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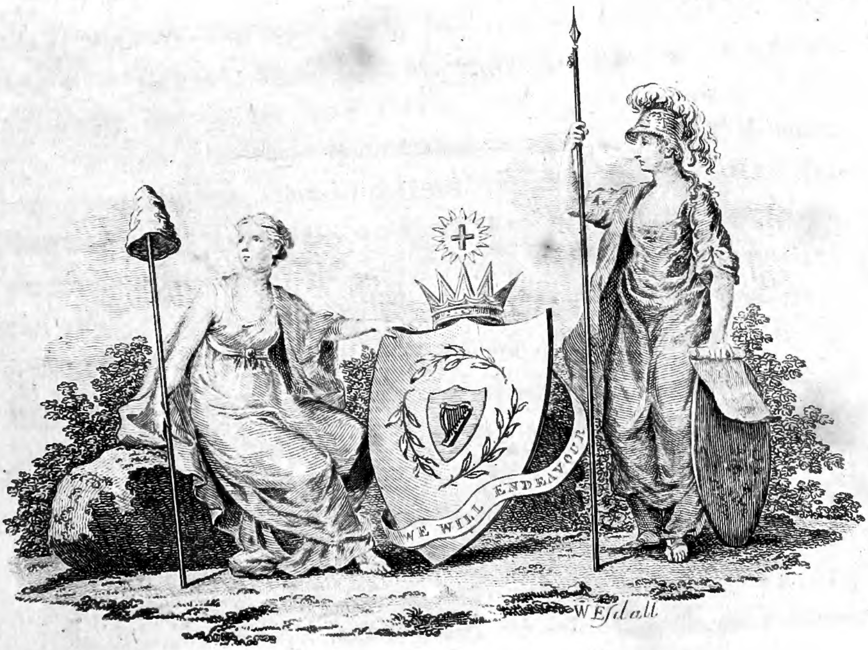

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S C I E N C E.

On the PRECESSION *of the* EQUINOXES. *By the Rev.* MATTHEW YOUNG, *D. D. S. F. T. C. D. & M. R. I. A.*

IT is univerfally acknowledged, that Sir Ifaac Newton has fallen into fome error in his calculation of the fun's force to produce the preceffion of the equinoxes, making it by one half lefs than the truth: but the particular fource of this error has not been fo generally agreed upon. Read April 1, 1797.

THOUGH feveral excellent mathematicians, of whom D'Alambert feems to have been the firft, have given genuine folutions of this problem, by proceffes entirely different from each other, perhaps it ftill may be worth while to endeavour to difcover diftinctly in what confifts the fallacy of Newton's reafoning, and whether in fome of the folutions of this curious queftion, which are received as genuine, there do not lie fome fecret and unobferved errors, which being equal and contrary, compenfate each other, and thus leave the refult correct, though the premifes from which it is deduced are faulty.

The first Lemma which Newton premises to the investigation of the precession is as follows:

Fig. 1. "If A P E P represent the earth, of uniform density, de-" scribed with the centre C, poles P, *p*, and equator A E; and "if with the centre C and radius P C, the sphere P *a p e* be sup-" posed to be described; and Q R be a plane perpendicular to "the right line joining the centres of the sun and earth; and "every particle of all the exterior earth P *a p* A P *e*, which is "higher than the inscribed sphere, endeavour to recede on "either side from the plane Q R, and the effort of each particle "be proportional to its distance from the plane; I say, first, "that the whole force and efficacy of all the particles in the "circle of the equator A E, disposed uniformly without the "sphere, throughout the whole circumference, in the form of a "ring, to turn the earth round its centre, is to the whole force "and efficacy of as many particles placed at the point A of "the equator which is most remote from the plane Q R, to "move the earth round its centre with a like circular motion, "as one to two. And that circular motion will be performed "round an axis lying in the common intersection of the equator "and the plane Q R."

The demonstration of this Lemma is given in the Principia, and allowed to be legitimate.

His

His fecond Lemma is as follows:

"The fame things being fuppofed, I fay, fecondly, that the "whole force and efficacy of all the particles without the fphere "to turn the earth round its axis, is to the whole force of as "many particles difpofed uniformly in the form of a ring, in "the circumference of the circle AE of the equator, to move "the earth, with a like circular motion, as two to five."

The demonftration of this Lemma is alfo given in the Principles, and is likewife received as unexceptionable.

Lemma 3.

"The fame things being fuppofed, I fay, thirdly, that the "motion of the earth round the axis already defcribed, com- "pounded of the motion of all its particles, will be to the "motion of the aforefaid ring round the fame axis in a ratio, "which is compounded of the ratio of the quantity of matter "in the earth to the quantity of matter in the ring, and of the "ratio of three fquares of the arch of a quadrant of a circle "to two fquares of the diameter; that is, in a ratio of matter "to matter, and of the number 925275 to the number "1000000."

This Lemma I fhall firft demonftrate in Newton's fenfe, and then correct the conclufion on the principles propofed by Simpfon and Frifi.

