocity, however great, would lose all its motion short of the point A; and consequently could never reach that point.

40. IN the third scholium of the same proposition Mr. Euler seems again surprized, that his computation should give an expression for the velocity, when in reality there can be no motion; but we have already explain'd this difficulty in our observations on the corollaries of his 40th proposition, and shall therefore say no more of it here.

41. AFTER the 53d, 54th, 55th, 56th and 59th propositions of Mr. Euler have been so excellently solved by Sir Isaac Newton, in the 8th and 9th propositions of the second book of his Principia; the weight of computation, with which Mr. Euler has filled them, can, I think, be no otherwise excused, than by supposing him unskilled in Sir Isaac Newton's treatise of Quadratures, to which recourse ought always to be had, in the analysis of such problems.

42. IN the solution of the 61st proposition, we have this equation $dx = \frac{k^m dv}{gk^m - v^m}$, x representing the space passed through, and v a quantity in the dupli-

cate

cate proportion of the velocity acquired at the end of that ſpace, k and g being known quantities; and we are told, that by the quadrature of curves (ope quadraturarum) v may be determined by x, however in the annexed ſcholium Mr. Euler diſmiſſes this equation from any future examination, becauſe its integral cannot be exhibited. But by the improvements the late Mr. Cotes has made to Sir Iſaac Newton's doctrine of Quadratures, which were ſoon after their publication demonſtrated by Dr. Pemberton, it is always poſſible by the circle and hyperbola to aſſign the relation of x and v, whether m be an integer or a fraction; and therefore a more ample proſecution of this equation might reaſonably have been expected from a perſon ſo much in love with computation.

43. IN the firſt ſcholium of the 62d propoſition we are told, that the velocity of the moving body always vaniſhes at C, becauſe the reſiſtance in that point is infinite. But this is falſe reaſoning; ſince every infinite reſiſtance will not inſtantaneouſly deſtroy a finite velocity. For example, ſhould the denſity of the medium be reciprocally in the ſubduplicate proportion of the diſtance from C, and the reſiſtance in the ſimple proportion of the velocity, which will be the caſe in this propoſition, when m denotes the fraction $\frac{1}{2}$; notwithſtanding ſuch a reſiſtance, the body may yet arrive at C, with any given degree of velocity however great.

44. IN the ſecond ſcholium to the 66th propoſition Mr. Euler obſerves, that the equation determining the quantity q in that propoſition will not be changed, although inſtead of p or R any multiples of thoſe quantities are taken. And as the magnitude of q in reality varies, when inſtead of R any multiple is taken; he accounts for this only by telling us in general, that a differential equation is more extenſive than the integral, from whence it is deduced.

45. THIS vague and indetermined reafon fhews, why Mr. Euler confidered this as a difficulty; which could fcarce appear fo to one duly apprized, that the fluxion of a logarithm of any quantity is the fluxion of that quantity, divided by the quantity; therefore whatever given number or quantity multiplies the quantity propofed, the fame multiplies both the numerator and denominator of the fluxion, whereby the fluxion remains the fame, however that number or quantity be varied; as it ought to do, becaufe the multiplying a flowing quantity by any given one only increafes its logarithm by the logarithm of the given quantity.

46. IN the fifth chapter, which treats of curvilinear motion, every propofition from the beginning to the end does immediately or confequentially depend on thofe erroneous equations, which we have taken notice of in the remarks on the 14th and 21ft propofitions; and therefore, however true the conclufions here given may be; yet, as they are founded on wrong pofitions, they owe their coincidence with truth to chance only, by the accidental intervention of contrary errors.

47. BUT admitting the principles here made ufe of to have been truly and accurately obtained; yet there are other important exceptions both to the method of this chapter, and to particular conclufions contained in it.

48. IN relation to the method of inveftigation here made ufe of, it is neceffary to obferve, that the ancient geometers in their analyfis, no lefs than in their compofition, brought in aid, as occafion required, whatever had been difcovered by others; and that inveftigation was by them the moft approved, which contained in it no procefs, that could be fupplied or contracted by any known propofition. On the contrary our algebraical gentlemen in the modern tafte lay afide all regard to order, concife-

nefs,

ness, or elegance in their conceptions; and confine their whole labour to the single ambition of rendring all previous knowledge as little necessary, as possible, to their present conclusions.

49. MR. Euler has here so closely pursued this scheme, as not to think of digesting even his own subject into any method, which might facilitate his analyses. Instead of premising these two general propositions, That of the equality of the areas described in equal times about the center of attraction, and That of the equality of the velocities at equal distances from that center both in the direct and curvilinear fall, whereby his following analyses might have been abbreviated; he plunges without preparation into compound propositions, and contents himself with slightly deducing these, as corollaries, from a proposition [prop. 74.], to which they should have been premised.

50. Two such propositions, which are or ought to be the very basis of all investigations of curvilinear motion, certainly merited to have been particularly discussed in their full extent, instead of being thus derived, as it were, by chance, from a proposition not so extensive as they are. This is inverting the natural order of the subject, and necessarily involves it in obscurity and perplexity.

51. BUT to be more particular, the deductions in the 70th and 71st propositions needed not that pomp of differential equations and canons; being easily deduced with more brevity without them, particularly the 3d and 4th corollaries of the 71st.

52. IN the 72d proposition, and its corollaries the effect of gravity compounded with a rectilinear motion is discussed with a degree of intricacy even surprizing in so simple a subject.

53. THE 73d, a very easy subject, is not much better treated.

54. IN the 74th propoſition the radius of curvature is unneceſſarily involved in the computation, to no other purpoſe than that of embaraſſing the proceſs, till it is exterminated.

55. THE ſolution of the 75th is unmeaſurably prolix; for if from the center C a circle be deſcribed, whoſe radius is unity, u will in this circle be the coſine of the angle MCP, and the ultimate proportion of the increment of CM or y to the increment of u will be compounded of the ratios of MT to TC, CM to the radius 1, and the radius to the ſine of the angle MCP, that is, of the ratios MT to TC, and CM to $\sqrt{1-uu}$; whence the differential equation $\frac{du}{\sqrt{1-uu}} = \frac{Ody}{y\sqrt{yy-QQ}}$ is immediately deduced; to arrive at which Mr. Euler has employed near 20 equations, ſubſtitutions, reſtitutions, *&c.*

56. BESIDES the general obſcurity, in which the 78th, 79th, 80th, and 81ſt propoſitions are here involved; there is in the 11th corollary of the 80th an error of ſo extraordinary a nature, as to merit a particular diſcuſſion.

57. IF the attraction directed to any center be reciprocally in the duplicate proportion of the diſtances from that center, and a body be projected from any point in a direction perpendicular to the line joining the center and that point with a certain degree of velocity; it is then confeſſed, that the curve deſcribed by this body will be an ellipſis, the center of attraction being its focus, and the point, from whence it ſet out, becoming that extremity of the axis, which is moſt removed from the focus. It is allowed too, that by diminiſhing the velocity of the projected body the ſpecies of this ellipſis is perpetually changing, approaching more and more to a right line; and that the time of half a revolution in this reduced ellipſis perpetually approaches to the time of the direct fall to the center. And ſince in theſe reduced ellipſes the

the body, after it arrives at its lower apsis, will perpetually return back again to the point, from whence it was originally projected; since, I say, this will be true, however small the velocity is, with which the body is originally projected, that is, however near the species of the ellipsis approaches to a right line: Mr. Euler has falsely concluded, that in the direct fall, when both the original velocity, and the ellipsis vanish, the same return will take place, that is, that the body, the moment it arrives at the center, instead of proceeding forwards with the velocity it has there acquired, will immediately return back towards the point, whence it fell.

58. Now though this monstrous supposition obviously manifests its own absurdity, and carries its own confutation with it to every one, that at all considers it; yet that Mr. Euler may not be confounded in searching for those erroneous deductions, which have here missed him, I will inform him, that the reason, why, when there was any transverse velocity originally impressed, however small, the body constantly returned, after it was got at some small distance below the center of attraction, is this, that then the attraction to the center never conspiring in direction with the motion of the body, the direction of that motion by this means perpetually changes, till below the center, where it acquires its greatest velocity, it becomes perpendicular to a line drawn to the center, and consequently returns from that point with a motion altogether similar to that, with which before it descended. But if there is no such original velocity, and consequently no obliquity, this reasoning no longer holds good; for the body will now be ever found in the right line passing through the center; and therefore, when it has once acquired a progressive motion in this line, it must continue it, till having passed the center its velocity is abolished by the contrary action of the centripetal force.

59. IF the reader, after the numerous miſtakes which this author has been guilty of, in relation to the perpendicular deſcent of bodies, is deſirous of ſeeing them all in epitome; he need only conſult the 2d ſcholium of the 91ſt propoſition, which will fully ſatisfy his curioſity, and afford him an ample field for aſtoniſhment. He will there find our author's former diſcoveries even improved; that bodies drawn to a center by centripetal forces, which increaſe perpetually their velocity, ſhall not only on a ſudden be ſtopt at the center in the midſt of their ſwifteſt motion; but be moreover annihilated *.

60. IN the demonſtration of the 98th propoſition we are told, that the radius of the curvature (Tab. IX. fig. 4.) at the point m, is $\frac{mv^2}{v\varepsilon}$. But this is the old miſtake of taking the radius inſtead of the diameter. For, ſince the line mn is a tangent in the point m, and V a ſuppoſed point of the curve, $\frac{mv^2}{2v\varepsilon}$, and not $\frac{mv^2}{v\varepsilon}$, is the radius of the curvature at that point; as we have formerly ſhewn in our remarks on the 2d corollary of the 21ſt propoſition.

61. IN the 4th corollary to this propoſition the ſecond differences of z and y are falſely aſſigned, being there but of half their proper magnitude. For to authorize the concluſions in this corollary M m ought to be the chord of the preceding arch continued; but ſince it is ſuppoſed to be the line, in which the body would proceed forwards from m, if not acted on, it muſt of neceſſity be conſidered as a tangent to that point m.

62. THE remaining part of this chapter is exceſſively obſcure, tedious and ten times more com-

* Corpus, ſtatim ac in centrum pervenerit, ibi evaneſcet.

pounded,

pounded, than it need to be, of which take the following examples.

63. THE 101ſt propoſition requires none of thoſe complicated equations, it ſo much abounds with. The true method of ſolution is to conceive the motion of the body to be compounded of two motions, one of them parallel to the axis, and the other performed in a plane perpendicular to the axis; then conſidering theſe ſeparately, the motion parallel to the axis will be ever uniform, being no ways changed by the attractive force; and the motion in the plane perpendicular to the axis will be reduced to the ſimple contemplation of the figure deſcribed by a body acted on by a known central force, and having its direction in the ſame plane with that force; then by combining theſe two motions together the ſituation of the body may at any time be known. From this method of ſolution the whole is reduced to principles already determined, and the concluſions in the two examples annexed to this propoſition are attained without any computation.

64. IN like manner the 102d propoſition abounds with a complication of differential equations no ways neceſſary. For if from the point A in the figure of that propoſition a line be drawn perpendicular to the plane APQ, let us denominate this line the axis. Then the force in AM may be always divided into two others, one of them parallel to this axis, and the other at right angles to it. But the parallel force being in the ſame direction with the force MQ, the body by the conjunction of theſe two forces will approach the plane APQ, and from the known quantities of theſe forces, and the known velocity towards this plane, the motion of acceding to this plane will be determined by the third chapter, it being under theſe circumſtances a rectilinear motion: and by the force perpendicular to the axis a curvilinear track will be deſcribed round that axis in a plane,

and

and to which that axis is always perpendicular; and as this curvilinear orbit may be determined by the consideration of the motion in a plane only, it is evident, that by this method the whole motion of the body will be determined by the means of known propositions, without the assistance of new principles, or the perplexity of new computations.

65. Of the superiority of this method of solution we will give the following instance.

66. Suppose a body at M to be attracted partly to the point A (Tab. II. fig. 1.) by a force directly as MA, the distance from that point; and partly to the plane APQ by a force directly as MQ, the perpendicular distance from that plane.

67. To determine this body's motion let the force AM be divided into two forces AQ, QM, one of them, as AQ, perpendicular to the axis, and the other QM parallel to it. The first of these will be directly as the distance from the axis, since AM is always as the force directed to the point A, and by this force there will be described in the plane, to which that axis is perpendicular, an ellipsis having the intersection of the axis with that plane for its center. The other force parallel to the axis will be as MQ, the distance of the body M from the plane APQ, but the original force, by which the body M was attracted to the plane APQ, was also as this distance MQ; therefore the whole united force, by which the body M is urged towards that plane, is as MQ; the motion then of acceding to that plane is the same, as that of the direct fall of a body towards a center, to which it is attracted in proportion to its distance from that center. Therefore the motion of the body in a plane perpendicular to the axis being known, and the motion of acceding in lines parallel to the axis to the plane APQ, or receding from it being known, the whole motion of the body is thereby determined.

68. From

68. FROM the known proportions between the force on M acting parallel to AQ, by which the ellipsis is described, and the separate forces, that by their conjunction in the direction MQ urge the body towards the plane APQ, the proportion between the time of one revolution in the ellipsis, and the time of one oscillation in a direction perpendicular to the plane APQ is easily assigned; and from hence the change of the position of the nodes, or of those points, in which the body passes through this plane in each revolution, is determined, the time of the bodies passing from one node to its opposite being the time of one oscillation. If therefore a line be supposed drawn from the center of the ellipsis to the node, in which the body was last found, the next position of the node will be determined by a line drawn also from the center of the ellipsis, containing with the first drawn line an area, that shall be in such proportion to the whole ellipsis, as is the time of one oscillation to the time of one revolution in the ellipsis.

69. THIS instance is the same, with the example Mr. Euler makes use of at the end of his 102d proposition (only an error of the press in the first line must be rectified, where instead of AP there is MP,) it also includes his 103d proposition, which is the determination of the nodes in this case. In the solution of this example Mr. Euler has filled several pages with the most abstruse and complicated computations. There are equations arising to the 7th, 8th and 9th powers, and involving the third power of first differences, and the second power of second differences; there are introduced logarithms both real and imaginary; nay the simple determination of the nodes cannot be dispatched without the relation of tangents and sines to their respective arches.

70. THE last chapter of this volume, which treats of curvilinear motion in a resisting medium, is equally

ly exceptionable with any of the preceding ones in respect to its method, and the operose computations, with which it is unnecessarily filled. But as we have already distinctly examined other parts of the book with regard to these particulars, we shall not enter into a farther discussion of them in this place; we shall only conclude our remarks upon this volume by obviating a most erroneous determination of the motion of a body in a resisting medium, in the scholium annexed to the 112th proposition.

71. IN this proposition Mr. Euler inquires, what law of resistance combined with the uniform force of gravity acting in the direction AC (fig. 6. Tab. XI.) will compel a body to move in a given semicircle, as BAMD.

72. THE determination is, that, if the resistance in every point, as M, is to the force of gravity, as three times the distance MQ to twice the radius AC, and this resistance is supposed to conspire with the motion of the body, when it moves upwards in the quadrant BA, but to be oppos'd to it, when it moves downwards in the quadrant AD; then the body by the combination of this resistance with the force of gravity, will describe the semicircumference BAD; and from hence in the scholium he concludes, the resistances in the quadrant AD being equal and altogether similar in their directions to those in the quadrant AB, that the body, when it arrives at D, will by means of these resistances already established return back through the semicircumference DAB to B.

73. THIS appears even to Mr. Euler wonderful, that a body at rest in D, and acted on by a force perpendicular to the line DC, for in this direction does the resistance act at the point D, should by means of this force be impelled in a line different from that perpendicular: but he thinks, he has solved the difficulty by telling us, that the force acting in the direction of the semicircumference at the point B is not abso-

absolutely perpendicular to the diameter in that point, but has an infinitely little deflection from the perpendicular.

74. What the angle measuring this infinitely little deflection is, Mr. Euler will never be able to explain; it being an unintelligible reason urged in support of a false fact. For when the body arrives at the point D, it cannot return in the circumference DMA, but by the intervention of some superadded force. What has here misled Mr. Euler, is his want of discerning, that the determination of the law of the resistance supposes the body, when its resistance is investigated, to move with a motion oblique to the action of gravity; it being only by means of this obliquity, that the comparison of the resistance with the force of gravity can in this case take place. And consequently the points B and D are excluded from all inferences formed on these principles; no more being determined, than that by such resistances, as are here assigned, the body will move from any one point of the semicircle to any other, provided the point, from whence the motion is supposed to begin, is neither B nor D.

REMARKS

REMARKS ON Dr. *SMITH*'s COMPLEAT SYSTEM of OPTICKS.

1. I Shall at present proceed no farther in my remarks on Mr. Euler's book; as I apprehend, what has been said, will be sufficient to prevent an intelligent reader from being misled by any of the mistakes of the succeeding part. And, I think, it appears, that the most of this gentleman's errors are owing to so strong an attachment to the principles, he had imbibed under that inelegant computist, who was his instructor, that he was afraid to trust his own understanding even in cases, where the maxims, he had learnt, seemed to him contradictory to common sense. But now I am to enter upon a more arduous undertaking, to pass censure upon the labours of a gentleman, who from his being a relation of the late most excellent Mr. Cotes, not only possessed all his papers at his death, but during his life enjoyed so uninterrupted a conversation with that great man, as furnished him with an opportu-

tity

nity of every kind of information, he could at any time desire. Therefore as we gather from Mr. Professor's proposals, that he has been imployed more than twenty years upon this work, and as he has informed us, that Mr. Cotes himself before his death had the same subject under consideration; it were reasonable to expect no small share of perfection in such a performance. I shall therefore endeavour to proceed in my remarks with all the circumspection, which these reflections may seem to require.

2. THIS treatise sets out * with a very unskilful representation of a capital proposition in Sir Isaac Newton's Opticks †, by omitting an essential part of it, which Sir Isaac Newton had expressed under the form of an exception. For this exception is so extensive, that the refractive power of water, the most common of all refrangible substances except the air, is almost half as much again in proportion to its density, as that power is in the air, in glass, and other terrestrial bodies. This power in all the oils is more than twice as much as in glass, and in one substance, the diamond, near three times as much. The repeating the proposition afterwards at art. 189. entire, is no excuse for omitting the exception in this place; since the conclusion, which Sir Isaac Newton makes from this proposition, depends absolutely upon that exception.

3. THERE soon follows, at art. 17, another instance of this author's imperfect knowledge of the true theory of the action between light and bodies, where it appears, that when a ray within a denser medium falls upon the surface of it, he did not apprehend, that it would be reflected till such times, as the sine of incidence bore a greater proportion to the radius than that, which measures the refraction; whereas the ray will be reflected, as soon as the sine

* Artic. 5. † B. II. prop. 10.

of

of the incident angle comes to that proportion. Upon this miſtake he proceeds afterwards in the mathematical part, art. 472, 487, and talks of rays emerging in the tangent of a circle.

4. THOUGH in this popular part of his treatiſe the author might be allowed to omit ſome circumſtances, which in mathematical demonſtrations could not be diſpenſed with; yet he ought never to have expreſſed himſelf, as he has done at article 21, where he deſcribes the rays, which in opticks may be conſidered as parallel, to be ſuch, in which the difference of their diſtances at any two given places is inſenſible: whence it ſhould follow, that the rays of the ſun are not to be conſidered, as parallel, unleſs their diſtances at the orbit of mercury and at the earth were not ſenſibly different.

5. IF in the figure referred to in article 32, O mean the place of the eye, for which it is uſed in the fourth chapter, the aſſertion in this article is not true. For the rays, which from P enter the eye, will none of them proceed from *p*, as here determined.

6. IN article 43 the ray, which within any glaſs lens makes equal angles with each ſurface, is ſaid to paſs through the middle point of the glaſs; whereas in all concavo-convex glaſſes that ray is inclined to a point in the axis, which is always without the glaſs. This overſight is indeed attempted to be corrected in the mathematical part, Art. 228; but very imperfectly. It is only ſaid, that in theſe glaſſes this point will be a little without the glaſs; whereas in ſuch there is no diſtance, which it may not exceed. For the thickneſs of the glaſs is to the difference between the ſemidiameters of the ſpheres, of which the two ſurfaces are portions, as this diſtance to the leſſer ſemidiameter.

7. IN article 95th this author has endeavoured to give ſuch a proof of the proportion, which he has

has assigned between the light, we receive from the moon, and that we receive from the sun, as he imagined, might be easily apprehended; though he informs us, that he discovered that proportion by other means. It follows, as far as it is just, directly from the proposition for the same purpose, of that excellent geometer James Gregory, in his Geom. Par. Univers. page 144; and the general proposition there mentioned for determining the proportion between the degrees of light received from any planet, and from the sun, is repeated in David Gregory's Astronomy *, a book in every one's hand. When this author was comparing moon-light with day-light, had he described them both in a similar manner, as was reasonable to have been done, he would have seen, how little the description, he has given of day-light, is to the purpose; but it likewise appears from the result. For he first informs us, that the proportion, he has set down, is not true, but upon supposition, that the moon reflects all the light, it receives from the sun; and yet his argument is drawn from comparing the light of the moon, seen in the day, with the light of the clouds, that is, is deduced from the quantity of light actually reflected by the moon.

8. AS this gentleman at art. 98th, &c. in relation to the apparent place and magnitude of the images of objects seen by reflection and refraction, has thought proper to depart from the principles delivered by the most approved writers on opticks; in order to form a judgment upon this innovation, I shall first set down distinctly the received opinion.

9. THE figure and colours of objects are immediately presented to the sight by the images made upon the bottom of the eye; but their distance is

* Lib. 3. prop. 58.

judged of by concomitant circumstances. This amongst other means is naturally suggested to us by the rangement of the several objects, that occur to our sight; those appearing the most remote, where the greatest number of other objects are perceived to intervene. This way of receiving information of distance by the sight has been anciently taken notice of; being at large insisted on by Alhazen the Arabian in his treatise of Opticks, Lib. 2. n. 39. In extensive views there is also another more definite circumstance, well known to the painters, which suggests different degrees of distance. This is the blueish tint, which the colours of objects receive from the interposition of the air between them and the sight; which is greater in proportion, as a greater quantity of air is interposed between the eye and the object. But as the last of these two causes can have no sensible effect in very small distances, where the eye is found to form an exact judgment, even without the assistance of the other, it is supposed, that the eye is possessed of some powers within itself, whereby it receives this information. When any object is viewed attentively, we always turn the sight directly to it, that the axis of the eye be pointed to the object; and therefore in viewing an object with both eyes, the axes of the eyes are more inclined to each other, when the object is near, than when it is remoter; and this being an animal action, is capable, in consequence of habit, of suggesting to the mind the different distances of objects by the different degrees of this conversion of the axes towards each other. Besides, as it is found, that the eye cannot see objects at the same time distinctly, which are at any considerable difference of distance, but can alternately fit itself to either; this change made in the eye is also by habit capable of suggesting distance. Both these causes are very well explained by James Gregory, in his Optica Promota. At the end of prop. 28, he thus expreſſes himſelf, "quotidiana experientia majoris vel minoris contorſionis [axium oculorum] natura eſt edocta, ut de diſtantiâ ſeu loco viſibilis conjecturam facere poſſit;" and of the ſecond in the next propoſition; "naturæ inſitum eſt, ut humores oculi fluidos & mobiles aliquo modo diſponat ad radiantia propinqua, ut diſtincte pingantur in oculi retinâ; ex quorum humorum mutatione magna vel parva dignoſcit natura (quotidianis experientiis edocta) num parvo vel magno intervallo diſtet radians ab oculo; atque ita de illius loco conjecturam facit." The firſt of theſe two cauſes was known before the latter, which was not apprehended, till after the manner of viſion by the refraction of the eye had been well conſidered; inſomuch, that Kepler, who has explained the firſt of theſe cauſes diſtinctly *, confines the judgment of diſtance from a ſingle eye to thoſe diſtances, which bear a ſenſible proportion to the breadth of the pupil †: and Aguilonius, who alſo admits of the firſt, lays down a propoſition in form (though contrary to experience) that one eye only is incapable of judging of diſtance ‖. That the firſt cauſe does operate to this effect, is from hence concluded, that we judge better of diſtance by both eyes than by one alone, as is uſually experienced by directing a perſon to endeavour to touch an object at ſome ſmall diſtance by a lateral motion of the finger with one eye ſhut. But it is alſo evident, that the ſingle eye will likewiſe in ſome degree direct to the diſtance; becauſe, if the object is not touched, the finger will not paſs at a great diſtance from it; and thoſe, who have the uſe but of one eye, will generally ſucceed in the experiment. In optick inſtruments, where the images of objects

* Paralipomen. in Vitellionem, prop. 8.

† Ibid. prop. 9.

‖ Optic. Lib. 3. prop. 1.

are viewed with one eye, this last cause is supposed to be that, which suggests the distance of those images; and that the eye sees them in that place, whence the rays proceed, which enter the eye from the several parts of the image.

10. THESE are the most direct means of our judging of distances. But as we do not always look at objects with the greatest attention, there are other circumstances accompanying objects, which offer themselves to our sight, whereby their distance is suggested. In particular the angle, under which an object appears, whose magnitude is supposed known, will give us an apprehension of a greater or lesser distance, according as that angle, or more properly the magnitude of the image made in the eye, is less or greater. Likewise if bodies are in motion, when the angle, under which they appear, increases, we shall judge the body to approach, or recede, when that angle diminishes. These and other collateral means of our receiving suggestions of distance, when they do not conspire with the former, often occasion deception in the sight, and cause us to mistake the real distance of objects.

11. THE same deceptions are also liable to take place in the images of objects seen by optick instruments; and the experiments, by which this author was induced to depart from the received opinion, are evidently of this kind; where the image of the object by its increasing or diminishing upon the motion of the eye or glass seems to approach or recede, contrary to what it ought to do upon the common principles *. But this gentleman not duly considering, that the eye was thus equally liable to deception in optick instruments, as in direct vision, has been so far confounded by these appearances, as to think it necessary to relinquish the common doctrine, and

* Art. 138. and the remarks upon it.

to

to advance in its stead a new rule for determining the apparent place of objects seen in those instruments.

12. According to this gentleman, the image of the object is always judged to be at that distance from the eye, where the object itself would be seen under the same apparent magnitude *. But this rule is in the first place deficient; as it can be no guide to the eye, when the original object has not first been seen. It is also contrary to fact in the most common and simple cases. In microscopes it is impossible, that the eye should judge the object to be nearer than the distance, at which it has viewed the object itself, in proportion to the degree of magnifying. For, when the microscope magnifies much, this rule would place the image at a distance, of which the sight cannot possibly form any opinion, as being an interval from the eye, at which no object can be seen. Yet this gentleman expressly asserts, at article 141, that the apparent distance of the image of an object seen either by reflection or refraction, is to the distance of the object seen by the naked eye, reciprocally as the apparent magnitude of the object to the apparent magnitude of the image. I believe, in general, whoever looks at an object through a convex glass, and then at the object itself without the glass, will find the object to appear nearer in the latter case, though it be magnified by the glass; and in the same trial with a concave glass, though by the glass the object be diminished, it will appear nearer through the glass than without. But the most convincing proof, that the apparent distance of the image is not determined by its apparent magnitude, is the following experiment. If a double convex glass be held upright before some luminous object, suppose a candle, there will be seen two images, one erect and the other inverted. The

* Art. 139.

first is made simply by reflection from the nearest surface, the second by reflection from the further surface, the rays undergoing a refraction from the first surface both before the reflection and after. If this glass has not too short a focal distance, when it is held near the object, the inverted image will appear greater than the other, and also nearer; but if the glass be carried off from the object, though the eye remain as near to it, the inverted image will diminish so much faster than the other, that it shall appear at length very much less, but still it will appear nearer. Here two images of the same object are seen under one view, and their apparent distances immediately compared; and here it is evident, that those distances have no necessary connection with the apparent magnitude. This experiment will be rendered still more convincing by the following means. If the glass be of some considerable breadth, let a small bit of paper, or the like object, be stuck upon it somewhere about the middle. Then let the inverted image be viewed through a short tube, or a hole at such a distance from the eye, that the frame of the glass or any circumambient objects may be concealed from the sight. When the eye has attentively considered this image, and fixed an idea of its apparent distance, let the glass be gently moved, while the eye accompanies the image, till the paper come also in sight; then as the image is actually formed nearer to the eye than the glass, so the paper will evidently strike the eye with the sense of its being more remote than the image. If the same trial be made with the upright image, the paper will appear nearest. This experiment succeeds best, when the eye is held as near to the glass, as it can bear to see the images distinctly.

13. Let us now consider Mr. Professor's arguments, in art. 138, against the received opinion. One of his reasons, that the different divergency of

rays cannot be the means of suggesting to us the place, whence those rays come, is, that "it is a "rational deduction from sense, which informs us, "that rays diverge from the points of an object; "which the majority of mankind are entirely ignorant of, and the ancient philosophers, who thought, "that something like rays proceeded from the eye to "the object, could distinguish distances as well as "we." This is quite as good a reason for the rays of light being no cause of our seeing at all; because those, who know the rays come from the object, and those who imagined they issued from the eye, could see equally well. But certainly, as the rays of light make such an impression upon the eye, as to raise in us the ideas of the objects whence they proceed, though we have no immediate perception of those rays; so the change made in the eye, in consequence of the different divergency of the rays, may be a sufficient cause to suggest to us difference of distance, without our having any direct perception of that divergency.

14. ANOTHER argument is, that our sensible ideas of the places of the remote parts of a long walk or gallery, and of the clouds over head, and of all celestial bodies, are quite different from the rational ideas of the places, from whence the rays diverge. But these are objects placed beyond the utmost distance of distinct vision, and our not receiving information from this power in the eye of the distance of objects placed beyond its reach, is no reason against its producing that effect within the distances, at which it operates.

15. THE argument, drawn from the success of painters in imposing on the sight by projections in perspective, by the representation of lights and shadows, and other parts of their art, will then deserve to be considered, when a picture shall be produced not distinguishable from the original object at a di-

 stance,

ſtance, in which the eye can ſee with perfect diſtinctneſs.

16. What is ſaid of objects ſeen through glaſſes by converging rays, is nothing to the purpoſe. For though we ſhould admit this gentleman's opinion, that the appearance of their approach is to be aſcribed to the augmentation of their viſible magnitude, this can only be conſidered as one of the caſes, in which the eye is ſubject to a deception from that cauſe.

17. Here this gentleman has alſo objected againſt our forming a judgment of diſtance from the inclination of the axes of the eyes; becauſe theſe angles are varied by turning the head ſide-ways, while we look at an object, though the diſtance remains the ſame. But as this is no proof, unleſs we ſaw things as perfectly by a ſide-view, as by a direct one; ſo in his remarks * on this article he corrects himſelf, and upon ſecond thoughts, it ſeems, will not diſpute, whether the feeling of the turn of our eyes in directing their axes ſucceſſively from a remoter object to a nearer may not alſo contribute to correct our judgment of its diſtance.

18. As our author has acknowledged for his predeceſſor, in his oppoſition to the received doctrine, a writer remarkable for the ſingularity of his opinions, and has even adopted his arguments; I ſhall here occaſionally obſerve, that that writer has in fact ſubſcribed to the doctrine, he imagined himſelf to be oppoſing, in regard to the power of both eyes in this buſineſs. This is evident by comparing his words in the 17th ſection of his Eſſay towards a new Theory of Viſion with the tranſcript, we ſet down above from James Gregory †. As Mr. Profeſſor has exactly

* Art. 245.

† "Becauſe the mind has by conſtant experience (quotidia-
" na experientia, &c. Opt. Prom. J. Gregor.) found the diffe-
" rent

actly followed the example of his original in assenting at last to the common opinion with regard to both the eyes, after having undertaken to oppose it; so possibly he might by his second thoughts have done the same in relation to one eye, had his author expressed himself somewhat more distinctly, where he has done so; for under the improper terms of straining the eye at Sect. 27, he has in reality acknowledged this consequence of the power, which fits the eye to each particular distance.

19. To apply this doctrine of apparent place to the assigning the apparent places of the images seen in optick instruments, it is necessary, that the apparent magnitude be assignable without regard to the apparent place, and for this end the antiquated doctrine contained in that ancient, but trifling piece of opticks ascribed to Euclide, that the apparent magnitude solely depends on, and is proportional to the angle, under which it is seen, is here at article 98 revived. Though Mr. Professor has delivered this maxim under the name of a definition; yet it is plain, he did not intend merely to impose an arbitrary sense upon the phrase apparent magnitude, but understood those words in their common acceptation for the idea of magnitude, which is excited in the eye upon the view of any object. Otherwise, what is said in the 104th and following articles concerning the apparent magnitude of objects seen by refracted or reflected rays, would be little more than tautology. Besides, he afterwards, at article 156, follows

"rent sensations corresponding to the different dispositions of the "eyes to be attended each with a different degree of distance "in the object, there has grown an habitual or customary con- "nection between those two sorts of ideas; so that the mind no "sooner perceives the sensation arising from the different turn, "it gives the eyes, in order to bring the pupils nearer or far- "ther asunder, but it withal perceives the different idea of di- "stance, which was wont to be connected with that sensation."

lows prop. 6th of the treatise abovementioned in assigning the diminution of the angle (without taking in any additional consideration) as the reason, why the distant parts of parallel lines seem to the eye not so wide asunder, as the nearer parts. He also repeats after prop. 10th and 11th of this treatise the like reason, why the remote parts of a walk or a floor appear to ascend gradually, and the cieling to descend; and after Aguilonius, whom the authority of Euclide's name led into the same error, that the upper parts of very high buildings seem to lean forward over the eye below, and that the distant parts of a line extended from the eye diminish to the sight *.

20. After all this, in the 169th and following articles Mr. Professor relinquishes this notion of apparent magnitude, and acknowledges, that it depends in part on our judgment of the distance; that two rows of trees which are parallel, by standing upon an ascent, whereby the more remote parts appeared farther off, than they really were, the trees were thought to diverge; that animals and all small objects seen in vallies contiguous to large mountains appear extraordinary small, because we think them nearer to us, than they really are; that in like manner, when things are placed upon the top of a mountain, or upon a large building, and are viewed from below, we think them extraordinary small for the same reason; and in the last place, that the sun and moon in the horizon appear to us larger under the same angle, because they are judged farther off.

21. To reconcile these contradictions, in the remarks, art. 301, it is said, that there are two sorts of apparent magnitude. I hope, this gentleman does not mean, that objects have two apparent magnitudes at the same time: and if he only means,

* Optic. Lib. 4. prop. 2. & Consect.

that

that in some circumstances objects give us the idea of one apparent magnitude, and under other circumstances of another; he ought to have shewn, how it can be brought to pass, that the idea of apparent magnitude, which is unalterable, either by the power of the imagination or any circumstances whatever, that do not affect the picture of the object upon the retina, can be obliterated, and another depending upon a collateral circumstance take place in its room.

22. AS this gentleman is very large upon the appearance of the horizontal luminaries; and, by his referring at art. 333 of the remarks to Riccioli and a paper of Mr. Molyneux published in the Philosophical Transactions, Numb. 187, for the history of what had been done before him, seems desirous, the world should be duly apprized, how much he has contributed to the explanation of that appearance; these accounts being very defective, I shall here give a fuller history of this matter. Ptolemy in his Almagest Lib. 1. c. 3. ascribes this appearance to a refraction of the rays by vapours, which actually enlarge the angle, under which the luminaries appear; just as the angle is enlarged, by which an object is seen from under water. Thus he is understood by his commentator Theon *, who explains distinctly, how the dilatation of the angle in the object immersed in water is caused. This passage of Ptolemy we find copied by the two Arabian astronomers Alfraganus † and Geber ‖.

