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THE

PRINCIPLES

OF

ANALYTICAL CALCULATION.

BY

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In hisce rebus, multis et gravibus præjudiciis laboramus; sed illa, acri atque iteratâ meditatione subigenda sunt vel potius penitus averruncanda.

BERKELEY *de Motu.*

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CORRECTIONS.

Page vi, l. 10, Preface, for *abtruſe*, read *abſtruſe*.
—— xvi, l. 14, Preface, for *ſubtangent*, read *ordinate* ÷ *ſubtangent*.
—— xvii, l. 17, Preface, after *no method*, inſert *producing right reſults*.
—— 8, l. 17, for *o*, read o.
—— 39, l. 2, in the place of the ſecond + put ×.
—— 40, bottom line, for *invention*, read *inventor*.

PREFACE

PREFACE.

ALTHOUGH a preface is not abſolutely neceſſary, to render intelligible the plan and object of the following work, yet as it differs very materially from all works on the ſame ſubject, I think it proper to explain the reaſons that have induced me, to depart from the notions and methods of preceding authors.

According to moſt of the treatiſes on abſtract ſcience, that from the time of Sir Iſaac Newton, have been publiſhed in this country, the binomial and other related theorems have an origin diſtinct from that of theorems and methods purely algebraical; or in other words, their demonſtration is not merely effected by what are called the ordinary operations of Algebra, but derived from a doctrine founded on a principle of motion.

It required no great ſagacity to perceive, that a principle of motion, introduced to regulate proceſſes purely algebraical, was a foreign principle, and that the notion of conceiving quantity, of all kind whatever, as generated by a continual flux, was partial and merely illuſtrative; accordingly, to the principle and connected method, objections have been ſtated, by foreign writers, but more fully and diſtinctly by a late diſtinguiſhed mathematician of this country. We have ſtill, however, adhered to the principle and method, either from veneration of their great inventor, or becauſe, if we did reject them, what was propoſed to be ſubſtituted in their ſtead, appeared deficient in perſpicuity and preciſion.

That

That of two methods, the beſt is that which is the moſt ſimple, natural, and direct, is a propoſition, the truth of which is involved in the ſignification of its terms; no progreſs is made in diſcuſſion, by an aſſent to ſuch a propoſition; ſince the difficulty and contention will be to determine what is moſt ſimple, natural, and direct. On this account I ſhall ſlightly trace out the courſe of calculation I purpoſe to follow, and the courſe uſually moved in by thoſe, who regard a principle of motion as the proper or eſſential baſis of the fluxionary and differential calculus, and leave the reader to the operations of his own judgment.

I regard the rule for the multiplication of algebraic ſymbols, by which addition is compendiouſly exhibited, as the true and original baſis of that calculus, which is equivalent to the fluxionary or differential calculus; on the direct operations of multiplication, are founded the reverſe operations of diviſion and extraction of roots, by inſpection and trial, and when theſe operations, direct and inverſe, are performed on a binomial $(x + a)$, the reſults, ſo it appears by inveſtigation, may be generally expreſſed under a common formula, called the binomial theorem. From this theorem, other forms, or modes of repreſenting quantity are deduced; and certain properties being common to the expanſion of theſe forms and of the binomial, they are ſtill farther comprehended under a general formula, called the expanſion, or developement of a function: from the ſecond term of this expanſion, the fluxion or differential of a quantity may immediately be deduced, and in a particular application, it appears to repreſent the velocity of a body in a motion.

The fluxioniſts purſue a method totally the reverſe; they lay down a principle of motion as the baſis of their calculus, thence deduce ſome of its firſt proceſſes, and eſtabliſh the binomial theorem, by which it is ſaid, the extraction of roots may be effected: by this method, the binomial theorem does not propoſe to comprehend under a general mode of expreſſion, the reſults of the rules of diviſion, extraction, &c. but rather

to

to ſuperſede thoſe rules. The project of extracting the ſquare and cube roots of algebraical quantities by a principle of motion, is ſurely revolting from common ſenſe, and the fact is, that except the methods of extracting quantities be previouſly examined, the binomial theorem cannot be demonſtrated.