By

By the revolution of the circle E A H C, and circumſcribed ſquare (fig. 2.) P Q S T round the common axis E H, let there be deſcribed a ſphere and circumſcribed cylinder. Let the radius A O be $= 1$, the periphery of the circle A E C H $= p$, the ordinate B R $= y$, abſciſſa B O $= x$. Then $1 : p :: x : px$, the periphery of the circle whoſe radius is O B; therefore $px \times 2y$ will be the ſurface generated by the ordinate R G, in the revolution of the circle A E C H round the diameter E H: but x will be the meaſure of the velocity of the point B, therefore $2px^2y$ will be the momentum of all the particles in that ſurface; and the fluent of the quantity $2px^2y\dot{x}$ will be the momentum of the entire ſphere, when x is equal to the radius A O. But $y = \overline{1-x^2}|^{\frac{1}{2}}$; therefore the fluxion $x^2\dot{x}y = x^2\dot{x} \times \overline{1-x^2}|^{\frac{1}{2}} = \frac{x^2\dot{x}}{\overline{1-x^2}|^{\frac{1}{2}}} - \frac{x^4\dot{x}}{\overline{1-x^2}|^{\frac{1}{2}}}$; and the fluent of $\frac{x^2\dot{x}}{\overline{1-x^2}|^{\frac{1}{2}}} = \frac{1}{2} \times$ circular arc E R $- \frac{1}{2}x \times \overline{1-x^2}|^{\frac{1}{2}}$, and the fluent of $-\frac{x^4\dot{x}}{\overline{1-x^2}|^{\frac{1}{2}}} = -\frac{3 \times \text{circular arc } ER - \overline{2x^3+3} \times x \times \overline{1-x^2}|^{\frac{1}{2}}}{8}$; therefore the whole fluent, when $x = 1$, is $\frac{1}{8} \times$ quadrantal arc E A $= \frac{1}{32}p$; and $2px^2\dot{x} \times \overline{1-x^2}|^{\frac{1}{2}} = \frac{1}{16}p^2$, the motion of the entire ſphere.

In a cylinder, the ordinate y becomes $=$ B R $= 1$; therefore the fluxion of the momentum of the cylinder $= 2px^2\dot{x}$, whoſe fluent, when $x = 1$, is $\frac{2}{3}p$. Therefore the motion of a cylinder is to the motion

motion of an infcribed fphere, revolving round the fame fixed axis, and with the fame angular velocity, as $\frac{2}{3}p$ to $\frac{1}{16}p^2$, or as 16 to $\frac{3}{2}p$, that is, as four equal fquares to three circles infcribed in them.

LET the quantity of matter in an indefinitely flender ring, furrounding the fphere and cylinder at their common contact A O C, be reprefented by the letter m, its velocity will be as A O $= 1$; and its motion $= m$, and therefore the motion of the cylinder is to the motion of the ring as $\frac{2}{3}p$ to m, or as $2p$ to $3m$.

THE motion of the annulus, uniformly continued round the axis of the cylinder, is to its motion revolving uniformly in the fame periodic time round one of its diameters, as the circumference of a circle to twice the diameter.

FOR (fig. 2) let A R $= z$, and let its fluxion $\dot{z}$ be given, R B $= y$, A B $= x$, and A O $= r$; let the motion be performed round the diameter A C, the velocity of the point R will be as R B or y; therefore the fluxion of the motion of the annulus round the diameter AC, is to the fluxion of the motion round the center O in an immoveable plane, as $\dot{z}y$ to $\dot{z}r$, that is, from the nature of a circle, as x to z; and therefore the motions themfelves are to each other in the fame ratio, that is, when $x =$ A C, as the diameter to half the circumference, or as twice the diameter to the circumference of a circle.

HENCE, by compounding all thefe ratios, the truth of the Lemma is manifeft.

BUT

But Simpſon in his miſcellaneous tracts has juſtly obſerved, that though this reaſoning be indiſputably true in Newton's ſenſe, yet there is a difference between the quantity of motion ſo conſidered, and the momentum, whereby a body revolving round an axis, endeavours to perſevere in its preſent ſtate of motion, in oppoſition to any new force impreſſed, which latter kind of momentum it is that ought to be regarded in computing the alteration of the body's motion in conſequence of ſuch force. In this caſe, every particle is to be conſidered as acting by a lever terminating in the axis of motion; ſo that to have the whole momentum, the moving force of ſuch particle muſt be multiplied into the length of the lever by which it is ſuppoſed to act; whence the momentum of each particle will be proportional to the ſquare of the diſtance from the axis of motion, as it is known to be in finding the center of percuſſion, which depends on the very ſame principles.

The correction ariſing from this change in the proceſs amounts only to about $1\frac{1}{2}''$, as will eaſily appear in the following manner:

The fluxion of the moment of a ſphere, from what has been ſaid already, is $2p\,x^3\,y\,\dot{x}$; from the nature of the circle, $x^2 = 1 - y^2$, as before; therefore $x\dot{x} = -y\dot{y}$, $x^3\,\dot{x} = y^3\,\dot{y} - y\dot{y}$, and $2p\,x^3\,y\,\dot{x} = 2p \times \overline{y^4\,\dot{y} - y^3\,\dot{y}}$, whoſe fluent is $\frac{4}{15}\,p$, when $y = 1$.

In

In a cylinder, $y = 1$, therefore the fluxion of the moment $= 2\,p\,x^3\,\dot{x}$; whose fluent is $\frac{1}{2}\,p$, when $x = 1$.

The moment of a ring revolving round its center is double the momentum of the same ring revolving round one of its diameters. For let $\dot{z}$ be the fluxion of the arch, y the ordinate, and x the abscissa, radius being unity; $\dot{z}\,y^2$ is the fluxion of the moment of the ring revolving round one of its diameters; but, from the nature of the circle, $\dot{z} = \frac{\dot{x}}{y}$, therefore $\dot{z}\,y^2 = \dot{x}\,y$, which is the fluxion of the area ABR; therefore when $x = 1$, that is, when the arch is equal to $\frac{1}{4}\,p$, the measure of the moment will be the area of a quadrant; and the measure of the moment of the entire ring will be equal to the area of the circle, or $\frac{1}{2}\,p$.

If the ring revolve round its center, in an immoveable plane, its moment will be equal to the ring multiplied into the square of its radius, that is, equal to p Therefore the moment in the former case is to that in the latter, as $\frac{1}{2}\,p$ to p, or as one to two.