23. BUT as soon as it was discovered, that there was no alteration in the angle, this cause was rejected. Accordingly we find, that Regiomontanus in his Epitome of the Almagest makes no mention of this cause assigned by Ptolemy, but refers to the

* Pag. 10.

† Elem. Astron. cap. 2.

‖ Astronom. Lib. 2. pag. 21.

optical

optical writers *, who agree in another opinion: Alhazen an Arabian author somewhat later than Alfraganus and Geber, though he admits, that some such refraction may at particular times cause a more than ordinary appearance of this phenomenon, yet he denies this to be the constant cause, and that the appearance is not owing to any real augmentation of the visual angle, but to an optical deception. After he has endeavoured to shew in distinct propositions, that the ordinary refraction of the air would rather diminish than increase the visual angle at the horizon; then to account for the appearance in question, he repeats from a former part of his work, that the sight judges of the magnitude of visible objects by comparing the visual angles, under which they are seen, with their distances †, and that, if the eye does not form a true judgment of the distances of objects, it will not form a just opinion of their quantities. Next he observes, that the sight perceives the colour of the sky, but not its form; and whenever the sight perceives any colour extended in length and breadth without discerning its true form, it will conceive it, as flat, by resembling it to the usual superficies, which occur to the sight, such as walls, or the like, and this is the appearance of all convex and concave surfaces at distances very remote ‡: therefore that the sight apprehends the surface

* In ea re sensum decipi perspectivis conclamatum est. Lib. 1. Conclus. 1.

† Dico, quod in secundo tractatu hujus libri declaravimus, cum tractavimus de magnitudine: quod si visus comprehenderit magnitudines visibilium: comprehendit illas ex quantitatibus angulorum, quos respiciunt visibilia apud centrum visus & ex quantitatibus remotionum, & ex comparatione angulorum ad remotiones. Optic. Lib. 8. n. 55. p. 280.

‡ Visus comprehendit colorem cœli, nec tamen certificat formam ejus nudo sensu. Et cum visus comprehenderit colorem aliquem in longitudine & latitudine; super hoc; quod comprehendit

face of the ſky, as flat, and judges of the ſtars, as it would of ordinary viſible objects extended upon a wide ſpace; and when the ſight views ſuch objects diſperſed over any extenſive place, and ſees them under equal angles, and perceives the quantities of their diſtances, then that, which is moſt remote, is eſteemed the biggeſt *. Again, as the ſight does not perceive the concavity of the ſky, and conſiders the ſtars, as placed in it, it perceives equal ſtars to be unequal, when in different places. For it compares the angle, under which a ſtar is ſeen near the horizon, to a remote diſtance, and the angle, under which the ſame is ſeen in a more elevated part of the ſky, to a near diſtance, and eſtimates a ſtar in or near the horizon to be greater than in the upper part of the ſky; and therefore eſtimates the ſame ſtar in different parts of the ſky to be of a different magnitude †. And after having argued againſt the refraction of the air's being the cauſe of this appearance, becauſe, though it conſpired to this effect, it would be too ſmall to

hendit figuram & formam: comprehendet ipſum planum: aſſimulabit enim ipſum aliquibus ſuperficiebus aſſuetis, ut parieti & aliis. Et hoc modo comprehendit ſuperficies convexas & concavas in remotione maximâ. Ibid.

* Viſus ergo comprehendit ſuperficiem cœli planam, & comprehendit ſtellas; ſicut comprehendit viſibilia aſſueta ſeparata, quæ ſunt in locis ſpatioſis. Et cum viſus comprehenderit aliqua viſibilia aſſueta in loco aliquo ſpatioſo, & comprehenderit illa angulis æqualibus, & comprehenderit quantitates diſtantiarum viſibilium: tunc illud, quod eſt remotius, comprehendetur majus. Ibid. p. 281.

† Viſus ergo comprehendit ſuperficiem cœli planam, nec ſentit concavitatem ejus, & comprehendit ſtellas ſeparatas in ipſo. Comprehendit ergo ſtellas æquales, ſeparatas inæquales: nam comparat angulum, quem reſpicit ſtella extrema, propinqua horizonti apud centrum viſus, ad diſtantiam remotam, & comparat angulum quem reſpicit ſtella in medio cœli, & propinqua medio, remotioni propinquæ. Et ſimiliter comprehendit ſtellam, quæ eſt in horizonte aut prope, majorem ea, quæ eſt in medio cœli aut prope. Comprehendit ergo eandem ſtellam & diſtantiam in diverſis locis cœli, diverſæ quantitatis. Ibid.

produce the appearance, he sums up the whole thus: "Causa ergo, propter quam videntur distantiæ stellarum in horizonte majores, quàm medio cœli aut prope: est illud: quòd sensus æstimat illas distare magis in horizonte, quàm in medio cœli. Et hoc, quòd visus comprehendit stellas in diversis locis cœli diversas in magnitudine: est error perpetuus: quia causa est perpetua: & est: quoniam visus comprehendit superficiem cœli planam, nec sentit concavitatem ejus & æqualitatem distantiæ à visu." Ibid. pag. 282.

24. This cause of the appearance is so fully set forth by this writer, that he has been almost universally followed ever since. Vitellio assigns the same *, and receives therein the approbation of his commentator Kepler †; insomuch that, where he seems to join the refraction of the air as an additional cause, Kepler ascribes it to a typographical error ||.

25. Our countrymen John Peckham Archbishop of Canterbury and the famous Roger Bacon have very distinctly expressed the same. The former in a treatise entitled Perspectiva Communis first lays down this proposition, "Distantiam horizontis majorem apparere quam alterius cujuscunque partis hæmispherii." His reason is, "Ubi major magnitudo interjacere videtur, necesse est, ut etiam major distantia esse videatur. Sed inter horizontem & videntem tota terræ latitudo interjacere videtur. At inter videntem & punctum cœli verticale nihil interjacere videtur ‡." Then his 82d proposition is this, "Stellas in horizonte majores apparere, quam in alia parte cœli," which he proves from the former proposition, "Quia magis distare videntur stellæ in horizonte, quam in alia cœli parte, ac tum in ortu, tum in medio cœli sub æquali an-

* Optic. Lib. 10. n. 54.
† Paralipom. in Vitell. p. 134.
|| Ibid. p. 132.
‡ Lib. 1. prop. 65.

"gulo

"gulo videntur; sequitur stellas in horizonte ma-"
"jores apparere quam alibi. Quia res ex æquali
"angulo ad majorem distantiam relata, major esse
"judicatur." The latter, though more concise, is equally explicite. "Quod visus judicat cœlum
"quasi planæ figuræ extensæ super caput in orien-
"tem & occidentem, quando aspicit ad alterum
"illorum. Sed quod videtur prope caput propin-
"quius videtur, & ideo stella quando est in medio
"cœli videtur esse propinquior, & ideo in horizonte
"videtur magis distare, sed quod magis videtur
"distare, videtur esse majus, postquam sub eodem
"angulo videtur." Perspectivæ Distinct. 3. cap. 6.

26. THIS was the opinion of the cause of this appearance, while the Arabick learning prevailed, and the same has been generally adopted since. Besides Kepler before mentioned, who proposes again the same opinion in his excellent Epitome of Astronomy, Lib. 1. pag. 81. Christopher Rothmannus somewhat earlier, being mathematician to the famous Landgrave of Hesse-Cassel, and con-temporary with Tycho Brahe, at page 109 of his Discourse on the Comet in 1585 in direct terms approves of the explanation of this phænomenon given by Alhazen. And Cardan still earlier has very dstinctly expressed the same opinion in the following words. "Astra
"omnia, dum oriuntur & occidunt, majora viden-
"tur, quam in cœli medio, quoniam terræ magni-
"tudo intermedia facit, ut oculus ea plus distare
"existimet, & ob id esse majora: nam & turris
"ulna major judicatur ab oculo illius distantiam
"comprehendente, tametsi minorem angulum in
"oculo faciat ulna ipsa *".

27. AGAIN, Martin Hortensius in his Dissertation on the appearance of Mercury in the sun anno 1631, at pag. 42 expresses himself to the same effect, "Quia cœlum longius a nobis putatur distare in

* De subtilitate. Lib. 3. ¶ 4.

"horizonte

"horizonte ob longissimum terræ tractum interjectum, in medio autem cœli videtur vicinius, quia nihil ei interjicitur. Apparent quoque stellæ majores in horizonte quam in medio cœli; cum eorum quæ sub eodem angulo videntur, quæ longius putantur abesse, majora appareant, quæ propius, minora."

28. A few years after, Des Cartes in his Dioptricks cap. 6. p. 93. gives expressly the same reason; "Hæc astra [sol & luna] circa meridianum in cœli vertice minora apparent, quàm cum sunt in ortu vel occasu, & occurrunt inter ipsa & oculos nostros diversa objecta, quæ judicium de distantiâ melius informant. Et astronomi cum suis machinis illa dimetientes satis experiuntur hoc, quod ita jam majora, jam minora appareant, non ex eo contingere, quòd modò sub majori, modo sub minori angulo videantur, sed ex eo quod longius dissita judicentur."

29. These are the words of Des Cartes, which Mr. Molyneux in the paper, to which our author refers his readers, has so strangely misrepresented. Nor has Mr. Hobbes fared any better under his hands. For though he erred in supposing, there was any necessary connection between the center of that arch, into which the sky appears, as formed, and the center of the earth; yet he accounts for the appearance truly from our being to sense out of the center of that arch *; nay has extended this so far, as

* "Causa quare sol, luna, & cæteræ stellæ majores apparent prope horizontem quàm ab horizonte remotiores, ex eo oritur, quòd cum oculus sit in superficie, non in centro terræ magis distat ab horizonte aspectabili, hoc est, ab horizonte cæruleæ illius superficiei quam vocamus cœlum, quàm ab ejusdem summo culmine. Etsi enim terra ad orbem stellæ cujuscunque rationem magnitudinis habeat inconsiderabilem, magna tamen est comparata cum distantia, quæ apparet ab ipsa ad loca stellarum apparentia." As this writer had very little mathematical skill, no wonder he should conceive the subject

as to observe this deception to operate gradually from the very zenith to the horizon, and that if the apparent arch of the sky be divided into any number of equal parts, those parts in descending towards the horizon will gradually subtend a less and less angle *.

30. James Gregory subscribes to the common opinion, though he agrees with Alhazen, that an extraordinary refraction by clouds may at particular times increase the appearance. Geom. Par. Univers. p. 141.

31. Father Malebranche also in the first book of his Recherche de la verité printed in 1673, has explained this phenomenon almost in the expressions of Des Cartes, and twenty years after in defending it, he supposed like Mr. Hobbes the vault of the sky to appearance spread into what, he somewhat improperly calls a demi spheroïde applati †. And he caused to be printed in the Journal des Sçavans ‡, an Attestation signed by M. le Marquis de l'Hôpital, M. Varignon, M. Sauveur, and M. L'Abbé de Catelan, signifying that his reasons were demonstra-

ject so indistinctly; but immediately after he gives a better reason for this difference in the apparent distance of the different parts of the sky. "Accidit oculo horizontem prospicienti distantiam ejus terram interpositam legendo æstimare, unde "magis videtur distare quàm a cœlo summo. Majorem autem "distantiam apparentem sub eodem angulo visorio necessario "sequitur imago major." Hobbes de Homine cap. 3. p. 17. If it be surprizing, that this author should here express himself in part so ill, in part so justly, it may be remembered, that he has been suspected of receiving assistance, in what he has writ both in geometry and opticks from the manuscript papers of a person of more skill, Mr. Warner the publisher of Mr. Harriot's Artis Analyticæ Praxis. See J. Wallisii Elench. Geom. Hobbian. p. 116 & Sethi Ward. Exercitatio in Philosophiam Hobbianam p. 356.

* Hobbes de Homine, cap. 7. p. 41.

† Reponse du P. Malebanche à M. Regis à Paris 1693.

‡ Ann. 1694, N°. x.

tive and clearly deduced from the true principles of opticks.

32. I wonder our author ſhould repreſent Mr. Molyneux's paper, as containing the laſt thoughts, which had been advanced upon this ſubject, excepting the fancy in the Eſſay toward a new Theory of Viſion, which is here ſo diffuſely examined; when in the ſame Philoſophical Tranſaction, in which Mr. Molyneux's paper was publiſhed, there follows a diſcourſe of Dr. Wallis, which alſo is quoted in the New Theory of Viſion, wherein the Doctor aſſigns the ſame cauſe as thoſe authors, we have here named. And ſince Mr. Profeſſor thought fit to examine in form ſo trifling an opinion, he ſhould not, methinks, have put us off with a bare referring to Riccioli for the hiſtory of the ſubject. For he has made mention of no other than the two opinions, which we have already taken notice of, excepting that of Gaſſendus; whereas Baptiſta Porta has given us no leſs than three others *, and Scheiner has likewiſe ſuggeſted a particular thought upon the ſubject, that the appearance might in part ariſe from the contraction of the perpendicular diameter of the luminaries, or the perpendicular diſtance of ſtars near each other, whereby we may be led into a wrong judgment, and inſtead of conſidering theſe, as leſſened, conceive the horizontal diameters of the luminaries, or the horizontal diſtance between two ſtars, as increaſed. Refract. Cœleſt. cap. 28. pag. 46.

33. But indeed none of theſe obſcure opinions deſerve any particular examination; nor even that of Gaſſendus, though an author of greater fame, who would aſcribe the appearance merely to a dilatation of the pupil, which he ſuppoſes to attend the view of celeſtial objects near the horizon †; for this is

* De Refract. L. 1. prop. 12.

† De apparente magnitudine ſolis humilis & ſublimis Epiſt. 1. art. 5.

incon-

inconsistent with the principles of opticks, and the author of it is so totally ignorant in that science, as to assert, that a dilated pupil magnifies an object for the same reason, as a convex glass does *. His letters upon this subject contain likewise divers other absurdities; that the office of the crystalline humour is to erect the images of objects, which otherwise would appear inverted †; that no object is ever seen by both the eyes at once for this strange reason, that because an object viewed first with one eye alone, and then with the other, appears to cover different places of a distant wall; therefore if the axis of both eyes were together directed to the object, both those parts of the wall, if not the whole interval, ought to be hid ‡; nay, great part of those letters is taken up in reviving and justifying the ridiculous Epicurean scheme, that objects are seen by species continually flying off from them.

34. Nor do we find, that this opinion of Gassendus gained any credit with the skilful, however a writer or two of little note may have fruitlessly endeavoured to make sense of it. On the contrary Huygens in his treatise upon the Parhelia, translated by our author, has approved, and very clearly explained the received opinion §.

35. This

* De apparente magnitudine solis humilis & sublimis Epist. 2. art. 13. pag. 39.

† Ibid. art. 4. pag. 18.

‡ Ibid. art. 17. pag. 52.

§ "The cause of this fallacy in short is this, that we think the "sun or any thing else in the heavens to be remoter from us, "when it is near the horizon, than when it approaches towards "the vertex, because we imagine every thing in the air that ap-"pears near the vertex to be no farther from us than the clouds "that fly over our heads; whereas on the other hand we are used "to observe a large extent of land lying between us and the ob-"jects near the horizon, at the far end of which the convexity

35. This apparent greater diſtance of the ſky near the horizon is ſo very obvious to the ſenſe, that we find it aſſumed, as indubitable, upon all occaſions. Aguilonius, though he does not conſider the effect of this appearance upon the luminaries, or the diſtance of ſtars, has in the very page quoted by our author, in article 160, for another purpoſe this propoſition, "Cœlum prope horizontem longius a nobis diſtare videtur quam juxta verticem *. And Merſennus in his Opticks, pag. 495, ſays, "Viſus "non deprehendit quantum aſtra diſtant a nobis, & "cœlum terræ in ambitu horizontis cohærere putat: "res enim ut plurimum propinquiores exiſtimantur, "quarum intermedium ſpatium non percipitur: hinc "cœlum prope horizontem longius quam juxta ver-"ticem a nobis diſtare videtur."

36. But Mr. Hobbes is the firſt, who has expreſsly conſidered this vaulted appearance of the ſky, as the real portion of a true circle, and Mr. Profeſſor has followed him herein ſo far, as to improve upon his thought of dividing this arch into equal parts; propoſing to find what portion of a circle this arch contains by obſerving the angle of the elevation of the middle point between the zenith and horizon.

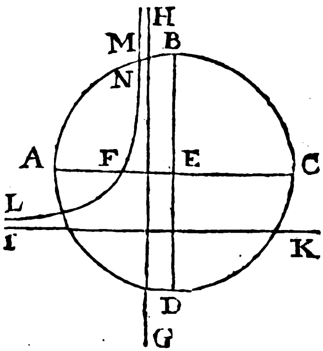
37. As this is the only addition, this gentleman has made, to what had been before advanced upon this ſubject, I am ſurprized to find the problem ſo very ſlightly paſſed over, ſince it is capable of a geometrical ſolution, which affords a better mechanical conſtruction than the inartificial one, he ac-

"of the ſky begins to appear; which therefore with the objects "that appear in it is uſually imagined to be much farther from "us. Now when two objects of equal magnitudes appear un-"der the ſame viſual angle, we always judge that object to be "larger which we think is remoter. And this is the true cauſe "of the deception we have been ſpeaking off." Art. 556.

* Optic. L. 4. Prop. 4. Conſect. 1.

quieſces

quiesces in; and besides admits of a computation so concise, as to be preferable to either. If in the circle ABCD, whose center is E, two Diameters AC, BD be drawn perpendicular to each other, and in AE, EF be taken to AE, as the tangent of the angle observed to the radius, this angle being less than half a right the point F will fall between A and E. Then EF being divided into two equal parts by the line GH parallel to BD, and ED also divided into two equal parts by the line IK parallel to AC, and to the asymptotes GH, IK the hyperbola LM being described through F cutting the quadrantal arch AB in N; the arch BN will be similar to half the apparent arch of the sky between the zenith and horizon. But the problem may also be solved by computation thus. Deduct the square of the tangent of the angle observed, and one third of that square each from the square of the radius. Then, as the radius is supposed unity, the arithmetical complement of the logarithm of the greater of these two numbers, half that complement, and the logarithm of the other number being added together give the logarithmick cosine of an angle, the cosine of the third part of which exceeds half the arithmetical complement of the greater of the two forementioned numbers by the sine of the angle, whose tangent is to the radius, as the apparent horizontal distance to the apparent perpendicular distance.



38. By this computation it will be easy to judge, how far the figure of the sky can be determined by observation upon this principle. As our author places the limits of the middle altitude within 18

and 30 degrees, if the altitude be 20 degrees, the horizontal diſtance will be ſcarce leſs than four times the perpendicular diſtance; but if the altitude be 28 degrees, it will be little more than two and a half.

39. After this account of what authors have ſaid upon this phænomenon, it plainly appears, how little reaſon Mr. Profeſſor has to aſſert in article 333 of his remarks, that ever ſince the time of Ptolemy the cauſe of it has been dubious and diſputed. Probably he ſcarce looked farther into this matter than the diſſertation of Mr. Molyneux, he here praiſes, who alſo for want of inquiry and miſtaking the authors, he had ſeen, had thus repreſented it. But we find on the contrary, that from the time of Alhazen the opinion of the beſt writers had been, as it were, unanimous. Where this gentleman learnt, that the firſt aſtronomers were not aware, that the intervals of ſtars ſeem much greater near the horizon than the meridian, I cannot conjecture: for ſurely it is impoſſible to have lookt at the ſtars without perceiving it. Nor can I find, where Ptolemy has given that piece of advice, our author here aſcribes to him. In the place, he has quoted, nothing is ſaid upon the ſubject. But I ſuppoſe, he intended to refer to the ſecond chapter of the ninth book of the Almageſt, which is cited by Kepler on this occaſion *, though nothing farther is there to be found than this ſhort obſervation, that the difference in the apparent diſtance between ſtars in different elevations created a difficulty in judging of the exact time, wherein the planets are ſeen ſtationary.

40. IN this particular inquiry, beſides ſhewing the true ſtate of the ſubject, I had alſo a farther deſign, to ſet forth how well the principles concerning the apparent magnitude of objects had been eſtabliſhed, and how generally approved, which this gentleman

* Paralipom. in Vitel. pag. 134.

has

has rejected for that crude conception concerning it, advanced in the firſt dawn of this ſcience. But it will not be amiſs to ſee farther, how diſtinctly optical writers have expreſſed themſelves upon this head in general.

41. ALHAZEN has here delivered himſelf in very diſtinct terms, that though the apparent magnitude of objects had been by moſt aſcribed to the magnitude of the viſual angle only, yet that this opinion was undoubtedly falſe. He aſſigns this reaſon, that within moderate diſtances it is evident to the ſenſe, that the object does not appear leſs to the eye by increaſing the diſtance. That for inſtance the eye does not perceive a thing at two cubits diſtance, as if it were leſs, than at the diſtance of one cubit, nor even though it were removed to the diſtance of three or four *. He confirms this farther by obſerving, that if two diameters are drawn in a circle, and the circle

* Plures opinantur, quòd quantitas magnitudinis rei viſæ non comprehenditur a viſu, niſi ex quantitate anguli, qui fit apud centrum viſus, quem continet ſuperficies pyramidis radialis, cujus baſis continet rem viſam: & quòd viſus comparat quantitates rerum viſarum ad quantitates angulorum, qui fiunt a radiis, qui continent res viſas apud centrum viſus, & non ſuſtentatur in comprehenſione magnitudinis, niſi ſuper angulos tantùm. Et quidam illorum opinantur, quòd comprehenſio magnitudinis non completur in comparatione ad angulos tantùm, ſed per conſiderationem remotionis rei viſæ, & ſitus ejus cum comparatione ad angulos. Et veritas eſt, quòd non eſt poſſibile, ut ſit comprehenſio quantitatum rerum viſarum a viſu ex comparatione ad angulos, quos res viſæ reſpiciunt apud centrum viſus tantùm. Quoniam eadem res viſa non diverſatur in quantitate apud viſum, quamvis remotiones ejus diverſentur diverſitate non magna. Quoniam quando res fuerit prope viſum, & ipſe comprehenderit quantitatem ejus: & poſtea fuerit elongata a viſu non multùm; non diminuetur ejus quantitas apud viſum, quando ejus remotio fuerit mediocris.——non comprehendit viſus rem viſam in remotione duorum cubitorum, minorem, quàm in remotione unius cubiti. Et ſimiliter ſi elongetur à viſu per tres cubitos aut quatuor, non videbitur minor, quamvis anguli, qui fiunt apud viſum, diverſentur diverſitate extranea. Opt. Lib. 2. n. 36.

ſo preſented before the ſight, that one diameter ſhall appear under a leſs angle than the other; or if the two ſides of a ſquare are offered to the ſight in the ſame manner; yet, if the diſtance be moderate, the eye ſhall notwithſtanding ſee one as a circle, the other as a ſquare *. To ſhew how conſiderable a power the diſtance of the object has upon the idea, we receive from the eye of its magnitude, he afterwards propoſes this obſervation; if a man ſhould cover with his hand before his ſight a great part of a diſtant wall, or of the ſky, and then upon the removal of his hand obſerve the idea raiſed in his mind from that part of the wall or ſky, which his hand before covered, he will perceive his imagination to be affected in no degree with the impreſſion of his hand being of a like apparent magnitude with that part of the wall or ſky, it covered †. This is ſo diſtinctly explained by this author, and ſo generally followed ‡, that Herigone has ſet it down as an optical axiom, "Quæ ſub eodem angulo videntur, "quæ longius putantur abeſſe, majora apparent ||."

42. This point therefore having been ſo generally received, I think Mr. Profeſſor's fellow-writer might have ſpared himſelf the trouble of his long diſſertation in ſupport of this opinion §; which can only ſerve to perſuade the world, that theſe gentlemen had but juſt come to the knowledge of ſo common a principle.

* Ibid.

† Ibid. n. 38.

‡ See Vitell. L. 4 prop. 27. Peckham Perſpec. L. 1. prop. 64. Bacon Perſp. Diſtinct. 3. c. 5.

|| Curſus Math. Tom. 5. pag. 15. This author conceived ſo little difficulty in the appearance of the horizontal moon, that he produces it as an indubitable proof of the axiom. "Ut luna, "quamvis in horizonte & medio cœli, ſub eodem angulo cer-"natur, major tamen in horizonte quàm in medio cœli apparet; "quod ejus diſtantia in horizonte major exiſtimetur propter in-"terjecta corpora."

§ Art. 171. of the Remarks.

43. When

43. WHEN Alhazen has expressed himself so distinctly upon this subject, as we have seen, it is a strange assertion of Tacquet, that every one of the ancient writers upon opticks supposed the visual angle alone to determine the apparent magnitude *. But this author can be of no account in this matter; since he had so little considered the operation of the eye, as to subscribe to the absurd assertion already mentioned of Gassendus, that the axes of both eyes are never directed to the same object †.

44. Aguilonius, though he adopts the same form of expression, yet by better considering the subject found himself obliged to acknowledge, that the eye does in fact estimate the magnitude of objects by comparing the visual angle with their distance: "Vim oculis natura concessit, qua simul rei distantiam percipiant, atque ex ejus collatione cum angulo pyramidis opticæ, veram magnitudinem quam proximè dignoscant. Manifestè enim deprehendimus errores omnes, qui in magnitudinis perceptionem irrepunt, ex distantiæ ignoratione originem ducere ‡." And he afterwards qualified the assertion, that those things, which appear under the same angle, are judged equal, and those, which appear under the greater angle, greater, by limiting it to the case, when the difference of distance is not taken cognizance of ‖.

* Quæ sub æquali angulo videntur, apparent æqualia; quæ sub majori, majora; minora sub minori; & qualis est angulorum opticorum, talis est & magnitudinum apparentium proportio. Id veteres optici ad unum omnes axiomatis loco habuere. Opt. Lib. 1. prop. 3.

† Ibid. prop. 2.

‡ Opt. Lib. 3. prop. 11.

‖ PROP. X.

Majoribus spectata angulis majora, minora minoribus, æqualibus æqualia videntur.

In hac propositione subintelligendum esse, ut ratio disparis intervalli, quo magnitudines ab aspectu distant, penitus ignoretur. Opt. Lib. 4.

45. WHAT

45. What Agbilonius has mentioned of the errors we fall into in relation to the apparent magnitude of objects being caused by a wrong judgment of their distance, Alhazen has particularly insisted on, when amongst the several causes of errors in vision, he sets down one to be great distance, and shews, that from thence will arise an error in our judging, both of the interval between distant bodies, and also of their magnitude †.

46. It is this error in our judgment of distance, which causes parallel lines to seem converging, and the like deceptions, which in the optical treatise ascribed to Euclide, and by our author are referred to the mere decrease of the angle. Trees are sometimes planted so as to appear in one point of view, as parallel. But in these the angles subtending their distances continually diminish, as these distances are farther removed from the sight, though not so much, as when the trees are really parallel. If the angle only determined it, trees, which extended in two lines from the point of view, must be seen as parallel.

47. But now to sum up the result of this disquisition concerning the apparent magnitude of objects; as we have shewn the concurrent opinion of all the best writers to be, that it is connected with our ideas of their distance; so this gentleman has not only admitted as much in many objects viewed by the naked eye; but also in terrestrial objects seen through telescopes *, that the object does not strike the beholder with the appearance of its magnitude being increased as much as the angle, under which it is seen, because it is thought nearer. And this is certainly the true cause of the appearance. But I am very much surprized to see it affirmed, that, what is here said, expresses the true sense of his definition of

† Optic. Lib. 3. n. 52. * Remarks, Art. 243.

apparent

apparent magnitude given in the 98th Article of his book, where it is asserted, that the greater visual angle not only causes a larger picture of the object on the bottom of the eye; but that this picture being larger or smaller causes a sensation of a greater or smaller visible extension,

48. In short, what has perplexed Mr. Professor is this, that the apparent magnitude of very distant objects is neither determined by the magnitude of the angle only, under which they are seen, nor is in the exact proportion of that angle compared with their true distance, but is compounded also with a deception concerning that distance; insomuch that if we had no idea of difference in the distance of objects, each would appear in magnitude proportional to the angle, under which it was seen; and if our apprehension of the distance was always just, our idea of their magnitude would be in all distances unvaried: but in proportion as we err in our conception of their distance, the greater angle suggests a greater magnitude. Our author not being apprized of this compound effect, has sometimes explained the apparent magnitude by the visual angle only, at other times by that angle compared with the distance, and by that means becomes thus inconsistent with himself.

49. However besides this gentleman's inconsistency, the method he has taken to determine the place of images made by reflection or refraction simply from the magnitude of the angle, under which they are seen †, is certainly erroneous. For not to mention other examples to the contrary, when the inverted image made by reflection from a convex lens appears less than the erect image; though the inverted image seems nearest, yet it not only looks less to the sense, but also in reality subtends a smaller

† Art. 139.

angle,

angle, as may be easily proved by bringing one of the images over the other.

50. I BELIEVE, I need not apologize for the length of my remarks upon this point; because it relates to a principle, that extends itself throughout this whole work, and in which Mr. Professor triumphs as a great discovery in opticks unknown even to Sir Isaac Newton or Mr. Cotes †. But I shall now conclude this head with one short remark upon what, he has said concerning the inventors of the telescope. For, as he has promised us the historical, as well as the other parts of the subject; this account would have been less deficient, if besides the translation of Huygens's history of the invention, and the descant he has made upon Mr. Molyneux's claim in behalf of Roger Bacon, he had taken notice of the pretensions of another of our countrymen Leonard Digges Esquire. For, if Roger Bacon had never compounded glasses together, it is certain, this gentleman had. For he expresly says in the 21st chapter of the first book of his Pantometria, published by his son Thomas Digges Esquire in 1571, "Marveylouse are the conclusions that may "be perfourmed by glasses concave and convex of "circulare and parabolicall fourmes, using for mul-"tiplication of beames sometime the ayd of glasses "transparent, whiche by fraction should unite or "dissipate the images or figures presented by the "reflection of other. By these kinde of glasses or "rather frames of them, *&c.*" Whoever reads the whole chapter, I think, can scarce doubt, but the author had seen in some measure the effects of viewing an object made by reflection from concave or convex surfaces through a lenticular glass.

51 HOWEVER, if Roger Bacon were quite ignorant of the telescope, yet I cannot agree, that the

† Remarks, Art. 197, 465, 494, 536.

use

ufe of glaffes of fmall convexity for the relief of the fight decayed by age was fo very imperfectly known by him, as this gentleman fuppofes *; for the more deficient or erroneous his theory is, the lefs probable is it, he fhould make fuch an affertion in terms fo exprefs ("hoc inftrumentum eft utile fenibus") if he had not feen the effect. The curfory manner, in which this is mentioned, perfuades me, it was not an invention of his own; but I cannot conceive, how he fhould have faid it fo directly, if this ufe of glaffes had not at that time begun to be in practice; efpecially in a chapter, which profeffes to exhibit examples of the doctrine before laid down. Mr. Profeffor's opinion, that the fryar had here only in his thoughts a plano-convex glafs with the flat fide laid immediately upon the object, I cannot fubfcribe to. Not to examine into the criticifm on the word "fuppofiti," the confequence drawn from Roger Bacon's confidering in his reafoning only the refraction of one furface is, I think, certainly invalid: for by the fame argument one might conclude, that telefcopes are now moft ufually made with a plano-convex object-glafs; becaufe Dr. Smith has determined the relation of their lengths to their apertures, charges, and power of magnifying upon that fuppofition †. Certainly fince Mr. Profeffor in fo ample a treatife has thought fit to conclude thefe points from the cafe, where one refraction only takes place; though the object-glaffes of thofe inftruments in their common fabrick have two furfaces, which refract; why may we not fuppofe this old writer in that very imperfect ftate of the fcience to have taken the like liberty.

52. IN what relates to the philofophical theory of light, this gentleman has had the caution to fet down Sir Ifaac Newton's difcoveries for the moft

* Remarks, Art. 88.

† B. 2. chap. 6, & 7.

part in his own words*; but has entirely omitted all, that he has eſtabliſhed in his ſecond book of Optics concerning the cauſe, why ſome part of the light is tranſmitted, and ſome part reflected at every pellucid ſubſtance. Now as this ſpeculation is the ſtrongeſt inſtance of the wondrous capacity of that great man in unravelling the moſt intricate and diſguiſed operations of nature, and eſtabliſhes at the ſame time ſuch a ſeries of properties belonging to light, as give us no ſmall reaſon to hope, that by diligently following the clue, he has here put into our hands, ſtill greater ſecrets of nature may one day be brought to view; Mr. Profeſſor ought not to have paſſed over ſo important and complicated a ſubject by a bare hypotheſis, that the paſſage of a ray of light at a ſurface, where it is reflected, is in a curve with one point only of contrary reflection at the very ſurface †. For if the light is inflected in its paſſage near the edges of bodies by the ſame principle, as it is reflected and refracted, and the alternate returns of eaſy reflection and eaſy tranſmiſſion will give the light in its paſſage by thoſe edges ſo many different bendings as to form three or even more viſible fimbriæ to the ſhadows of bodies, why may not the ſame alternate diſpoſitions at refracting ſurfaces produce in the light a more compound motion?

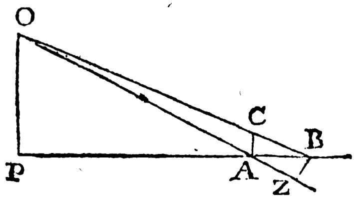
53. IN the remarks upon the cauſe of refraction and reflection, ſince Mr. Profeſſor thought proper to explain the hypotheſis of Leibnitz ‡ more at large than any other; it would not have been amiſs for him to have aſcribed it to its true author, Monſ. Fermat ||; nor would it, I believe, have diſpleaſed a geometrical reader in ſo large a work, containing ſo

* B. 1. chap. 6, 7, 8.
† Article 191.
‡ Article 413.
|| See Varia Oper. Math. Pet. Fermat, pag. 156, &c.

many

many collections, to have seen Huygens's most elegant demonstration from his treatise of light *.

54. I SHALL now proceed to consider the 2d book of this treatise, in which we may discover perhaps one cause of Mr. Professor's falling into the abovementioned mistakes. For here, I think, appears throughout that inexpertness in demonstration, which shews him to have been too little conversant in the writings of the best geometers. Of this we might have before taken notice of some examples. For instance, in article 157 it is said, that the apparent magnitude of a given line AB seen very obliquely at a given distance OA (meaning the angle A O B, which it subtends at the eye in O) increases and decreases in proportion to the increase or decrease of OP, the perpendicular distance of the eye from the line AB produced; provided that the distance AO be very large in comparison to AB. But though this assertion may be admitted, his proof is not to be justified. First, he says, if the ray BO cut a line AC perpendicular to AB in C, while the eye is raised or depressed in the perpendicular OP, the line AC will increase and decrease, as OP does: whereas this is false; for AC is to OP as BA to BP, which is not a given proportion, since neither BP nor AP are given, but AO. Again the angle AOB does not so properly vary in the same proportion with the line AC, as with a line let fall from A perpendicular on OB, or from B perpendicular on OA; and it is this perpendicular BZ, which is in a given ratio to OP, BZ



* Traité de la Lumiere, chap. 3 p. 40.

being to OP as AB to AO. From this wrong turn given to the demonſtration he was obliged to put an unneceſſary reſtriction upon the propoſition; for it is equally true, when the diſtance AO bears a very great proportion to AB, whether AB be ſeen with a great or ſmall degree of obliquity.