The ſubject of the fluxionary and differential calculus, I now purpoſe to treat diffuſely: and, if on a plain caſe I appear to argue tediouſly and unneceſſarily, let it be recollected, that I am only paying a tribute, which is always exacted, when an opinion long received or ſanctioned by great authority, is controverted.

According to the notions of the great inventor of the method of fluxions, quantities are ſuppoſed generated by motion, and the degree of tendency, or velocity with which they are generated, is called the fluxion: perhaps this method may aid conception, yet as tendency preſcinded from its effect is not intelligible, and as velocity is a term not of itſelf accurately underſtood, ſome definition is neceſſary to fix its meaning: and when it is defined to be the ſpace which would be deſcribed by the motion continued uniform, that ſpace being unknown, becomes the object of computation; ſo that the very principle ſet out from, ſo far from being ſimple, is only eſtabliſhed ſubſequently to calculation; and conſequently the doctrine of fluxions cannot be derived ſolely from the conſideration of velocity.

Of his own method, Newton left no ſatisfactory explanation: thoſe who attempted to explain it, according to what they thought the notions of its author, ſoon diſcerned that the very principle of the method was itſelf a ſubject of calculation, and endeavoured to eſtabliſh it, by reaſonings which fairly may be called tedious and prolix. Of the commentators on the method of fluxions Maclaurin is to be eſteemed moſt acute and judicious, but his Introduction exhibits rather the exertions of a great genius ſtruggling with difficulties, than a clear and diſtinct account of the ſubject he was diſcuſſing.

To remove the prolixity into which diſcuſſions to eſtabliſh the doctrine of fluxions, on the bare conſideration of velocity ran, it was propoſed conformably to the notions of Newton, to call in as an auxiliary principle, the doctrine of prime and ultimate ratios, or of limits: theſe new terms, however, not explaining themſelves, it became neceſſary to fix their meaning by definition: what has been given, although laboured into correctneſs by conditions and limitations, does not readily excite diſtinct ideas. According to this new view of the theory of fluxions, the buſineſs of computation was to determine the limiting ratios of quantities, after this propoſition had been eſtabliſhed, to wit, that the ratio of the velocities with which quantities are generated, is the ſame as the limiting ratio of the increments generated in a given time.

Euler and Dalembert went a ſtep farther: they rejected the notion of the generation of quantities by motion, but retained the doctrine of limiting ratios: what the former author has ſaid is not ſatisfactory; he ſoon quits the diſcuſſion of principles for the work of calculation, and the explanation of Dalembert, if luminous, is only ſo by the partial illuſtration of an example.

In all explanations of the differential calculus, the chief point and difficulty is in the method of deducing the ſecond term of the expanſion of an algebraical function of x, when x is made $x + \Delta x$; now, to draw a tangent to a curve, to find the velocity of a moving body at a given point, &c. it is neceſſary to have this ſecond term called the differential or fluxion computed: hence it is clear, that if by any independent method or train of reaſoning, we could find the value of the ſubtangent of a curve, or the value of the velocity of a body in motion, reverſely, we might explain, what a fluxion or differential was, by reference to the properties