Hence, from what has been demonstrated, the momentum of a sphere is to the momentum of a cylinder, revolving round their axes with the same angular velocity, as $\frac{4}{15}$ to $\frac{1}{2}$; the momentum of a cylinder is to the momentum of a ring revolving round its centre, in like manner, as $\frac{1}{2}\,p$ to m; and the momentum

 of

of a ring revolving round its centre, is to the momentum of the ſame ring revolving round one of its diameters, as two to one; therefore compounding theſe ratios, and *ex æquo* the momentum of a ſphere revolving round its axis, is to the momentum of a ring revolving round one of its diameters, as $8p$ to $15m$, or as 800000 × quantity of matter in the ſphere, to 1000000 × the quantity of matter in the ring.

If therefore $9'' \, 7''' \, 20^{iv}$, viz. the quantity of the preceſſion, which according to Newton's calculation ariſes from the action of the ſun alone, be encreaſed in the ratio of 925725 to 800000, it will become $10'' \, 33'''$.

But it is well known, that the true quantity of the preceſſion, ariſing from the action of the ſolar force, is nearly double this quantity. Since therefore the correction of this 3d Lemma will not account for the great difference between the reſult of Newton's calculation and the truth, we muſt look for the cauſe of the difference elſewhere. Simpſon is of opinion, that it ariſes from this, that the momentum of a very ſlender ring revolving about one of its diameters, is only the half of what it would be if the revolution were to be performed in a plane, about the centre of the ring; and therefore, that all concluſions, which do not take this into the account, muſt be two little by juſt one half. But it is evident, that this cannot be the true cauſe of the difference, becauſe Newton did actually conſider, that the motion of a ring round one

of

of its diameters was leſs than when it revolved round its centre, though he has differed from Simpſon in the ratio which he has aſſigned of their motions in theſe two caſes; and when the ratio of their motions is admitted to be as one to two, and the other corrections propoſed by Simpſon are alſo made, the total error on theſe accounts is found to be but 1, 5″, as has been already ſhewn.

Mr. Milner, in his paper on this ſubject in the 69th vol. of the Philoſophical Tranſactions, agrees with Friſi in thinking, that the error lies in Newton's aſſumption, that the receſſion of the nodes of a rigid annulus and a ſolitary moon, revolving in the perimeter of the annulus, are equal; whereas in truth, as they aſſert, (though erroneouſly, as we ſhall preſently ſhew), the receſſion of the latter is but one half of that of the former.

Let us therefore examine particularly whether the receſſion of the nodes of a rigid annulus be indeed double the receſſion of the nodes of a ſolitary moon, as has been aſſerted.

Let A E (Fig. 1.) repreſent the rigid annulus, indefinitely ſlender, projected into its own diameter, P p its axis; let the line of the nodes be at right angles to S C, the line joining the centres of the ſun and earth. From C take the arch C L, and draw L M parallel to D B; let g = the gravity of any given quantity of matter, as a cubic inch; h = the ſpace deſcribed in 1″ by a

 body

body falling freely by the force of gravity; $p =$ the periphery of a circle whofe diameter is unity; alfo let A C = 1; S. angle D C A = s; Cos. D C A = c; arch C L = z; fine of C L = y. Then L M = cy, and C M = sy.

The difturbing force of the fun is equal to $f \times$ L M (Cor. 17. Prop. 66. Lib. 1. Princip.) and the force of a particle of matter at L to move the annulus about the centre, in the direction PQAD, is C M $\times f \times$ L M, acting by the power of the lever C M; that is, the force of this quantity of matter at L is $= csfy^2$; therefore the fluxion of the force of the matter in a quadrant of the annulus is $csfy^2 \dot{z} = csf \times \frac{y^2 \dot{y}}{\sqrt{1-y^2}}$; but the fluent of $\frac{y^2 \dot{y}}{\sqrt{1-y^2}}$ is $\frac{1}{2}z - \frac{1}{2}y \times \overline{1-y^2}|^{\frac{1}{2}}$, and therefore the whole fluent is $\frac{1}{2}csfz - \frac{1}{2}csfy \times \overline{1-y^2}|^{\frac{1}{2}}$; and when $y = 1$, the force of the matter in a quadrant of the annulus is $= \frac{csfp}{4}$, and the force of the whole annulus is $pcsf =$ to the fimple force $\frac{pcsf}{\sqrt{\frac{1}{2}}}$ acting at the diftance $\sqrt{\frac{1}{2}}$ from the centre, that is, at the diftance of the centre of gyration from the centre of the annulus. This is the force of the fun, to difturb the annulus, when at the greateft diftance from the nodes; call this fimple force Fcs.

THE

The quantity of matter in the annulus is $2p$, and the diſtance of the centre of gyration from the centre of the earth is $\sqrt{\frac{1}{2}}$; and by the property of that centre, if the whole matter of the annulus were collected into that point, any force applied to move it about the centre C, would generate the ſame angular velocity, in the ſame time, as it would do in the ring itſelf. And ſince this force Fcs acts at the ſame diſtance $\sqrt{\frac{1}{2}}$ from the centre of the annulus, it is the ſame thing as if it were directly applied to the body to move it. Now to find the motion generated, ſince the ſpace deſcribed in a given time, is as the force directly, and the matter moved inverſely, therefore $g : h :: \frac{pcsf}{2p\sqrt{\frac{1}{2}}} : \frac{hfcs}{2\sqrt{\frac{1}{2}}\,g}$ = the ſpace deſcribed by the centre of gyration in 1″. And $2p\sqrt{\frac{1}{2}}$ (the circumference of the circle whoſe radius is the diſtance of the centre of gyration from the centre of the annulus): $360° :: \frac{hfcs}{2\sqrt{\frac{1}{2}}\,g} : 360 \times \frac{hfcs}{2pg}$ the angle through which the ring is drawn in 1″ by the action of the ſun, when at the greateſt diſtance from the nodes.