55. But in the ſecond book we find perpetual proofs of this unſkilfulneſs in regard to geometrical demonſtrations. Hence his complaint againſt the great Huygens's demonſtrations for being tedious, intricate and embarraſſing to the reader, by what he is pleaſed to call formal compoſitions and reſolutions of ratios *; the inartificial method of marking the ſame points with different letters †; demonſtrating by folding up the paper ‡; grounding his demonſtrations ‖ concerning cauſtics upon the deſcription of them in the popular part of his treatiſe, where eye ſight is appealed to for the light being condenſed at them; the unuſual phraſes of proportionable, middle proportional; conjointly to ſignify every method of adding the terms of proportionals, and diſjointly both for diviſion, converſion, and taking the difference of the antecedents and of the conſequents. To the ſame cauſe was owing the great inaccuracy, with which he expreſſed himſelf in relation to what, he calls the center of a lens. This likewiſe could be the only reaſon, which ſhould induce him to take refuge in algebraical calculations **, in a caſe, whoſe demonſtration is no more than this. XY being to TX as AP to PT, and $X\tau$ to XY as TP to $P\beta$, by equality $X\tau$ is to TX as AP to $P\beta$, and by compoſition $T\tau$ to TX as $A\beta$ to βP, or as $B\beta \times \beta A$ to $B\beta \times \beta P$. Again TF being to $F\tau$ as APq to $P\beta q$, by converſion TF is to $T\tau$ as APq to $A\beta \times \beta B$. Therefore $T\tau$ being to TX as $A\beta \times \beta B$ to $P\beta \times \beta B$,

* Remarks, Art. 421, 475.
† Fig. 440 & 443.
‡ Art. 214.
‖ Art. 329.
** Art. 339.

by

by equality TF is to TX as APq to $P\beta \times \beta B$. Consequently TX is greatest, when the rectangle under $P\beta B$ is greatest, that is, when it is equal to one fourth of the square of PB or AP, and then, since TF is to TX as APq to $P\beta \times \beta B$, TX will be equal to a fourth of TF, and XY equal to one fourth of FG.

56. OUR author seems himself conscious of this imperfection, when he takes advantage from his subject to express the lemma, whereon Sir Isaac Newton builds his demonstrations by prime and ultimate ratios in such terms, as he thinks, may be allowed in a physical subject, but in a demonstration purely mathematical have certainly no meaning. For, when it is said, that quantities and their proportions, which so approach to a state of equality as to become equal at last, may be taken for equal in a state immediately preceding the last, were he asked to explain, what that state is, which can be understood immediately to precede the last in any subject of geometrical demonstration, it were impossible to make sense of the question. Nay, so apprehensive is he of a defect in his geometrical reasoning, that he even refers to computation (a method, he has in another place * censured) as an additional confirmation †.

57. BUT besides such imperfections we find several direct errors. The fourth chapter has affixed to it a very pompous title, though it contains no more than two propositions, and those relating to the very

* Remarks, Art. 421.

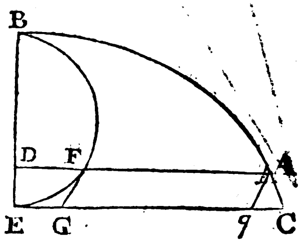
† The whole lemma runs thus. "Quantities and their proportions, which so approach to a state of equality as to become equal at last, may be taken for equal in a state immediately preceding the last; and also in a state somewhat remote from the last without sensible error in physical subjects: and the same may be said of figures, which continually approach to a state of similitude; especially if these errors, when computed, are found inconsiderable." Art. 204.

 easiest

easiest cases. The second of which gives only an approximation except in the sphere. The first is scarce true in any kind of object besides a plane or curve line parallel to the refracting surface. It is here only proved, that the distances of any intermediate point of a streight line from the extremes are in the same proportion in the image, as in the original. But streight lines are never in geometry said to be similar, because they are proportionally divided. If this term is to be applied to the right line, it must be said without restriction, that all streight lines are similar. In every other object not parallel to the refracting surface, the proposition is directly false. If the object were the arch of a circle in any plane not parallel to the plane of refraction, its image, as here assigned, would be elliptical. It is also false in every plane object, not parallel to the refracting surface; and much more so in every solid object.

58. Indeed the demonstrations in the first chapter of the foci, besides the imperfection of the principle, upon which they are grounded, are on another account also inconclusive. As Dr. Barrow has searched with much more exactness than any, who had gone before him, into the place, where rays are collected by reflection or refraction, and where the images of objects formed by this means are seen; so his exact inquiry into the line, in which each ray passes, and in what degree they condense together in every circumstance drawing out his disquisition into some length, others have endeavoured to give a more concise, though less perfect idea of this subject. For this purpose, in particular, Dr. Gregory in his Elements of Opticks has by a method of demonstration similar to that of our author contented himself with shewing, whereabouts those rays would cross the axis of a reflecting or refracting spherical surface, which would enter the eye placed in that axis; and thus

thus far his demonstration may be admitted as just. But our author having used the same method to assign the place of the focus independent of the eye; his demonstrations are by no means so free from exception. For in the first part of his work *, he has always represented as necessary to constitute a focus, that the rays be in that place condensed together; but his form of demonstrating may be applied to cases, where there is no condensation of rays. For example, if rays parallel to the base of the cycloide BAC generated by the revolution of the semicircle BFE upon the line EC be reflected, as the ray DA is reflected into Aq, as the point of incidence A approaches towards C, the lines Eq, EC continually approach toward equality, and become equal, when the distance DE vanishes; yet here is no condensation of the rays on the axis at the point C; because the intervals between the points q appertaining to equidistant rays increase, while DA approaches EC. This is evident, because Aq is parallel to FG, the tangent to the semicircle BFE at the point F, where the rays intersect that semicircle, and Cq is equal to the excess of the arch EF above the tangent.

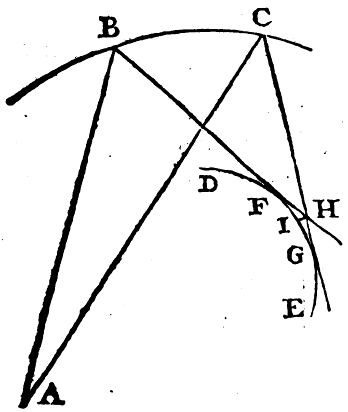


59. ARTICLE 468 contains a false conclusion from the preceding principles. For the density of the light, which is proportional to its heat or warmth, is to be estimated by a perpendicular section of the reflected rays, and to be determined by the aberration of the rays from the point of contact at the caustic after this manner.

60. IF rays diverging from any point A are reflected by the surface BC into the caustic DE, all

* Book 1. chap. 2.

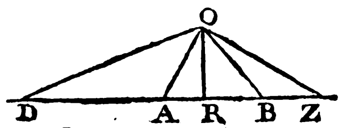
the rays in the pencil BAC will be ſpread through the part of the cauſtic FG, ſuppoſing F and G to be the points, in which the reflected rays BH and CH touch the cauſtic. Here the denſity of the light upon the cauſtic muſt not be meaſured by the magnitude of FG, but by a perpendicular ſection of thoſe rays, where they are moſt condenſed. Thus if HI be drawn from H perpendicular to the curve, every ray of the pencil BAC will paſs through that line HI. This line then will nearly repreſent the magnitude of a perpendicular ſection of thoſe rays at I; and therefore if two pencils proceeding from A are taken ſo that that the quantity of light, or, which is the ſame, that the angles made by the extream rays may be equal in each, the denſities of the light at the middle of the parts of the cauſtic formed by each pencil will be reciprocally as the line IH in each. But IH is ultimately the chord of $\frac{1}{4}$ the angle BHC to the radius GH or GI. The denſity then eſtimated by this means will be reciprocally as GI, or its double GF compounded with the reciprocal of the angle BHC.

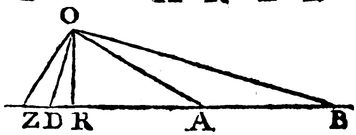


61. Hence it follows, that when to equal angles made by the incident rays at A unequal angles are made by the reflected rays at H, the denſity of the rays will not be juſtly eſtimated by the reciprocal of the part FG of the cauſtic included between the reflected or refracted rays. For inſtance, though in our author's caſe, at art. 465, when the rays reflected

reflected by a circle issue from a point in its circumference the density of the light in the superficial caustic, which is proportional to its heat, is, as this gentleman has computed, reciprocally as bH × HI: yet in his 467th art. when the rays are parallel, this density is not in the simple ratio of BD directly and the triplicate of CD inversly, as he has determined; but in the duplicate ratio of DB directly and in the triplicate of CD inversly.

62. Whereas this gentleman professes to have rendered the doctrine of the rainbow more general, than it has been yet handled; all that he has added to what Dr. Halley had in the Philosophical Transactions Numb. 267. already done upon that subject, is the applying to the computation of the inverse problem a general theorem of Mr. John Bernoulli published in the Acta Eruditorum *, for finding from the tangent of an arch the tangent of any multiple of that arch. The lemma in article 508, from which this general theorem is deduced, might have been demonstrated thus. If ROA be one angle, ROB the other, and ROD their sum or difference. Draw OZ perpendicular to AO, then the angle ROZ is equal to the angle RAO, and therefore



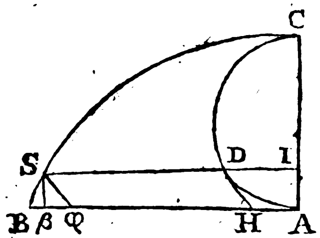


the angle ROB being equal to the angle AOD, that under RDO is equal to that under BOZ; whence as DZ to OZ so is OZ to BZ, but as OZ to ZR so is AZ to OZ, so that by equality, as DZ to RZ, so is AZ to BZ, and by division as DR to RZ, so is AB to BZ, or by permutation DR is to AB as RZ to BZ, or as RZ × AR to BZ × AR, that is, as ORq

* Ann. 1722. Mens. Jul.

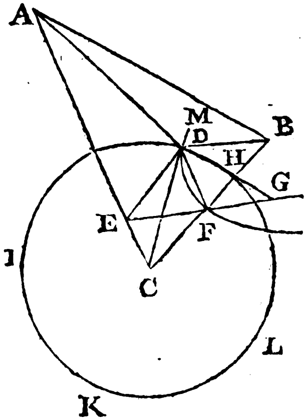
to $ORq—AR \times RB$, when AB is the ſum of the given tangents and RD the tangent of the ſum of the arches, but as ROq to $ROq+AR \times RB$, when AB is the difference between the given tangents, and RD the tangent of the difference of the arches.

63. The propoſition contained in article 602 is not univerſally true, but fails, whenever the curvature at the vertex of any one of the curves cannot be compared with that of the circle. For inſtance, in the cycloide ABC, when any ray parallel to the baſe AB is reflected at S, Bφ, the aberration from B, is equal to the arch AD leſſened by the tangent DH or HA, and Bβ is equal to AD—DI; but AD—AH bears not ultimately a finite proportion to AD—DI. And hence the corollary in article 603 is not true, when one or more of the ſurfaces are cycloides or curves of the like curvature.



64. But the firſt propoſition of this chapter is both defective and erroneous upon the author's own principles. The place of the image cannot be determined, as is here required, till it be ſhewn how to find the line, in which it is ſeen, that is, how to aſſign the reflected or refracted ray, which ſhall enter the eye. But of this our author has been abſolutely ſilent. The problem is indeed in moſt caſes too complex to admit of a ſimple and elegant ſolution; but this is no excuſe, why in a treatiſe promiſed to be compleat, no notice ſhould be taken at leaſt of what had been done by others in this particular. Alhazen's problem to find the ray, which iſſuing from a given point ſhould be reflected by a ſpherical ſurface to the eye in any given poſition, was thought worthy to exerciſe the invention of two

of the principal mathematicians of the laſt age. The hyperbola, by which Huygens ſolved the problem, eſpecially as determined by Sluſius in the Philoſophical Tranſactions, Numb. 98. admits of a very conciſe demonſtration. Suppoſe a ſpherical ſurface IKL, whoſe center is C, and A to be a point, whence the ray proceeds, and B the eye. AC and BC being joined, and AC×EC likewiſe BC×CF made equal to the ſquare of the ſemidiameter, and EF drawn and continued; if an equilateral hyperbola FM be deſcribed to the diameter EF, whoſe ordinates ſhall be parallel to AB, the ray, which paſſing from A falls on the ſurface in the point D, where this hyperbola interſects the ſurface within the angle ACB, ſhall be reflected to the eye at B. Draw DHG parallel to AB, then by the hyperbola EG is to GD as GD to GF, ſo that the triangles EGD, DGF will be ſimilar, and the angle DEG equal to the angle FDG. Again, AC × CE and BC × CF are equal; therefore AC is to CB as CF to CE, the triangles ACB, FCE are ſimilar and the angle CEF equal to ABC or DHF. Conſequently the angles DEG, FDH being alſo equal, the angles DEC and DFC will be equal. But if CD be drawn, AC is to CD as CD to CE; therefore the angle ADC is equal to DEC. For the ſame reaſon the angle BDC is equal to DFC; conſequently the angles ADC, BDC are equal; and ſo the ray AD will be reflected to B.

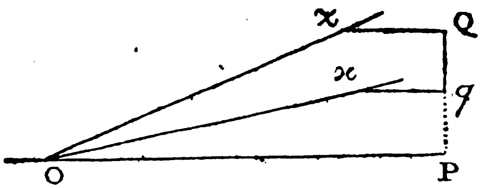


65. If the oppoſite hyperbola be deſcribed, it will paſs through the point C; and where it cuts the

ſpherical ſurface within the angle made by AC and BC continued will give the point in the concave ſurface, whence the ray from A will be reflected to B.

66. NEITHER ought Dr. Barrow's excellent ſolution given in his Optical Lectures * of the problem in plane refracting ſurfaces to have been omitted; eſpecially ſince it admits of an improvement from his Geometrical Lectures †.

67. BUT farther in this propoſition, what relates to the viſible place of the image is inconſiſtent with Mr. Profeſſor's general doctrine. For example, if



the object be Qq a part of the line QP, it is here determined, that Qχ being drawn parallel to OP to meet the reflected or refracted ray, in which the point Q is ſeen, and $q$$x$ drawn likewiſe paralled to OP, till it meet the like ray in x; then the object Qq will be ſeen as extended from χ to x; but the general precept was, that the object appears in that place, where it would fill the angle, under which it is ſeen; whereas here the diſtance χx is greater than Qq.

68. WE have now gone through the two firſt books of this treatiſe, which are intended to comprehend as much of the ſubject, as is the proper object of mathematicks. In the next book, which promiſes the mechanical part, to moderate our expectations the author ſets out with informing us, that he had himſelf no ſkill, in what relates to the figuring, grinding and poliſhing of glaſſes; and therefore has contented himſelf with giving us what collection, he could gather from others. Thus

* Lect. 5. §. 12. † Lect. 6. §. 2.

there

there is no account of the common methods made uſe of; but only a particular artifice invented by Huygens, as better ſuited than the uſual ways of working, to the formation of glaſſes for teleſcopes of an extraordinary length. But I ſhall take leave to obſerve upon this part of the work, that as our author profeſſes to deliver the hiſtory and progreſs of the ſubjects, he has handled; in the deſcriptions of the aſtronomical inſtruments, which he has here inſerted, he ſhould not have ſubſcribed to the vulgar miſtake, that the method of graduation uſed in theſe inſtruments was the invention of Petrus Nonius *. For his method of diviſion, which he explains at large in his very curious treatiſe de Crepuſculis † printed at Liſbon in 1542, is widely different. It conſiſts in deſcribing within the ſame quadrant 45 concentric arches, dividing the outermoſt into 90 equal parts, the next within into 89, the next into 88, and ſo on till the innermoſt was divided into 46 only. By this means, in moſt obſervations, the plum line or index muſt croſs one or other of thoſe circles very near a point of diviſion. Whence by computation the degrees and minutes of the arch might eaſily be counted ‡. This method of diviſion is alſo deſcribed by Mr. William Barlowe in his book, entitled, The Navigator's Supply printed at London in 1597; and an actual example of it is to be ſeen in the

* Art. 861. † Part. 2. prop. 3.

‡ This artifice Nonius likewiſe 20 years afterwards deſcribes briefly in his excellent treatiſe De Arte atque Ratione Navigandi, where he would perſuade himſelf, that it was not unknown to Ptolemy, ſaying, "Ita enim exiſtimo Claudium Ptolemæum "feciſſe. Nam ſi maximam ſolis declinationem idcirco (ait) re-"periſſe partium 23. m. 51. ſe. 20. quia ea proportio inventa "fuiſſet totius circuli ad arcum inter tropicos quam 83 habent "ad 11. Conſtat igitur aliquem quadrantem intra ambitum in-"ſtrumenti deſcriptum, in ipſos 83 æquales partes deſtributum "fuiſſe, quarum arcus inter tropicos 44 continebat. Neque "enim tanta fuit illius inſtrumenti quo Ptolemæus utebatur "magnitudo, ut in eo prima atque ſecunda minuta notari poſ-"ſent." Lib. 2. Cap. 6.

first

first figure of Tycho Brahe's Astronomiæ Instauratæ Mechanica, printed in the same year, But as this divides the degrees very unequally, and is also very difficult to execute with exactness, especially when the numbers, into which the arches are to be divided, are incomposite, of which there are no less than nine; though Tycho in his second instrument endeavoured to lessen the latter difficulty by the use of other numbers; yet to that instrument he also added diagonals, which he so much preferred, that at length he left Nonius's way quite off *, and used only diagonals in his mural Arch and the rest of his instruments.

69. This method of diagonals was first published by Thomas Digges Esquire, in an ingenious treatise of his entitled Alæ seu Scalæ Mathematicæ, printed at London in 1573, occasioned by the memorable new star, which had then lately appeared in Cassiopea, on which he made his observations by a fore-staff thus divided. But he acknowledges, it was not a discovery of his own, having been long practised in England; and, as he had been informed, was the invention of one Richard Chanseler a most skilful artist †. Tycho, in that part of his Progymnasmata, which treats of the same star, takes notice of this passage of Mr. Digges, and affirms, when he studied at Lipsick, he used such an instrument divided after this manner, which was communicated to him

* De Cometa Ann. 1577. pag. 461.

† —"libere fateor illam partiendi radium in plurimas sensibiles "partes rationem a me inventam non fuisse, sed diu hic in Anglia "a plurimis peritissimis mathematicis usurpatam. Primus tamen "qui ea divisionum ratione usus est, quemadmodum ex auditu "accepi, quidam fuit nomine Richardus Chanslerus, peri-"tissimus & ingeniosissimus artifex mathematicus, cujus nomen "eo libentius publicare decrevi, quòd jam e vita discesserit, ne-"que monumentum suarum virtutum ullum publicum reliquit, "præter instrumenta quædam summa arte fabricata, & dulcissi-"mam suæ singularis peritiæ (in nonnullorum mathematico-"rum adhuc superstitum animis) memoriam." Capitul. 9.

him from one Homelius, a famous mathematician of that place *. This was about 1565; for in a letter written to Rothman in 1587, he says, he was then 17 years of age †.

70. BUT Jacobus Curtius, Vice-Chancellor of the empire, improved at different times the division by concentric circles. At first, after describing within the limb of the quadrant a number of such circles, he extended the first to 91 degrees, the second to 92, the third to 93, and so on; and then divided each of these into 90 equal parts. This, when he was the Imperial minister at Rome, he communicated to Clavius, who published it there Anno 1586 in a treatise, where he describes an instrument for delineating of dials ‡; and Clavius himself proposes a variation in this method by describing 39 such concentric circles within the limb of the quadrant, continuing the second of these to 91 degrees, the third to 92, and so on, till the 39th extended to 128 degrees. Each of these circles he divided into 128 equal parts, which is performed by the simple method of bisection §. But a calculation is necessary, or at least a table ready computed, for knowing the degrees and minutes, which answer to the divisions of the several circles in both these methods; and Curtius made a second improvement, which required no such computation.

* "Ego certè multo ab hinc tempore, videlicet annis plus "minus 28, cum Lipsiæ studiis incumberem, ejuscemodi partitionem radii in usu habebam, ex clarissimi mathematici Homelii officina, beneficio Bartholomei Schulteti, qui illi inservierat, "mihi communicatam. Unde autem Homelius hanc hauserit, "aut an ipsemet eam adinvenerit, apud me incertum est. Sit "cujuscunque velit, ingeniosa certè & apprimè utilis est destributio, quam & ego postea arcualibus graduum subdivisionibus in quadrantibus, sextantibus & armillis non inconcinnè aut "infrugeferè applicui." Pag. 671.

† Epist. Astronomic. pag. 62.

‡ Fabrica & usus Instrumenti ad Descriptionem Horologior. Cap. ult. p. 114. § Ibid. pag. 116.

71. THIS

71. This he sent about 1590 both to Clavius and Tycho, which the first published in his book of the Astrolabe, Anno 1593 *; and Tycho printed Curtius's letter with the account of this improvement, at the end of the abovementioned treatise, where he gives a description of his instruments. This method consists in setting off upon the first concentric arch within the outermost, the 60th part of such a portion of that arch as answered to 61 degrees, and from that division continuing on through the whole arch the intervals of single degrees. By this means every division in this arch is advanced one minute forwarder than in the first. At the beginning of the next arch he takes off the 60th part of 62 degrees, and from that point continues through the whole arch the intervals answering to single degrees; whereby each division in this arch is advanced two minutes beyond the degrees of the first. And thus he proceeds, till the degrees are divided into the whole number of minutes they contain †.

72. But

* Lib. 3. Canon. 1. Schol. pag. 566.

† This worthy gentleman Jacobus Curtius was the great patron of Tycho with the Emperor Rodulph the second, when Tycho had been so ill used by his countrymen. He was also a very ingenious person, as appears from these methods of division, as also from a scale of his invention for readily drawing the hour-lines, and a quadrant, which by the help of a table of sines and tangents is of excellent use in astronomy and practical geometry. Both these are described by Clavius in the 16th, and last chapters of his abovementioned book of dialling. Curtius likewise made an improvement in trigonometry, which he takes notice of in his letter to Tycho. This was occasioned thus. In 1588 one Nicolas Raymarus Ursus, of whom both Tycho and Rothman often complain as a plagiary, published at Strasburgh a sort of rhapsody, which he called Fundamentum Astronomicum, where amongst many things good and bad, at fol. 16 and 17 he gives a scheme and directions how to resolve several cases in trigonometry by addition and subtraction; this Curtius informs Tycho, he had improved so as to include all cases. He does not there directly set down his method, but only hints, how it might be

72. But as this laſt improvement is very operoſe, a moſt excellent compendium of it has been ſince introduced. This was firſt communicated to the world by Peter Vernier a perſon of diſtinction in the Franche-Comté, in a very ſmall tract, entitled La Conſtruction, l'Uſage & les Proprietez du Quadrant nouveau de mathematique, &c. printed at Bruſſels in 1631. In his dedication to the Infanta of Spain, Iſabella Clara Eugenia governeſs of the Netherlands, having ſhewn its preference to what Nonius and Clavius had done in the affair he adds, "Le mien ayant

be done. Pitiſcus at the beginning of the fifth book of his trigonometry in 1600 gives a method to this purpoſe; but Clavius had ſeveral years before in his abovementioned treatiſe of the Aſtrolabe, Lib. 1. lem. 53. performed it much better. This method may be briefly repreſented thus. The product ariſing from the multiplication of any two numbers is equal either to half the ſum or to half the difference of the coſines of the ſum and of the difference of the arches, to which the numbers propoſed ſhall be the ſines, multiplied by the radius; the ſum of the coſines is to be taken, when the ſum of the arches exceeds a quadrant, and the difference of the coſines, when that ſum is leſs than a quadrant. If either or both the numbers exceed the radius, ſo as not to be found among the ſines, ſuch numbers muſt be divided by 10, 100, or 1000, &c. till they can be found in the table, and the produce multiplied accordingly. In like manner the quotient ariſing by the diviſion of any number by another, will be equal to half the ſum or half the difference under the condition abovementioned of the coſines of the ſum and of the difference of two arches, one of which has for its ſine the dividend, and the other for its co-ſecant the diviſor, that ſum or difference being divided by the radius. But if the dividend exceed the radius, it muſt be divided as before; and if the diviſor be leſs than the radius, it muſt be multiplied in like manner, till it can be found among the ſecants. This directly follows from theſe two propoſitions; The firſt, that the rectangle under the ſines of two arches is equal to the rectangle under the radius and half the ſum or half the difference of the coſines of the ſum and of the difference of the arches. The other is, that the ſine and co-ſecant of the ſame arch are reciprocals. A report of this artifice ſeems to have put lord Napier upon the noble invention of logarithms. See Tycho Brahe's life by Gaſſendus p. 109. 165, as alſo Anthony Wood's Athenæ Oxonienſes, Vol. I. col. 549.

"tous ces advantages sur les autres ce n'est pas sans "subject que je l'appelle nouveau et de mon inven-"tion." In the preface also he claims it as his own invention, and says, thereby a quadrant of three inches is rendered capable of determining minutes *. In his book he shews how to apply it to instruments of different dimensions. His contrivance is a moveable arch divided into equal parts, one less in number than the divisions of the portion of the limb corresponding to it †.

73. In 1634 Joan. Baptista Morinus, one of the Royal Professors of mathematicks at Paris, printed a book entitled Longitudinum Cœlestium atque Terrestrium Scientia. The way there delivered for finding the longitude had been publickly examined by commissioners appointed by Cardinal Richelieu. The author in graduating the instruments for making observations to sufficient exactness commends two methods. The first, he says, was the invention of Joannes Ferrerius, a most industrious and accurate artist ‡. This is performed by circular diagonals, which

* "—d'un instrument du tout admirable de mon invention, "& qui n'a jamais esté veu, &c.——en un quart de cercle de "trois poulces d'estendue, on demesle les minutes entieres du "cercle quoy qu'il ne soit divisé qu'à l'ordinaire en nonante de-"grées, à l'ayde toutefois d'une petite portion de cercle mobile "contenant seulement quinze parties esgales; qui sont toutes les "subdivisions necessaires au dict instrument.——

† In his first instrument after having supposed a quadrant to be divided into half degrees, he adds, "La seconde partie de l'in-"strument est une planche construicte en la forme & figure d'un "secteur de cercle, la circonference de laquelle comprendra juste-"ment un angle de trente & un demy degrées, non toutefois "formés ny descrits, mais bien divisé en trente parties esgales "seulement.—" p. 10.

‡ "Prima. Est accurata & geometrica divisio cujuslibet gra-"dus in 60 minuta, quæ super Quadrante 2 pedum, imò unius "pedis semidiametri commodè applicari potest; quam adinvenit "D. Joannes Ferrerius instrumentorum mathematicorum soler-"tissimus & accuratissimus fabrefactor, qui præsens aderat; ipsis "notissimus

which continued ſhall paſs through the center of the quadrant, and which he demonſtrates to anſwer to geometrical rigour *. But to ſave the trouble of drawing particular diagonals for every degree he adviſes one only to be affixed to the moveable index, which being divided into 60 equal parts, by its interſection with the ſtreight lines proceeding from the center and diſtinguiſhing the degrees on the limb of the inſtrument, will give the overplus minutes †. The other method is Vernier's ‡, which Morinus particularly explains, and ſhews, if the radius of the quadrant be half a foot, one foot, two feet, or three feet, by a ſmall arch divided into 30 equal parts anſwering to 31 of the diviſions of the limb, along which it moves either as a ſector, or as joined to the arm that carries the ſights, angles may be meaſured true to one minute, 30 ſeconds, 15 ſeconds, or 6 ſeconds reſpectively. This he juſtly prefers to the other method ||, and deſcribes it again in the continuation of his book, printed in 1636 §, and again

" notiſſimus Commiſſariis, quibus inventionem ſuam pridem " detexerat." Long. Scient. Par. 1. p. 17. This Ferrerius muſt have been then very old, if he was the ſame, that is praiſed for finding out a new method of drawing the hour-lines upon dials by Clavius, in the preface to his abovementioned book printed in 1586, in theſe words; " Inventor primus hujus ratio- " nis, quæ præclariſſima eſt, Hiſpanus quidam dicitur, nomine " Joannes Ferrerius, homo in primis acutus, & in rebus invenien- " dis admodum ſagax.

* Long. Scient. Part 2. pag. 51, &c.

† " Hæc enim particula ſemper adhærens alhidadæ & ſecta à " lineis quæ diſtinguunt gradus inſtrumenti, oſtendet in ſectione " minuta gradibus addenda." Ibid. p. 52, 53.

‡ " Secunda eſt accurata etiam diviſio 31 graduum ſive par- " tium Quadrantis in 30 partes æquales; inventa & in lucem " edita à Nobili viro Petro Vernerio in comitatu Burgundiæ " caſtelli Dornanſii Capitaneo; quâ cum Quadrante 2 pedum " quarta pars unius minuti ſenſibiliter menſuratur." Ibid. Par. 1. pag. 18.

|| Ibid. Par. 2. pag. 53.

§ Par. 6. pag. 187.

in

in a further continuation printed in 1639, where he recommends an azimuth quadrant of 5 feet radius, which by this artifice would meaſure angles to two ſeconds *; and laſtly, in his anſwer to Longomontanus, printed in 1641, he mentions both theſe methods †.

74. NOTWITHSTANDING all this, in 1643 one Benedictus Hedræus a Swede, publiſhed at Leyden a ſmall treatiſe, entitled Nova & Accurata Aſtrolabii geometrici Structura, &c. where in the preface he takes occaſion to deſcribe circular diagonals without mentioning Ferrerius, and aſſumes to himſelf Vernier's method. This in his book he applies to a Theodolite and Protractor, by dividing ſuch a part of a moveable circle as anſwers to 61 degrees of the limb of the inſtrument, into 60 equal parts ‡; he ſhews alſo how this moveable circle may be otherwiſe divided §, and likewiſe deſcribes a large aſtronomical quadrant, which by a ſmall moveable arch correſponding to 5 degrees 5 minutes of the limb of the inſtrument, each degree of which limb being divided into 12 equal parts, would meaſure angles to 5 ſeconds ‖, or even half that.

75. THIS method is alſo ſuccinctly explained by Tacquet in his Opera Mathematica ** printed in 1669, where he would aſcribe the original of it to a propoſition of Guido Ubaldi in his book of Aſtronomical Problems †† publiſhed after the author's

* Par. 9. Cap. 8.

† Coronis Aſtronomiæ, p 14.

‡ "Limbo ſic diviſo, numerentur ab una linearum, quas vocant fiduciæ, ut in delineatione AB, in quam partem placuerit, 61 gr. ab A uſque in C, & ſpatium illud in circulo mobili, quod hiſce 61 gr. reſpondet, dividatur in 60 partes æquales." Par. 1. Memb. 1. cap. 2.

§ Ibid. cap. 6.

‖ Par. ult. Memb. 1. cap. 2 & 3.

** Geom. Pract. lib. 1. cap. 4. pag. 23.

†† Lib. 1. Prob. 1.

death

death in 1609. But this was delivered very distinctly by Clavius in 1586 *, as the invention of Fabritio Mordente, the Emperor Rodulph's mathematician, who discovered it long before, as appears from a treatise of his brother printed at Antwerp in 1584, entitled Il Compasso del Signor Fabritio Mordente: con altri istromenti mathematici ritrovati da Gasparo suo fratello. Though this can at most be considered only as a very faint rudiment of these methods, being no more than a proposal to find the number of minutes, seconds, &c. of any arch less than a degree, by multiplying this arch 60 times, whereby the number of degrees, it measures, will express the number of minutes in the original arch; and if the multiple exceed an exact number of degrees, the excess multiplied in like manner will exhibit the seconds, whereby the arch given exceeds an exact number of minutes.

76. IN 1673 the famous Hevelius published the former part of his Machina Cœlestis, where he adopts this method with due applause; being the first that reduced it to practice in large astronomical instruments. He describes it from Hedræus, and appears not to have known, that it had been mentioned by other writers.

77. IN 1674, one Gerard à Gutschoven, Professor of the Mathematicks and Anatomy in the University of Louvain in a very small piece entitled Usus Quadrantis Geometrici printed at Brussels gives this method without taking notice of any preceding author. He applies it in the form of a sector † to a

* Fabrica & Us Instrum. &c. cap. ult. pag. 120.

† " *Cursor* est sector circuli mobilis, cujus partes sunt *arcus* " *minutorum*, arcus nempe in 30 partes æquales divisus, ipsos " Quadrantis gradus lambens." in præf.

ſmall quadrant, whereby minutes may be meaſured*. This quadrant, which he ſo much boaſts of, as not the production of his cloſet, but as brought into uſe in the fields and woods, has the rectilinear ſides of the quadrant projected into two circular arches to make room for the ſinical diviſion in the middle †, which had been deſcribed by others ‡.

78. Lastly, it is this method of Vernier, that Mr. Profeſſor Smith has alſo deſcribed, which is indeed preferable to any form of diagonals, though Dr. Hook in oppoſition to Hevelius labours to prove the contrary §. The doctor has indeed removed the objection againſt the abſolute exactneſs of rectilinear diagonals, when divided by equidiſtant concentric arches, by ſhewing how to divide them more truly by the help of a table of tangents ‖. But as his arguments

* "Quadrans totus diviſus eſt in 90 gradus; Curſor autem in "30 partes æquales qui comprehendit accurate 31 gradus qua-"drantis. Quiſque Curſoris gradus ſingulos Quadrantis gradus "excedit unâ trigeſimâ gradus, id eſt, duobus minutis primis, "unde fit ut ſequenti methodo non gradus tantum, ſed etiam "ſingula minuta prima à Regula fiduciæ abſciſſa numerare valea-"mus."——Prob. 1.

† "Uſui & praxi maximè appropriatum [inſtrumentum] mihi "excogitavi non in Muſæo vel Cathedra, ſed in ipſis agris & "ſilvis, in ipſa manuali operatione." In præf. "And after-"wards Habes Scalam altimetram ad arcum reductam variis uſi-"bus deſtinatam:—Tandem planum opplevimus lineis cancella-"tis, magnum in triangulis ſolvendis uſum habentibus; & hæc "etiam de penu meo deprompta ſunt."

‡ The Arabians, who introduced the uſe of ſines into trigonometry, ſeem to have been the authors of this device; ſee Catalog. MSS. Angl. p. 461. N°. 3309-6. But Petrus Apianus in his Introductio Geographica, printed at Ingolſtad in 1533, has given a figure of it with this inſcription on the limb, "Qua-"drans univerſalis a Petro Apiano jam recens inventus;" premiſing this character of it: "Quadrans novus quem univerſalem "ſeu generalem libuit appellare, eò quod quicquid in primo mo-"bili quæri, excogitari, aut proponi poteſt per hunc licet inve-"nire."

§ See his Animadverſions on Hevelius's Mach. Cœleſt.

‖ Ibid. pag. 22. and 23.

guments for their preference are very weak; so he in vain would alledge the authority of Tycho on his side: for it does not appear from Tycho's works, that this compendium was at all known to him. The citations * thence given by the Doctor, refer only to that little improvement upon Nonius mentioned above.

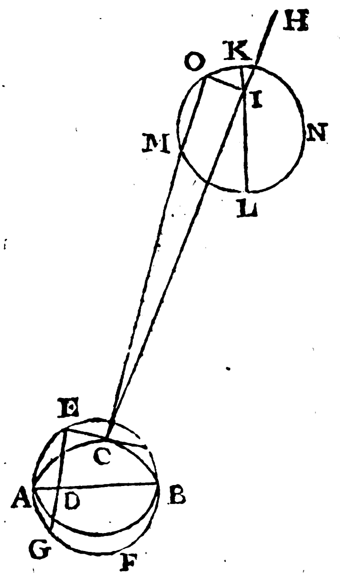
79. THE truth is, neither of these methods are of any use unless executed with exactness; and one great excellence of that of Vernier is its readily discovering the least error committed in the divisions. For which reason none but an able artist dare undertake it. But it is now performed very accurately in various instruments by our most skilful workmen, and its superior usefulness is sufficiently established by its having a place in the mural arch of our Royal Astronomer †; whom the same ardor still supports, which above 60 years ago carried him to St. Helena to settle the southern constellations; so that we may expect from his great abilities, and unwearied diligence, a series of observations upon the Moon's motion, that will far exceed, whatever has been produced since the very beginning of astronomy.