of

of curves or of motion *, and this in fact is the method in the doctrine of fluxions: for, as it will appear in the following pages, if x the space be a function of the time t, or be thus expressed, $x = \phi t$, then increasing t by $\dot{t}$, ϕt becomes $\phi t + \frac{(\phi t)^{\cdot}}{\dot{t}} \dot{t} + \frac{(\phi t)^{\cdot\cdot}}{1.2\dot{t}^2} \dot{t}^2 +$, &c. in which series $\frac{(\phi t)^{\cdot}}{\dot{t}}$ denotes the velocity, $\frac{(\phi t)^{\cdot\cdot}}{1.2\dot{t}^2}$ the accelerating force; hence, since $\frac{(\phi t)^{\cdot}}{\dot{t}}$, or $\frac{\dot{x}}{\dot{t}}$ denotes the velocity, if we could independently of the above method, and by some new process determine the velocity, it is clear we might call the fluxions of quantities, velocities, and thence determine $\frac{\dot{x}}{\dot{t}}$: and consistently with this definition of fluxions, all quantities must be conceived to be generated by motion, and the language of analysis must consist of figurative expressions borrowed from the doctrine of motion. And here it is easy to explain why in discussing the principles of fluxions, authors fell into so many absurd and unintelligible expressions: If x be a function of the time (t) and t becomes $t + \dot{t}$, then x becomes $x + \frac{\dot{x}}{\dot{t}}.\dot{t} + \frac{\ddot{x}}{1.2.\dot{t}^2}.\dot{t}^2 + \frac{\dddot{x}}{1.2.3.\dot{t}^3}.\dot{t}^3 +$, &c. and the space described in time $(\dot{t}) = \frac{\dot{x}}{\dot{t}}\dot{t} + \frac{\ddot{x}}{1.2.\dot{t}^2}\dot{t}^2 +$, &c. in which collection of partial motions, $\frac{\dot{x}}{\dot{t}}\dot{t}$ is the space described in an uniform motion with a velocity $= \frac{\dot{x}}{\dot{t}}$, $\frac{\ddot{x}}{1.2.\dot{t}^2}\dot{t}^2$ is the space described in a motion uniformly accelerated, by an accelerating force $\frac{\ddot{x}}{1.2\dot{t}^2}$, but the other terms as $\frac{\dddot{x}}{1.2.3\dot{t}^3}$, $\frac{x^{\text{iv}}}{1.2.3.4.\dot{t}^4}$, since they cannot be referred

* See an instance of this reverse mode of reasoning, by which from the properties of motion, those of fluxions are obtained in Robin's Tracts, p. 34. Vol. ii.

referred to any known motion are not deſignated by any name, as $\frac{\dot{x}}{\dot{t}}$, $\frac{\ddot{x}}{1.2\dot{t}^2}$ are, which refer to motions known in nature. The firſt fluxion could then be called the velocity, the ſecond the tendency of the velocity, or the rate of the increaſe of the firſt velocity ſuppoſing it not uniform, or whatever was equivalent to the accelerating force, but the third, fourth, &c. fluxions could only be called by analogy and circuitouſly, the tendency of the tendency of a velocity, &c. &c. phraſes, to which no preciſe notions could be attached, and which occurring in a ſcience, that ought to poſſeſs, if any other, perſpicuity and accuracy, diſguſted men of ſound minds, and alienated them from the ſtudy of the *abtruſe and fine geometry*.

As in the equation $x=\varphi t$ when $\frac{\dot{x}}{\dot{t}}$ is deduced $=\frac{(\varphi t)^{\cdot}}{\dot{t}}$; $\frac{\dot{x}}{\dot{t}}$ does not neceſſarily mean the velocity, except x be uſed to denote a ſpace deſcribed, and t the time of deſcribing it, ſo, if $x=\varphi t$, expreſſes the relation between any other ſubject of inveſtigation, $\frac{\dot{x}}{\dot{t}}$ may have a correſponding meaning, and be expreſſed by an appropriate term; and hence, as in the principle of motion, if the fluxion or differential of a quantity be defined to be that term, and by any proceſs we can compute it, the new calculus may be eſtabliſhed accordingly, and its figurative expreſſions, muſt be all borrowed from the new ſubject of inveſtigation *.

Since the method of fluxions leads to right reſults, it may no doubt be explained into perſpicuity and preciſion, but it ſurely is a partial theory and its language figurative and alluſive: and if ſo, is it more commodious,

* Thus a fluxion of a quantity might be defined by means of the ſubtangent of a curve, then the difficulty in computation would be, to find the value of the ſubtangent. To compute the velocity of a body in motion; to draw a tangent are problems of the ſame kind and difficulty; if you can do the one, you can the other.

commodious, and logical, by principles and reaſonings new in mathematics, to eſtabliſh that theory, and by expreſſions borrowed from it, to form the general language of Analyſis; or from the principles and ordinary operations of Algebra, to form a language in no ways figurative, and to employ it as the common inſtrument in the inveſtigation, not only of the properties of motion, but in mathematical inveſtigation of all kind whatever?