But the force of the ſun when at any other diſtance from the nodes, as at H, will be leſs; and the mean quantity of the force may thus be inveſtigated. Draw the great circle $pHGP$, and making radius = 1, let the arch $CH = z$, ſine of $CH = y$; then in the ſphærical triangle CHG, Rad (1): S. CH (y) :: S. angle DCA (s): S. HG = sy. But it has been already proved, that

that the force of the ſun is equal to F × by the product of the ſine and coſine of his height above the plane of the annulus, therefore the force of the ſun at H is equal to $F\,sy \times \overline{1-s^2y^2}|^{\frac{1}{2}}$. But this force acts entirely in the plane P G H p, therefore we muſt reſolve it into two forces, one acting in the plane P Q A, which is that we are looking for, the other in the plane P C p, perpendicular to the former; this latter force is deſtroyed by an equal and contrary force, when the ſun is equidiſtant on the other ſide of the line of the nodes; but the other force always acting in the ſame direction, is that only by which the ring is annually affected. The Cos. G H: Cos. angle **D C A**:: Rad.: Sin. angle **H** (Caſ. 11. Sph. Trig.) and Rad: Sin. angle **H**:: Sin. C H: Sin. C G (Caſ. 2.) ∵ Cos. G H $(\overline{1-s^2y^2}|^{\frac{1}{2}})$: Cos. D C A (c):: Sin. C H (y): Sin. C G $= \frac{cy}{\overline{1-s^2y^2}|^{\frac{1}{2}}}$. Then, to find the part of the force acting in the plane P Q A, Rad. (1.): $F\,sy\sqrt{1-s^2y^2}$ (the whole force):: S. G C $\left(\frac{cy}{\sqrt{1-s^2y^2}}\right)$: $F\,csy^2$, the force in the direction P Q. And hence to find the mean annual force, we muſt find the ſum of all the $F\,csy^2$ in the circle, or the fluent of $F\,csy^2\dot{z} = \frac{F\,csy^2\dot{y}}{\sqrt{1-y^2}}$; whoſe fluent, found as before, is $\frac{1}{2}F\,csz - \frac{1}{2}F\,csy\sqrt{1-y^2}$; and when $y=1$, the fluent becomes $\frac{1}{4}F\,csp$, and in the whole circle $= F\,csp$; this divided by the whole circumference $2p$, the mean force comes out $\frac{1}{2}F\,cs$, that

that is, half the greateſt force, when the ſun is at the greateſt diſtance from the nodes.

Now to compute the force of the ſun to produce the anticipation of the nodes of a ſingle moon at A, the nodes of the orbit being in quadrature; the force of the ſun $= fcs$; the quantity of matter in the moon is $= 1$. Then $g : b :: fcs : \frac{bfcs}{g}$ the ſpace deſcribed in $1''$; and $2p$ (the circumference of a circle whoſe radius is unity, or the diſtance of the moon from the earth) : $360^\circ :: \frac{bfcs}{g} : 360 \times \frac{bfcs}{2pg} =$ the angle deſcribed in $1''$ by the plane of the orbit of a ſolitary moon in ſyzige.

And by a proceſs exactly ſimilar to that uſed before in the caſe of a rigid annulus, it may be ſhewn, that the mean force of the ſun to diſturb the moon, conſtantly in ſyzige, is but half its force when at the greateſt diſtance from the nodes.

It follows therefore, from what has been demonſtrated, that the greateſt force of the ſun to move the annulus in the direction PQA is equal to its greateſt force to move the plane of the moon's orbit, the moon being conſtantly in ſyzige, and that the mean force in both caſes is half the greateſt force; conſequently the mean force of the ſun to move the plane of the annulus in the direction PQA is equal to its mean force to move the plane of a ſolitary moon in ſyzige, in the ſame direction

direction. But by Cor 2 Prop 30. Lib. 3. Principia, in any given position of the nodes, the mean horary motion of the nodes of a solitary revolving moon, is just half the horary motion of the nodes of a moon continually in syzige. And Mr. Landen, in his memoir, has shewn, that when a rigid annulus revolves with two motions, one in its own plane, and the other about one of its diameters, half the whole motive force acting upon the ring is consumed in counteracting the centrifugal force of the ring, by which it endeavours to revolve round a momentary axis, in consequence of its two motions; and the other half only is efficacious in producing the angular motion of the ring about its diameter; so that the motion of the nodes of a detached rigid annulus, being produced by half the mean solar force, is exactly equal to that of the orbit of a solitary moon. For in the case of a solitary moon no centrifugal force to produce a revolution round a momentary axis can take place, there being nothing for the body to act upon; but in a rigid ring, its two motions compounded will give the ring a tendency to revolve about an axis neither perpendicular to nor in the plane of the ring, and therefore this axis cannot be permanent; since each particle of the ring will act by its centrifugal force to impress on it a new motion about an axis perpendicular to the former. But if the rigid annulus, so revolving, be attached to the equator of a sphere, the case will be widely different; for the whole motive force is here employed in giving motion to the annulus and sphere together about

about a diameter of the equator; therefore the part of it which is employed in giving motion to the ring, bears a very ſmall proportion to the whole force, and it is this ſmall part only which is counteracted and rendered inefficient; for the ſphere itſelf has no centrifugal force, whereby it endeavours to revolve round a momentary axis. Hence the motive force being given, viz. the force on the ring, the angular motion generated will be inverſely as the inertia of the matter moved; now the inertia of the annulus is = the matter of the annulus $\times \sqrt{\tfrac{1}{2}}$ (the diſtance of its centre of gyration from the centre of the ring); and the inertia of the ſphere and ring together is = the matter in them $\times \sqrt{\tfrac{2}{5}}$; therefore the angular velocity of the ring muſt be diminiſhed in the ratio of the inertia of the ring to the inertia of the ring and ſphere together, in order to have the angular velocity which now will be produced in the ring, in conſequence of its connection with the ſphere, by the counteracting force. That is, if a be the angular velocity of the ring and ſphere united, the angular velocity which that part of the force which is counteracted could produce in the ring will be $= a \times \dfrac{\text{inertia of the ring}}{\text{inertia of the ſphere}} = a \times \dfrac{1}{250}$. The 250th part therefore of the whole force only is now efficient in moving the ring round its diameter; but this part is = the centrifugal force, and therefore it is this part only of the whole ſolar force which is counteracted.