80. BUT to return to our author, whose fourth and last book promises a compleat history of the discoveries, that had been made in the heavens by the telescope, selected from a great variety of books, memoirs and observations of the best astronomers; which books are these, Galileo's Nuncius Sydereus the first on the subject, the Systema Saturnium of Huygens, Du Hamel's History of the Royal Academy of Sciences at Paris, the Memoirs of that Academy, our Philosophical Transactions, and a late treatise of Bianchini, entitled, Hesperi & Phosphori nova Phænomena. But as in relation to the late wonderful discovery of an apparent motion in the fixt stars, which is occasioned by the gradual

* Ibid. pag. 3, & 4. † Dr. Halley.

progrefs of light, wherein the worthy Savilian Profeffor of Aftronomy at Oxford, Mr. Bradley *, has fhewn equal diligence and fkill in obfervation in detecting the circumftances of the appearance, as fagacity in fearching out the caufe; fince our author has only dilated upon what Mr. Bradley has himfelf writ, I fhall conclude my remarks on this treatife with a fhort account of an improvement communicated in 1729, as foon as Mr. Bradley's difcovery was known, by the late great geometer Dr. Taylor to fome of his friends; whereby the particulars of this appearance may be exhibited according to the exact theory of the earth's motion.

81. LET ABC be the orbit of the earth, in which it moves from B to C and A round the fun in D. Then the earth being in C, if the tangent CE be drawn, and upon the tranfverfe axis AB as a diameter the circle AEBF be defcribed, ED being drawn and continued to G, DE is perpendicular to the tangent EC; and therefore the velocity of the earth in B is to the velocity in C reciprocally as DB to DE; that is, directly as AD to DG, fince the rectangles under ADB and EDG are equal.



Now CH being drawn from the earth in C toward

* Now Dr. Bradley, and the Royal Aftronomer at Greenwich, as well as Profeffor at Oxford.

the

the true place of any ſtar, take CI to AD as the velocity of light to the velocity of the earth in B. Through I draw KIL in a ſituation perpendicular to AB, and making KI equal to AD and IL equal to DB, upon the diameter KL deſcribe the circle KMLN parallel to the plane ACBF. Laſtly, make the angle under KIO equal to that under ADG, which is nearly the arithmetical mean between the mean and true diſtance of the earth from the aphelion, and draw CO. Now KL being in a ſituation perpendicular to AB and the angles under KIO, ADG equal, OI will be equal to DG, and be ſituated perpendicularly to GE, and therefore is parallel to CE or to the direction of the earth's motion in C; and is to KI as the velocity of the earth in C to its velocity in B. Conſequently ſince KI or AD is to CI as the velocity of the earth in B to the velocity of light, by equality OI is to IC as the velocity of the earth's motion in C to the velocity of light. Therefore the ſtar will be ſeen from the earth at C in the direction CO. Hence it follows, that every fixed ſtar is viewed from the earth as moving in a circle parallel to the plane of the ecliptic, the diameter of that circle which is parallel to the leſſer axis of the earth's orbit being divided by the true place of the ſtar in the ſame proportion, as the greater axis of the earth's orbit is divided by the ſun.

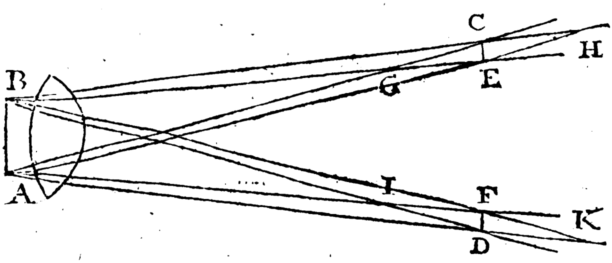
REMARKS ON Dr. JURIN's ESSAY UPON Distinct and Indistinct VISION.

1. TO these animadversions upon Mr. Professor Smith's compleat System of Opticks I shall add some observations upon the Essay subjoined by his friend and fellow-writer Dr. Jurin. This discourse consists of four general heads. The first relates to the difference in the appearance of an object when seen distinctly, and when confusedly; the second to that faculty in the eye, whereby it accommodates itself to the different distances of objects; the third to a particular phænomenon in indistinct vision, which is the multiplication of the object; and the fourth is taken up in considering the dazling, to which the eye is subject by the immediate contrast of light and darkness.

2. AS it appears, that this gentleman learnt the principles of opticks from his friend's book; so in his first head he has expatiated on the simple case of a uniform white object upon a black ground seen indistinctly more in detail, than one can suppose, a writer

writer would have done, to whom the subject was not new: And after all he has treated of it but imperfectly, proceeding on a supposition, that the light is uniformly spread through his circles of dissipation; whereas he confesses at art. 220. that the density of the light is different in different parts of these circles. Nay, he had so ill fixt the subject in his thoughts, that when he was put to such hard shifts how to account, that the astronomers without the assistance of telescopic sights should observe the diameter of the moon nearer to the truth, than he could have expected, he had certainly forgot, that the magnitude of his circle of dissipation depends upon the breadth of the pupil; for as those observations were made either through a small hole, or slit much narrower than the pupil, the dilatation of the moon's image from its indistinctness must have been proportionably lessened.

3. When he comes to consider at art. 90. an object but a little more complex, a black circular line seen upon a white ground, or two black parallel lines viewed in like manner, he has much mistook. It is a fact, that such a circle at one distance from the eye will appear to have a black spot in the middle, and the middle space between two parallel lines near together in the like situation will become dark; but



this is owing to a very different cause from what he has assigned: For if AB be the diameter of the pupil, and CE, FD the distinct images of the transverse ſections of the circumference of the circle, or of the parallels; and from the extremities of the pupil to the extremities of theſe images the rays AC, AE, AF, AD, and BC, BE, BF, BD be drawn, the ſpaces GCHE and IFKD will be deprived of light; but all without theſe ſpaces will be ſo much illuminated, that the axis of the eye can no where be deprived of light ſufficiently to produce ſo diſtinguiſhable a degree of darkneſs. Neither is the appearance truly deſcribed by our author, for the middle darkneſs in the parallel lines, and the central ſpot in the circle do not extend into a ſimple penumbra gradually vaniſhing, but that ſhade is compoſed of a number of different images, and appears uniform, only when thoſe images become ſo numerous, as not to be eaſily diſtinguiſhed. And the true cauſe of this appearance is the multiplication of the image found in objects ſeen indiſtinctly; which I apprehend to ariſe from ſome corrugation, or inequality of ſurface, to which that part in the eye, which is changed for the different diſtances of objects, is ſubject in its extreme tenſion either way, whereby the image of an object looked at out of the limits of diſtinct viſion is multiplied. From this multiplicity of images two parallels, as each is multiplied, at firſt are ſeen as four lines, or even more, but at length the two innermoſt unite into one; and in the circle by a more multifarious appearance a ſpot in the center is in one ſituation produced by the union of the different images in ſome one part.

4. MOREOVER the defence of Hevelius againſt the objections of Dr. Hook, which this gentleman is here pleaſed to bring in, ſerves only to ſhew, he did not underſtand the method of obſerving practiſed by that great aſtronomer. It is very well known, that Dr. Hook paſſed too ſevere a cenſure upon Hevelius's inſtruments, and his method of obſervation. But this mere imagination of an extraordinary

ordinary length of ſight, which Hevelius might acquire from age and long exerciſe is a very trifling defence. But the truth is, his method of obſervation required no ſuch length of ſight. This might be juſtly objected to the method of obſerving practiſed before Tycho Brahe; but that noble reſtorer of aſtronomy invented a form of ſights continued in uſe by Hevelius not chargeable with this inconvenience. The ſight next the eye is a plane furniſhed with two parallel chinks, both of which, not one only, as this gentleman * has repreſented, are made uſe of in every obſervation. The other ſight is either a plane likewiſe equal in breadth to the interval between the chinks, or a cylinder of that thickneſs. Now when theſe ſights were pointed towards any ſtar, a little before they came into the exact direction of the ſtar, the light of the ſtar would ſhine through one of the chinks, but be intercepted from the other by the interpoſition of the remoter ſight, and when the ſights had paſſed the direction of the ſtar, the chink before obſcured would now receive the light of the ſtar, and the other chink be covered from it. But in the intermediate ſituation, which is the very direction of the ſtar, the light of the ſtar would paſs through both chinks. Therefore it was the buſineſs of the obſerver to bring the ſights into ſuch a ſituation, wherein by looking through each of the chinks, one immediately after the other, his eye would receive the light of the ſtar through both. This method of obſervation therefore is little influenced by the larger or ſhorter ſight of the obſerver, but its accuracy depends upon the proportion of the breadth of the chinks to the diſtance between the ſights: and thoſe chinks were ſo framed, that by a ſcrew or ſome other like contrivance they might be equally dilated, or contracted together. In this manner of obſerving Dr. Hook's aſſertion is evi-

* Eſſay upon diſtinct and indiſtinct Viſion, art. 183.

dently

dently falſe, that a leſſer angle cannot be diſtinguiſhed better by a long radius than by a ſhort one *.

5. IN the ſecond part of this eſſay, after informing us, that he had deceived himſelf by certain trials to favour the ſingular opinion of Monſ. de la Hire, that the eye was not poſſeſſed of the faculty, he now admits of, he tells us, that Dr. Porterfield in his diſſertation lately publiſhed by experiments better contrived has cauſed him to change his mind, and ſubſcribe to the common opinion.

6. IN relation to the new ſcheme, he advances, concerning this change in the eye; before it is any further conſidered, I think, we muſt aſk the anatomiſts, whether a muſcle ſtrong enough to keep the eye tenſe can be inviſible.

7. IN his third head this gentleman has had the hardineſs to advance beyond the inſtructions of his maſter; and has adventured to expatiate upon the diſcoveries of Sir Iſaac Newton, concerning the fits of eaſy reflection and tranſmiſſion of light, where Mr. Profeſſor has choſe to be very conciſe. And as our compleat ſyſtem of opticks is upon this argument ſo very deficient, that theſe alternate fits of reflection and tranſmiſſion of light are not ſo much as mentioned by name, where he has undertaken to exhibit Sir Iſaac Newton's diſcoveries in optics †; before I come to conſider this writer's ſpeculations upon this point, I ſhall briefly give ſome account of this matter.

8. IT is well known, that when light falls upon any the moſt tranſparent ſubſtance, a part only enters the ſubſtance, ſome part being reflected at its ſuperficies. Again, when the light, which paſſes into the ſubſtance, is arrived at its farther ſuperficies, the whole is not tranſmitted through that ſuperficies, but there alſo a part is reflected back.

* Animadverſions on Machina Cœleſtis, pag. 7, & 8.

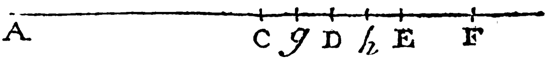
† Book 1. chap. 6, 7, 8.

When any colourless transparent substance is exposed to the open light of the day; what is reflected from its first superficies is always white, and so is the light returned from its farther surface, unless the substance be very thin; for in that case the light is coloured; and the colour differs according to the particular thickness of the substance. If such a thin transparent plate be unequally thick, the light reflected from its farther surface will be variegated with different colours. Such is the appearance seen in a bubble blown up with water rendered tenacious by the dissolution of a little soap. When this bubble is placed under a glass to protect it from being disturbed by the agitation of the air, the water, which composes it, will gradually and regularly descend from the upper to the lower part, rendering the bubble continually thinner and thinner till it breaks; the upper part also by this means will be always thinner than the lower. This bubble, when grown very thin, will be overspread with a great variety of colours, which Sir Isaac Newton has very particularly described. Likewise, if the object-glass of a very long telescope be laid upon a glass, that is flat, so as to touch it in one point, the small convexity of the telescope glass will leave between the two a thin plate of air of a considerable breadth perforated in the middle, where the glasses touch, and from thence gradually thickening. Here, where the glasses touch, and for some little distance round, the light is so freely transmitted, that no reflection is seen, but this dark spot will be encompassed round with a great variety of colours of the same kind with those seen upon the water-bubble. When the plate of air begins to be a little thick, these colours become of a very compound kind, and soon after end in perfect whiteness. In like manner the light reflected from the bubble of water at first, while its coat is thick, is uniformly white; and on the contrary, when it becomes extremely thin, a dark

spot

ſpot appears on the top by the reflective power being in that place nearly quite loſt.

9. The cauſe of theſe phænomena Sir Iſaac Newton found to be, as follows. The rays of light in their motion through the air, or any other tranſparent medium, are not alike diſpoſed in every part of their paſſage in regard to reflection and refraction; in ſo much that in ſome places, if they meet with another tranſparent ſubſtance, they ſhall enter it freely, ſuffering only a refraction at the ſurface upon its tranſmiſſion through into the tranſparent ſubſtance, but in other places the ſame ray ſhall be diſpoſed to be reflected back from any ſuch ſubſtance in its way. Theſe alternate diſpoſitions are found to return at equal intervals after this manner. Suppoſe AB to repreſent the path of a ray of light. Then if at the point C the ray be



diſpoſed to enter freely any tranſparent ſubſtance interpoſed in its way, but at D be diſpoſed to be reflected, from the ſurface of ſuch a ſubſtance; if DE be taken equal to CD, at E it ſhall again be in a diſpoſition freely to be tranſmitted through the ſuperficies of ſuch a ſurface, and if EF be taken of the ſame length with the former intervals, the ray at F ſhall be again in a diſpoſition to be reflected. And thus will theſe two diſpoſitions change and return at equal intervals throughout the whole progreſs of the ray within the ſame medium. But theſe alternate diſpoſitions are not to be underſtood, as confined to theſe preciſe points; but if theſe diſpoſitions are ſuppoſed to operate ſtrongeſt at theſe points, they muſt be conſidered, as decreaſing gradually from them both ways. Sir Iſaac Newton divides theſe intervals into two equal parts, as here CD is divided at *g*, and DE at *h*; and conſiders the whole fit of reflection to be extended between *g* and *h*, but ſtrongeſt at D, and

from

from thence gradually weakening toward *g* and *h*, yet with this caution that it ſhould not be conceived as preciſely limited at theſe points *g* and *h*, but to decay indefinitely.

10. BUT farther, as the rays of light differ one from another in colour, and refrangibility, inſomuch that there are rays of all degrees of refrangibility indefinitely from the leaſt to the greateſt; ſo the lengths of theſe fits of eaſy reflection and tranſmiſſion are as much varied, and every degree of refrangibility is accompanied with a peculiar length of theſe fits, the leaſt refrangible rays having their fits the longeſt, and the moſt refrangible rays the ſhorteſt within the ſame medium.

11. AGAIN in different mediums, and according to the different angle, under which the ray enters any medium, theſe fits are in the ſame rays different.

12. THE inequality in the length of theſe fits belonging to rays of different refrangibility and colour, is very great within the ſame medium, the intervals between the middle points of two neareſt fits either of reflection or refraction in the moſt refrangible rays being to that length in the leaſt refrangible as 63 to 100; whence it follows, that when the medium through which the light paſſes, is ſo thin a plate, that its whole depth contains but a ſmall number of theſe fits in any one colour, the light of ſome colours may at its farther ſurface be in a fit of eaſy reflection, and the light of other colours in a fit of eaſy tranſmiſſion, in which caſes the light reflected will be coloured. Sir Iſaac Newton exhibits for an inſtance of this the thickneſs of a plate, wherein the green light ſhall at the farther ſurface be in its third fit of eaſy reflection; there the yellow, and great part of the blue will in ſome degree be in a fit of eaſy reflection alſo; but no other part of the light: therefore the reflected light will be pure green compounded only with ſome yellow and blue.

13. However to produce this coloured appearance the plate must be very thin; for these intervals in every one of the colours are very small; in the least refrangible rays, when the light falls perpendicularly upon a plate of air, the interval between the middle of two successive fits of easy reflection, that is, DF in the preceding figure, is not the 78000th part of an inch, and in the most refrangible rays scarce the 125000th part. In water these fits are about $\frac{3}{4}$ only of this length, and in glass not quite $\frac{2}{3}$. If the plate of air were but the 800th part of an inch, whereby according to computation from Sir Isaac Newton's numbers, the least refrangible rays would arrive at the farther surface in the 99th fit of reflection, a ray at the confine of the red and orange will have 107 fits of reflection within the plate, a ray at the confine of orange and yellow will have 111 fits of reflection, a ray at the confine of yellow and green 119 of those fits, a ray at the confine of green and blue 129 fits, a ray at the confine of blue and the indico 139 fits, a ray at the confine of the indico and violet 145 fits, and lastly the most refrangible rays 156 of these fits. Hence many species of the rays of each denomination of colour will be found in fits of easy reflection at the farther surface of such a plate; all which together can compose nothing short of perfect whiteness in the reflected light, and consequently the light transmitted will be equally free from any tinge of colour. Nay so soon does the light lose all diversity of colour, that Sir Isaac Newton found it in his observations to become white, as soon as it had more than 8 or 9 fits in passing through a thin plate *.

14. Another circumstance is here still to be considered. While the transparent plates in Sir Isaac Newton's experiments were so thin as to exhibit rings of colours; the rings, where the plate was thinnest,

* Optics B. 2. part 1. observ. 12.

re-

reflected the greater quantity of light. The reason of this is evident from the method he has laid down for finding the colour reflected in each case, whereby it appears, that in the most luminous part of the rings, where the plate was thinnest, a greater proportion of the whole light was in a fit of reflection at the farther surface of the plate. But when any plate comes to be so thick, that the light of each denomination of colour has many parts reflected, and many parts transmitted, by reason that the whole contains many species, which shall have different numbers of fits of easy reflection or easy transmission; then the light reflected will not only be white, but the number of the species of light in fits of reflection will be so equal to the number of those in fits of transmission, that the proportion between the quantity of reflected and transmitted light will not be varied by any increase of the thickness of the plate.

15. For instance in the plate of air before mentioned the 800th part of an inch in depth the extreme red light in the middle of a fit of easy transmission at the first surface, would arrive at the farther surface in the middle of the 99th fit of easy reflection, and the extreme violet in the middle of a fit of easy transmission at the first surface, would arrive at the farther surface rather beyond the verge of the 156th fit of easy transmission; insomuch that, as there would be at least 57 different species of rays, which would arrive at the farther surface after a different number of fits of reflection, so there could not be reckoned above one less in fits of transmission. Were the plate the tenth part of an inch in thickness, the number of species arriving at the farther surface after different fits of reflection would be no less than 4629, and the number of species in fits of transmission cannot at most differ from this above one. Hence it comes to pass, that in all thick plates, and consequently in all the common appearances, the light reflected

reflected from every point of either surface of colourless transparent substances, whatever be the figure of their superficies, is not only always white, but also the same in quantity of light, the same proportion of the whole light arriving at each point of such surfaces in fits of easy reflection.

16. HENCE appears how absolutely this gentleman has erred in this whole matter, when, in order to account for the double, or more compounded appearance of objects seen indistinctly he imagined, that by reason of these alternate fits of easy reflection and transmission whenever a pencil of rays, from a distant object falls upon the cornea; that because some parcels of its rays will be transmitted into the eye, while other parcels are reflected back into the air, therefore, if the light were received upon a plane placed before the crystalline, it would consist of a middle circle surrounded with rings dark and luminous alternately.

17. NEITHER can any diversity be made, as this gentleman farther supposes, in the transmitted light at either surface of the crystalline; for the brightest part of the light in passing between the cornea and crystalline, which is the least of the two intervals, and that wherein the fits are longest, will have more than 12000 fits of reflection, or refraction.

18. BUT as little as this gentleman appears to have understood the subject, he had far other thoughts; for he has not contented himself with applying this curious doctrine, as he terms it, to vision, but even takes upon him to correct and improve upon Sir Isaac Newton by making the fits of reflection of different extent from the fits of transmission in a manner contrary to what Sir Isaac Newton has represented. From what we said above, it appears, that in common half the light incident upon any surface is in a fit of transmission, and the other half in a fit of reflection. Hence arises this obvious question, Why is

is not half the light reflected at all surfaces? But in regard to this it must be considered, that Sir Isaac Newton has pursued his speculations upon this subject no farther, than what he could determine by indubitable experiments and observations, and he has found, that at all surfaces, where the light is acted upon by bodies, as a part is transmitted into pellucid substances, so a part is reflected; and he likewise finds, that as the transmitted light is diverted from its straight course by a refractive power, where that power is strongest, the greatest quantity of light is reflected. From this and other considerations he concludes, that it is one and the same power, by which the light is refracted and reflected; and that the light, which is reflected, is only such, as is in a fit of easy reflection, when it arrives at the surface; and as these fits in the passage of the ray to the transparent substance are alternately changed for fits of transmission, and return at certain intervals; so each fit likewise continues for a certain space, and that the intervals of these returns, and the space, through which each continues, depend altogether upon the medium, through which the light passes, and not upon that, whereon the light falls. But as Sir Isaac Newton had not discovered, wherein this power in bodies of refracting and reflecting light expresly consists; so he is silent in relation to the cause, why more light is reflected from some bodies than from others.

19. Now this gentleman imagines this defect in Sir Isaac Newton may be supplied by correcting him in regard to the extent of each fit of reflection, and supposing that to be varied according to the medium, upon which the light falls; for instance, when the light passing through air upon glass, or through glass upon the air, the extent of the fit of easy reflection should not be what Sir Isaac Newton supposes, half the interval between the middle of the nearest fits of

 reflection,

reflection, but only $\frac{11}{100}$ of that interval, and at surfaces between air and water $\frac{6}{100}$ of that interval. But this supposition is absolutely inconsistent with the very experiments, from which Sir Isaac Newton deduces his whole doctrine. If the extent of these fits varied thus, not only the quantity of light reflected from a transparent surface would be varied by changing the contiguous medium, but also the colour, which, Sir Isaac Newton says, he could never observe. The colours, which arose successively upon the bubble of water, Sir Isaac Newton has described very distinctly with all the concomitant circumstances; but were it examined according to the new regulation of our author, what colours ought to have appeared, we shall find, that from the yellow, which Sir Isaac Newton ranges in the 5th order of colours, to the total blackness, each colour ought to emerge almost uncompounded, being, where the least simple, scarcely mixed with the contiguous colours of the same order, and never with those of any other. In particular the purple in the 5th order, which, Sir Isaac Newton says, was much inclined to red, must have been a pure violet without any mixture, and a dark interval have been seen between that and the red of the next order; even in the 7th order, where they are most compounded, no thickness of the bubble can be assigned, at which more than three colours will be reflected; insomuch that in no part of this 7th order of colours any whiteness could appear: nay, were the extent of the fit of easy reflection any thing less than what, Sir Isaac Newton supposes, a sensible interval void of reflection must have appeared between this and the order preceding. This shews, that the proportion assigned by Sir Isaac Newton between the extent of each fit of easy reflection and the interval of the fits was the result of mature

mature deliberation, and neceſſary towards producing the appearances.

20. HENCE it appears, that whether more or leſs light be reflected from a body, the extent of the fits of reflection in the incident rays is the ſame, and the difference in the quantity of reflected light is owing to the greater or leſs portion of that part of the light, which arrives at the body in a fit of eaſy reflection, being thrown back by the reflective power of the body. The cauſe this gentleman has been pleaſed to aſſign for this, is ſo abſolutely inconſiſtent with the eſſential principles of Sir Iſaac Newton's doctrine, that the thought could never have been entertained by one of the leaſt degree of ſkill in the ſubject. But this gentleman ſeems ſo great a ſtranger to the matter, he has writ upon, that he does not appear in the leaſt to have known, that rays of different colours have their fits of reflection and tranſmiſſion of different lengths. For from this it follows, that, when a thin tranſparent plate, whether the bubble of water in Sir Iſaac Newton's experiments, or the plate of air between the flat and convex glaſs, is expoſed to the open light of the day, ſcarce any part of it is exempted from reflecting light, except the central ſpot only, but the reſt appears ſpread over with different colours contiguous to one another. But had this gentleman been at all apprized of this, he ſhould at leaſt have ſuſpected, that if the phænomenon, he is here endeavouring to account for, depended upon the cauſe, he aſſigns for it, inſtead of a multitude of ſeparate images a broad one ſhould have preſented itſelf variegated with colours.

21. AFTER it has been thus made appear, how totally unſkilled this gentleman is, in this doctrine relating to the fits or diſpoſitions of eaſy reflection and of eaſy tranſmiſſion accompanying the rays of light; I know not, whether it may be thought worth

while to take notice, how little this gentleman's erroneous opinions concerning it will ſerve his purpoſe. By computing on ſuppoſition that rays of all colours have the ſame intervals between their fits, and the extent of each fit what this gentleman aſſigns, the magnitude and number of the rings, that are formed at the cornea, when the rays are incident in ſuch a manner as to make the rings the largeſt, will be above 670, if the aperture of the pupil be ſuppoſed $\frac{1}{10}$ of an inch; and if the magnitude of the firſt or largeſt dark ring be deſired, it will be found, that, if the rays falling on the middle of the cornea be in a fit of eaſy tranſmiſſion, the breadth of the firſt dark or reflected ring will be about the $\frac{1}{500}$ of the whole pupil; but if the rays falling on the middle of the cornea, be in a fit of eaſy reflection, then the breadth of the firſt dark ring will be but about the $\frac{1}{660}$ part of the whole pupil; if we conceive now the viſion to be ſo indiſtinct, that a luminous point appears under an angle of 20 minutes (a degree of indiſtinctneſs five times greater than that, under which he ſuppoſes theſe rings to be perceptible) the breadth of the firſt or broadeſt dark ring cannot, under all theſe favourable circumſtances, ſubtend a greater angle at the eye than $2\frac{2}{5}$ ſeconds; and conſequently none of theſe rings could, even on his own repreſentation of this doctrine, be ever ſenſible. It is evident then, that, when our author, to account for the double appearance of objects ſeen by indiſtinct viſion, ſuppoſes the viſibility of theſe rings, it was not only neceſſary, that he ſhould utterly miſapprehend Sir Iſaac Newton's doctrine; but that he ſhould alſo be ignorant of the moſt obvious conſequences of thoſe abſurd conceptions, he had formed to himſelf on this ſubject.

22. IN the laſt part of this eſſay our author has diſcourſed very diffuſely upon the dazzling, to which the eye is ſubject from the immediate contraſt of light and darkneſs, and tells us, he ſhould not have been ſo particular, had he not ſuſpected, that Sir Iſaac Newton was once led into a miſtake by an appearance ſimilar to thoſe, he here treats of; at leaſt, ſays this gentleman, I do not find, he has any where accounted for the extraordinary ſtrength of the ring of light next the central black ſpot ſpoken of in Obſervation 23, Part 1. of his ſecond book of Opticks; nor do I ſee any way of accounting for it, but from ſome ſuch conſideration.

23. Here it is ſuppoſed, firſt, that Sir Iſaac Newton, in attributing any ſuperior degree of brightneſs to this ring, was deceived by the neighbourhood of the central black ſpot; ſecondly, that Sir Iſaac Newton has no where accounted for this extraordinary brightneſs; and laſtly, that no account can be given of it, but what our author here aſſigns.

24. If this were indeed a miſtake of Sir Iſaac Newton, it muſt be owned a very ſignal one; ſince he ſo little ſuſpected this appearance to be owing to any ſuch incidental cauſe, that when he is ranging the colours of bodies under the ſeveral rings produced in theſe experiments, he places the light of white metals under the claſs of this ring, on account of their ſuperior brightneſs. But it is further evident, that he was not impoſed upon by any ſuch deception, from the method he took to ſatisfy himſelf of the extraordinary brightneſs of the light of this ring, by comparing two bubbles of water blown at diſtant times; in the firſt of which the whiteneſs appeared, which ſucceeded the colours, and in the other the whiteneſs, which preceded them all *; and before † he tells us, this whiteneſs, which ſucceeded the colours, would often ſpread and dilate itſelf over

* Opticks, pag. 196. † Ibid. pag. 190.

the greater part of the bubble, before that even the ſucceeding blue emerged on the top; nay, would decay gradually, and quite vaniſh, before the black ſpot appeared.

25. AGAIN, though Sir Iſaac Newton has not ſeparately diſcuſſed the cauſe of this particular appearance, yet it is ſo unneceſſary to have recourſe to this gentleman's chimerical imagination concerning it, that it is a moſt obvious conſequence from the method, Sir Iſaac Newton gives for inveſtigating the ſeveral colours exhibited by thin plates. For it thence appears, that at this thickneſs of the plate no part of the light, which enters it, arrives at its farther ſurface in a fit of eaſy tranſmiſſion, which never happens in any greater thickneſs.

26. THUS then has this learned gentleman unhappily attempted the application and improvement of a doctrine, of which, it appears, he was not qualified to deſcribe even the very rudiments.

27. FOR the firſt principle laid down by Sir Iſaac Newton, in the ſecond part of his ſecond book of Opticks, is the inequality of the length of the fits of tranſmiſſion and reflection in rays of different colours, and it is only from the combinations of theſe different lengths in plates of a different thickneſs, that all the appearances deſcribed in the firſt part are deduced.

28. BUT the deſcription and application of this doctrine, as given us by our author *, do neceſſarily ſuppoſe the lengths of theſe fits to be the ſame in rays however different in colour.

29. AGAIN, Sir Iſaac Newton in the ſame ray makes the fit of reflection equal in length to the fit of tranſmiſſion, the experiments in the firſt part neceſſarily requiring it.

30. ON the contrary Dr. Jurin tells us, that in water the fit of reflection is in length but $\frac{6}{94}$ of the

* Art. 209, 210, 211, &c.

fit of transmission; an assertion that absolutely destroys the whole theory established by Sir Isaac Newton, setting it at variance with almost every appearance, it is intended to solve.

31. LASTLY, The immediate consequence of Sir Isaac Newton's theory, as described in the second book of Opticks, is, that the light of the first ring surrounding the central spot should be much stronger than that of the succeeding rings, all the rays being there in a fit of reflection.

32. YET so little was our author versed in this doctrine, that he conceives the remarkable strength in the light of this ring to be only an illusion, suspects Sir Isaac Newton to have been mistaken in his description of it, and tells us, that he does not see, that its reality can be any way accounted for.

*** *THUS Mr.* Robins *shewed the fallacy of what had been advanced in the Essay upon distinct and indistinct vision. But its author was not a man to be convinced even by demonstration; for he published a Reply, whereof Mr.* Robins *soon printed a Full Confutation; there exposing also the many errors he had further committed. But the above Remarks are abundantly sufficient to prevent any intelligent person from being imposed on by Dr.* Jurin's *Essay, or by whatever may be urged in its defence. For the Doctor laboured in vain to account for an appearance, that depended solely on mixt and heterogeneous light, from a property of the most pure and unmixt light, such as exists no where. I shall therefore only give two paragraphs from Mr.* Robins's *last Tract, as they contain a certain proof of a particular, he had delivered merely as a conjecture.*

" To spare this gentleman (Dr. Jurin) the
" labour of framing a new hypothesis, and be-
" wildering himself further in this subject, I will
" inform him of some particulars relating to the
" multiple appearance of objects seen indistinctly,
" that will not only evince, that the fits of trans-

"mission and reflection have nothing to do in this
"affair, but will also prove, that the cause, I had
"hinted at in my Remarks *, is undoubtedly the
"genuine one.

"It is true, that a narrow line of light, such
"as the interval between the edges of a parallel
"ruler brought very near together, will appear
"bordered with other lines of light, when held
"either too near or too remote for distinct vision.
"If Dr. Jurin's assignment of the cause of this
"appearance can at all take place, the stars ought
"not to appear radiated, but inviron'd with con-
"centric circles of light; as the forementioned line
"of light is bordered with parallel lines. A very
"small pin-hole in a piece of paper being held up
"against the light, first at a distance from the eye,
"wherein it may be seen distinctly; and then, while
"the eye is directed to it, drawn gradually nearer;
"this hole, as soon as it ceases to be seen distinctly,
"will also appear radiated like a star, not incompassed
"with luminous circles. If the appearances in this
"experiment are duly attended to, they will account
"for the lines of light in the other observation: for
"these rays will most of them, if not all, have a lucid
"speck in them; so that if a slit were cut in the pa-
"per, through the pin-hole, of the breadth of the
"hole, each of these specks will become a line of
"light parallel to the slit. But circles of light not
"appearing about the pin-hole in this experiment,
"shews plainly, that no such luminous circles are
"formed in the bottom of the eye, by rays issuing
"from a luminous point, either beyond, or within,
"the limits of distinct vision †."

* Remarks on Dr. Jurin, §. 3. p. 280. l. 16.

† Robins's Full Confutation of Dr. Jurin's Reply, pag. penult.

APPEN-

APPENDIX

BY THE

PUBLISHER.

AS my friend, besides the abusive treatment, which he received from Philalethes Cantabrigiensis, has not escaped censures from other quarters; I here purpose to vindicate his memory from those aspersions. This will lead me to obviate some late insinuations against Sir Isaac Newton himself. And I shall farther make a few observations on that great man's early writings; having been enabled from papers, that have fallen into my hands, to determine more fully, than has been done, not only the order, in which the several tracts, he had from time to time composed, were writ; but also, a point which I conceive to be of much greater consequence, by what steps, he gradually corrected the crude ideas, he had received of indivisibles; till he perfected the doctrine, which Mr. Robins has distinguished himself by explaining.

Dr. Robert Simson, the learned professor of the mathematicks in the university of Glasgow (whom I must always honour for his zeal towards reviving a just taste in geometrical subjects) at the end of the second edition of his Conick-Sections, printed at Edinburg in 1750, after having given the solution of a celebrated problem of Alhazen, has surprized me by the following insinuation against Mr. Robins's candour, "Solutionem hanc, tam analysin quam "compositionem, diu ante annum 1729 inveni; ex "eo autem anno discipulis eam quotannis prælegi, "iisque etiam scripto communicavi. Anno autem "1739 vir clarissimus Benjaminus Robins, in libello "cui titulus, Remarks upon Mr. Euler's Treatise "of Motion, p. 75, 76, &c. * compositionem a "præcedente non multum abludentem publicavit. "Num eam ipse invenerat, aut aliunde acceperat, "non dicit vir doctissimus. Verisimile autem "est eum analysin hujusmodi problematis, ope "cujus ad compositionem retrogradaretur, non ha-"buisse; dicit enim problema in multis casibus "magis complicatum esse, quam ut solutionem "simplicem et elegantem accipiat. Præcedens "vero analysis, sive solutio æque simplex est, ac "compositio ex ea derivata, eamque excogitave-"ram diu antequam Slusii constructionem vide-"ram; et in compositione analysin ita presse secu-"tus sum, ut non observaveram rectam LM "parallelam esse junctæ BC, ante visam Slusii con-"structionem. Hæc autem monenda duxi, ne "quis putet me tantum analysin præcedentem Slusii "constructioni aptasse."

Here Dr. Simson has in the first place wholly mistaken Mr. Robins in regard to the problem, which he speaks of as too complex in most cases to admit of a simple and elegant solution; for Mr. Robins does not say this of Alhazen's problem, which he had not yet mentioned, but has reference to the proposition of Dr. Smith there animadverted

* Remarks on Dr. Smith, §. 64.

on.

on, in its full extent; and Dr. Smith's propoſition is this: "When a large picture, or a plane object "of any given ſhape, ſtands perpendicularly to the "common axis of any number of refracting or re-"flecting ſurfaces, its apparent ſhape, ſituation, "magnitude and diſtance from the eye, at any "point of the axis, may be found as follows *." And, Mr. Robins charges the explanation of this propoſition with being defective, becauſe there is no notice taken, that "the place of the image "cannot be determined, as here required, till it "be ſhewn, how to find the line, in which it is "ſeen; that is, how to aſſign the reflected or "refracted ray, which ſhall enter the eye †." Though, to avoid the cenſure of unreaſonable captiouſneſs, he acknowledges, that "the pro-"blem, thus generally propoſed, is, in moſt caſes, "too complex to admit of a ſimple and elegant "ſolution;" but, however, thinks this no juſt ex-cuſe for Dr. Smith's paſſing over in ſilence, what ſolutions had been given of particular caſes; and ſpecifies two, this problem of Alhazen, and another concerning refraction given by Dr. Barrow in his Optical Lectures.