The theory of fluxions, founded on a reverſion of the natural and logical order of ideas, ought on the ground of compenſation to confer on abſtract ſcience, the advantages of perſpicuity and illuſtration, ſince it has diminiſhed the accuracy of its demonſtrations; to be convinced of this, let us attend to the demonſtration of the famous binomial theorem as given by Maclaurin and ſubſequent writers.

Firſt, $(1+x)^n$ is put $= 1 + \text{A}x + \text{B}x^2 + \text{C}x^3 +$, &c.
And here, as Landen obſerves, we muſt firſt conceive $\text{A}x$ to be a line generated by motion, $\text{B}x^2$ to be another line likewiſe generated by motion, &c. and then, that the ſum of all theſe lines, generated by different kinds of motions, to equal the line $(1+x)^n$, generated likewiſe by motion. Here is a very wide departure from any thing like ſimplicity; but is the method rigorous and accurate? in the next ſtep

$$n\dot{x}\,(1+x)^{n-1} = \text{A}\dot{x} + 2\,\text{B}x\dot{x} + 2\,3\,x^2\dot{x} +, \text{\&c.}$$

Or the fluxions are made equal, becauſe, when two quantities generated by motion are always equal, their velocities or their fluxions are equal: but is $(1+x)^n$ really and metaphyſically equal to $1 + \text{A}x + \text{B}x^2 + \text{C}x^3 +$ &c.? no ſuch thing, for when n is a negative number or fraction, the ſeries $1 + \text{A}x + \text{B}x^2$, &c. runs on in infinitum, and conſequently there can be no abſolute equality, between $(1+x)^n$ and $1 + \text{A}x + \text{B}x^2 +$, &c. when ſpecific numbers are ſubſtituted for x: this ſtep, then according to the principle of the generation of quantities by motion at leaſt wants proof, in all caſes, except when n is a poſitive integer number.

In

In the next ſtep, dividing by $n\dot{x}$ there reſults $(1+x)^{n-1}=\frac{A}{n}+\frac{2Bx}{n}+$, &c. and it is ſaid, that ſince the equation is true, *whatever* x is, it follows that, making $x=0$, $1=\frac{A}{n}$: now, to make this good logic, one of the *whatever values* of x muſt be o: but as in the uſual and proper meaning of whatever, o is not comprehended, it ought formally to have been comprehended: but if o be comprehended, then it is in fact aſſerted, that $(1+x)^{n-1}=\frac{A}{n}+\frac{2Bx}{n}+$, &c. when $x=0$, or in other words, it is aſſerted, that $1=\frac{A}{n}$, which if true, at leaſt wants proof, ſince it is no conſequence from what has preceded *.

The

* This mode of tacitly including the particular value o, under the term *whatever*, is to be found in many demonſtrations: Euler frequently employs it 'Dubium hic ſuboriri poſſet, an, ſi duæ hujuſmodi ſeries fuerint inter ſe æquales

$$A+Bz+Cz^2+, \text{\&c.}=a+bz+cz^2+, \text{\&c.}$$

neceſſario inde ſequatur, coefficientes ſimilium poteſtatum ipſius z inter ſe eſſe æquales; ſeu an ſit $A=a$; $B=b$, &c. Hoc autem dubium facile tolletur, ſi perpendamus hanc æqualitatem ſubſiſtere debere *quemcunque* valorem obtineat variabilis z. Sit igitur $z=0$, atque manifeſtum eſt fore $A=a$, &c. Analy. p. 177. inf.