Hence therefore it appears, that Newton rightly ſuppoſes the preceſſion of the nodes of a rigid, detached annulus, and of a ſolitary moon to be equal; though the principles on which he argues are inſufficient, becauſe he did not, as was neceſſary, conſider the operation of the counteracting centrifugal force. And when he comes to apply this deduction, his concluſion is erroneous, becauſe, omitting the conſideration of the centrifugal force as before, he conceived, that the motion of a ſolitary annulus and of a ring attached to a ſphere were produced by the ſame efficient force; whereas in this latter caſe, the centrifugal force of the annulus vaniſhes, and therefore the whole force of the ſun becomes efficient; that is, the efficient force in the caſe of a ring adhering to the equator of a globe, is double the efficient force in the caſe of a ſolitary ring; and therefore the quantity of the preceſſion, eſtimated on this falſe hypotheſis, comes out too little by juſt one half.

Bishop Horsely, in his commentary on this problem, obſerves, that if this aſſertion, to wit, that the motion of the nodes of a rigid annulus and of a ſolitary moon are the ſame, be true, he cannot ſee how the quantity of the preceſſion of the equinoxes can be different from that which is aſſigned by Newton; but he refrains from any abſolute deciſion: " Si hoc " vere dictum ſit (ſays he) ſc. quod par eſt ratio nodorum " annuli lunarum terram ambientis, ſive lunæ illæ ſe mutuo " contingant, ſive liqueſcant, & in annulum continuum for- " mentur,

" mentur, five denique annulus ille rigefcat, & inflexibilis " reddatur, nefcio qui fieri poffit, ut alius fit punctorum equi- " noctialium motus a vi folis oriundus, quam calculi Newtoniani " fuadent. Quem tamen longe alium invenere viri permagni " Eulerus & Simpfonus noftras, quos velim lector confulas. " Ipfe nil definio." Now from what has been faid it clearly appears, how the motion of the nodes of a folitary moon and rigid annulus may be equal, and yet the quantity of the preceffion affigned by Newton erroneous in the ratio of one to two; the efficient motive force of an attached annulus being double the efficient motive force of a ring revolving folitarily, with a compound motion round its centre and one of its diameters.

If then the corrected quantity of 10″ 33‴, be further corrected, by augmenting it in the ratio of two to one, the refult will nearly agree with the quantity inveftigated by other eminent mathematicians; thus Simpfon makes it 21″ 7‴, Landen 27″ 7‴, D'Alambert 23″ nearly; Euler 22″; Frifi 21¼″; Milner 21″ 6‴, and Mr. Vince, 21″ 6‴; fee Phil. Tranf. vol. 77.

From this review of the folutions of this problem, it appears that Mr. Landen has the honour of having firft detected the particular fource of Newton's miftake, by difcovering that when a rigid annulus revolves with two motions, one in its own plane and the other round one of its diameters, half the motive force

 acting

acting upon the ring is counteracted by the centrifugal force arising from this compound motion, and half only is efficacious in accelerating the plane of the annulus round its diameter. As Mr. Landen has not expressly demonstrated this proposition, I am persuaded I shall afford the mathematical reader much gratification, by here laying before him the following very elegant demonstration, communicated to me by the learned Mr. Brinkley, Professor of Astronomy in the University of Dublin.

Prop. If a rigid ring $n\,q$ N Q revolves with two motions (fig. 3.), one in its own plane, and the other about the diameter q T Q; and if a motive force, acting at the point Q, be supposed equivalent to the whole motive force acting upon the ring, then half this force is efficacious in accelerating the motion of the point Q (in a direction perpendicular to the plane of the ring) and the other half is consumed in counteracting the centrifugal force, arising from the motion of the particles of the ring about a momentary axis P T p.

In the great circle $n\,b$ let a point b (fig. 3.) be taken indefinitely near to n, and in the ring a point r, so that $n\,b$ and Qr may represent the angular velocities about the diameter and the centre of the ring. Let d and c represent these velocities, and r the radius of the ring. Draw $r\,s$ perpendicular to the plane of the ring, and meeting the great circle bQs in s; then

then will rs reprefent the accelerating force of the point Q perpendicular to the plane of the ring; but $rs : nb :: Qr :$ Rad. (r), therefore $rs = \frac{cd}{r}$.

CONSEQUENTLY, if R = the matter of the ring, a motive force acting upon the point $Q = \frac{cd}{r} \times \frac{1}{2} R$ will be equivalent to the whole efficacious motive force on the ring.

THE momentary axis PTp is in a plane perpendicular to the plane of the ring, and which paffes through Qq. Make PT = the radius of the ring, and draw Pr perpendicular to Qq, and we have $Pr : Tr :: d : c$, or $Pr = \frac{dr}{\sqrt{c^2+d^2}}$, and $Tr = \frac{cr}{\sqrt{c^2+d^2}}$. Let PT (in fig. 4.) reprefent the momentary axis, and QEN a quadrant of the ring. From any point E of the ring draw Ev perpendicular to PT, and vw perpendicular to QT. The centrifugal force of E : centrifugal force of N :: Ev : NT, or the centrifugal force of E = centrifugal force of N $\times \frac{Ev}{NT} = \frac{c^2+d^2}{r} \times$ particle $E \times \frac{Ev}{NT}$, becaufe the velocity of $N = \sqrt{c^2+d^2}$. But the efficacious part of this force in a direction perpendicular to the plane of the ring = whole $\times \frac{vw}{Ev}$; and a force acting at Q equivalent