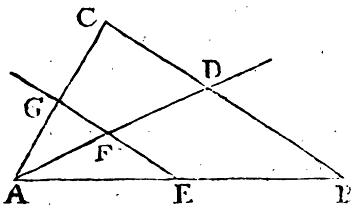
Again, with reſpect to Alhazen's problem in particular, I am morally ſure, Mr. Robins was wholly unapprized of Dr. Simſon's having ever had it under conſideration. For whenever Mr. Robins was in London, there was ſcarce a day paſſed, wherein I was not in his company; and he could not have failed mentioning to me a thing of this kind. The demonſtration publiſhed in his Remarks, had been communicated to him by me from the papers of my friend Dr. Pemberton ‡, who had con-

* Compleat Syſtem of Opticks, book 2. chap. xx. prop. 1.
† Remarks on Dr. Smith, §. 64.
‡ This the Doctor permitted Mr. Robins to publiſh, but not to make uſe of his name.

ſidered this problem above 40 years ago, when we were fellow ſtudents at Paris, upon the following occaſion.

The treatiſe of Apollonius De Sectione rationis having then lately fallen into his hands, he was greatly ſtruck with the elegance of the ancient method of ſeeking the ſolution of problems, delivered there more fully and explicitly, than in any of their works, which had before been known to the modern world; though ſeveral ſpecimens of their methods, ſcattered about in thoſe writings, had enabled ſome to diſcover in part this ancient form of analyſis. My friend's admiration of this method, induced him frequently to apply it to the problems, he met with ſolved by the calculations of algebra. And in particular, turning over Sir Iſaac Newton's treatiſe on Algebra, with the intention of conſidering his problems in this light, where, among others, this occurred. To find the locus of the vertex of a triangle, wherein the angles at the baſe ſhould have a given difference; he applied to this the ancient method of analyſis in the following manner.

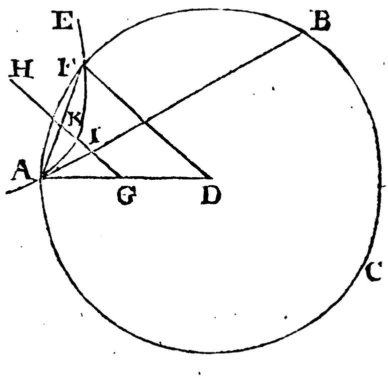
Suppose, in the triangle ACB, the angle CAB exceeds the other angle at the baſe CBA, by the given angle BAD.



Then the angle CAD will be equal to CBA, and the triangles CAD, CBA ſimilar; whence BC is to CA as AC to CD, and the rectangle under BCD equal to the ſquare of AC. Hence if AB be biſected by EFG parallel to BC, the rectangle under EGF will be equal to the ſquare of AG, or of GC. Conſequently the point C will be in an equilateral hyperbola, in which EFG will be a diameter, E the center, CA ordinately applied to that diameter, and AF a tangent to the hyperbola in in the point A. Therefore the given line AB will alſo be a diameter, and the line AF, given in poſition, the tangent of the hyperbola at the vertex A; ſo that the point C is in an equilateral hyperbola given in poſition.

THIS very analyſis was afterwards applied by Dr. Pemberton for aſſigning the vertex of a triangle, whoſe angles at the baſe differ by an angle given, in the ſecond of two problems concerning the rainbow, publiſhed by him in the Philoſophical Tranſactions, N° 375, for the months of January and February 1723 *. Where it is likewiſe noted, that if a line is drawn from the vertex of this triangle, here the point C, ſo as to be ordinately applied to the diameter AB of the hyperbola, which is alſo the baſe of the triangle, this line will make an angle with AC equal to the angle ABC †. But this is the principle, whereon the demonſtration, publiſhed by Mr. Robins of Alhazen's problem, is founded.

BUT this analyſis of Sir Iſaac Newton's problem, immediately led my friend to the following method of triſecting a circular arch.



LET the arch to be triſected be AB. In the circle ABC, draw the ſemidiameter AD, and to AD, as a diameter, and to the vertex A, draw the equilateral hyperbola AE, to which the right line AB, the chord of the arch to be

* Viz. the triangle CML in the figure of page 253.

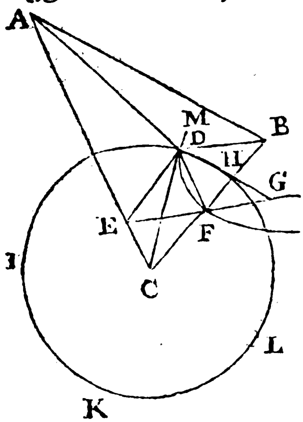
† Viz. the line LT in the figure of page 255 ſhewn to be ordinately applied in the hyperbola to the diameter MC, and the angle SCL to be equal to the angle TLM.

triſected,

trisected, shall be a tangent in the point A; then the arch AF, included within this hyperbola, is one third of the arch AB.

Draw the chord of the arch AF, divide AD into two equal parts at G, so that G be the center of the hyperbola, join DF, and draw GH parallel to DF, cutting the chords AB, AF in I and K; then the hyperbola being equilateral, every diameter is equal to its latus rectum, and the rectangle under GKI will be equal to the square of AK, that is, GK will be to KA as AK to KI, the triangles GKA, AKI similar, and the angle KAI equal to AGK, which equals the angle ADF. Now the angle ADF, at the center of the circle, being equal to KAI at the circumference; the arch AF will be equal to half FB, and therefore equal to one third of AB.

At the same time, his determination of this locus suggested to him, how easily it might be applied to Alhazen's problem: since, if from the point D (I refer to Mr. Robins's figure) right lines were drawn to the points E and F, assigned according to Huygens and Slusius, the angles CED, CFD would, as the Marquis de l'Hôpital had observed *, be equal, whereby EF being joined, the angles DEF, DFE would have the same difference, as the angles CEF and CFE in the given triangle ECF.



And now I hope, from what has been said, Dr. Simson will no longer retain any suspicion, that Mr. Robins could be guilty of an act of meanness, whereof (if I may be allowed to know

* Sections Coniques, Liv. x. Exempl. vii.

the

the temper and disposition of one, with whom I conversed so intimately for many years) I may pronounce him utterly incapable in any occurrence of life whatever.

But I must beg leave further to add, that though Dr. Simson's analysis leads very directly to the demonstration published by Mr. Robins; yet, if exhibited as an adequate solution of the problem, it is incompleat. We see by the treatise above mentioned of Apollonius, that the custom of the ancients, in their analysis, was to inquire into all the various cases of a problem, and the limitations, to which the solutions in each were subject; and Dr. Simson has justly observed, that "sine determinationibus, sive διορισμοῖς, problema rite solutum minime habendum est *." But here Dr. Simson has imitated Huygens and Slusius in attending only to the general solution of this problem, as Dr. Barrow had done before in his Optical Lectures; whereas Alhazen has taken into consideration the several cases, though in some measure unsuccessfully.

The problem admits of solution only within the angle ACB, and in the angle vertical to it, comprehended by the lines AC, BC continued backwards †. Therefore, when both the points are given without the speculum (though the hyperbolas cut the circle in four points) they exhibit but two solutions only; one on the convexity, where the hyperbola, whose vertex in Mr. Robins's figure is F, passes through the surface of the speculum within the angle ACB; and another in the concave part, where the opposite section to this hyperbola, that whose vertex is E, passes through the speculum within the angle vertical to that under ACB.

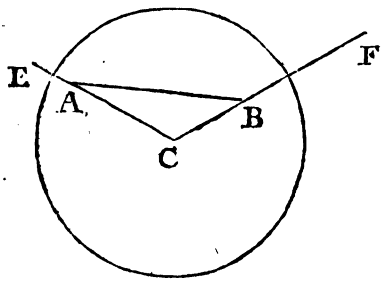
But when one of the points, and more especially, when both are given within the speculum,

* Preface to his Conics, p. vii. † Alhazen, lib. 5. prop. 66.

the

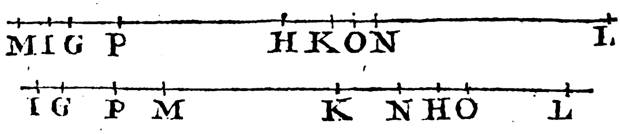
the problem is ſubject to a greater variety of ſolutions.

In relation to the hyperbola, whoſe vertex is E, as it paſſes through the center of the ſpeculum, it muſt always exhibit one ſolution within the angle vertical to ACB; but when the point A ſhall be within the ſpeculum, as in the annexed ſcheme, the point E ſhall be without, and this hyperbola ſhall enter the ſpeculum within the angle ACB, and exhibit another ſolution of the problem within that angle.



In the next place, in regard to the hyperbola, whoſe vertex is F, when B is within the ſpeculum, F is without. And here it is evident, that if the angle ABC is either obtuſe, or right; the hyperbola, whoſe vertex is F, cannot meet the ſpeculum within the angle ACB, whether the point A be within or without the ſpeculum; ſince the tangent of this hyperbola in the vertex F, being parallel to AB, muſt paſs on this ſide wholly without the ſpeculum.

But when the angle ABC is acute, if GH be a third proportional to BC and the ſemidiameter of the ſpeculum, and GI being taken to IH in the duplicate ratio of the difference between AC and



CB to their ſum, IK be taken equal to AC, and prolonged to L, that the ratio of IK, that is AC, to KL be the duplicate of four times the rectangle under

under AC, CB to thrice the rectangle under their ſum and difference; and if GM, equal to HK, be added to HG, when IK exceeds IH, otherwiſe taken from it, and to IK be added KN, a third proportional to thrice ML and KL, and NO, which with ML ſhall make a rectangle equal to that under GKH, being taken from IN, when IK exceeds IH, otherwiſe added to it; then if OP, taken from O towards G, ſhall be to LP, as the ſquare of LP to thrice the rectangle under KLM, GP will be the verſed ſine, to the diameter GH, of half the angle ACB, when the hyperbola, whoſe vertex is F, touches the ſpeculum.

But from this analogy LP, and conſequently GP, may be eaſily found by the methods too well known to make any particular explanation here neceſſary.

Only it muſt be obſerved, that when two points are found by this analogy within GH, that neareſt to G gives the verſed ſine, which determines the angle ACB.

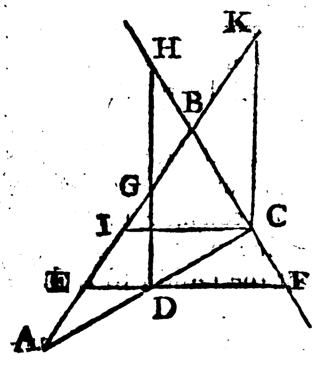
If AC is leſs than IG, or if the angle ACB be leſs than this now found, the hyperbola, whoſe vertex is F, will not reach the ſpeculum within the angle ACB; but if greater, this hyperbola will paſs through the ſpeculum, and exhibit two ſolutions of the problem.

On the other hand, if the angle ACB is given, with the two diſtances AC, CB; the ſemidiameter of the ſpeculum, wherein the hyperbola, whoſe vertex is F, will touch it, may be found thus. Find an angle, the cube of whoſe tangent, applied to the ſquare of the radius, ſhall be to the tangent of half the angle given, as the difference of AC from CB to their ſum; then the rectangle under this ſemidiameter of the ſpeculum, and the exceſs of AC above CB, will be to the rectangle under AC and CB, as the cube of the ſine of the angle now found, applied to the ſquare of the radius, to the ſine of half the angle given.

Moreover, if the angle ACB and the ſemidiameter of the ſpeculum were given, together with one of the diſtances AC or CB; the other may be aſſigned, wherein the hyperbola, whoſe vertex is F, ſhall exhibit a ſingle ſolution by touching the ſpeculum.

To ſet down at length the inveſtigation of all theſe particulars, would exhibit an extenſive example of applying the ancient analyſis to the determining the limits of the higher order of problems; of which Dr. Pemberton has long ago given a very diſtinct ſpecimen, in the two problems concerning the rainbow above referred to. But as this would carry me too far from my principal deſign; let it ſuffice here to obſerve upon the whole, that as the point A may be either without the ſpeculum, within, or on the ſurface, while the point B is within; when B and A are both within the ſpeculum, the problem may admit of two, three, or four ſolutions; for two will be given by the hyperbola, whoſe vertex is E; but when the point A is in the ſurface of the ſpeculum, or without, this hyperbola will exhibit but one ſolution, and the problem may have that ſingle ſolution only, or by the concurrence of the other hyperbola have, in the whole, three ſolutions, or perhaps only two.

And thus I ſhall take leave of this problem, after adding the two following ſhort remarks.



In the firſt place, Huygens's conſtruction of this problem will admit of a conciſe demonſtration by the following propoſition.

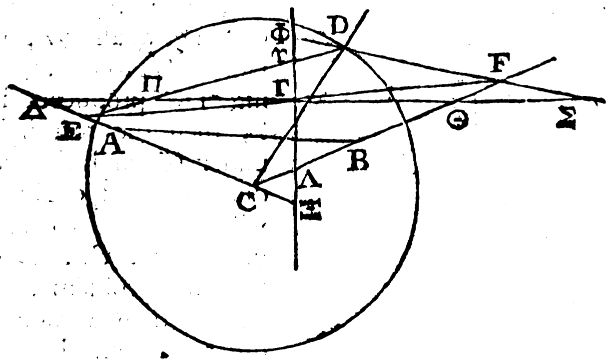
In any triangle ABC, if the baſe AC is biſected in D, and EDF, DGH be drawn, each making equal angles with the legs AB, CB, the

the four segments AE, CF, BG, BH will be all equal.

For CI being drawn parallel to EF, BI will be equal to BC and the difference between AB and BC, and AE, EI each equal to half that difference, being equal to each other, since AD is equal to DC: but CF is equal to EI; therefore AE and CF are equal.

In like manner if CK is drawn parallel to DH, BK is equal to BC, and BG half the difference of AB, BK, being also equal to BH, as the angles BGH and BHG are equal; therefore GB and BH are each equal to AE and CF.

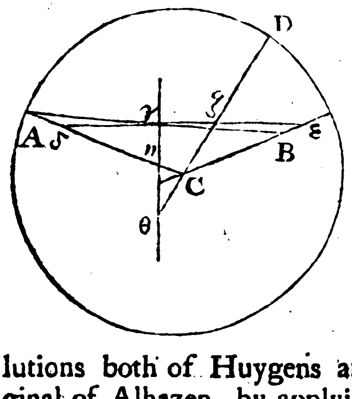
Now Huygens's construction is this. The line EF being bisected in Γ, and the lines ΔΓΘ, ΓΛΞ



drawn to make each of them equal angles with EC, CF, the opposite hyperbolas having these lines for asymptotes, and one of them passing through the center C, will be intersected by the surface of the speculum in the points, wherein the reflection sought should be made. Here if D be the point, where the reflection is made, the angles CED, and CFD are equal; and EF being bisected in Υ, and ΔΓΘ, ΓΛΞ making each equal angles with CE and CF, if ΔΘ intersect ED in Π, and DF produced in Σ, also ΓΛ intersecting ED in Υ, and FD in Φ, in the triangles ΔΕΠ, ΘΦΣ, the angles ΔΕΠ, ΘΦΣ are equal, also the

the angles ΕΔΠ, ΘΣF are equal; consequently the angles ΔΠΕ, ΘΣF are equal, and ΠΣ drawn through Γ, wherein EF, being the base of the triangle EDF, is bisected, makes equal angles with the legs ED, DF. In like manner, the angles ΕΞΥ, FΛΦ being equal, the angles ΕΥΓ, ΓΦF are equal; whence, by the preceding proposition, ΕΠ, ΥD, ΦD, FΣ are all equal: therefore the points E, D, and F are in opposite sections passing through E and F, having for asymptotes ΔΘ, ΓΛ: and in the triangle ECF, ΕΔ being equal to CΞ, the hyperbola through E passes also through C.

I shall also further add, that if AB be bisected in γ, and $\delta\epsilon$ be drawn making equal angles with AC, CB, and intersecting CD in ζ, as also $\gamma\eta$ perpendicular to $\delta\epsilon$, meeting DC produced in θ; then the rectangle under $\theta\zeta$, and the radius of the speculum, will be equal to that under AC, CB. And thus the problem may be solved in a manner different from the solutions both of Huygens and Slusius, or the original of Alhazen, by applying a line, as $\theta\zeta$, which shall be to one of the lines AC, CB, as the other to the radius of the speculum, through the point C, within the lines $\delta\epsilon$, $\gamma\theta$, given in position.



I shall now proceed to defend my friend from reflections, that come from another quarter.

Though Mr. Robins, in his Tracts on fluxions and prime and ultimate ratios, had given a very just and clear account of their nature and use, conformable to the sense of their great inventor; yet, it seems, it was so different from the notions, many had enter-

tained

tained of these speculations, that divers objections were made to what he had published. Amongst other things it was pretended, that the way of demonstrating followed by the ancient geometers, so much commended by him, was tedious and perplexed, if not inconclusive; that he had done ill in distinguishing the method of fluxions, from that of prime and ultimate ratios *; that he had been presumptuous in saying Sir Isaac Newton was too brief in his explications †; that he had explained wrong Sir Isaac Newton's expressions of the prime and ultimate ratios of the quantitates nascentes and evanescentes, by having maintained, that those prime and ultimate ratios were only the limits of all the different ratios, these variable finite quantities bore to one another, while they were rising or vanishing together; and hence that he had misrepresented the first Lemma of the Principia, by endeavouring to shew, that its demonstration did not prove the actual coincidence of the inscribed and circumscribed rectilinear figures with their respective curvilinear limits, and by asserting that Sir Isaac Newton never intended such a coincidence; and lastly, that Mr. Robins had quite perverted Sir Isaac Newton's description of the momentum.

The gentleman, who under the name of Philalethes Cantabrigiensis had undertaken a defence of Sir Isaac Newton against Dr. Berkeley the author of the Analyst, not brooking, that Mr. Robins's explanation of these doctrines should be inconsistent with his own, refrained not from casting out against him these and other reproaches; all which might indeed, nay perhaps ought to have been, passed over without any notice, as solely the effects of anger and disappointment, and as having been already answered by Mr. Robins himself; had not

* Account, §. 40. Dissertation, §. 9, 10. † Discourse, §. 4, 94. Account, §. 28.

 the

the behaviour of Philalethes, in these contests, been seriously commended, and Mr. Robins as much censured, by M. de Buffon, in the Preface to his Translation of Sir Isaac Newton's Treatise of Series and Fluxions, printed at Paris in 1740, with a train of general invective, without citing any one expression from Mr. Robins's writings in support of it; merely, as it seems, from the high notions he had been inspired with of Philalethes, and his manner of writing; for mentioning his Tracts against the author of the Analyst, M. de Buffon is pleased to say, "qui sont admirables pour la force de "raison et la finesse de raillerie qu'on y trouve par "tout—." But perhaps, when he calls to mind a small pamphlet, entitled Lettre à Monsieur de Buffon par M. Jurin, writ in his own language, he may not be unwilling, that this high strained compliment should be ascribed to his want of a perfect knowledge in ours.

I am surprized at M. de Buffon's imagining, that the author of the Analyst could find any cause of triumph in the representation Mr. Robins had given of Sir Isaac Newton's doctrine, which at once destroyed all his objections; while Philalethes by allowing, at least in part, Dr. Berkeley's interpretations of Sir Isaac Newton, denied the consequences in vain; and was so far from demolishing the Doctor's writings, or humbling his metaphysical pride, as M. de Buffon expresses it *, that Philalethes left him room enough to write on without end; but on the appearance of Mr. Robins's book, the author of the Analyst thought fit to be silent.

But M. de Buffon gave so little attention to what he scrupled not to treat with so much ill manners, that he asserts of Mr. Robins, that "il avouë que "la géometrie de l'infini est une science certaine, "fondée sur des principes d'une vérité sûre—;"

* Preface, p. xxvii.

whereas

whereas Mr. Robins always contended, that what is ſtyled the geometry of infinites was no certain ſcience, nor built on ſure principles; and therefore he has ſhewn, according to Sir Iſaac Newton, that the proportions of fluxions are not the proportions of any quantities imagined to be infinitely ſmall, but only the limits of the varying ratios of finite quantities, that diminiſh together, till they vaniſh.

WITH the ſame precipitancy M. de Buffon concludes by aſſerting, that the mathematicians paid no regard to what Mr. Robins had ſaid ("il n'y a pas "eu moyen de leur faire croire un ſeul mot de tout "cela.") But the contrary is evident; for the beſt writers ſoon after trod in Mr. Robins's ſteps, and expreſſed themſelves conformably to his ſentiments and phraſeology without reſerve.

EVEN M. de Buffon's own countrymen are at length come into Mr. Robins's way of explaining theſe ſubjects; for M. de Bougainville, in the Preface to his Traité du calcul integral *, ſays, "Le "calcul de Newton eſt independant de la réalité "des quantités infiniment petites—." And M. d'Alembert has, at ſeveral times, ſhewn his diſlike to the notion of infinitely little quantities. In his Traité de Dynamique †, ſpeaking of the Méthode des infiniment petits, he ſays, "Que les commen- "çans qui n'en pénétrent pas toujours l'eſprit, pour- "roient s'accoutumer à regarder ces infiniment petits "comme des réalités; c'eſt une erreur contre la- "quelle on doit être d'autant plus en garde, que "de grands hommes y ſont tombés, et qu'elle- "même a donné occaſion à quelques mauvais livres "contre la certitude de la geométrie." In Tom. I. of the Encyclopédie ‡ he has theſe words, "Il n'y "a point réallement de quantités infiniment peti- "tes,—. Cette idée des *limites* eſt tres-nette et tres-

* Printed at Paris in 1754. p. viii. † Ibid. in 1743. p. 36. ‡ Ibid. in 1751. p. 845.

"utile pour reduire la geométrie des infiniment
" petits à des notions claires." And in Tom. IV. *
" Il [Newton] n'a jamais regardé le calcul *differen-*
" *tiel* comme le calcul des quantités infiniment pe-
" tites, mais comme la méthode des premieres et
" dernieres raisons, c'est-à-dire la méthode de trouver
" les limites des rapports."

But more particularly Mr. Maclaurin published, within seven years after Mr. Robins, his Treatise of Fluxions at Edinburg in 1742; wherein he conformed himself entirely to Mr. Robins's sentiments, in regard to Sir Isaac Newton's doctrine. And though he was joined in a long course of intimacy with the chief of Mr. Robins's opposers, and personally unknown to him; yet in the preface he has publickly commended Mr. Robins's performance.

Nay, Mr. Maclaurin not only concurs with Mr. Robins in his interpretation of these doctrines, but has even expresly followed his plan in treating the subject.

Mr. Maclaurin in the Account he gives himself of his book in the Philosophical Transactions, N° 468, p. 325, thus begins. "The author's first
" design, in composing this Treatise, was to establish
" the method of Fluxions on Principles equally
" evident and unexceptionable with those of the
" ancient Geometricians by Demonstrations deduced
" after their manner in the most rigid form †, and
" by illustrating the more abstruse Part of the Doc-
" trine, to vindicate it from the imputation of un-
" certainty or obscurity." Again, in the introduction to his book, p. 3, Mr. Maclaurin says, "The
" method of demonstration, which was invented by
" the author of fluxions, is accurate and elegant ‡;
" but we propose to begin with one that is some-
" what different; which, being less removed from

* Printed at Paris in 1754. p. 985, 986. † Discourse, §. 40, 151. Account, §. 6. ‡ Discourse, §. 3, 94, 142, 150.

" that of the ancients, may make the transition to his
" method more easy to beginners, and may obviate
" some objections that have been made to it *."

All this is an exact description of the method Mr. Robins pursued. For though Mr. Maclaurin has expressed himself thus tenderly, yet his method does not indeed differ essentially from that of the ancients.

He immediately subjoins, " But, before we pro-
" ceed, it may be of use to consider the steps by
" which the ancients were able, in several instances,
" from the mensuration of right-lined figures, to
" judge of such as were bounded by curve lines † ;
" for as they did not allow themselves to resolve
" curvilineal figures into rectilineal elements, it is
" worth while to examine by what art they could
" make a transition from the one to the other ‡."
This also is the very order in which Mr. Robins proceeded in regard to the doctrine of prime and ultimate ratios. Mr. Maclaurin farther adds, that
" as they were at great pains to finish their demon-
" strations in the most perfect manner, so by fol-
" lowing their example, as much as possible in
" demonstrating a method so much more general
" than theirs, we may best guard against excep-
" tions and cavils, and vary less from the old foun-
" dations of geometry." And he illustrates this by the very example, which Mr. Robins ‖ had given before him, in explaining the methods of the ancients. Thus [p. 8.] " when Archimedes demon-
" strated, that the area of a circle is equal to a
" triangle upon a base equal to the circumference
" of the circle, of a height equal to the radius, it
" was not by supposing it to coincide with a circum-
" scribed equilateral polygon of an infinite number

* Discourse, §. 40, 151. Account, §. 6. † Discourse, §. 89. Dissertation, §. 12, 103. ‡ Ibid. §. 13. ‖ Discourse, §. 90.

" of

"of sides, but in a more accurate and unexcep-
"tionable manner."

Mr. Maclaurin so perfectly approved of the course Mr. Robins took of justifying Sir Isaac Newton's doctrine, by comparing it with the practice of the ancients, that he chose to expatiate largely in describing their methods, and in comparing them with the practice of the moderns before Sir Isaac Newton.

Thus at p. 33. "It is often said, that curve lines
"have been considered by them as polygons of an
"infinite number of sides. But this principle no
"where appears in their writings. We never find
"them resolving any figure or solid, into infinitely
"small elements. On the contrary, they seem to
"avoid such suppositions, as if they judged them
"unfit to be received into geometry; when it was
"obvious that their demonstrations might have been
"sometimes abridged by admitting them. They
"considered curvilineal areas as the limits of cir-
"cumscribed or inscribed figures of a more simple
"kind, which approach to these limits (by a bi-
"section of lines, or angles, that is continued at
"pleasure *,) so that the difference betwixt them
"may become less than any given quantity †. The
"inscribed or circumscribed figures were always
"conceived to be of a magnitude and number that is
"assignable; and from what had been shewn of these
"figures, they demonstrated the mensuration or the
"proportion, of the curvilineal limits themselves, by
"arguments *ab absurdo* ‖. They had made frequent
"use of demonstrations of this kind from the be-
"ginning of the elements; and these are in a par-
"ticular manner adapted for making a transi-
"tion from right-lined figures to such as are

* Dissertation, §. 13, 107.

† The use made in geometry of the above expression *any given quantity*, Philalethes never could understand, which occasioned many of his mistakes. Review, §. 18. Dissertation, §. 42, 108, &c.

‖ Account, §. 28.

"bounded

"bounded by curve lines *. By admitting them "only, they established the more difficult and sub- "lime part of their geometry on the same founda- "tion as the first elements of the science. Nor "could they have purposed to themselves a more "perfect model †."

This is exprefly contrary to what Philalethes contended for, in opposition to Mr. Robins, that Sir Isaac Newton's method, by proving the varying quantities came up to their limits, was more perfect than that of the ancients. Whereas Sir Isaac Newton never claimed such superiority; and had Philalethes comprehended the ancient method of demonstration, he would have known it to be impossible to excel it in point of evidence; and not have laid his friend Mr. Maclaurin under the necessity of thus contradicting him. Sir Isaac Newton contented himself with asserting, that his methods were consonant to that of the ancient geometers. The coincidence contended for, and thus highly praised by Philalethes, is the very essence of indivisibles.

But Mr. Maclaurin proceeds farther, and confirms all, Mr. Robins had said against the tediousness and perplexity, which Philalethes objected against the ancient mode of demonstration.

In p. 35 he says, "His [Archimedes] method "has been often represented as very perplexed, and "sometimes as hardly intelligible ‡. But this is not "a just character of his writings, and the ancients "had a different opinion of them. He finds it "necessary indeed to premise several propositions to "the demonstration of the principal theorems; and "on this account his method has been excepted "against as tedious. But the number of steps is "not the greatest fault a demonstration may have; "nor is this number to be always computed from "those that may be proposed in it, but from those

* Dissertation, §. 12. † Discourse, §. 40. ‡ Dissertation, §. 53.

"that

" that are neceſſary to make it full and concluſive.
" Beſides, theſe preliminary propoſitions are generally
" valuable on their own account, and render our
" view of the whole ſubject more clear and com-
" pleat. In his treatiſe of the ſphere and cylinder,
" for example, by his demonſtrating ſo fully the
" menſuration of the ſurfaces and ſolids, generated
" by the internal and external polygons, we not only
" ſee how the ſurface and ſolid content of the ſphere
" itſelf is determined, but we acquire a more perfect
" knowledge of this theory, and of all that relates
" to it, with a ſatisfaction * that we are ſenſible is
" often wanting in the incompleat demonſtrations of
" ſome other methods."

Indeed the perplexity of a demonſtration does not ariſe from the number of ſteps in it; but from the diſorderly diſpoſing thoſe ſteps, and which are generally increaſed from want of order. This Philalethes could never underſtand; not being able to diſtinguiſh between a long and a perplexed demonſtration.

The conſequences of the moderns, departing from this rigour in demonſtration uſed by the ancients, in introducing infinites, are thus expreſſed by Mr. Maclaurin at p. 38, 39. " But when the principles
" and ſtrict method of the ancients, which had hi-
" therto preſerved the evidence of this ſcience entire,
" were ſo far abandoned, it was difficult for the
" Geometricians to determine where they ſhould ſtop.
" After they had indulged themſelves in admitting
" quantities of various ſorts, that were not aſſigna-
" ble, in ſuppoſing ſuch things to be done as could
" not poſſibly be effected, (againſt the conſtant prac-
" tice of the ancients,) and had involved themſelves
" in the mazes of infinity, it was not eaſy for them
" to avoid perplexity, and ſometimes error †, or to
" fix bounds to theſe liberties when they were once

* Diſſertation, §. 54. † Account, §. 3, 38. Diſſertation, §. 30, 31, 32, 33.

" intro-

"introduced. Curves were not only considered as po-"lygons of an infinite number of infinitely little sides, "and their differences deduced from the different "angles that were supposed to be formed by those "sides; but infinites and infinitesimals were admitted "of infinite orders, every operation in geometry and "arithmetick applied to them with the same freedom "as to finite real quantities, and suppositions of this "nature multiplied, till the higher parts of geometry "(as they were most commonly described) appeared "full of mysteries.

"From geometry the infinites and infinitesimals "passed into philosophy, carrying with them the "absurdity and perplexity that cannot fail to ac-"company them."

At page 46 Mr. Maclaurin says; "We may "perceive from these instances, that it is not by "founding the higher geometry on the doctrine "of infinites we can propose to avoid the apparent "inconsistences that have been objected to it;—."

Accordingly, at page 49, it is said, "Sir "Isaac Newton accomplished what Cavalerius wished "for, by inventing the method of fluxions, and "proposing it in a way, that admits of strict de-"monstration *, which requires the supposition of "no quantities but such as are finite, and easily "conceived. The computations in this method "are the same as in the method of infinitesimals; "but it is founded on accurate principles, agreeable "to the ancient geometry †. In it, the premises "and conclusions are equally accurate, no quan-"tities are rejected as infinitely small, and no part of "a curve is supposed to coincide with a right line."

Farther, Mr. Maclaurin has expresly copied Mr. Robins's representation of fluxions in these words, Art. 10, page 56, 57. "The velocity

* Discourse, §. 3, 94, 142, 150. Account, §. 4. Dissertation, §. 1. † Discourse, §. 3, 142, 150.

"with

"with which a line flows, is the same as that of "a point which is supposed to describe or generate "it *. The velocity with which a surface flows, "is the same as the velocity of a given right line, "that, by moving parallel to itself, is supposed to "generate a rectangle which is always equal to the "surface †. . . . In this method likewise quantities "of the same kind may be represented by right "lines, and the velocities of the motions by which "they are supposed to be generated, by the ve-"locities of points moving in right lines ‡."

And the sense of this paragraph is delivered by Mr. Maclaurin in the Philosophical Transactions, N° 468, page 329; as follows.

"Lines are supposed to be generated by the "motion of points. The velocity of the point that "describes the line is its fluxion, and measures the "Rate of its Increase or Decrease ||. Other magni-"tudes may be represented by lines that increase "or decrease in the same Proportion with them; "and their Fluxions will be in the same Proportion "as the Fluxions of those lines, or the Velocities "of the Points that describe them ‡||."

The uses to be made of these velocities, Mr. Maclaurin thus expresses, in conformity with Mr. Robins, in the last Article of his book.

"In this doctrine, when the velocity of a motion "is determined, it is always with relation to the "velocity of some other motion; and when we "enquire at what rate the ordinate, for example, "increases or decreases, it is always in relation "to the base, or some other magnitude, with which "it is compared." And thus Mr. Robins, "It is "by means of this proportion only, that fluxions "are applied to geometrical uses; for this doctrine

* Discourse, §. 6. Dissertation, §. 3. † Discourse, §. 49. ‡ Ibid. §. 10, 11, 39. Dissertation, §. 7. || Discourse, §. 6. Dissertation, §. 3, 6. ‡|| Discourse, §. 10, 39. Dissertation, §. 7.

"never

"never requires any determinate degree of velocity "to be assigned for the fluxion of any one fluent*."

How Sir Isaac Newton applies his method of prime and ultimate ratios to the quantitates nascentes and evanescentes, whence are demonstrated the proportions of these velocities or fluxions, Mr. Maclaurin in Article 502 thus declares. "... In order to "avoid such suppositions" [infinitesimals] "Sir "Isaac Newton considers the simultaneous incre-"ments of the flowing quantities as finite, and then "investigates the ratio which is the limit of the "various proportions which those increments bear "to each other, while he supposes them to decrease "together till they vanish †; which ratio is the "same with the ratio of the fluxions by what was "shewn in Art. 66, 67 and 68 ‡. In order to "discover this limit, he first determines the ratio "of the increments in general, and reduces it to "the most simple terms so as that (generally speak-"ing) a part at least of each term may be inde-"pendent of the value of the increments them-"selves; then by supposing the increments to de-"crease till they vanish, the limit readily appears."

Concerning this limit Mr. Maclaurin further adds [Art. 505] that it is called the *first* or *prime ratio* of these increments, and may with equal propriety be said to be the *last* or *ultimate*; because though the ratio of those increments continually varies, when the motion is continually accelerated or retarded, yet the ratio of the generating motion is the term or limit from which the variable ratio of the increments proceeds, or sets out to increase or decrease; and soon after he observes, that a ratio may limit the variable ratios of the increments, though it cannot be said to be the ratio of any real increments ||.

* Discourse, §. 15. † Account, §. 31. Review, §. 5. Dissertation, §. 65. ‡ Discourse, §. 146, 149. Review, §. 5. Dissertation, §. 65. || Discourse, §. 104, 105, 107, 108. Account, §. 31. Dissertation, §. 58. 73.

Now

Now this is the very doctrine Mr. Robins always insisted upon, and which Philalethes pretended to treat with so much scorn and contempt.

But to conclude, I ought not, perhaps, to omit, that Mr. Maclaurin has once indeed spoke of infinitesimals, which he has thus expresly condemned, more favourably than Mr. Robins has done, when he says [Introduction, p. 47] "we would not be understood to affirm, that the method of indivisibles "and infinitesimals, by which so many uncontested "truths have been discovered, are without a foundation."