Now, if *quemcunque* valorem is made to mean o as well as all other values, it muſt in fact be aſſerted, that $A+Bz+$, &c. $=a+bz+$, &c. when $z=0$, but what is this but arbitrarily making $A=a$? Is it not true, then, it may be aſked, that, if ſuch an equation ſubſiſts whatever z is, $A=a$, $B=b$? I anſwer, if the equations terminate at terms, Mz^m, $M'z^m$, by continually eliminating A, a, B, b, &c. we at length arrive at an equation of the form $(M-M')$, &c. $=0$, and conſequently, $M=M'$, and then by reverſing the ſteps, we find $L=L'$ and $B=b$, $A=a$: but if the ſeries do not terminate, and as propoſed, they are never ſuppoſed to terminate, then the equality of the coefficients of the like powers of the arbitrary quantity cannot, I apprehend, be ſtrictly proved: and in fact, the ſtatement that $A+Bz+$, &c. ad inf. $=a+bz+$, &c. ad inf. has no diſtinct meaning.

'But this method of indeterminate coefficients is a uſeful method and leads to right reſults.' True; but in its application the conditions are different from what they are when the problem is generally and abſtractedly propoſed: for inſtance, $A+Bz+Cz^2$, &c. may expreſs the evolution of an expreſſion as of $\frac{2-(\alpha+\beta)z}{1-(\alpha+\beta)z+\alpha\beta z^2}$, and $a+bz+$, &c. the evolutions of expreſſions as $\frac{1}{1-\alpha z}$, $\frac{1}{1-\beta z}$ in which the equality inſtituted between the infinite ſeries would have meaning:

but

The principle of motion being itſelf an object of calculation, and by no means natural in proceſſes purely algebraical, different baſes have been propoſed for the fluxionary calculus. Newton in the Principia, having employed the doctrine of prime and ultimate ratios *, Dalembert in the Encyclopedie, firſt formally propoſed as a baſe that doctrine, or what is equivalent, the doctrine of limits. Landen in his Reſidual Analyſis, ſubſtituted not ſo much a new baſis, as a new calculus, by which all problems within the province of fluxions, might be ſolved: M.Carnot, within theſe few years, has by peculiar arguments, and with great ingenuity, given the Metaphyſics and Logic of Leibnitz's infiniteſimal calculus.

Theſe methods I now examine, and ſhall endeavour to ſhew that in all there is the ſame kind of difficulty, when the paſſage is made from finite to infinite, or from diſcrete to continued quantity: and that all are equally liable to the objection of Berkeley, concerning the fallacia ſuppoſitionis, or the *ſhifting of the hypotheſis*. Of the infiniteſimal calculus of Leibniz I ſhall take no notice, becauſe its principles as formally laid down by him, are acknowledged to be inadmiſſible; and it is not neceſſary to controvert, what no man undertakes to defend †.

In

but the theorem ſeparately and abſtractedly propoſed, as it is found in Euler and other authors, is not ſelf-evident and therefore cannot properly be laid down as a principle, and not only, is not proved as it is attempted to be, but ſeems incapable of being proved. See Arbogaſt. p. xii. Preface. Carnot, p. 40. Reflexions ſur la Metaphyſique du calcul. inf. Couſin, p. 124.

* M. Couſin in the preface to his Treatiſe on the Differential Calculus, ſays, that before Dalembert had publiſhed his thoughts in the fourth Volume of the Encyclopedie, the true metaphyſics of the Differential Calculus, the method of limits, was abſolutely unknown: was the ſtep then from Newton's doctrine of prime and ultimate ratios to that of limits ſo difficult an one?

† From ſeveral paſſages in the letters of this great man, he appears, however, to have had juſt conceptions of the principles of his calculus: it is to be regretted, that neither he nor Newton complied with the wiſhes of Wallis, 'Optaverim ita, ut tibi vacet tuum calculum Differentialem et 'Newtono ſuam Fluxionum methodum juſto ordine exponere; ut quid ſit utrique commune, et quid 'interſit diſcriminis, et utramque diſtinctius intelligamus.' Ex Epiſtolis Wallifii ad Leibnitium. Julii 30, 1697.