valent to this = whole $\times \frac{vw}{Ev} \times \frac{Tx}{TQ} = \frac{c\ + a^2}{r} \times E \times \frac{Ev}{NT} \times \frac{vw}{Ev} \times \frac{Tx}{TQ} = \frac{c^2 + d^2}{r} \times E \times \frac{vT \times Pr \times Tx}{TQ^3}$. Now if great circles be conceived drawn through P, Q, and P, E; (by Sph. Trig.) cos. PE (vT) × Rad. (TQ) = cos. PQ (Tr) × cos. QE (Tx). Therefore a motive force at Q equivalent to the motive, efficient, centrifugal force of E $= \frac{c^2 + d^2}{r} \times E \times \frac{Tr \times Pr \times Tx^2}{TQ^4}$; therefore the ſum of all theſe quantities = the motive force at Q equivalent to the ſum of all the efficient centrifugal forces, or the centrifugal force of the ring. But it is eaſily ſhewn, that the ſum of all theſe quantities $= \frac{c^2 + d^2}{r} \times \frac{1}{2} R \times \frac{Tr \times Pr \times TQ^2}{TQ^4} = \frac{c^2 + d^2}{r} \times \frac{1}{2} R \times \frac{cdr^2 \times TQ^2}{c^2 + d^2 \times TQ^4} = \frac{cd}{r} \times \frac{1}{2} R$. Hence the motive force at Q, equivalent to the ſum of all the efficacious centrifugal forces, is expreſſed by the ſame quantity $\frac{cd}{r} \times \frac{1}{2} R$, as the force at Q equivalent to the whole motive, efficacious force on the ring. Q. E. D.

Mr. Simpson has pointed out the miſtakes in the ſolutions of this problem propoſed by M. Silvabelle and Walmeſley; but neither is his own calculation entirely faultleſs; and his concluſion appears to be correct, only becauſe the errors in the premiſes compenſate each other. Thus he ſuppoſes, that the whole motive force,

force, acting on a detached rigid ring, revolving with a two-fold motion, one round its centre, the other round a diameter, is equal to the efficient force by which the plane of the ring is moved round its diameter; whereas the former is to the latter as two to one; half the whole motive force being counteracted and rendered inefficient by the centrifugal force. 2dly, He fuppofes, that the whole efficient motive force, acting on a detached rigid annulus, revolving in the fame manner as before, is equal to the whole efficient motive force acting on an annulus, attached to and connected with a fphere, which is alfo falfe in the ratio of one to two; the centrifugal force in the cafe of an attached annulus vanifhing; and therefore no part of the whole force is rendered ineffectual; and confequently half the motive force in the latter cafe will produce an equal effect as the whole in the former, half of the force in the former cafe not contributing in any degree to the motion of the annulus round its diameter, but being totally employed in counteracting the tendency of the ring to revolve round a momentary axis.

Mr. Milner's and Frifi's calculations become likewife correct in the refult, in the fame manner as Simpfon's, by the mutual counteraction of equal and contrary errors. Thus they both hold, that the preceffion of a rigid annulus is double that of a folitary moon, whereas they are equal, as we have already demonftrated, by which the preceffion would come out twice greater than the truth; but they likewife are of opinion, that the pre-

ceffion

ceſſion of an attached and ſolitary annulus are equal, whereas the former is double that of the latter; this error therefore counterbalances the former.

Mr. Emerson has given two ſolutions of this queſtion, which are both erroneous, one in his Miſcellanies, the other in his Fluxions. In the former he adopts the ſame principles with Newton, in ſuppoſing the preceſſion of a ſolitary moon, a detached rigid annulus, and an attached annulus to be equal. In the latter he determines the direction in which a body would move in conſequence of a uniform motion impreſſed on it in one direction, and a uniformly accelerated motion in another, to be the diagonal of a parallelogram, whoſe two ſides repreſent the ſpaces deſcribed from quieſcence, in the ſame time, by the two forces; which, as Mr. Milner has juſtly obſerved, produces an error of one half in the concluſion. For let A D be the ſpace deſcribed by the uniform motion (fig. 5.), while the body would deſcribe A B by the accelerated motion; ſince the time is indefinitely little, the accelerating force may be conſidered as conſtant, and therefore the body will in fact deſcribe the parabola A G C; and the direction of the motion at C will be the tangent E C; but the angle D E C = D A C + A C E = 2 D A C nearly, becauſe the tangents A E, C E, are very nearly equal (Ham. Con. Cor. 1. Prop. 3. Lib. 2. and Prop. 3. Lib. 3.); that is, the true angle of deviation D E C, is very nearly double the angle of of deviation DAC, as determined by the diagonal of the parallelogram.

In this solution Mr. Emerson says, "the earth being an oblate "spheroid, the sphere is encompassed with a solid crust going "round the equator in the manner of a ring; now the effect of "the forces of the sun and moon upon this crust, and the motion "communicated thereby to the whole body of the earth, is what "we are to enquire after." He then calculates the force of the sun upon the annulus, and supposes this whole force efficient; he next supposes this whole motive force to act at the distance of the centre of gyration from the centre of the earth, and thence deduces the motion generated in the plane of the equator about one of its diameters. It appears therefore, that he supposes the whole motive force of the sun to be efficient on the annulus, separately considered: and 2dly, that this efficient force is equal to the efficient force on the same annulus, when connected with the earth; which, exclusive of the error detected by Mr. Milner, are the very same false hypotheses with those adopted by Simpson.

But here a question naturally arises, if the error of Newton's calculation be as great as is pretended, whence comes it to pass that the result of his calculation agrees so exactly with phænomena; for on supposition, that the precession arising from the force of the sun alone is but $9'' \; 7'''$, the precession caused by the moon will be $40'' \; 52''' \; 52^{iv}$, and the whole preceſſion, ariſing from both cauſes conjoined, will be $50'' \; 0''' \; 12^{iv}$, according to obſervation.