What this supposed foundation is, Mr. Maclaurin has no where explained; and whether this was said by him in compliment to his friend Philalethes, or the effect of prejudices remaining from former opinions, I leave undecided. It is certainly inconsistent with all, that has been here cited from him.

Thus, upon the whole, I cannot doubt, but M. de Buffon, when he shall have calmly perused what Mr. Robins has writ, and observes, how exactly he has been followed by others; he will see reason to wish, that all, he has said against my friend with so much insult, may be understood to proceed from his not having duly himself considered Mr. Robins's tracts, but taken upon trust the suggestions of some other, which were the effects of prejudice and ill will. And indeed when he talks of the obscurity of Mr. Robins's ideas, the insignificancy of his phrases, and the unintelligibleness of his style; he gives the most certain proof, that he had never carefully read his writings, or has a very imperfect knowledge of the language in which they are writ; for Mr. Robins is much admired here for the contrary excellencies, on whatever subjects he has employed his pen.

M. de Buffon attempts to justify his abusive treatment of Mr. Robins, from his having the presumption to censure the Grand Bernoulli, as he styles him. This wonderful man, it seems, my friend has presumed to call an inelegant computist *; which assertion he was very able to justify from that writer's numerous productions. But the principal motive of his using this freedom was the indignation, he had early conceived at M. Bernoulli's insufferable vanity and indecent carriage, without any cause, towards many eminent mathematicians. In particular, Dr. Keill he had abused in a most outrageous manner; which scandalous behaviour the Doctor has treated, as it deserved, in his Letter to M. Bernoulli †, which Letter being in Latin, I wonder, it was not reprinted by the collectors of Dr. Keill's works: for though it has some personal reflexions, all justifiable from the usage he had received (a usage much blamed even by some of M. Bernoulli's best friends ‡ and greatest admirers) yet it also contains curious particulars relating to the mathematicks; especially the applying second and third fluxions, in which many have committed errors. There the Doctor's assertions are supported by irrefragable arguments, the weakness of his antago-

* Remarks on Dr. Smith, §. 1. † J. Keill Epistola ad J. Bernoulli. Lond. 1720. in 4to.

‡ What an opinion M. de Montmort had of M. Bernoulli's behaviour, appears from his letters to Dr. Taylor. In one it is said, " Il est certain que M. Bernoulli est trop jaloux d'honneur " et porte trop loin ses pretentions dans la dispute avec son " frere, ils avoient tort tout deux mais luy à mon avis beau- " coup plus que Jacques;" in another, " J'ay déja pris la " liberté de dire à M. Bernoulli que bien de gens trouvoient " qu'il n'en usoit pas bien à l'egard de feu M. de l'Hospital;" and in a third, " Il y a un endroit contre M. Keill que je blame " extremement—J'en suis faché pour M. Bernoulli qui n'a pas été " en droit de faire un pareil reproche à M. Keill quelque sujet " qu'il puisse avoir de s'en plaindre." Keill Epist. ad J. Bernoulli, at the end.

nist's reasons clearly evinced, his abuses exposed, and the whole written in a very elegant manner. Insomuch that I am apt to believe, if M. d'Alembert had duly examined this dispute, and seen this letter; he would scarce have said of M. Bernoulli, "Peut-être "étoit-il excusable à l'egard de M. Keill, qui avoit "en quelque maniere violé les régles du droit des "gens, et dont les procédés n'étoient pas moins "blâmables que les discours *."

Again, my most dear and worthy friend Dr. Taylor, a gentleman of fortune; who allotted some part of his time to mathematical studies, and has published works of genius and invention; as his Methodus Incrementorum †, with several curious Dissertations in the Philosophical Transactions ‡. And observing the imperfect manner the art of Perspective was delivered in all the books he had seen, he wrote his treatise on that subject ||; which is highly esteemed by our most knowing painters ‡||. This excellent person M. Bernoulli fell upon without ceremony, to which attack the Doctor gave a sufficient answer in the Philosophical Transactions, N° 360;

* Eloge Historique de M. Jean Bernoulli, in M. d'Alembert's Melanges de Literature, &c. † An account of this book is given by the author himself in the Philosophical Transactions, N° 345. ‡ Ibid. N° 336, 352, 353, 354, and 367. || See an account of this Perspective by the author, ibid. N° 344.

‡|| As Dr. Taylor's treatise is written briefly, not to say obscurely; some have attempted to explain it. Though I have reason to think my friend found out by himself the excellent method, he described; yet he is not here an original inventor. For that very method was long before published by Guido Ubaldi, in his Perspective printed at Pesaro in 1600. Where it is delivered very clearly, and confirmed by most elegant demonstrations. In the last book, Ubaldi applies his method to the delineating the scenes of a theatre. And in this particular, with regard to the practice, he is followed by Signor Sabbattini in his Practica di Fabricar Scene; whereof was made, at Ravenna in 1638, a new edition, to which was added a second book, containing a description of the machines used for producing the sudden changes in the decorations of the stage.

setting

setting forth, at the same time, the gross errors his antagonist had committed in the solution of the Isoperimetrical problem. But M. Bernoulli persisting, that Dr. Taylor had stole from him the method of determining the center of oscillation, the Doctor demonstrated, that his own had been published in the Philosophical Transactions, N° 337, before that of M. Bernoulli's *. On this clear conviction, M. Bernoulli had recourse to the low shift of pretending, that what Dr. Taylor had published in the Transactions, was but a sketch of what he afterwards produced in his Methodus Incrementorum †. Now in contradiction to this, it is certain, what was delivered in the Transactions, is more full and explicit, than what is contained in the Methodus Incrementorum, though even that treatise had been printed before M. Bernoulli's discourse was published.

Lastly, in regard to Sir Isaac Newton, this great, modest, and generous person, has often, by M. Bernoulli, been most unreasonably insulted, and more particularly in a paper, that was published in an infamous libel, dated 29 Julii 1713 ‡, and there styled Judicium primarii Mathematici. That this primarius Mathematicus was M. Bernoulli, could not long remain a secret; as the paper bore such genuine marks of its real parent. However, M. Bernoulli three years after, in a letter to M. Leibnitz, wonders how it came to be known ‖. But the weakness of this judgment has been fully exposed

* Act. Erudit. Sept. 1722. † Act. Erudit. Supplem. Tom. viii. Sect. v. ‡ This libel was translated into French, and printed at the Hague in the Journal Literaire for Nov. and Decemb. 1713. p. 448. ‖ "Miror quomodo Newtonum "scire potuerit me auctorem esse epistolæ illius, quam inseruisti "chartæ illi contra Newtonum publicatæ; cum tamen nemo "mortalium sciverit me illam scripsisse, nisi tu ad quem scripta "est, et ego a quo scripta est." Leibnitii et Bernoullii Commercium Epistolicum Lausanæ 1745. Tom. ii. Epist. 231.

by Sir Isaac Newton himself, at the end of the Commercium Epistolicum. See also the Journal Literaire for July and August in 1714; where the gross abuses thrown on Sir Isaac Newton by the author of the libel itself, in relation to Dr. Hook and Mr. Flamstead, are excellently well answered by Dr. Keill. As to what was afterwards said in the Acta Eruditorum for March 1720, in giving an account of M. de Fontenelle's Elogium of M. Leibnitz, where an opportunity is taken of upbraiding Sir Isaac Newton for having been beholden to M. Huygens and Dr. Hook; it may be replied, that he had quoted them both in his Principia, Lib. i. Prop. iv. in the Scholium.

AGAIN, Mr. Robins's Remarks on a Treatise of M. Euler, are by M. de Buffon brought into the account. And here I cannot but observe the difference in the behaviour of a person of knowledge, who thinks for himself, and of one carried away with the unjust reports of envy and malice. M. Euler, highly to his honour as a gentleman, far from being influenced by any private disgust, not only has publickly praised, but has taken the pains to communicate to his countrymen in their own language, a work * of Mr. Robins's, which he thought worthy these marks of his esteem.

BUT to be more particular upon the railings thrown out against Mr. Robins in regard to Sir Isaac Newton.

M. de Buffon has charged Mr. Robins with want of respect towards that great man, by making him assert, that Sir Isaac Newton had not well considered the ancient geometers, that his Lemma was obscure and hypothetical, and his ideas not clear, and even setting himself above him.

* New Principles of Gunnery, translated into High Dutch by M. Euler. Printed at Berlin in 1745.

As

As M. de Buffon has not referred to any particular passages in Mr. Robins's writings; it is not easy to guess, where he pickt up all this string of accusations against one, who has always professed the greatest deference for Sir Isaac Newton *. Mr. Robins has indeed supposed, for which he had very good grounds, that, when Sir Isaac Newton invented his method of fluxions, he had not then much studied the ancients †. But is this asserting, that he had never well considered them?

Neither has Mr. Robins ever represented Sir Isaac Newton's Lemma as obscure, much less as in any measure hypothetical ‡; nor has he any where insinuated, that Sir Isaac Newton was ever deficient in the perspicuity or distinctness of his ideas, declaring, in the strongest terms, that his express design was to shew, that the misapprehensions of others had given rise to all the objections, that had been advanced against Sir Isaac Newton's doctrine; which objections will be at once removed by a careful choice of expressions, adequately accommodated to Sir Isaac Newton's real meaning ||.

Mr. Robins has indeed made mention of the great brevity with which Sir Isaac Newton wrote ‡||, a fact well known to every one, that reads him. If to declare this be a crime, he is followed in it by Mr. Maclaurin ||†. But this was the most complaisant

* Discourse, §. 129, 159. Account, §. 24. Dissertation, §. 93, 94, 96, 98. Remarks on Dr. Smith, §. 52. New Principles of Gunnery, in the preface, p. 51. † Dissertation, §. 93. ‡ Ibid. §. 42. || Ibid. §. 93. To the same purpose, it is said by M. d'Alembert, speaking of the differential method, "Il faut "avoüer que si ce calcul a eu des enemis dans sa naissance, "c'est la faute des geométres ses partisans, dont les uns l'ont "mal compris, les autres l'ont trop peu expliqué." Encyclopédie, Tom. iv. p. 988. But here is the difference, it is impossible to justify M. Leibnitz's notions. ‡|| Discourse, §. 4. Account, §. 28. ||† "What he [Sir Isaac Newton] has given "on this subject being very short, his conciseness may be sup- "posed

plaisant reason, Mr. Robins could alledge, why the author of the Analyst might not understand Sir Isaac Newton. It was in opposition to Dr. Berkeley, that Mr. Robins composed his treatise; all that he wrote afterwards against Philalethes, that gentleman brought upon himself by his impatience to see another enter the lists against his antagonist, with different weapons from his own; and turning his assault against him (as M. de Buffon expresses it) "comme defenseur de la verité s'est chargé de "lui signifier ce qu'on entendoit fort bien Newton "sans Robins *;" but unfortunately for himself, as it now appears, that either he, or Mr. Maclaurin, as well as Mr. Robins, did not understand Sir Isaac Newton.

But if Mr. Robins had ascribed to Sir Isaac Newton some degree of negligence †, it will be no more than what was freely admitted by Dr. Saunderson, Professor of the Mathematicks at Cambridge, who, no body believes, ever wanted respect for Sir Isaac Newton; yet did own, "that the great Inventor, "never expecting, to have it" [doctrine of fluxions] "canvassed with so much trifling subtility and cavil, "had not thought it necessary to be guarded every "where by expressions so cautious, as he might "have otherwise used ‡."

The truth is, Sir Isaac Newton at first made the same use of indivisibles, others had done: in his Analysis per æquationes numero terminorum infinitas, he expresly says, "Nec vereor lo- "qui de unitate in punctis, sive lineis infinite "parvis ‖;" and in his Lectiones Opticæ he

"posed to have given some occasion to the objections which "have been raised against his method." Maclaurin's Treatise of Fluxions, p. 2.

* M. de Buffon's preface, p. xxviii. † Dissertation, §. 101, 102. ‡ See Memoirs of Dr. Saunderson's life, before his Algebra, p. xv. ‖ Comm. Epist. p. 85.

demon-

demonstrated by indivisibles *; where he also attempts, by their aid, to accommodate the definition of proportionality in commensurable quantities to such as are not commensurable †. But when he composed his Principia, he proposed to establish a more accurate method of conception; and explicitly instructs his readers, how they are to understand, whatever expressions may occur, that may appear similar to what are used in the doctrine of indivisibles, in saying, "Siquando quantitates "tanquam ex particulis constantes consideravero, "vel si pro rectis usurpavero lineolas curvas; "nolim indivisibilia, sed evanescentia divisibilia, "non summas et rationes partium determinatarum, sed summarum et rationum limites semper "intelligi ‡."

But experience has shewn, that notwithstanding this caution, many had mistook Sir Isaac Newton's meaning, even after he had explained, in the Introduction to his Treatise on Quadratures, his doctrine in terms guarded against every objection, that could be raised.

Again, M. de Buffon is too fond of his talent for invective to confine it to one object, but must needs extend it to Sir Isaac Newton himself also in the same heedless manner; that because Sir Isaac Newton died not long after the last publication of his Principia, he takes upon him to presume, that while that work was under his revisal, his intellects were so impaired by age, that he was not master of his own thoughts, without considering in the least the cause of his death, which, notwithstanding his years, was not occasioned by the mere failure of

* Dissertation, §. 93. See also a demonstration of Sir Isaac Newton's, by indivisibles, in Hunt's Gauger's Magazine, printed at London in 1687. † Lect. Optic. Part. 1. Sect. 3. Lem. 6. Corol. 1. ‡ Princip. Lib. I. Lem. xi. in the Scholium.

the principles of life; but from one of the most tormenting diseases, that human nature is subject to *; for excepting some degree of that defect in memory in regard to recent occurrencies, which is the usual concomitant of age, he is well known to have retained his intellectual faculties to the last. And this unjust reflection is thrown out upon Sir Isaac Newton only; because he had not pleased M. de Buffon in omitting a passage in his book relating to M. Leibnitz.

Now the original design of inserting this passage was to shew, that Sir Isaac Newton had before found out the method of fluxions. Of that affair he indeed had given an account in a letter written to Mr. Collins, of an earlier date † than that ‡ alluded to in the passage, whose omission is complained of by M. de Buffon. But for above 20 years after the Principia had been published, Sir Isaac Newton did not know, that this letter to Mr. Collins existed, or that a copy of it had been sent to Slusius, M. Tchurnhans, and M. Leibnitz. This letter contained a particular description of the method, he had delivered in the book of series and fluxions, which he had written the year before; and as he was prevailed on by Dr. Pemberton ‖, while the last edition of his Principia was carrying on, to suffer this treatise of series and fluxions, after it should have been revised and augmented by himself, to come abroad; he thought proper to exchange the passage relating to M. Leibnitz, for what he had inserted from the abovementioned letter to Mr. Collins; for as it was written before M. Huygens's Horologium Oscillatorium was pub-

* See his elogium by M. de Fontenelle, in the History of the Royal Academy of Sciences at Paris for the year 1727.

† 10 Dec. 1672. ‡ 24 Octob. 1676. ‖ View of Sir Isaac Newton's Philosophy, in the preface.

lished;

liſhed; ſo it mentioned Sir Iſaac Newton's having then determined the curvature of curves *.

As to the paſſage, that regards M. Leibnitz, it may be obſerved, that Sir Iſaac Newton has ſaid, "He [M. Leibnitz] pretends, that in my Book of "Principles, p. 253, 254, I allowed him the in-"vention of the *calculus differentialis*, independently "of my own †; and that to attribute this invention "to myſelf, is contrary to my knowledge there "avowed. But in the paragraph there referred "unto, I do not find one word to this purpoſe; on "the contrary, I there repreſent, that I ſent notice "of my method to M. Leibnitz before he ſent no-"tice of his method to me; and left him to make "it appear that he had found his method before the "date of my letter, that is eight months at leaſt "before the date of his own ‡." And afterwards Sir Iſaac Newton adds, "As for the Scholium upon "the ſecond Lemma of the ſecond Book of the "Principia Philoſophiæ Mathematica, which is ſo "much wreſted againſt me; it was written not to "give away that Lemma to M. Leibnitz, but on the "contrary to aſſert it to myſelf ||." And accordingly it is referred to and quoted at length in the diſpute about the invention of fluxions **.

BUT Sir Iſaac Newton finding M. Leibnitz, as well as others, had miſrepreſented this paſſage; he reſolved to leave it out. On which account M. de Buffon aſks, "Pourquoi ſupprimer cet article? "puiſqu'on l'avoit laiſſé ſubſiſter dans la ſeconde "edition

* Comm. Epiſtolic. p. 105. † Recueil de Diverſes Pieces ſur la Philoſophie, &c. printed at Amſterdam in 1720, Tom. ii. p. 35, 42. ‡ Raphſon. Hiſtor. Fluxion. p. 115. As alſo in Recueil, &c. Tom. ii. p. 87. See alſo Philoſ. Tranſ. Nº 342. p. 218; or Com. Epiſt. p. 51. || Raphſon. Hiſt. Fluxion. p. 122. Or Recueil, &c. Tom. ii. p. 108. ** Journal Literaire 1714. p. 340. Philoſ. Tranſ. Nº 342. p. 198; or Comm. Epiſtolic. p. 30; and Recueil de Diverſes Pieces, &c. Tom. i. in the preface, p. xiii.

"edition en 1713. c'est-à-dire, dans le tems de la "chaleur de la contestation." I answer, that edition was in the press, and great part printed off, before the dispute about the invention of fluxions began, before Sir Isaac Newton knew, that his letter to Mr. Collins was in being, and long before the passage of the Principia had been misrepresented. And now there was no farther occasion for that passage; since Sir Isaac Newton's right to the invention of fluxions had been fully demonstrated in the Commercium Epistolicum. Besides, it did not become him to be upon any terms with such persons as M. Leibnitz and M. John Bernoulli, who not content to usurp his inventions, had loaded him with calumnies. The former, not to insist on the invention of fluxions, pretended to the discovery of the principal propositions of the Principia; but the paralogisms he committed, betrayed the plagiarism *. The latter, with no small degree of vanity, claimed to himself the integral calculus †, when what he had produced was but a very inconsiderable part of what is contained in Sir Isaac Newton's letters sent to M. Leibnitz ‡, and whoever shall consider the seven theorems, he long afterwards published ‖ with the same degree of boasting, must be surprized at his obstinacy in persevering so to neglect, what Sir Isaac Newton has done, as not to know, that the four first of those theorems were but a small pittance of the series for the quadrature of curves, exhibited in the abovementioned letters; even after the use of it had been farther explained in the Treatise of Quadratures; nor that the three last were only one very simple case of the seventh proposition of that book, *viz.* when the part of the ordinate under the vinculum is a bi-

* Philos. Trans. N° 342. p. 208. Or Commer. Epist. p. 41, &c. Journal Literaire 1714. p. 348. † Acta Erudit. 1716. p. 299. ‡ Keill Epist. ad Bernoul. p. 3. ‖ Acta Erudit. 1719. Mens. Jun. p. 269.

nomial,

nomial, and the index of that vinculum a negative integer. They talk indeed of their exponential calculus as a great discovery *; but this Sir Isaac Newton himself has hinted to be really of no use †.

Thus, I presume, I have sufficiently vindicated both my friend Mr. Robins and Sir Isaac Newton from the pragmatical reflections of M. de Buffon. But as Sir Isaac Newton has at different times been censured on many points; I hope I may, without blame, here add, to what others have already writ in his justification, some animadversions on such objections, as I find to have been lately advanced or revived.

The brevity, with which he has writ, particularly in his Principia, I observe still to be complained of ‡; however M. de Buffon has been pleased to reproach Mr. Robins for making any concession on that head. But to such as have duly prepared themselves by a sufficient knowledge in geometry to enable them to understand his meaning (and who else ought to attempt it?) this conciseness is most agreeable ‖. In the beginning of his book, what relates to the laws of motion, and centripetal forces, is delivered most clearly; and therefore it ought not to have hindered any person of skill from comprehending, what he had demonstrated of the influence gravity has in the system of the universe. But his conciseness perhaps may have discouraged others less expert from giving

* "Ut taceam *calculi exponentialis*, qui transcendentis perfectissimus, est gradus, quem L....us primus exercuit, Johannes vero Bernoullius proprio marte etiam assecutus est, nullam N....o aut ejus discipulis notitiam fuisse." The paper dated 29th of July, 1713. Or Journ. Liter. 1713. p. 453.
† Philos. Trans. N° 342. p. 212. Or Comm. Epist. p. 46.
‡ "Dans la plupart des endroits difficiles il [Newton] emploie un trop petit nombre de paroles à expliquer ses principes." Mem. de l'Acad. Royal. des Sciences à Paris. 1745. p. 329.
‖ "Prolixitas inutilis est obtusis et fastidiosa ingeniosis. Illis enim multa nil profuit; his vero pauca sufficiunt." Maurolyci. Opuscul. Mathem. p. 48.

due

due attention to what he has delivered very distinctly; if they had, his doctrine of the extensiveness of the power of gravity could never have been conceived to be an attempt to establish that operation as an original power in nature, for which no cause was to be inquired after; and putting it upon the foot of the ancient principle of occult qualities. Certainly, when Galileo and Torricellius shewed the line, that projectiles would move in by the action of gravity upon them (so far as they were not resisted by the medium they passed through) they were never charged with introducing, under the name of gravity, an occult quality; nor will any one object to the explaining any of the effects of the spring of the air by those, who ascribed them to that elastic power, which the air is known to be possessed of, as asserting an occult quality, and reject those explanations, as unphilosophical, though the cause of that elasticity was, and still remains, wholly unknown to us. If the effects of no power in nature were to be inquired after, till that power could be traced up to its first original cause, all natural philosophy would be at an end.

Now no writer was ever more cautious, than Sir Isaac Newton has been, to avoid all objections of this kind. In the former part of his book, where he treats of the effects of centripetal powers in general, and of the tendency bodies may be supposed to have towards each other; he frequently repeats in the most explicit manner possible, that under the name of attraction he had no intention of confining it to any particular physical cause whatever *.

AND

* "——considerando vires centripetas tanquam attractiones, "quarum fortasse, si physice loquamur, verius dicantur impul-"sus." Newton. Princip. Lib. i. Sect. xi. at the beginning. Again, "Vocem attractionis hic generaliter usurpo pro corporum conatu "quocunque accedendi ad invicem: sive conatus iste fiat ab "actione corporum, vel se mutio petentium, vel per spiritus "emissos

And when in the third book he applies the mathematical speculations of the first to the system of the world, he does not give the name of gravity to the centripetal powers, he finds to operate on the moon and other planets; till he has shewn, that action in the moon to be the same as that, which here upon the earth we call gravity. And from that time he substitutes this gravity in the room of the term attraction, he had before made use of *. In all this there is not the least ground to imagine, that he had any other idea of gravity, than what he had before expressed of attraction as a cause, whose effects he treats of without taking upon him to determine, whence that power arose.

I am as much surprized on the other hand, that what, Sir Isaac Newton has suggested at the conclusion of the latter editions of his book concerning the possibility of the operation of gravity being owing to some subtle fluid extended through the universe, should be considered as drawn from him by these objections, I have been speaking of, against the general tenour of his former sentiments †; when in one of the passages ‡ just now cited, which occurs in

" emissos se invicem agitantium; sive is ab actione ætheris, aut " aeris, mediive cujuscunque seu corporei seu incorporei oriatur corpora innatantia in se invicem utcunque impellentis." Ibid. Lib. i. at the end of Sect. xi. in the Scholium. See also Philos. Trans. N° 342. p. 222, 223. Or Com. Epist. p. 55, 59.

* " Propterea vis illa, qua luna retinetur in orbe suo ea ipsa " erit quam nos gravitatem dicere solemus." Newton. Princip. in Schol. Prop. iv. Lib. iii. Again, " Hactenus vim illam qua " corpora cœlestia in orbibus suis retinentur centripetam appel- " lavimus. Eandem jam gravitatem esse constat, et propterea " gravitatem in posterum vocabimus." Ibid. in Schol. Prop. v. Lib. iii.

" † Le principe d'une matiere subtile—que M. Newton lui- " même, plus interessé que personne à ne le pas admettre, n'a " pû s'empêcher d'adopter—." Hist. de l'Acad. Royal des Sciences à Paris 1749. p. 53.

‡ " Sive conatus iste fiat ab actione ætheris" &c. Princip. Lib. I. Sect. xi. at the end.

in the first edition as well as in the following, he has so explicitly enumerated this among the other causes, he has suggested as possibly capable of producing the effect.

However, as effects and causes cannot be extended in infinitum, there must be some first cause, to which the subsequent are owing. But that Sir Isaac Newton was not so hasty as to conclude gravity to be such a first cause, is evident, from what has been here said. Though I find M. d'Alembert rather supposing that he considered it as such; confirming himself in that opinion from Mr. Cotes having adopted that sentiment in a preface, which M. d'Alembert presumes to have been writ under Sir Isaac Newton's inspection, and to have received his entire approbation *.

Now Sir Isaac Newton has himself informed us †, "Quæ novæ Principiorum editioni præmissa "sunt, Newtonus non vidit antequam Liber in "lucem prodiit;" and it is well known, that he was much dissatisfied with that preface for more reasons than one; which contains things unworthy of him, in particular towards the end, where mention is made of Dr. Bentley; and that he therefore once intended to have made another edition of his book, in order to omit that preface, being much displeased with Dr. Bentley on that account. However, his mild temper could not long retain resentment against one, with whom he had for a great part of his life a familiar acquaintance; so that when at length he came to make the last edition of his Principia;

* —"il [Newton] a souffert que M. Côtes son disciple "adoptât ce sentiment sans aucune réserve, dans la préface qu'il "a mise à la tête de la seconde edition des *Principes*; préface "faite sous les yeux de l'auteur, et qu'il paroît avoir ap- "prouvée." Encyclop. Tom. i. p. 854. The same thing is repeated in another place of the Encyclopédie, Tom. vii. p. 876.

† Comm. Epistolic. in the preface, pag. penult.

in

in regard to the memory of Mr. Cotes, and an unwillingness to offend Dr. Bentley, who was then alive, and to whom he had been for many years reconciled, he suffered that preface to be again prefixt to his book.

Though this preface is one, and even the most considerable of those writings, which gave occasion to what he was wont to complain of, That opinions, which he never had entertained, have been imputed to him; because they were found in the writings of others, whom some had been pleased to call his disciples. But that no opinions may be attributed to this great man, which he never held; see what he has said excellently well himself in the Philosophical Transactions, N° 342, p. 222 *, about his method of philosophising. See also his letter to Signor Conti †.

Here M. d'Alembert intended to concur with Sir Isaac Newton; though he has been mistaken in ascribing to him an opinion, he never held. In another place ‡ he joins with the Bernoulli's, when they charged Sir Isaac Newton with having committed a mistake in determining second fluxions §. And though that affair had been already cleared up from their objections by Sir Isaac Newton himself in the Philosophical Transactions, N° 342. p. 209 ‡‖; yet M. d'Alembert has again renewed the accusation, which he supports by insisting, that Sir Isaac Newton had confounded the idea of a real curve with an imaginary form, which should consist

* Com. Epist. p. 55, &c. † Raphs. Hist. Fluxion. p. 100; or Recueil des Pieces, &c. Tom. ii. p. 16. ‡ Encyclop. Tom. iv. p. 988. § Mem. de l'Acad. Royal des Sciences Ann. 1711. ‡‖ Or Com. Epistol. p. 40. See also what Dr. Keill has said in the Journal Literaire in 1714. p. 343; and in 1716. p. 428; and in his letter to Bernoulli, p. 16. See likewise Riccati's letter, at p. 24. of Suzius's Disquisitiones Mathematicæ, printed at Venice 1725. In so many places has this affair been settled.

of

of polygons *. Whereas the fact is, those, who wrote upon curves according to the principles of indivisibles, were guilty of confounding these two ideas together; and Sir Isaac expresly distinguished them. Those, who had considered curves as consisting of polygons of an infinite number of sides, supposed the tangent to be one of these sides continued; whereby the portion, intercepted between the curve and tangent in an ordinate parallel to that through the point of contact, has for its ultimate limit the second fluxion of that ordinate; but Sir Isaac Newton has considered the tangent not as coinciding with any particle of the curve; but as making angles with every line drawn from the point of contact within the curve either way; and thus the ultimate limit of the aforesaid portion, intercepted between the curve and tangent, is half only of what was assigned by others.

Sir Isaac Newton seems also to be blamed by M. d'Alembert in the Encyclopédie † for having used synthetick demonstrations; as if he did it to conceal his method of investigation. But Sir Isaac Newton has himself told his real motives ‡; and so long ago as 1671 he advised, after a problem is once solved by algebra, to demonstrate the solution; "that laying aside all algebraical calcu-"lations, as much as may be, the theorem may be "adorned, and made elegant, so as to become "fit for publick view ‖." And in his algebra he has blamed the introducing the expressions of arithmetick into geometry. He there says, "Æqua-"tiones sunt expressiones computi arithmetici, et in

* "Il (Newton) a confondu la courbe polygone avec la "rigoureuse." Encyclop. Ibid.

† Tom. vii. p. 637.

‡ Philos. Trans. N° 342. p. 206; or Comm. Epist. p. 39.

‖ Method of series and fluxions, Prob. ix. §.107. I have above quoted the words of the English translation; because the French translation gives not the full force of the passage.

"geo-

"geometria locum non proprie habent.—Multi-
"plicationes, divisiones, et ejusmodi computa in
"geometriam recens introducta sunt; idque incon-
"sulto, et contra primum institutum scientiæ hujus.
"—Proinde hæ duæ scientiæ confundi non debent.
"Veteres tam sedulo distinguebant eas ab invicem,
"ut in geometriam terminos arithmeticos nun-
"quam introduxerint. Et recentes utramque con-
"fundendo amiserunt simplicitatem in qua geo-
"metriæ elegantia omnis consistit *."

And other writers before him had spoke to the same purpose †. But his example, M. d'Alembert is of opinion, has had an ill influence on the English mathematicians, who hence becoming great espousers of the synthesis, have not gone such lengths in geometry, as might otherwise have been expected from them; and after enumerating many improvements in the physical astronomy, adds, "Qu'on "essaye d'employer la synthese à ces recherches, "on sentira combien elle est incapable."

Here this ingenious gentleman confounds the analysis and synthesis together; an inadvertency, he might have avoided, had he recollected, what he

* Arithmetica Universalis, p. 288. at London in 1722.

† "Neque vero placet barbarum idioma, id est algebricum; "geometrica geometrice tracto." Vietæ Op. p. 305. "Artifex "geometra quanquam analyticum edoctus, illud desimulat et tan-"quam de opere efficiundo cogitans profert suum syntheticum "problema, et explicit." Ibid. p. 335. "Solutio non est "geometrica, siquidem algebra geometrica non est—." Kepler de Motu Martis, p. 93. "Monemus tantum viros clarissimos "[Wallis et alios] ut sepositis tantisper speciebus analyseos "problemata geometrica viâ Eucledianâ et Apollonianâ exe-"quuntur, ne pereat paulatim elegantia et construendi et de-"monstrandi, cui præcipue operam dedisse veteres innuunt satis "et data Euclidis et alii a Pappo enumerati analyseos libri." Fermat's Epistle to Sir Kenelm Digby, first printed in Dr. Wallis's Commercium Epistolicum in 1658, and afterwards reprinted in Vol. II. of his works in 1693. This letter is not in M. Fermat's Mathematical Works printed at Thoulouse in 1679.

has said in another place *, where he acknowledges the ancients to have had an analysis of their own different from ours; he could not but have seen, how insufficient the terms of synthesis and analysis are to distinguish the ancient geometrical analysis from the modern algebra.

Analysis is in general the unravelling the state of a question, and conducting it back to some known principles; and this may either be effected by the ancient method, which extends itself through the whole compass of geometry in search of the nearest and most appropriate principles; or by the modern computations of algebra, which are confined to a very few elementary propositions only †. And in like manner the solution of any problem may be proved after the ancient method of synthesis or composition from such propositions already known, as will most directly lead to the point in question; or by the round-about synthesis of an algebraical calculation, composing the proof from the most remote principles, in compliment to such, as have learnt only two or three elementary propositions. This algebraical synthesis abounds in the modern writers ‡; but such as were apprized of the excellency of the ancient demonstrations, whatever use they might have made of the algebraic analysis, thought it a necessary improvement of their solutions, to prove them by a synthesis of a more elegant form.

And here, it may be remarkt, that an observation made by M. d'Alembert, on the analysis of the ancients, requires correction; who says, "nous ignorons en

* Encyclop. Tom. i. p. 401.

† See a letter of Des Cartes to the Princess Elizabeth, being the 80th of the 3d Vol.

‡ See an eminent instance at p. 331. of the 1st Vol. of Cartes's Geometry, where Schooten endeavours to make out by algebra in a dozen pages, what James Gregory demonstrated in half that number of lines in his Geometriæ pars Universalis, p. 130.

" quoi

"quoi consistoit precisement leur analyse *;" when Pappus has given so distinct and particular a relation of it with many examples. And it has been formerly practised and described by divers of the modern geometers †; but perhaps no where more distinctly explained than in the Scholia on Vieta's Isagoge, printed at Paris in 1631; which particular scholia were omitted by Schooten, when he published Vieta's works. Joannes della Faille promised to write upon it ‡; and Hugo de Omerique in his Analysis Geometrica, printed at Cadiz in 1698, treats of it, though as a discovery of his own. I observed above, that the perfectest knowledge of it is to be learnt from Dr. Halley's edition of Apollonius de Sectione rationis.

But the practice of this analysis requires more knowledge in geometry, than M. d'Alembert seems to think necessary; conceiving that much reading in the mathematicks may hinder making discoveries, by so filling the head with foreign ideas, as to leave no room for the admission of new ones of our own ‖. This fancy is confuted by the examples of Vieta, Fermat, Huygens, Slusius, Barrow, and James Gregory, who, though they were great espousers of the synthesis, were justly esteemed the principal improvers of the modern geometry; and their thorough acquaintance with the writings of their predecessors, I presume, laid no restraint on their own invention: if his Hero Bernoulli had

* Encyclopédie, Tom. i. p. 677. † "At algebra quam "tradidere Theon, Apollonius, Pappus et alii veteres analystæ "geometrica est." Vietæ Op. p. 339. "Subjiciam igitur "aliquot problemata, quæ sub algebram non cadunt, ea-"que resolvam et componam methodo quæ veteres in re-"solvendis et componendis problematibus utebantur." Ghetaldus de Resolutione et Compositione Mathematica, printed at Rome in 1630. p. 330. I mentioned M. Fermat's authority above. ‡ In the preface to his book De Centro Gravitatis circuli, &c. ‖ Encyclopédie, Tom. i. p. 401.

imitated them, he would have ſaved himſelf five years puzzling about ſo eaſy a problem, as the determining the time of the ſhorteſt twilight *; ſeeing it already done with great elegance by Petrus Nonius †. And if he had duly conſidered the works of Sir Iſaac Newton, and others, and not depended ſo much on his own uninformed invention; he might, by gaining of time, and acquiring a better manner of writing, have made ſtill further improvements, and given a more elegant turn to his inveſtigations.

I COME laſtly to give ſome account of Sir Iſaac Newton's diſcoveries; not forgetting his defence, as occaſion ſhall offer.

M. de Buffon, at the beginning of his preface, has ſaid, " L'ouvrage dont on donne ici la Traduction " a été commencé en 1664 et achevé en 1671." But this is not exact; for Sir Iſaac Newton compoſed it in a very ſhort ſpace of time; juſt before Chriſtmas in 1671. He never finiſhed much above half of what he deſigned ‡. The reſolution he had taken towards the end of his life, of reviſing and augmenting it, being prevented by his death; it came into the hands of his heirs, in the ſtate it was originally written. They indeed would have put it forth, in the condition it was left; had not the bookſeller declined the terms, upon which it was offered him.