In the theory of ultimate ratios, or of limits, the quantities employed in the calculation are not what are called infiniteſimal quantities, nor the ratios of ſuch quantities, but the limiting ratios of ſuch quantities, and expreſſed by finite terms: thus if $y = x^m$, increaſe x by i, and $y + h$, (h its increment,) becomes $x^m + m x^{m-1} i +$, &c.

$$\because h = m x^{m-1} i + m . \frac{m-1}{2} x^{m-2} i^2 +, \&c.$$

and $\frac{h}{i} = m x^{m-1} + m . \frac{m-1}{2} x^{m-2} i +$, &c.

decreaſe i, h is decreaſed, and the expreſſion on the right hand ſide of the equation approaches more nearly to the value of $m x^{m-1}$; make $i = 0$, and $m x^{m-1}$ is called the limit of the ratio of h to i, or $L \frac{h}{i} = m x^{m-1}$.

To ſhew its application, conceive in a parabola, of which the equation is $a x = y^2$, two ordinates MP, mp rightly applied, and through the points P, p of the curve a line pPT to be drawn cutting the axis in T; and moreover, ſuppoſe pT′ to repreſent the tangent, (the line that meets the curve but does not cut it,) and a line pn to be drawn parallel to the axis.

then $\frac{pn}{Pn} = \frac{PM}{TM}$; but PM, $y = a^{\frac{1}{2}} . x^{\frac{1}{2}}$,

and pm, or $PM + pn = a^{\frac{1}{2}} . (x + i)^{\frac{1}{2}}$, ($Mm = i$),

$\therefore pn = a^{\frac{1}{2}} \left(\frac{x^{-\frac{1}{2}} i}{2} - \frac{x^{-\frac{3}{2}} i^2}{8} +, \&c. \right)$

and $\frac{pn}{Pn} = a^{\frac{1}{2}} \left(\frac{x^{-\frac{1}{2}}}{2} - \frac{x^{-\frac{3}{2}} i}{8} +, \&c. \right)$

make $i = 0$ and $L \frac{pn}{Pn} = \frac{a^{\frac{1}{2}} x^{-\frac{1}{2}}}{2}$, but when i the interval between the ordinates diminiſhes, $\frac{PM}{TM}$ approaches more nearly to the value of $\frac{pm}{T'm}$, and the

the limit of $\frac{\text{PM}}{\text{TM}}$ is $\frac{pm}{\text{T}'m}$, hence, $\frac{pm}{\text{T}'m} = \frac{a^{\frac{1}{2}}x^{-\frac{1}{2}}}{2}$, or $\text{T}'m = \frac{2a^{\frac{1}{2}}x^{\frac{1}{2}} \times x^{\frac{1}{2}}}{a^{\frac{1}{2}}} = 2x$ the value of the ſubtangent; according to the differential notation $\text{L}.\frac{pn}{\text{P}n}$ is expreſſed by $\frac{dy}{dx}$, according to the fluxionary, by $\frac{\dot{y}}{\dot{x}}$.

The theorem on which Landen's method eſſentially depends, is this,

$$\frac{x^{\frac{m}{n}} - v^{\frac{m}{n}}}{x - v} = x^{\frac{m}{n}-1} \times \frac{1 + \frac{v}{x} + \left(\frac{v}{x}\right)^2 + \left(\frac{v}{x}\right)^3 \ldots\ldots (m \text{ terms})}{1 + \left(\frac{v}{x}\right)^{\frac{m}{n}} + \left(\frac{v}{x}\right)^{\frac{2m}{n}} +, \text{\&c.}\ (n \text{ terms})},$$

which (when v becomes x), $= x^{\frac{m}{n}-1} \times \frac{m}{n}$.

In M. Carnot's very ingenious view of the ſubject, he ſhews that Leibniz's method in which infiniteſimal quantities are introduced, leads to right reſults by a compenſation of errors *. In his examples he procures what

* A note of M. Lagrange's affixed to a memoir De l'Infini abſolu, contains notions very ſimilar to M. Carnot's: the following is an extract from the note.