To this objection a ſatisfactory anſwer is ſuggeſted by Newton himſelf, where he ſays, that the preceſſion will be diminiſhed if the matter of the earth be rarer at the circumference than at the centre. The reaſon of which is evident from what has been already demonſtrated, for the quantity of matter in the earth being given, the diſtance of the centre of gyration from the centre of the earth will be leſs, the more the matter of the earth is accumulated towards the centre, and therefore the leſs will be the angular motion generated by the ſun and moon.

Fig. 1.

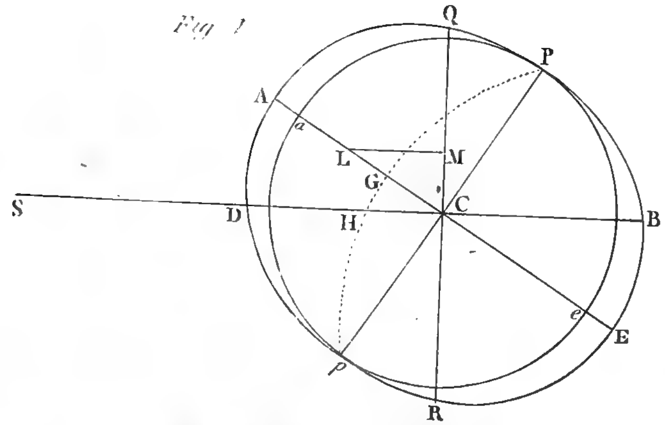


Fig. 5.

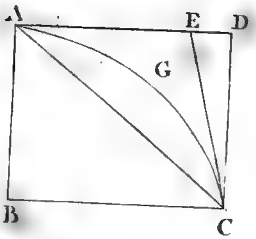


Fig. 2.

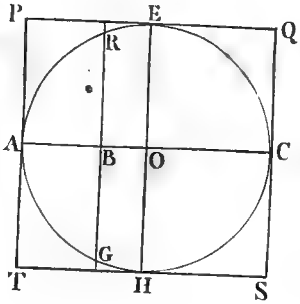


Fig. 3.

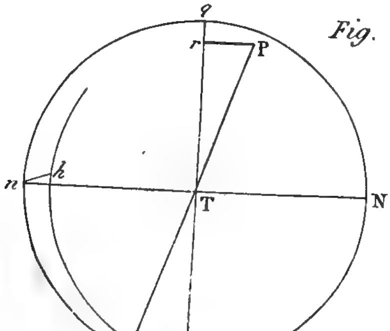
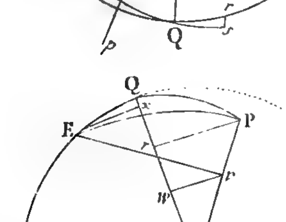


Fig. 4.



GENERAL DEMONSTRATIONS *of the* THEOREMS *for the* SINES *and* COSINES *of* MULTIPLE CIRCULAR ARCS, *and also of the* THEOREMS *for expressing the* POWERS *of* SINES *and* COSINES *by the* SINES *and* COSINES *of* MULTIPLE ARCS; *to which is added a* THEOREM *by help whereof the same* METHOD *may be applied to demonstrate the* PROPERTIES *of* MULTIPLE HYPERBOLIC AREAS.

By the Rev. J. BRINKLEY, *A.M. ANDREWS' Professor of Astronomy, and M.R.I.A.*

Read May 6, 1797.

THEOREMS by help of which the chords of multiple circular arcs may be found in terms of the chord of the simple arc were first given by Vieta, and afterwards in a different manner by Mr. Briggs, which are very fully explained in the Trigonometria Britannica, and their uses in constructing trigonometrical tables shewn. From these may readily be deduced theorems for the cosines of multiple arcs in terms of the cosine of the simple arc, and for the sines in terms of the sine of the simple arc when the multiplier is an *odd* number, and consequently the series first given by Sir Isaac Newton for the sine of a multiple arc when the multiplier is an odd number, the only case in which that series terminates—Afterwards similar

 theorems

theorems for the ſine and coſine of multiple arcs, when the multiplier is any whole poſitive number even or odd, were given by ſeveral authors—But all the writers on this ſubject that I have ſeen, except Dr. Waring, have deduced the law of the ſeries from obſervation in a few inſtances without a general demonſtration of its truth——Dr. Waring has (Curv. algebr. Propr. Theor. 26 & Cor.) by help of his admirable theorem for finding the ſums of the powers of the roots of an equat. given a general demonſtration of the ſeries for finding the chord of the ſupplement of a multiple arc in terms of the chord of the ſupplement of the ſimple arc, and conſequently a general demonſtration of the theorem for the coſine of a multiple arc in terms of the coſine of the ſimple arc, and alſo of the ſine of a multiple arc when the multiplier is an odd number. But in the caſe where the multiplier is an even number no demonſtration, as far as I have ſeen, has ever been given by any author. Dr. Waring's method of demonſtration cannot be applied to this caſe—The following demonſtration extends to every multiplier whether even or odd. The demonſtrations for the ſine and coſine of the multiple arc in terms of the coſine of the ſimple arc, from whence the other theorems are immediately deducible, are of this kind—The probable law is deduced from obſervation in a few inſtances and then the general truth of that conjecture is proved. Dr. Waring's demonſtration, although by a very different proceſs, being founded upon the properties of algebraical equations, is alſo of this kind, as it depends

upon

upon his theorem for the ſums of the powers of the roots of an equation, of which he has given the ſame kind of demonſtration—Previous to the demonſtrations of theſe theorems I have given a demonſtration of the theorems for expreſſing the ſine and coſine of multiple arcs in terms compounded of the ſine and coſine—Theſe theorems alſo have been given by many authors, and the only general demonſtrations of them have been deduced from the hyperbola and the conſideration of impoſſible quantities—However uſeful impoſſible quantities may be in diſcovering mathematical truths they ought never to be uſed in ſtrict demonſtration, and it muſt ſeem a very circuitous mode to apply the properties of the hyperbola to demonſtrate thoſe of the circle—Theſe demonſtrations are from the properties of the circle and the theorems for combinations.