WE know from Sir Iſaac Newton himſelf ‖, that he was firſt led to his method of exhibiting by infinite ſeries the areas of curves not quadrable in finite terms, from conſidering the method Dr. Wallis propoſed in his Arithmetick of infinites.

* Eloge Hiſtorique de M. Jean Bernoulli, in M. d'Alembert's Melanges de Literature, &c. † Nonius de Crepuſculis, printed at Liſbon in 1542. ‡ General Dictionary, under the article Newton. See alſo Com. Epiſt. p. 165. ‖ Com. Epiſt. p. 159.

The Doctor for this purpose considered the subject thus. That if the ordinate of any curve consist of two terms, one of which was an invariable quantity, and the other some power of the abscifs; he shews, not only, how this curve, but others also, whose ordinates should be the several integral powers of this in succession, were to be measured; and then takes into consideration, how the areas might be expressed of curves consisting of fractional powers, which should be inserted intermediately between each of the terms of this series with integral powers, so as to compose a series of this form, 1, $\overline{1-xx}|^{\frac{1}{2}}$, $1-xx$, $\overline{1-xx}|^{\frac{3}{2}}$, $\overline{1-xx}|^{2}$, &c. For these Dr. Wallis found an infinite series to be necessary; and discovered such a series upon this principle for the mensuration of the circle. Sir Isaac Newton, in considering this method, observed, that by what is now usually called his Binomial theorem, the several terms, which compose the areas of the original series of quadrable curves, might be assigned: and concluded, that the same might be applied to the intermediate curves, with this difference, that in the curves, whose ordinates were an integral power of the first, the series would exhibit a certain finite number of terms; but when applied to the intermediate terms with fractional powers, the series would run on in infinitum. And this gave the first rise to this celebrated theorem.

But soon after he observed, that this theorem might more commodiously be applied to express those intermediate ordinates themselves; and then, in searching after a more direct proof of the truth of this theorem, he found, he could extend the methods of division and extraction of roots, taught in books of algebra, into the very same series; and appears to be the first, who thought of continuing on those operations in this manner: Dr.

 Wallis

Wallis obſerves of himſelf, that though he was not unappriſed of thoſe operations [in his treatiſe of Arithmetick he had deſcribed them *] yet this continuation of them had eſcaped him †.

These diſcoveries Sir Iſaac Newton made in a ſhort ſpace of time, between the years 1664 and 1665. Then alſo he found out a method of tangents, like that Sluſius afterwards produced, and how to determine the curvature of curves ‡.

In the ſummer of 1665, beſides employing himſelf in making trials upon the facility, wherewith his ſeries for meaſuring hyperbolick ſpaces might be applied to the computing logarithms to a great number of figures ‖; he firſt thought of his method of expreſſing univerſally the 59th propoſition of Dr. Wallis's Arithmetick of infinites by an indefinite index.

These diſcoveries, with others, were communicated to M. Leibnitz in 1676, by Sir Iſaac Newton's letters. In them was the binomial theorem, where it was ſhewn, how by it to raiſe any nomial whatever to any power, and to extract its root ‡‖; there alſo M. Leibnitz was inſtructed in the uſe of indefinite indexes; yet it is cuſtomary to attribute

* Cap. xx. reprinted in the 1ſt Vol. of his Op. Mathematica.

† "Non quod ego neſciverim diviſionem et radicum extractionem poſſe in ſpeciebus inſtitui: ſed quod in hujuſmodi caſibus res abitura ſit in infinitum, pariter ac ſi velim fractionem $\frac{1}{3}$ ad decimalem redigere 0.3333 &c. aut numeri non quadrati 2 radicem quadraticam exquirere 1.4142 &c." Walliſii Op. Math. Vol. II. p. 376.

‡ General Diction. Art. Newton. Concerning this curious ſpeculation it may be obſerved, that Apollonius had in his 5th book determined the point, where only one perpendicular could be drawn to the conic-ſection; and this point is the center of the circle, which is now conſidered as of equal curvature with the conic-ſection at the point, where the perpendicular falls: and Kepler [p. 76. in his Paralipomena to Vitellio] makes expreſs mention of theſe circles in the parabola, and obſerves them to be different in different parts of the curve.

‖ Comm. Epiſt. p. 164.

‡‖ Ibid. p. 132.

their

their introduction into analysis also to that gentleman, even prior to Sir Isaac Newton *.

As to the invention of series, there has been raised some doubts. It is said, Sir Isaac Newton himself has acknowledged, that he found them out at first by very indirect means †.

How he discovered and compleated that invention has been above related; all which he accomplished, three years before Lord Brounker and Mr. Mercator published their quadratures of the hyperbola by a series ‡. The former indeed had found out that series long before; for it is mentioned by Dr. Wallis in 1656 ‖; but such a series is not a general method. As to Mr. Mercator, he lived 16 years after, and made no farther progress or pretensions ‡‖. He could not but know, what Sir Isaac Newton had done, either from Mr. Collins, or at least from Sir Edward Sherburne's Translation of the Sphere of Manilius (which he had read ‖†), where, in the Appendix p. 116, Sir Isaac Newton's inventions are mentioned; and indeed Mr. Mercator kept a correspondence ** with Sir Isaac Newton himself; and in his excellent Astronomy, printed in 1676, at p. 286, speaking of the librations of the moon, he says, "Harum tam variarum atque implicitarum librationum causas hypothesi elegantissimâ explicavit nobis Vir cl. Isaac Newton, cujus humanitati hoc et aliis nominibus plurimum debere me lubens profiteor." Sir Isaac Newton's additions to Mercator's Astronomy were mentioned in the Appendix to Sherburne's Manilius, printed in 1675.

* Dict. Universelle de Mathematique et de Physique par M. Saverien à Paris 1753. Tom. i. p. 361. §. 5. † Ibid. p. 423. ‡ Com. Epist. p. 95, 103, 130; and also General Dict. Art. Newton. ‖ Oper. Math. Tom. i. p. 232. ‡‖ Philos. Trans. N° 342. p. 174. Or Com. Epist. p. 3. ‖† See the Preface to his Astronomy. ** Princip. Prop. xvii. Lib. iii. of the last edition.

In the year 1665, Sir Isaac Newton also found out the direct method of fluxions, and the following year the inverse method, as likewise the theory of light and colours, and considered the effects of gravity on the moon, and on the planets gravitating towards the sun *.

But as to the invention of fluxions, Sir Isaac Newton himself, in answer to what M. Leibnitz had boasted of his own discoveries, has said, " And am " not I as good a witness that I invented the " methods of series and fluxions in the year 1665, " and improved them in the year 1666, and that I still " have in my custody several mathematical papers, " written in the years 1664, 1665, and 1666, some " of which happened to be dated; that in one of " them, dated the 13th of Novemb. 1665, the direct " method of fluxions is set down in these words.

" PROB. An equation being given, expressing " the relation of two or more lines x, y, z &c. " described in the same time by two or more " moving bodies A, B, C &c. to find the relation " of their velocities p, q, r &c.

" Resolution. Set all the terms on one side of " the equation, that they become equal to nothing. " Multiply each term by so many times $\frac{p}{x}$ as " x has dimensions in that term. Secondly, Mul- " tiply each term by so many times $\frac{q}{y}$ as y hath " dimensions in it. Thirdly, Multiply each term " by so many times $\frac{r}{z}$ as z hath dimensions in it, " &c. The sum of all these products shall be " equal to nothing. Which equation gives the

* " Aussi grand au moins par ses experiences d'optique que " par son systême du monde," says M. d'Alembert, speaking of Sir Isaac Newton in describing his philosophy. Encyclop. Tom. vi. p. 298.

" rela-

" relation of p, q, r &c. And that this resolution " is there illustrated with examples and demon- " strated, and applied to problems about tangents, " and the curvature of curves. And that in an- " other paper, dated the 16th of May 1666, a " general method of resolving problems by motion " is set down in seven propositions, the last of " which is the same with the problem contained " in the aforesaid paper of the 13th of Novemb. " 1665. And that in a small tract written in " Novemb. 1666 the same seven propositions are " set down again, and that the seventh is improved " by shewing how to proceed without sticking at " fractions or surds, or such quantities as are " now called *Transcendent*. And that an eighth " proposition is here added, containing the inverse " method of fluxions so far as I had then attained " it, namely, by quadratures of curvilinear fi- " gures, and particularly by the three rules upon " which the *Analysis per æquationes numero termi-* " *norum infinitas* is founded, and by most of the " theorems set down in the *scholium* to the tenth " proposition of the book of quadratures. And " that in this tract, when the area arising from " any of the terms in the valor of the ordinate " cannot be expressed by vulgar analysis, I repre- " sent it by prefixing the symbol ▭ to the term. " As if the *abscissa* be x, and the ordinate $ax -$ " $b + \frac{bb}{a+x}$, the area will be $\frac{1}{2}axx - bx + ▭$ " $\frac{bb}{a+x}$. And that in the same tract I sometimes " used a letter with one prick for quantities in- " volving first fluxions; and the same letter " with two pricks for quantities involving second " fluxions *. And that a larger tract which I

* In his problem for determining the curvature of curves.

" wrote

" wrote in the year 1671, and mentioned in my
" letter of the 24th of Octob. 1676 *, was founded
" upon this smaller tract, and began with the
" reduction of finite quantities to converging
" *series*; and with the solution of these two pro-
" blems: 1. *Relatione quantitatum fluentium inter*
" *se data, fluxionum relationem determinare.* 2. *Ex-*
" *posita æquatione fluxiones quantitatum involvente,*
" *invenire relationem quantitatum inter se.* And
" that when I wrote this tract, I had made my
" analysis composed of the methods of series and
" fluxions together, so universal, as to reach to
" almost all sorts of problems, as I mentioned in
" my letter of the 13th of June 1676 †, and this
" is the method described in my letter ‡ of the
" 10th of December 1672 ||."

About the year 1708, all the papers of Mr. John Collins, who had kept a correspondence with the most eminent geometers in Europe, fell into the possession of Mr. William Jones, then a teacher of the mathematicks in this city. Amongst them were copies of some treatises of Sir Isaac Newton, particularly of his Analysis per æquationes numero terminorum infinitas ‡||. These Mr. Jones communicated to their author, who thereupon lent him the abovementioned tracts, which Mr. Jones transcribed, and used to distribute fragments of them to his scholars. So that many such are in the hands of different persons.

Amongst them was a small tract divided into two parts. In the first leaf was written "Con-" structiones Geometricæ Æquationum per D. Isaac" Newtonum. Ex Apographo Dni Collins."

The first part is a sort of Elements of geometry,

* Commerc. Epistolic. p. 165. † Ibid. p. 141.
‡ Ibid. p 105. || Raphs. Hist. Fluxion. p. 115. Or Recueil &c. Vol. II. p. 87. ‡|| Phil. Transf. N° 342. p. 176. Or Com. Epist. p. 5.

where

where motion is introduced, in order to effect several problems after a different manner from what is common, and there is given constructions of solid problems (some of which are transcribed into his Algebra) by placing a streight line of a given length between two other lines given in position, so as to pass through a given point, &c. and from the facility, wherewith this may be performed mechanically, by moving the given line between the others, till it arrives at the position required; he proposes to admit into geometry such motions, as an additional postulate to those of the elements.

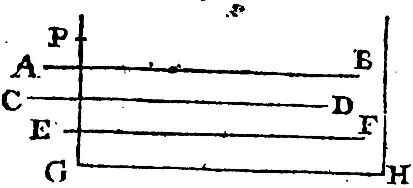
The second part, which answered to the title, somebody had prefixt to the whole, delivered in eight problems the constructions of quadratic, cubic, biquadratic, and equations of higher powers by the circle, conic-sections, and cubical parabola. The 9th and last problem was this, "Quomodo problemata solvenda sunt, ubi per intricatam terminorum complicationem non licet ad æquationes commode pervenire;" but the solution was wanting. And indeed this copy was very imperfect; as I learn from some passages extracted from another manuscript. The construction by the cubical parabola * Sir Isaac Newton speaks of in a letter to Mr. Collins, dated July 13, 1672 †.

As some of these constructions are particularly accommodated for obtaining the first figures of the roots of equations ‡; so Sir Isaac Newton found out two other expedients for the same purpose. The first of these he takes notice of in the abovementioned letter to Mr. Collins, and it was described by Mr. Oldenburgh in a letter to M. Leibnitz, dated the 24th of June 1675 ‖. But I shall here give a particular account of both.

If

* Enumeratio lin. tertii ordin. Sect. vii. † General Dictionary Art. Newton, p. 782. ‡ Newton Arith. Univers. p. 285. ‖ "Dominus *Newtonus*, beneficio "logarithmorum graduatorum in scalis *παραλλήλως* locandis ad "distantias

If within a frame ſeveral equal lines of logarithmetick numbers, ſuch as are deſcribed on Gunter's



ſcale, AB, CD, EF and GH are placed equidiſtant from one another, and all but the laſt GH upon ſeparate moveable ſliders, and upon the fixt ſide of the frame a point P be placed at the ſame diſtance from the firſt of thoſe lines. Then, when the beginning of all theſe lines ſtand directly under the point P, a ruler laid from P to any number upon the firſt line AB ſhall mark upon the ſecond line CD the ſquare of that number, upon the line EF the cube, and upon GH the fourth power of the ſame. But if any one of the ſliders be moved backwards, the ruler ſhall mark upon that ſlider the reſpective power of the forementioned number multiplied by the number, which in this ſlider in its preſent ſituation ſtands directly under the point P.

Therefore any equation $z^4 - az^3 - bz^2 + cz = m$ being given, if the ſlider EF be moved back, till in that ſlider the number a ſtand directly under the point P, and if the number b in the ſlider CD, and the number c in the ſlider AB, be likewiſe brought under P, then, if the ruler laid from P mark upon GH the true value of z^4, it will mark upon EF the value of az^3, upon CD the value of bz^2, and upon AB the value of cz, and theſe collected together

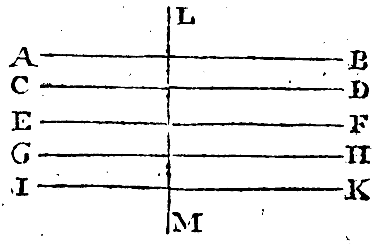
" diſtantias æquales, vel circulorum concentricorum eo modo " graduatorum adminiculo, invenit radices æquationum. Tres " regulæ rem conficiunt pro cubicis, quatuor pro biquadraticis. " In harum diſpoſitione reſpectivæ cœfficientes omnes jacent in " eadem linea recta; a cujus puncto tam remoto a prima re- " gula ac ſcalæ graduatæ ſunt ab invicem, linea recta iis ſuper- " extenditur, una cum præſcriptis conformibus genio æquationis, " qua in regularum una datur poteſtas pura radicis quæſitæ." Comm. Epiſt. p. 123.

under

under their proper ſigns will be equal to m; but when the numbers markt out by any ſituation of this ruler, being thus collected together, are either greater or leſs than m, then the ruler does not mark upon the line GH any true value of z^4, but by moving its ſituation, and collecting the numbers markt upon the ſliders, all the values of z^4, and thence all the roots of the equation, may by repeated trials be found.

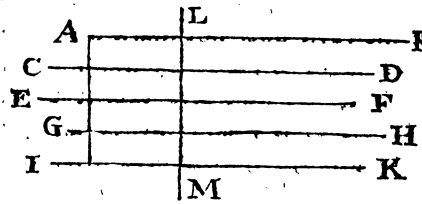
His ſecond method, which is more commodious, was thus.

If within a frame ſeveral lines of numbers are placed after this manner. Suppoſe the firſt AB fixt upon the ſide of the frame, but the reſt upon parallel ſliders, the firſt of which CD ſhall be the ſame with AB, the ſecond EF ſhall have its diviſions but half as long, the



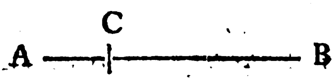
diviſions of the third GH one third, and the diviſions of the fourth IK one fourth, of the diviſions of the firſt; then, when the beginning of theſe lines ſtand under each other, if any line LM be drawn perpendicularly croſs them, it ſhall mark upon the firſt of the ſliders the ſame number as on the fixt line, upon the ſecond ſlider the ſquare of that number, upon the third the cube, and upon the fourth the fourth power of the ſame. But if any one of theſe ſliders be moved backwards, then ſhall the tranſverſe line LM mark upon that ſlider the reſpective power of the forementioned number multiplied by the number, which in this ſlider, in its preſent ſituation, ſtands directly under the point A, the beginning of the fixt line.

Now let any equation be given $az + bz^2 - cz^3 + dz^4 = m$. If the ſlider CD be moved back, till in in that slider the number a stand directly under A; and if the number b in the slider EF, and the number c in the slider GH, and the number d in the slider IK, are also brought under A; then, if the transverse line



LM mark upon the line AB the true value of z, the root of the equation, it will give upon CD the value of az, upon EF the value of bz^2, upon GH the value of cz^3, and upon IK the value of dz^4; insomuch that the numbers markt upon the sliders by the transverse line LM, collected together under their proper signs, shall be equal to m.

But what Sir Isaac Newton had done by these sliding rules, may be effected by any single line of artificial numbers, though not so expeditiously; for if AB be such a line, in which the number expressing the value of the



root z of the foregoing equation be at C, the distance AC taken from the number a will give upon this line az, and double this distance taken from b will give bz^2, and triple the same distance taken from c will give cz^3, and four times the distance taken from d will give dz^4. Which indeed may be performed most commodiously by these numbers inscribed on a circle after Oughtred's manner *.

There were also papers containing problems relating to the center of gravity; and to curves intersecting right lines given in position in given angles; and several examples of finding fluents from fluxions according to what is said at the end of the quadratures, "Fluens pro lubitu assumi potest, et assumptio

* See his Treatise, called The Circles of Proportion, &c. printed at London in 1633.

"cor-

"corrigi, ponendo fluxionem fluentis assumptæ "æqualem fluxioni propositæ, et terminos homo- "logos inter se comparando."

There was one paper intitled, Notæ in Act. Erudit. An. 1689, 1706. This ended thus, "Prop. 19, col- "ligitur ex duabus, falsis propositionibus 12ª et 15ª: "Nam 12ª non valet nisi in circulo viribus concen- "trico, et prop. 15. falsissima est. Leibnitzius "igitur non invenit prop. 19. per calculum differen- "tialem, sed inventam computare conatus est, ut "suam faceret, et computando bis erravit *." But these paralogisms of M. Leibnitz have been quoted even as preferable to Sir Isaac Newton's most clear and accurate demonstrations. After M. Leibnitz had been shewn these paralogisms, he was far from acknowledging his mistakes, saying, "Paralogismus, "quem mihi Keilius imputat, nihil est, et redit ad "*modum loquendi.*" Epist. Leibn. ad Bern. Tom. ii. p. 347. What a judge of demonstration was this gentleman!

The said Mr. Jones also gave to Dr. Pellet a copy of the small tract of fluxions, written by Sir Isaac Newton in Nov. 1666. This copy of Dr. Pellet a friend lent me, and I found it agreed exactly with Sir Isaac Newton's description.

As it is in English, had it been published †, some mistakes, that have been made about the nature of fluxions, perhaps occasioned by Sir Isaac Newton's brevity ‡, might have been avoided.

Here the author often calls the fluxions of lines, as well as of other quantities ‖, the *velocities* of *increase*; the *velocities* with which they *increase*, &c. as will appear from the following quotations.

* Philos. Trans. N° 342. p. 208. Or Comm. Epist. p. 41. See also Journal Literaire 1714. p. 348, &c.

† Why he did not publish this tract may be learnt from the Commercium Epistolicum, p. 164; he not thinking himself then at an age proper for writing.

‡ Discourse, §. 4.

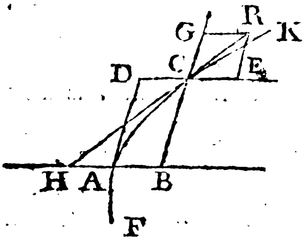
‖ Dissertation, §. 6.

In

In the scholium to his problem for finding fluxions he says, "Note, that if there happens to be in any "equation either a fraction, or surd quantity, or a "mechanical one (*i. e.* which cannot be geometri-"cally computed, but is expressed by the area or "length or gravity or content of some curve or "solid &c.) to find in what proportion the un-"known quantity *increases* or *decreases* do thus." What follows is suitable to what is delivered in the Treatise written in 1671. Prob. 1. §. §. 5—12.

In the Problem for drawing tangents it is said, "With what *velocity*, they" [the flowing lines] "increase or decrease—."

Again, "If the *celerity* of the *increasing* or fluxion of AB be called p, and of BC be q, I make CE : "GC :: p : q :: (CE : "ER ::) HB : CB; and "the point C shall move "in the diagonal line CR "(by Lem. 7.) which is "therefore the required "tangent.



"Now the relation of "p and q may be found "by the foregoing equation * (p signifying the in-"crease or fluxion of x and q of y) to be $4px^3 -$ "$9pyx^2 + 2payx - 2py^3 - 3qx^3 + qax^2 - 6qy^2x$ "$- 4qy^3 = 0$. And therefore $HB = \frac{py}{q} =$ "$\frac{3yx^3 - ayx^2 + 6y^3x + 4y^4}{4x^3 - 9yx^2 + 2ayx - 2y^3}$; which determines the "tangent.

"Hence may be observed this general Theorem "for drawing tangents to curve lines thus referred "to streight ones. Multiply the Terms of the "Equation, &c." See the abovementioned Trea-"tise, Prob. iv. §. 4.

* viz. $x^4 - 3yx^3 + ayx^2 - 2y^3x + a^4 - y^4 = 0$.

"Again,

Again, in his example of curves described by the interfections of right lines revolving about centers, he says, "That is the celerity of the *increase* "of *x* being called *p* and of *y* being *q*, DE : EF ":: *p* : *q*. Then shall the diagonal CE be the required tangent." See the abovementioned Treatise, Prob. iv. §. 39.

In the Problem for determining the curvature of curves are these expressions, "The *velocity* of the "*increase*, or fluxion of *v*," and "The motion "of the point C from B (that is the *velocity* with "which *y* = CB increases) will be *q*."

In the Problem about Quadratures, he says, "The *velocities* with which they" [areas] "*increase*, "will be as BE to BC" [ordinates]. Again, "The *velocities* with which the areas ABC and "DEF increase."

Now some of these expressions are rendered in Latin thus. In the Principia, "*Velocitates incrementorum* ac *decrementorum* *;" in Dr. Wallis's Works †, "Per earum fluxionem intelligit" [Newtonus] "*celeritatem incrementi* vel *decrementi*;" and in the Quadratures, "*Incrementorum velocitates*," and "*celeritates crescendi*."

But the expression *Velocitates incrementorum* ac *decrementorum* being usually translated, The *velocities* of the *increments* and *decrements* ‡, has occasioned Sir Isaac Newton to be misunderstood by the unskilful ‖; they not attending to his expression *Celeritates crescendi*, which might have pointed out to them the real sense of the other expressions ‖†.

* Lib. 2. Lem. 2. p. 244. and Dissertation, §. 77.

† Vol. II. p. 391.

‡ See Mr. Motte's Translation of the Principia, as also the Translation of the Quadratures in Dr. Harris's Lexicon Technicum, Vol. II.

‖ Discourse, §. 87. Account, §. 4, 5. Review, §. 1. Dissertation, §. 6, 97.

‖† Thus his expression *quantitates nascentes* having been always translated *nascent quantities*, has led many into mistakes. See Discourse, §. 107. Dissertation, §. 72.

In this Tract Sir Isaac Newton determines the relations of fluxions both in the manner he had done in his paper of the 13th of Novemb. 1665, and also by multiplying the terms of the equation by arithmetical progressions. In the demonstration he used indivisibles *; saying in the premised Lemma, "And though they" [the bodies or points describing the flowing lines] "move not uniformly, "yet are the *infinitely little* lines, which each moment they describe, as the velocities, which they "have, while they describe them." But in the following Problems he makes no use of indivisibles at all, nor of his method of prime and ultimate ratios †, ever expressing fluxions by finite right lines, that are to one another in the same proportion with them ‡.

As in drawing tangents, he supposes no part of the tangent to coincide with the curve, but to lie in the diagonal of a parallelogram, whose finite sides are to each other as the fluxions of the varying lines, by whose intersections the curve is described. Hence in his description of fluxions, in the Introduction to the Quadratures, he says of them; "exponi autem possunt per lineas quascunque quæ "sunt ipsis proportionales." And he seems to have had this old tract in his eye, when he said

* Account, §. 45. Dissertation, §. 93. † Ibid. §. 10, 93.

‡ In his papers concerning the center of gravity, he indeed uses indivisibles: as in the second Problem, which is this, "To find such plane figures, which are equiponderate to any "given plane figure in respect to an axis of gravity in any "given position," he thus expresses himself, "The *motions* "whereby *x* and *y* do *increase* or *decrease*, I do call *p* and *q*. "Now the ordinates multiplied into their motions *p* and *q* "may signify the *infinitely little* parts of those areas, which "each moment they describe, which *infinitely little* parts do "equiponderate (per Lem. 1 & 2.) if they multiply'd by their "distances from the axis AK do make equal products;—And "if all the respective *infinitely little* parts do equiponderate, the "superficies must do so too."

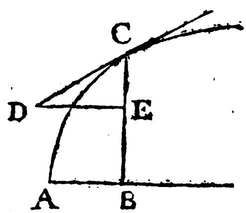
(speak-

(speaking of the comparison of his method with that of M. Leibnitz) "It is more elegant, because in "his calculus there is but one *infinitely little* quan"tity represented by a symbol, the symbol o. We "have no ideas of infinitely little quantities, and "therefore Mr. Newton introduced fluxions into his "method, that it might proceed by finite quan"tities as much as possible *."

Next he determines "The quantity of the "curvature of lines" by finding "that point fixed "in the curve line's perpendicular, which is then "in least motion; "for" [says he] "it is the "center of a circle which passing through the given "point is of equal curvature with the line at "that given point." Here the velocities of the points in the radius of the curvature intersecting the curve and the abscisse are most ingeniously expressed by finite right lines, that are used in the solution of the problem.

Thence he determines the points between the concave and convex portions of curves, as also where they are most or least curved.

The methods of squaring and rectifying curves are there given after the same manner, as in his book written in 1671. But there is one problem relating to the latter subject thus delivered.



To find the nature of a curve whose length is expressed by any given equation (when it can be.)

RESOLUTION.

Let AB, BC, AC be expressed by x, y, z, and their *motions* by p, q, r, respectively, and let the relation between x and z be given. Then finding the relation between p and r, make $\sqrt{rr - pp} = q$;

* Phil. Trans. N° 342. p. 205. Or Com. Epist. p. 38.

for drawing CD a tangent to the curve at C, and DE perpendicular to BC, the lines DE, EC, DC, shall be as p, q, r; but $\sqrt{DC^2 - DE^2} = EC$, therefore $\sqrt{rr - pp} = q$. Now the ratio between x and $\frac{p}{q}$ being thus found, find y, and the relation betwen x and y determines the nature of the curve.

In this discourse Sir Isaac Newton finds infinite series's by division and extraction of roots. But as he here for extracting the roots of adfected equations refers to Vieta's Exegesis; he seems not to have then discovered the Compendium, which he found out a very little time after, and fully described in the Tract, that was sent to Mr. Collins in 1669 *.

It has been lately said † of Sir Isaac Newton, — "il a trouvé le calcul differentiel, en ne faisant "que généraliser la méthode de Barrow pour les "tangentes." Whereas Sir Isaac Newton found out his method by considering the proportions of the velocities of increasing, or fluxions of quantities generated together by motion. These proportions he demonstrated in the beginning by indivisibles, and afterwards by his method of prime and ultimate ratios. It was Mr. Robins, who first demonstrated them by exhaustions.

Again, as it has been supposed, that M. Leibnitz indeed deduced the differential calculus from Dr. Barrow's method of tangents; it has been concluded ‡ "en ce cas ce ne seroit, ni Newton, "ni Leibnitz, ce seroit Barrow qui auroit trouvé "le calcul differentiel." But is this logic? For though we believe, M. Leibnitz by the hints given him in Sir Isaac Newton's letters, was enabled to deduce his method from that of Dr. Barrow; yet we know, that Sir Isaac Newton had discovered

* Comm. Epist. p. 75. † Encyclopédie, Tom. vii. p. 119. ‡ Ibid. Tom. iv. p. 988.

his

his method about five years before Dr. Barrow's Geometrical Lectures were publiſhed *.

And Dr. Barrow approved of theſe inventions, and was ſo well ſatisfied with young Newton's ſuperior genius, that he gladly ſubmitted his own works to his examination and correction †; and reſigning to him the Profeſſorſhip of the Mathematicks, he wholly dedicated his future ſtudies to Divinity; which produced thoſe moſt excellent diſcourſes, that have acquired him a univerſal and laſting fame.

Sir Iſaac Newton has often by foreigners been repreſented as a ſcholar of Dr. Barrow; but this is a miſtake, he having had no inſtructor (as indeed in theſe ſtudies is often the caſe) and falling on mathematical treatiſes by chance, ſuch as were very improper for a beginner; yet without any one's aſſiſtance, by his own unequalled ſagacity and invention, he not only overcame the difficulties of the ſubjects, increaſed by the manner, they were delivered in the books he read; but immediately made improvements, and very ſoon laid the foundation for all the marvellous diſcoveries, he afterwards produced. As he was very rapid in the courſe of his inventions; ſo he ſeems to have been from time to time tired in their purſuit ‡; and to have reſumed thoſe ſtudies at two or three repriſes only. Inſomuch that a ſmall portion of his long life was employed in theſe ſpeculations.

But to return to our narration; Mr. Jones alſo gave the ſaid Dr. Pellet a copy of Sir Iſaac Newton's Treatiſe of ſeries and fluxions, written in 1671 ||.

* If we may preſume to gueſs, perhaps Sir Iſaac Newton took the hint of his method of fluxions from what Merſennus ſaid of M. de Roberval's inventions. See his Balliſtic. p. 115.

† Barrow. Lect. Optic. in the preface. ‡ Comm. Epiſt. p. 141. || He alſo let Dr. Taylor ſee this Treatiſe, whence it is referred to in his Methodus Incrementorum, p. 64.

This was deficient in several places; for Mr. Jones was wont to curtail or otherwise disguise the papers, he communicated to his scholars, that none might make out a compleat book.

The translation Mr. Colson has published of this treatise was from Mr. Jones's own copy; which, I believe, was very perfect, as far as Sir Isaac Newton had at first composed it; as well as I can remember from my having read many years ago the original manuscript, when it was in my friend Dr. Pemberton's custody.

In this treatise Sir Isaac Newton chose the method of determining the relation of fluxions, by multiplying the terms of the equation by arithmetical progressions, which he had before described in his abovementioned Tract written in November 1666. "This indeed," says the Translator in his Comment, p. 242, "is not so short as the method of "taking fluxions, which he elsewhere delivers *, "and which is commonly followed; but it makes "sufficient amends by the universality of it, and "by the great variety of solutions it will afford. "For we may derive as many different fluxional "equations from the same given equation, as we "shall think fit to assume different arithmetical "progressions."

M. Van Hudden in his Tract, printed at the end of the first volume of Cartes's Geometry in 1659, on the maxima and minima, after having given a rule † for determining these from an equation containing one variable quantity only, which is altogether similar to Sir Isaac Newton's first method of taking fluxions; subjoins the same remark Mr. Colson has here made, that by the use of such arithmetical progressions [as Sir Isaac Newton proposes] the method would be rendered more general and useful ‡. But

* In Dr. Wallis's Op. Math. Vol. II. p. 392; and in the Quadratures. † Pag. 511. ‡ Ibid. p. 513.

this

this implex method Sir Isaac Newton afterwards laid aside, observing the other to be preferable *, and for this he had very good reason: for in reality nothing more is effected by these different progressions, than combining with the simple form of the fluxionary equation some multiple of the original equation divided by the flowing quantity.

HERE, as in the Analysis per æquationes numero terminorum infinitas, moments, as infinitely small parts, are introduced; accordingly in Dr. Wallis's works it is said †, "quamvis *fluentes quantitates* et "earum *fluxiones* prima fronte conceptu difficiles "videantur, earum tamen notionem cito faciliorem "evasuram putat [Newtonus] quam sit notio *mo-* "*mentorum* aut *partium minimarum* vel *differentiarum* "*infinite parvarum*.—Attamen non negligit theoriam "talium partium—." Hence Sir Isaac Newton, in the Introduction to his Quadratures, says, "Peragi "tamen potest analysis in figuris quibuscunque seu "finitis seu infinite parvis quæ figuris evanescentibus "finguntur similes, ut in figuris quæ per methodos "indivisibilium pro infinite parvis haberi solent, "modo caute procedas."

THIS tract Sir Isaac Newton intended to finish and publish with his Lectiones Opticæ ‡, that he was then reading in the university of Cambridge, and wherein he had mentioned it ||. But disputes arising from what was already extant of his on the subject of light and colours in the Philosophical Transactions, he resolved not to print those Lec-

* In his remarks on the Judicium primarii Mathematici, he observes the method delivered in his Quadratures is the most perfect of any in these words; "Methodus fluxiones omnes "capiendi, seu [in the style of M. Leibnitz] differentiandi "differentialia habetur in Propositione prima Libri de Quadraturis: et est verissima et optima." Comm. Epist. p. 249.

† Vol. II. p. 391.

‡ Second letter to Mr. Oldenburgh in Comm. Epistolic. p. 165.

|| Part. i. Sect. iv. Prop. 33, 34.

tures *. However, his Treatiſe of ſeries and fluxions was to accompany a tranſlation out of Dutch of Kinckheyſen's Algebra †, that Mr. Pit, an eminent bookſeller of London, had procured; and which Sir Iſaac Newton was deſired to improve and publiſh. But he at length, not liking this propoſal, inſerted many of his improvements in the public Lectures, intended to contain a general ſyſtem of algebra, which he read about the year 1675.

In that Treatiſe, he mentions prime and ultimate ratios ‡; but he never finiſhed it, being then employed in diſcovering ſome of the propoſitions, that make the ſubjects of his Principia, and in writing his Treatiſe of Quadratures ‖; a caſe of the 5th propoſition of which, together with the forms of curves, that might be compared with the conic-ſections, as alſo cyphers, containing the two firſt problems of his Treatiſe of ſeries and fluxions, written in 1671, with a deſcription of his method of extracting the fluents out of equations involving fluxions, and alſo of aſſuming an arbitrary ſeries, were ſent to M. Leibnitz, in a letter dated Octob. 24, 1676 †‖. Notwithſtanding all which M. Leibnitz has, even long after the cyphers had been explained, aſſerted, he believed Sir Iſaac Newton at that time had no knowledge of the characteriſtic and algorithm of fluxions ‡‖, that is, he was ignorant of what he had written in a paper ſo long ago as the 13th of November, 1665.

Whereas Sir Iſaac Newton has truly ſaid, that he had then made his method of fluxions much more univerſal, than the differential method of M. Leibnitz is at preſent. "For when the method of

* Comm. Epiſt. p. 165. † Ibid. p. 101. ‡ Pag. 236, 237, of the ſecond edition at London, in 1722. ‖ Phil. Tranſ. N° 342. p. 202, 206. Or Com. Epiſt. p. 34, 40. †‖ Com. Epiſt. p. 166, 167, 173, 188. ‡‖ Raphſ. Hiſt. Fluxion. p. 97. or Recueil de Divers Pieces, &c. Tom. ii. p. 4.