'In en eſt ici comme dans la methode des infiniment petits, ou le calcul redreſſe auſſi de lui même les fauſſes hypoteſes que l'on y fait. On imagine, par exemple, qu'une courbe ſoit un poligone d'une infinité de petit côtés, dont chacun etant prolongé devienne une tangente a la courbe. Cette ſuppoſition eſt reellement fauſſe; car le petit côté prolongé ne peut jamais être autre choſe qu'une veritable ſecante: mais l'erreur eſt detruite par une autre erreur qu'on introduit dans le calcul en y negligeant comme nulles des quantités, qui ſelon la ſuppoſition ne ſont qu'infiniment petites. C'eſt en quoi conſiſte, ce me ſemble, la metaphyſique du calcul des infiniment petits; tel que l'a donné M. Leibniz. La methode de M. Newton eſt au contraire tout a fait rigoureuſe ſoit dans les ſuppoſitions, ſoit dans les procédés du calcul. Car il ne conçoit qu'une ſecante devienne tangente, que lorſque, les deux points d'interſection viennent tomber l'un ſur l'autre, et alors il rejette de ſes formules toutes les quantités que cette condition rend entierement nulles. Cette methode exige abſolutement qu'on regarde comme evanouiſſantes, c'eſt à dire comme nulles, les quantités dont on cherche les premieres, ou derniéres raiſons: et c'eſt ce qui rend ſouvent les demonſtrations longues et compliqueés. La ſuppoſition des infinitement petits ſert a abrégér, et a faciliter ces demonſtrations: mais ce n'eſt qu'apres avoir prouvé en général que l'erreur qu'elle fait naitre eſt toujours corrigée par la maniere dont on manie le calcul, qu'il eſt permis de regarder les infiniment petits comme des realités et de les emploier comme tels dans la ſolution des problemes'. Miſc. Taur. 1760.

what he calls imperfect equations, which become true, when the arbitrary or infinitesimal quantities disappear: the two members in the imperfect equations are unequal, but may, by changing the infinitely small, or arbitrary quantities, be made to approach each other within any assigned limits of accuracy, and their ultimate ratio, is a ratio of equality: vanishing quantities, the author considers as the limits of infinitely small quantities, having their relative value assigned by the law of continuity.

Against the method of limits, or of prime and ultimate ratios, there are several objections, on the points of perspicuity, of logical precision, and of commodiousness in the processes of calculation: and first, the method is not perspicuous, inasmuch as it considers quantities in the state, in which they cease to be quantities; for when h and i are finite, the ratio between them is sufficiently clear and intelligible; not so, when h and i vanish, or become nothing at the same time; to conceive this ratio, the mind feels considerable difficulty, and to explain it, many reasonings and illustrations are requisite.

The second objection is, that, the fluxions of quantities in this method of limits, are not obtained by just and logical inference: for, when x is increased by i, x^m becomes $x^m + mx^{m-1}i + m.\frac{m-1}{2}x^{m-2}i^2$, &c. and $\frac{h}{i}$, $mx^{m-1} + m.\frac{m-1}{2}x^{m-2}i$, &c. and putting $i = 0$, the right hand expression becomes mx^{m-1}: but since $mx^{m-1} + m.\frac{m-1}{2}x^{m-2}i$, &c. was produced on the express supposition, that i is some quantity, if you make $i = 0$, the hypothesis, is as Berkeley says *, shifted, and there is a manifest sophism in

* To this point the following queries in the Analyst are directed.

Qu. 28. Whether the shifting of the hypothesis, or (as we may call it) the fallacia suppositionis, be not a sophism, that far and wide infects the modern reasonings, both in the mechanical philosophy, and in the abstruse and fine geometry?

Qu. 43.

in the procefs for obtaining $\text{L}\,\frac{h}{i}$: fince, if the hypothefis be deftroyed, the confequence ought not to be retained.