"fluxions

" fluxions proceeds not in finite equations, he re-" duces equations into converging series by the " binomial theorem, and by the extraction of " fluents out of equations involving or not in-" volving their fluxions. And when finite equa-" tions are wanting, he deduces converging series " from the conditions of the problem, by as-" suming the terms of the series gradually, and " determining them by those conditions. And " when fluents are to be derived from fluxions, " and the law of the fluxions is wanting, he finds " the law *quam proxime* by drawing a parabolick " line through any number of given points *." All this, together with the comparing curves with others, that are more simple, is shewn in the small treatise entitled Analysis per series, fluxiones et differentias quantitatum, published by Mr. Jones at London in 1711.

Thus Sir Isaac Newton had been successively, by sundry accidents and his own unwillingness to appear in publick †, prevented hitherto from publishing his treatises on fluxions. This delay was not the effect of any formed resolution to conceal his inventions, that he might be the more admired in what he should be enabled to perform by their assistance in any future work: it does not appear, that he had any express intention of writing his Principia, till Dr. Halley accidentally suggested to him that design.

On the coming out of that immortal book, these discoveries of Sir Isaac Newton became to be much talked of; so that Dr. Wallis pressed their author to publish something further of his method of

* Phil. Transf. N° 342. p. 193. Or Comm. Epist. p. 25.

† " Je ne parle point ici de l'art avec lequel il [Newton] " avoit caché sa méthode des fluxions, la chef de toutes ses " sçavantes recherches—." Says M. Clairaut in the Mem. of the Royal Academy of Sciences for 1745. p. 329.

fluxions;

fluxions; which occasioned Sir Isaac Newton to send to him, with other things relating to the subject, the first proposition of his Treatise on the quadratures of curves, wherein is determined the several orders of fluxions. These particulars the Doctor printed in the second volume of his mathematical works in 1693 *; three years before M. Leibnitz had given his rules for finding those orders in differentials.

NOTWITHSTANDING this, it was said in the preface to the Analyse des infiniment petits in 1696 by M. de Fontenelle (who, we are more than once told †, wrote that preface), "Mais le caracteristique de M. *Leibnitz* rend le sien beaucoup plus "facile et plus expeditif; outre qu'elle est d'un "secours merveilleux en bien des rencontres ‡." Does not this answer to what M. d'Alembert has said on another occasion, "Notre fureur d'ecrire "avant que de penser, et de juger avant que de "connoître ||." For M. de Fontenelle had no reason to give the preference to M. Leibnitz's method on any account whatever, unless he had persuaded himself, that a *d* was more easily writ than a tittle.

BUT this partiality is still kept up, and one of the authors of the Journal des sçavans †||, in giving an account of Mr. Maclaurin's book, has thought fit to say, "M. Leibnitz a répresenté les *differences* "par la caracteristique *d*, ce qui est infiniment com-"mode." This fancy can only arise from having been accustomed to that letter; and so, on the contrary, others think Sir Isaac Newton's characteristics for distinguishing the different orders of fluxions

* Pag. 392. † Encyclopédie, Tom. i. at Paris in 1751. p. 677. and Tom. vii. Ibid. An. 1757. p. 630. ‡ See Philos. Transact. N° 342. p. 204. Or Com. Epist. p. 37.

|| Encyclop. Tom. v. in the Advertisement, p. 4. at Paris in 1755. †|| For the month of August 1750.

by

by as many points. to be much more commodious *.

There has indeed of late been alledged important reasons for the preference of that of M. Leibnitz. To make this the more apparent, there has been presented to our view a symbol with a dozen points over it, to denote a fluxion of that order; as if any such order would ever be used, or that 12 points would not be as easily told, as 12 *ds*.

If it is not ridiculous to be serious upon so trifling an object, may it not be reasonably urged against the use of this letter *d*, that it took its rise from the term and idea of differentials, which are either erroneous quantities, or have no being in nature; so that *dx*, *dy*, &c. were introduced to denote false quantities not bearing the proportions to one another, that were required; or imaginary quantities incapable of any proportion.

But it is asserted, "Introduire ici le movement, "c'est y introduire une idée étrangere, et qui n'est "point nécessaire à la demonstration: d'ailleurs on "n'a pas d'idée bien nette de ce que c'est que la "vîtesse d'un corps à chaque instant, lorsque cette "vîtesse est variable. La vîtesse n'est rien de réel; "c'est le rapport de l'espace au temps, lorsque la "vîtesse est uniforme. Mais lorsque le movement est "variable, ce n'est plus le rapport de l'espace au "temps, c'est le rapport de la differentielle de "l'espace à celle du temps; rapport dont on ne "peut donner d'idée nette, que par celle des *limites*. "Ainsi il faut nécessairement en revenir à cette "derniere idée, pour donner une idée nette des "fluxions." Encyclopédie, Tom. vi. p. 923.

That motion is not foreign to geometry, we know from the example of the ancients, who ad-

* Philos. Trans. N° 342. Or Comm. Epist. p. 30. And Journ. Liter. for July and August, p. 342.

mitted

mitted it in the descriptions of figures *; and Proclus has said, "that this science takes cognisance of "magnitudes and figures, and of the limits attending them, and of the ratios they bear to each "other, and of the affections, various positions, "and motions, to which they are subject †."

THE introducing motion into geometry may be of great use. Some have thereby endeavoured to overcome the difficulty about parallel lines, which has been so much canvassed amongst the mathematicians. And after the great Galileo had shewn, how bodies descended with an accelerated velocity, whereby being projected obliquely, they described in their motions parabolas; the geometers, as Torricellius, Roberval and others began to consider the effects of such motions in the descriptions of curvilinear figures, and applied that idea to the determining their tangents, &c. See Torricellius de Motu gravium, p. 121. Lettera di Timauro Antiati [Sign. Dati] p. 14. Mersen. Ballistic. p. 115. Dettonville [M. Pascal] de la Roulette, p. 6.

THIS gentleman in his second objection contradicts himself, first in asserting that there is nothing real in velocity, which is as much as to say, that it has no existence in nature, and at the same time pretends to define it, though indeed in terms, that have no real meaning; for he tells us, that velocity is the proportion of space to time, thus confounding the two separate ideas of velocity and proportion, and without specifying what space and what time he means; nor is it very necessary to inquire, for however explained, the definition is absurd; since space and time being heterogeneous quantities, are incapable of bearing proportion one to the other.

* See Sextus Empericus, Ed Genev. p. 96, 97. Also Barrow, Lect. Mathem. An. 1664. Lect. iv. p. 69. Newton's Quadratures, in the Introduction; and Philos. Trans. N° 342. p. 205. Or Com. Epist. p. 38. † Proclus in Euclid. lib. 11. p. 16. at Basil in 1533.

The idea of motion, and its affection velocity, are very distinct from the ideas either of time or space; for both of these may be present to the mind without the other. True it is, that different degrees of velocity in the motion of any body are in the same proportion to each other, as the spaces, which would be described by those respective velocities, were they uniformly continued for any one specified portion of time, whether those velocities remained long enough uniform for the moving body actually to describe those spaces or not. And how such finite spaces are to be applied for measuring the degrees of velocity at each point of time in bodies, whose motion is continually varying, is distinctly explained by Mr. Robins in his Treatise of fluxions, without having recourse to any imaginary differential of time, as this author expresses himself, of which he truly says, we have no clear idea; he should have said no idea at all.

But at length Sir Isaac Newton himself gave an accurate account of his method of fluxions in the Introduction to his Treatise of Quadratures, printed at the end of his Opticks in 1704. This Treatise of Quadratures he was prevailed on to publish; because, as he had lent it out *, he observed some things had been copied from it †. The Introduction exhibited an idea of his method of fluxions entirely free from the notion of infinitesimals ‡.

M. Leibnitz also had took upon him to correct the notion of indivisibles, but unhappily. For as he had called his differential method Analysis indivisibilium,

* Advertisement before the Opticks; Comm. Epist. p. 222, in the note ‡; and Raphson. Hist. Fluxion. p. 2. † Joan. Craig. de Calculo Fluentium, printed at London in 1718, in the preface. ‡ "In finitis quantitatibus analysin sic instituere, "et finitarum nascentium vel evanescentium rationes primas vel "ultimas investigare, consonum est geometriæ veterum, et volui "ostendere quod in methodo fluxionum non opus sit figuras infi-"nite parvas in geometriam introducere." Introd. ad Quadrat.

bilium, so now he began to name it Methodus incomparabilium. He says, "siquis nolit adhibere "*infinite parvas*, potest assumere tam parvas quam "sufficere judicat, ut sint incomparabiles, et errorem "nullius momenti, imo dato minorem producunt." Again "et possunt adhiberi triangula communia *in*-"*assignabilibus* illis similia" &c. And afterwards he adds, "potest etiam exprimi conatus centrifugus "per differentiam radii et secantis ejusdem anguli "cujus differentiæ discrimen a sinu verso est infinities, "infinities, infinite parvum, adeoque nullissimum "respectu radii *." Against this arrant nonsense, which however was esteemed as containing profound mysteries †, Sir Isaac Newton seems to have levelled that passage of the Introduction to his Quadratures, "errores quam minimi in rebus mathe-"maticis non sunt contemnendi;" and in his demonstration of his rule for finding fluxions, he has carefully expunged, whatever might favour indivisibles.

Did this description of Sir Isaac Newton, in opposition to M. Leibnitz's crude conception, move the spleen of the authors of the Acta Eruditorum ‡, when in giving an account of the Quadratures, they represented, that, what is taught there, was published by Mr. Craig and Dr. Cheyne ||? But their writings contain only an inconsiderable part of what was sent to M. Leibnitz in 1676. And Mr. Craig has owned, that he was obliged to Sir Isaac Newton for correcting a deficiency in his method ||†.

* Act. Erudit. Mens. Feb. 1689.

† M. d'Alembert has well observed, "en general les hommes ne haissent point l'ob-"scurité, pourvû qu'il en resulte quelque chose de merveilleux." Encyclop. Tom. iv. p. 988. Perhaps if it had not been for this marvellous obscurity, the differential method would never have acquired the pompous name of the sublime geometry.

‡ Act. Erudit. Mens. Januar. 1705.

|| M. Leibnitz in a letter to M. Bernoulli shews his judgment in speaking slightingly of Sir Isaac Newton's Quadratures. Epist. Leibn. ad Bern. Tom. ii. p. 124.

||† Philos. Trans. for 1686. N° 183.

AGAIN,

Again, as in that account insinuations were thrown out against Sir Isaac Newton's candour *; it occasioned the controversy about the invention of fluxions.

This account was from its style, and other circumstances, supposed to have been written by M. Leibnitz himself †. He therefore, in a letter to M. Bernoulli ‡, pretended to be ignorant of it, and gives a very odd reason, why he ought not to be thought its author. There, says he, I hear is praised M. Tchurnhaus's discoveries, which I do not esteem. But M. Leibnitz has not disdained such artifices; for in publishing the Judicium primarii Mathematici, in order to conceal the writer, he rendered his friend M. Bernoulli ridiculous, by making him quote himself under the title of eminens quidam mathematicus ||. However, M. Leibnitz afterwards justified the account, and endeavoured though in vain to explain away its most exceptionable sense.

Concerning the invention of fluxions, there have been published many tracts †||, whence may be learnt the merits of the cause; but more especially from the second edition of the Commercium Epistolicum, printed at London 1722 in 8°; where Sir Isaac

* This rancour M. Leibnitz had long suppressed. In a letter to M. Bernoulli, dated 23 Aug. 1696, he says, "certum est me domino Newtono, ante viginti annos meæ methodi differentialis "fundamenta communicasse, antequam ille mihi quicquam de suis "huc spectantibus. An nonnihil inde profecerit haud satis scio: "neque ideo dicere aussim." Leibn. et Bern. Epist. Tom. i. p. 195.

† Ibid. Tom. ii. p. 308. ‡ Ibid. p. 313. || Ibid. p. 330. Here M. Leibnitz did not prudently follow his friend's advice, who had signified before to him, "rogo vero, ut quæ hic scribo, "iis recte utaris, neque me committas cum Newtono ejusque po-"pularibus; nollem enim immisceri hisce litibus." Ibid. p. 311. And indeed it proved of ill consequence; for it engaged him in controversy with Dr. Keill, who exceeded M. Bernoulli in common sense, though he fell far short of him in mathematical invention. †|| Journal Literaire for July and August 1714, and for 1716. Tom. viii. Part 2. Raphs. Hist. Fluxion. at the end; and Recueil des Divers Pieces sur la Philosophie, &c. Tom. i. in the preface, pp. 11—66, and Tom. ii. pp. 3—124.

Newton

Newton has, in the Preface, Account and Annotation, which were added to that edition, particularly answered all the objections M. Leibnitz and M. Bernoulli were able to make, since the Commercium first appeared in 1712.

This Commercium is composed of ancient letters, and parts of letters digested in the order of the time they were written, and relates to the invention of the methods of infinite series, moments and fluxions. At the bottom of the pages are added several judicious notes, that tend greatly to illustrate the whole affair.

To this Commercium M. Leibnitz long threatened to oppose one of his own *; and upon M. Bernoulli's hearing there was a large account of it published in the Philosophical Transactions †, he urged M. Leibnitz to hasten the fulfilling his promise ‡.

* Mihi consilium est, edere aliquod Commercium Literarium meum, unde apparebit quam in aliis quoque Newtonus olim tenuis fuerit. Leibn. et Bern. Epist. 30 Decemb. 1714. Tom. ii. p. 341.

† N° 342. This account is also published in Latin before the second edition of the Commercium Epistolicum. It was wrote by Sir Isaac Newton himself, and the arguments there used are unanswerable, which M. Bernoulli, in complaisance to M. Leibnitz, without having seen them, is pleased, in a letter to that gentleman, to call after him, *argutationes*, Tom. ii. p. 364, 367. He in another place, Ibid. p. 377, supposed Sir Isaac Newton had acted under a mask; and the authors of the Acta Erudit. [An. 1720. Mens. Mar. p. 137] say, Sir Isaac Newton's arguments were the same with Dr. Keill's. As to that, it cannot be doubted but the Doctor was assisted by Sir Isaac's conversation, and by the excellent notes in the Commercium Epistolicum. But the Doctor, in his writings, making use of some asperities, suitable to the affronts he had received, not being agreeable to Sir Isaac Newton's temper, he thought fit to publish a dispassionate account of the affair himself. At last Sig. Conti prevailed on Sir Isaac Newton to appear himself in this controversy. What he wrote was published in English at the end of Raphs. Hist. Fluxion. and in French in the Recueil &c. The substance of which Sir Isaac Newton afterwards included in the preface to a second edition, he made, of the Commercium Epistolicum.

‡ "Necesse puto, ut nonnihil matures alterum illud, quod "paras, Commercium Epistolicum priori ab *Anglis* edito opponendum." Leibn. et Bern. Epist. 19 Jan. 1716. Tom. ii. p. 367.

M. Leib-

M. Leibnitz loudly complained of the partiality of the collection, and represented the notes as malignant and false *. He attempted to give instances of the first, but quite missed his aim †; and against the notes he never produced any thing besides assertions; so that Sir Isaac Newton himself has well observed in their justification, "interpretationes illæ nullius "quidem sunt auctoritatis, nisi quam ab epistolis "derivant, sed male fundatas esse Leibnitius nun-"quam ostendit ‡."

AGAINST the Commercium, M. Leibnitz and M. Bernoulli used to urge, that it regarded the business of infinite series alone; whereas there is sufficiently considered not only that speculation, but also the affair of moments and fluxions ||. It is no wonder, these gentlemen disliked the treating here so largely on infinite series; when thence was manifest M. Leibnitz's unfair practices.

AGAIN they pretended, that it was not likely Sir Isaac Newton found out the method of fluxions; since in his Principia, published in 1687, he had mistook in regard to their higher orders, and that he gave, in the Scholium at the end of his Quadratures published in 1704, a faulty rule for determining those orders †||. But it has been often demonstrated, that in the Principia he committed no error on that account ‡||; and in 1693 was published by Dr. Wallis ||† the first proposition of Sir Isaac Newton's Quadratures, containing a right rule for finding all the orders of fluxions; and in 1711 Dr. Keill shewed by a series how to make out those

* Raphs. Hist. Fluxion. pp. 97, 104; or Recueil &c. Tom. ii. pp. 5, 36, 47, 52, 53. † Ibid. pp. 98, 101, 105, 113; or Ibid. pp. 5, 18, 53, 82; and Comm. Epist. at the beginning of the preface. ‡ Com. Epist. in the preface, p. 3. || Journ. Liter. for July and August, p. 322. Raphs. Hist. Fluxion. p. 112; or Recueil &c. Tom. ii. p. 80. †|| Com. Epist. p. 246. ‡|| See above, in p. 335, the note markt ‡||.
||† Op. Math. Vol. II. p 392.

orders *, which he learnt from Sir Isaac Newton †. As to the mistake in the Scholium to the Quadratures, it may be observed, that Sir Isaac Newton gave to M. Bernoulli's nephew, when in London, a corrected copy ‡; but as this mistake was afterwards much urged against Sir Isaac Newton, both Dr. Keill and he made it plainly appear to have been a mere oversight ||.

They further said, it was not probable, that M. Leibnitz did not find out the infinite series for measuring the circle; when both Sir Isaac Newton and Dr. Gregory allowed him that invention.

Now in 1669, amongst other series's of Sir Isaac Newton, one for finding the arch of a circle from the sine †||, and in 1671 another of Mr. Gregory for finding that arch from the tangent ‡||, were sent to Mr. Collins; who was very free in communicating these discoveries. In 1674 M. Leibnitz mentions, in a letter to Mr. Oldenburgh, his being possessed of the first series §||; and the next year Mr. Oldenburgh sends both Sir Isaac Newton's and Mr. Gregory's series's to M. Leibnitz ||*. But in 1676 M. Leibnitz, dropping his pretensions to the first series, not being able to demonstrate it, sends to Mr. Oldenburgh, as his own, that of Mr. Gregory with a demonstration **. Sir Isaac Newton and Dr. Gregory scrupled not to allow M. Leibnitz found out this series; for they knew nothing of Mr. Oldenburgh's letter, which lay buried for above thirty years amongst the papers of the Royal Society †*. This discovery startled at first M. Bernoulli ‡*;

* Com. Epist. p. 232. † J. Keill. Epist. ad J. Bern. p. 19. ‡ Epist. Bern. ad Leibn. Tom. ii. p. 310. || Journ. Liter. July and August 1714, p. 348. Epist. J. Keill ad Bern. p. 15; and Philos. Transf. N° 342. p. 208; or Com. Epist. p. 41.
†|| Comm. Epist. Collin. p. 85. ‡|| Ibid. p. 98.
§|| Ibid. p. 116. ||* Ibid. p. 118, &c. ** Ibid. p. 148.
†* Ibid. p. 118. ‡* Leibn. et Bern. Comm. Epist. Tom. ii. p. 309, 317.

but

but he was too deeply engaged in M. Leibnitz's defence to retract, though M. Leibnitz once expressed his apprehensions of it *. In short, M. Leibnitz was at last, in 1713, forced to acknowledge Mr. Gregory to be the original author †; but why he did not acknowledge as much at first, cannot be fairly accounted for.

Of the same force were produced, by these acute reasoners, in opposition to the Commercium Epistolicum, other arguments, consisting only of surmises and presumptions against evident matters of fact.

But from that Commercium it is most manifest, that Sir Isaac Newton had discovered his method of fluxions many years before M. Leibnitz's pretensions. Hence M. de Montmort has candidly said, "Pour "moy je soutient icy et je l'ai toujours soutenu "hautement que M. Newton a été maître du calcul "differentiel et integral avant tout autre géometre, "et que de l'année 1677 il sçavoit tout ce que les "travaux de M. Leibnitz et M. Bernoulli ont de-"couvert depuis ‡."

Besides, M. Leibnitz, from the whole tenour of his conduct, may be justly suspected of having learned by information, what he pretended to be the inventor of. For he pretended to Mouton's differential method ||; to a property of a series, that had been published by M. Pascal †||; to Sir Isaac Newton's series for measuring a circular arch from the sine ‡||; to a series of James Gregory for the same thing from the tangent §||; to four other series's of Sir Isaac New-

* Leibn. et Bern. Comm. Epist. Tom. ii. p. 313.

† Ibid. p. 341. ‡ Letter to Dr. Taylor, dated Jan. 22, 1717. at the end of Keill's letter to Bernoulli.

|| Philos. Trans. N° 342. p. 183; or Comm. Epist. p. 13.

†|| Ibid. p. 215; or Ibid. p. 49. ‡|| Ibid. p. 184; or Ibid. p. 14. §|| Journal Liter. for July and August 1714, p. 353, &c. and Philos. Trans. N° 342. p. 185; or Com. Epist. p. 15; and Keill Epist. ad Bernoulli, p. 19.

ton *; to a method of regreſſion †; to the Differential Analyſis at a time, when it was certain, he was ignorant of it ‡; and, laſtly, to ſome of the principal propoſitions of the Principia ‖.

By theſe inſtances it abundantly appears, how freely M. Leibnitz could appropriate the inventions of others; and it is farther ſhewn, how eaſily, from the manner he firſt produced this diſcovery in his letter to Mr. Oldenburgh, dated the 21ſt of June 1677 †‖, it might be deduced from Dr. Barrow's Method of tangents, by the aſſiſtance of what is declared in Sir Iſaac Newton's letters ‡‖, which had been ſent to M. Leibnitz, eſpecially in that of the 10th of December 1672. And hence it is, that the committee of the Royal Society in their report refer particularly to that letter ‡*.

No doubt others, by the help of theſe hints, might have diſcovered this method; as Mr. Gregory did ſo from much leſs information; though his candour would not permit him to prevent the firſt inventor ‖*.

But M. Leibnitz's different manner of producing this method, firſt in his letter to Mr. Oldenburgh, and afterwards in the Acta Eruditorum **, is very obſervable. In the former, it appears readily deducible from Dr. Barrow's Differential method of tangents; in the other, the reſemblance is diſguiſed as much as poſſible, and it is delivered with an affected and aſtoniſhing obſcurity.

Besides, there is good grounds to ſuſpect M. Leibnitz had a ſight of Sir Iſaac Newton's Analyſis

* Phil. Tranſ. N° 342. p. 188; or Com Epiſt. p. 19.
† Ibid. p. 189; or Ibid. p. 20. ‡ Ibid. p. 192; or Ibid. p. 24. ‖ Journal Liter. for July and Auguſt 1714, p. 348; and Philoſ. Tranſ. N° 342. p. 208; or Com. Epiſt. p. 42.
†‖ Comm. Epiſt. p. 192. ‡‖ Journal Liter. 1714, p. 325; and Philoſ. Tranſ. N° 342. p. 191, 192, 196, 197; or Com. Epiſt. p. 23, 24, 27, 28, 31. ‡* Com. Epiſt. p. 242, 243. ‖* Com. Epiſt. p. 104. ** Ann. 1684. Menſ. Octob.

per

per æquationes numero terminorum infinitas *; for he early put in a claim to a series contained in that Tract †, and afterwards has owned, he had seen before, in the hands of Mr. Collins, letters of Sir Isaac Newton ‡; Mr. Collins being very forward to boast of, and shew to mathematicians, Sir Isaac Newton's discoveries ‖.

However, it has been said †‖, no doubt to make us believe, M. Leibnitz was capable of inventing the Differential calculus, that he could not learn from Sir Isaac Newton's letters the artifice mentioned in them ‡‖, of performing the operation without being obliged to reduce the equation.

A remark like this has already been taken notice of by Dr. Keill ‖*; and I shall farther observe, that the artifice here alluded to is not difficult to find out when once proposed; especially by one, who could be no stranger to M. Fermat's general rule of freeing equations from fractions and surds †*. Yet it is highly probable, M. Leibnitz did not discover this artifice by himself; but might have had it of M. Van Hudden. For M. Leibnitz, in his letter to Mr. Oldenburgh, dated Amsterdam, 28 Novemb. 1676, says, "Amstelodami cum Huddenio locutus sum; "cui negotia civilia tempus omne eripiunt.—Præ-"clara admodum in ejus Schedis superesse certum "est. Methodus Tangentium a *Slusio* publicata du-"dum illi fuit nota. Amplior ejus Methodus est,

* How the method of fluxions might be gathered from that treatise, see the notes on it, as it is published in the Commercium Epistolicum. † Philos. Trans. N° 342. p. 184; or Com. Epist. p. 14. ‡ Raphs. Hist. Fluxion. p. 98, 106; or Recueil &c. Tom. ii. p. 5, 56; and Epist. J. Keill ad J. Bernoul. p. 5. ‖ Journal Literaire for July and August 1714, p. 328. †‖ Encyclopédie, Tom. iv. p. 988. ‡‖ Com. Epist. p. 105, 107, 150. ‖* Journal Literaire for July and August 1714, p. 325. †* See amongst Cartes's Letters, Vol. III. Let. LXXV, LXXVI; and Fermat's Varia Opera Mathematica, p. 60.

"quam quæ a *Slusio* fuit publicata *." Now this method of tangents has been since published †, and the editor observes, that from M. Van Hudden's papers it appears, he had improved his method; so that it proceeded without being obliged to reduce the equation. Besides, M. Leibnitz has owned, M. Huygens was master of the same artifice; whom he has acknowledged to have been his instructor in these studies ‡.

Thus M. Leibnitz, from the notices given him by Sir Isaac Newton, and thereupon consulting the writings of Van Hudden, Barrow, and Slusius ‖, at length got at what Sir Isaac Newton had described; and though in his letter, of the 21st of June 1677, he says, what he there explained, he believed to be what Sir Isaac Newton concealed; yet afterwards in publishing it, he did not, as he ought, declare the information, he had received ‖†; and what is worse, when it had been demonstrated, that Sir Isaac Newton had long before discovered these methods, he and M. Bernoulli by weak arguments endeavoured to shew the contrary.

These have often been fully answered by Sir Isaac Newton himself. In his doing of which M. Leibnitz has complained of want of politeness. But is it not a jest for any one to expect insipid flattery, which mostly passes under that denomination, from another, whom he had so grossly and falsely abused **?

* See Wallis's Op. Math. Tom. iii. p. 646; and Comm. Epist. p. 190, 191. † In the Journal Literaire for July and August 1713, p. 465, in a letter of M. Van Hudden to M. Van Schooten, dated Leyden 21 Novemb. 1659. ‡ Raphs. Hist. Fluxion. p. 98. "ce n'est qu'en France que j'ay pris entré et "M. Huygens m'en donna l'entrée." This passage is omitted at p. 5. Vol. ii. of the Recueil &c. See also Raph. Hist. Flux. p. 107; or Recueil &c. Vol. ii. p. 58. ‖ Comm. Epist. p 192; also Philos. Trans. N° 342. p. 192, &c. and p. 216, 219. or Comm. Epist. p. 23, &c. and p. 50, 53. ‖† Phil. Trans. N° 342. p. 220; or Com. Epist. p. 54. ** In an infamous paper, dated 29 July 1713.

M. Leibnitz

M. Leibnitz insisted to the last, in contradiction to what had been demonstrated, that he was first possessed of the characteristic and algorithm of the method. Now to produce no other argument against this idle pretence, in Sir Isaac Newton's letter of October 24, 1676, M. Leibnitz knew, there was included this sentence "Datâ æquatione quotcunque "quantitates fluentes involvente, fluxiones inve- "nire, et vice versa," which was the substance of the two first problems of his treatise, written in 1671. How then is it possible to solve these two problems without a knowledge of what M. Leibnitz is pleased to style the characteristic and algorithm? Can it be imagined, Sir Isaac Newton had no marks for what he called fluxions? It is certain, that so long ago as 1665 he used those of *p*, *q*, *r* &c. to denote the fluxions of *x*, *y*, *z* &c. and afterwards $\dot{x}$, $\dot{y}$, $\dot{z}$ &c. for the same purpose, and also other symbols as occasion and conveniency required*. Must not therefore M. Leibnitz's assertion appear very unpolite, not to say worse, to Sir Isaac Newton; who though he had by him tracts written in 1665, 1666 and 1671, &c. replete with this characteristic and algorithm; yet never appealed to their authority, till provoked by M. Leibnitz's unfair proceeding in so freely using his own testimony in his own behalf?

HENCE I think it very plain, who was the real inventor of these methods; and as the accuracy of Sir Isaac Newton's demonstrations depends on the first Lemma of the Principia, this has been objected to. M. Huygens soon animadverted on it, which occasioned Sir Isaac Newton, in the second edition of his book, to make some alterations in the expression. Notwithstandng this, its sense has been mistaken

* Journ. Liter. 1714. p. 341, &c. also Philos. Trans. N° 342. p. 204, &c. or Com. Epist. p. 37, &c.

by

by many. For which reaſon it was truly and fully explained by Mr. Robins; as alſo by Dr. Pemberton, he being called upon ſo to do. What Mr. Robins has written of it, is delivered in ſeveral places of the preceding tracts; and I ſhall conclude this Appendix with Dr. Pemberton's explanation, as it was publiſhed in the Hiſtory of the Works of the Learned for January 1741.

LEMMA.

Quantities and the ratios of quantities, which conſtantly tend towards equality during any whatever finite ſpace of time, and before the end of that time approach nearer together than by any whatever difference given, become ultimately equal.

HERE are ſuppoſed the following conditions.

1. THAT the quantities or ratios approach more and more towards equality.

2. THAT they be known ſo to approach during a ſpace of time, whether longer or ſhorter, which is ſufficient to anſwer the ſubſequent conditions.

3. THAT whatever difference, how minute ſoever, ſhall be named, theſe quantities or ratios ſhall be known to approach, till they come within that difference.

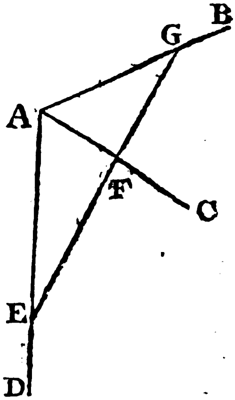
4. THAT after they have paſſed that difference, they ſhall be known ſtill to continue their approach.

UPON theſe conditions the quantities and ratios in this Lemma are ſaid to become ultimately equal.

OR more briefly, varying quantities or ratios are here ſaid to become ultimately equal, if they ſo approach, that, whatever difference be propoſed, it can be ſhewn, that within a finite ſpace of time, thoſe

those quantities or ratios will come nearer than by that difference, and still be approaching.

FOR instance, if from the same point A three streight lines AB, AC, AD be drawn, and in the line AD the point E be taken, whence the line EFG shall be drawn at pleasure, it is manifest, that EG is greater than EF. But if the line EG be supposed to turn round the point E towards the line EA, it is also evident, that whatever difference between EG and EF be proposed, the line EG may move so long towards EA, till the difference between GE and EF shall be less than the difference proposed, and also the lines GE and EF shall still continue to approach.

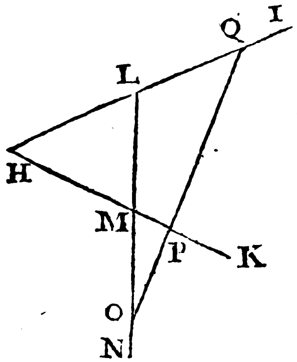


AGAIN, though the triangle GAE is greater than the triangle FAE, by the motion of the line GE, these triangles will also approach in the same manner.

FARTHER, if any polygon be inscribed in a circle, by the subdivision of the arches subtended by each side of the polygon, another may be inscribed of a greater number of sides, which latter will differ less from the circle than the first; and by the same means, a third polygon may be inscribed nearer in magnitude to the circle than the second, and the like process may be continued without end; insomuch that no difference how small soever can be proposed; but polygons may be inscribed, till that difference be surmounted, and yet other polygons still less different from the circle be describable.

MOREOVER,

Moreover, if the three lines HI, HK, LMN be given by position, and from the point O in LN



the line OPQ be drawn, the ratio of QO to PO will be greater than the ratio of LO to MO. But if the line OQ move round the point O towards OL, no ratio can be proposed greater than the ratio of LO to OM, how small soever be the difference, but the line OQ may approach towards OL, till the ratio of QO to OP becomes less than that proposed, this line still continuing in motion, and the ratio of QO to OP yet farther decreasing.

In like manner the ratio of the triangle QLO to the triangle PMO is greater than the duplicate of the ratio of the line LO to MO; but by the motion of OQ the ratio of those triangles will approach to the duplicate of that ratio in the same manner as before.

In the last place, the ratio of LQ to MP is greater than that compounded of the ratio of HL to HM, and of the ratio of LO to OM; but no ratio can be proposed greater than this, which by the motion of the line OQ shall not be surmounted, before the ratio of LQ to MP ceases its approach towards that ratio.

Now in every one of these instances all the conditions expressed in the proposition are complied with; therefore by this proposition EG must be said to become ultimately equal to EF, and the triangle GAE to FAE, as likewise the polygon inscribed in the circle ultimately equal to the circle itself; also the ratio of QO to OP is concluded to be ultimately the same with

with the ratio of LO to OM, and the ultimate ratio of the triangle LOQ to the triangle MOP, the duplicate of the ratio of LO to MO; and lastly, the ratio of LQ to MP ultimately the same with the ratio compounded of the ratio of HL to HM, and of the ratio of LO to MO.

The only difficulty, which here occurs, is, that whereas GE and EF become actually equal, both coinciding at last with the line AE, and the ratio of QO to OP becomes at last actually the same with that of LO to OM, the like is not found in any of the other instances; for the triangle AGE can never be in reality equal to the triangle FAE, nor the polygon by any subdivision of the arches be made equal to the circle, wherein it is inscribed. Again, the ratio of the triangle OLQ to the triangle OMP must ever be greater than the duplicate of the ratio of OL to OM, and the ratio of LQ to MP always greater than that compounded of the ratio of HL to HM, and the ratio of LO to MO.

But if it be said, that therefore in these cases nothing more can be concluded from the conditions proposed in the proposition, than that these magnitudes and ratios are not ultimately unequal; to this the answer is, that this negative conclusion is fully sufficient for all the purposes, to which Sir Isaac Newton has applied this Lemma, and that the only advantage, which accrues from the affirmative form of expression used by him, is some additional degree of brevity. And this affirmative form of speech is borrowed, not from the doctrine of indivisibles, but from the writers on geometrical progressions, with whom it has been usual to call the limit of the sum of the terms in an infinite progression, the sum of the whole series, though no number of terms in such a progression will amount to that limit. In particular, Gregory of St. Vincent, who avoids the use of indivisibles,

indivisibles *, first defines this limit thus †. "Terminus [the limit] progressionis est seriei finis; ad quem nulla progressio [by the addition of its terms] pertinget, licet in infinitum continuetur; sed quovis intervallo dato propius ad eum accedere poterit;" and then in his exposition of this definition has these words; "Terminus igitur progressionis talis est, quemadmodum explicuimus, cùm scilicet aggregatum, sive summa terminorum progressionis, quantumvis continuatæ, nunquam excedit quandam magnitudinem; excedit verò omne minus illa magnitudine, atque ita posset etiam dici productum sive quantitas totius, datæ progressionis, et magnitudo illa æqualis dicetur toti progressioni datæ; hoc est omnibus terminis proportionalibus simul sumptis."

The triangles GEA, FEA, and the triangles QOL, POM, also the lines LQ, MP, are by Sir Isaac Newton called quantitates evanescentes, or vanishing quantities, because they are supposed continually to diminish, till they vanish or come to nothing. If the motion be supposed to begin the other way from the line EA or OL, then Sir Isaac Newton calls those quantities quantitates nascentes; and the ratio, which is the limit according to the conditions specified in this Lemma, is called ultima ratio quantitatum evanescentium, and prima ratio nascentium, though the quantities never actually bear that ratio.

* See Prop. 45. Lib. de Duct. Plani in Planum. † Definit. 3. Libri de Progression. Geometricis.

End of the SECOND VOLUME.