The third objection relates to the inconvenience, that muft appear in the fundamental propofitions and firft proceffes of calculation, fince the quantities employed cannot be confidered feparately from each other, but muft appear connected, two and two, as in $\text{L}\,\frac{h}{i} = m x^{m-1}$, $\text{L}\,\frac{h}{i} = a^{x}$ when $y = a^x$ and h is the increment of y, when i is the increment of x; add to this, that the principle of the method, the definition of a limit, is neither fimple nor concife; and that, in finding the limiting ratio of $(x+i) - x$ to $(x+i)^m - x^m$, it is not clearly fhewn, that by diminifhing i, the rejectaneous quantity $m.\frac{(m-1)}{2} x^{m-2}\, i + \frac{m.(m-1)(m-2)}{1.2.3} x^{m-3}\, i^2 +$, &c. may be made of any degree of fmallnefs.

The method of Landen is liable to two objections, firft $\frac{x^{\frac{m}{n}} - v^{\frac{m}{n}}}{x - v}$ is obtained $= x^{\frac{m}{n}-1} \frac{\left(1 + \frac{v}{x} + \left(\frac{v}{x}\right)^2, \text{\&c.}\right)}{\left(1 + \left(\frac{v}{x}\right)^{\frac{m}{n}} + \left(\frac{v}{x}\right)^{\frac{2m}{n}} +, \text{\&c.}\right)}$

on the hypothefis that x is greater than v, therefore in good logic, you cannot retain the conclufion, when the hypothefis is deftroyed, or when x is

Qu. 43. Whether an algebraical note, or fpecies can at the end of a procefs be interpreted in a fenfe, which could not have been fubftituted for it in the beginning? Or whether any particular fuppofition can come under a general cafe which doth not confift with the reafoning thereof?

Qu. 46. Whether, although algebraical reafonings are admitted to be ever fo juft, when confined to figns, or fpecies as general reprefentatives of quantity, you may not neverthelefs fall into error, if, when you limit them to ftand for particular things, you do not limit yourfelf to reafon, confiftently with the nature of fuch particular things? And whether fuch error ought to be imputed to pure algebra?

is made $=v$: ſecondly, the method in its application is found to be very incommodious and embarraſſing.

The infiniteſimal calculus, as explained by M. Carnot, conducts to very eaſy proceſſes and operations, but it is in ſome ſort ſubjected to the objection of the fallacia ſuppoſitionis, and his three theorems, reſt on conſiderations two fine and remote from the common and ſimple principles of calculation.

Theſe methods are equally liable to the ſame objection, and they all employ nearly the ſame artifice, in effecting the paſſage from diſcrete to continued quantity.

In the methods of limit, the limit of $\frac{h}{i}$ is obtained by firſt conſidering i as a quantity, and then by putting $i=0$, and in this caſe $\frac{h}{i}$ is expreſſed by $\frac{0}{0}$.

In Landen's method $\frac{x^{\frac{m}{n}}-v^{\frac{m}{n}}}{x-v}$ is obtained by firſt conſidering $x > v$, and then by putting $x=v$, in which caſe, $\frac{x^{\frac{m}{n}}-v^{\frac{m}{n}}}{x-v}$ is expreſſed by $\frac{0}{0}$.

In the infiniteſimal calculus, arbitrary quantities are introduced to expreſs the conditions of the problem and then eliminated: a diſtance is firſt ſuppoſed between two points or lines, &c. and then that diſtance is made to diſappear; in this caſe what are called infinitely ſmall become vaniſhing quantities, whoſe ratio although vaguely expreſſed by $0 : 0$, is aſſigned by the *law of continuity* *.

Theſe

* In Deſcartes's method of drawing tangents, may be diſcerned the kindred notion, on which depends the paſſage from diſcrete to continued quantity: he firſt finds an equation with unequal roots to repreſent the interſection of a curve with a circle, and then makes the roots equal, in which caſe there is no interſection.