The theorems hitherto mentioned are more particularly applicable to the conſtruction of trig. tables and the reſolution of certain equations—In conſequence of the great advances that have been made in phyſical aſtronomy ſince the time of Sir Iſaac Newton, it has been found neceſſary for facilitating the calculation of particular fluents to expreſs the powers of the ſine and coſine in terms of the ſines and coſines of multiple arcs, and theorems for this purpoſe have been given by ſeveral authors. They have all however either deduced the general law from obſervation without demonſtration, or generally demonſtrated it by help of impoſſible logarithms—The demonſtrations

demonſtrations here given are general, and deduced from the circlė by help of the doctrine of combinations.

As the hyperbola has been ſo frequently uſed to demonſtrate properties of the circle, I have ſubjoined a theorem by which the connection of multiple circular areas, and multiple hyperbolic areas is more fully apparent than by any other that I have met with, and from whence by the doctrine of combinations, theorems may be deduced for hyperbolic areas ſimilar to thoſe of the circle.

I. Theorem. Let s and c be the ſine and coſine of any arc a, then, radius being unity, and n any whole number,

1. The ſine of $na = nc^{n-1}s - \frac{n.\overline{n-1}.\overline{n-2}}{1.\ 2.\ 3} c^{n-3}s^{3} +$ &c.

2. The coſine of $na = c^{n} - \frac{n.\overline{n-1}}{1.\ 2} c^{n-2}s^{2} +$ &c.

In each the powers of s increaſe by 2, and thoſe of c diminiſh by 2, till the laſt becomes 1 or 0. In the ſine the coefficient of $c^{n-v}s^{v} = \pm \frac{n.\overline{n-1}.\overline{n-2} - - (\text{to } v \text{ terms}}{1.\ 2.\ 3 - - - v}$ + when $\frac{v-1}{2}$ is even and — when odd. And in the coſine the coefficient of $c^{n-v}s^{v} = \pm \frac{n.\overline{n-1}.(\text{to } v \text{ terms}}{1.\ 2.\ 3.\ v}$ + when $\frac{v}{2}$ is even and — when odd.

Demonſtration

Demonſtration—Let a, a', a'', a''', &c. repreſent any arcs
s, s', s'', s''', their ſines
c, c', c'', c''', their coſines

Then by the common theorem for the ſine and coſine of the ſum of two arcs,

$$\left.\begin{array}{l}\text{The ſine}\\ \text{The coſine}\end{array}\right\} \text{of } a + a' = \left\{\begin{array}{l} s c' + s' c \\ c c' - s s' \end{array}\right.$$

$$\left.\begin{array}{l}\text{The ſine}\\ \text{The coſine}\end{array}\right\} \text{of } a + a' + a'' = \left\{\begin{array}{l} s c' c'' + s' c c'' + s'' c' c - s s' s'' \\ c c' c'' - c s' s'' - c' s s'' \end{array}\right.$$

&c. &c.

The following obſervations may be readily made by conſidering the way which in theſe ſucceſſive values are formed.

1. In both ſine and coſine of the ſum of n arcs ($a + a' + a''$ &c. the number of factors $s s'$ - - - $c c'$ in any term is equal to n and that the ſines s, s', s'', &c. and alſo the coſines c, c', c'', &c. are concerned exactly alike in the whole quantity.

2. In the ſine of the ſum of n arcs ($a + a' +$ &c.) the greateſt number of coſines c, c', c'', &c. together in any term $= n - 1$. This number diminiſhes by 2, and conſequently the number of s, s', &c. increaſes by 2.

3. In

3. In the cofine of the fum of n arcs the greateft number of c, c', c'', &c. in any term $= n$. the next lefs number $n-2$, &c. and confequently the number of s, s, &c. increafes by 2.

4. With refpect to the figns of the different products—In the fine of n arcs ($a + a' + a'' +$ &c) when 1, 5 or $4p + 1$ (p being any number) s, s', s'' &c. are united together, the fign is $+$ otherwife —. In the cofine of n arcs when 2, 6, 10 or $2p$ (p being odd) s, s', s'' are united together the fign will be — otherwife $+$.

5. In no term can the fine and cofine of the fame arc occur.

6. In any term $s\,s'\,s''$ - - - $c\,c'\,c''$ - - - whether of the fine or cofine if m be the number of the cofines and confequently $m-n$ the number of the fines: then, becaufe each of the quantities s, s' &c. and alfo c, c' &c. are concerned exactly alike in the fine of the fum of n arcs ($a + a' + a'' +$ &c.), and alfo in the cofine of the fum of n arcs ($a + a' + a'' +$ &c.) and likewife becaufe the fine and cofine of the fame arc cannot occur in the fame term, it follows that the number of terms $s\,s'\,s''$ (m terms) - - - $c\,c'\,c''$ - - - ($\overline{m-n}$ terms) = the number of combinations of n things taken m together $= \dfrac{n \cdot \overline{n-1} \cdot \overline{n-2} \;\text{- - -}\; \overline{n-m-1}}{1 \cdot 2 \cdot 3 \;\text{- - -}\; m}$.

From

FROM thefe obfervations it immediately follows, if a, a', a'', &c. are all equal, that the fine of $na = nc^{n-1}s - \frac{n.\overline{n-1}.\overline{n-2}}{1.2.3}c^{n-3}s^3 +$ &c. and that the cofine of $na = c^n - n.\frac{n-1}{1.2}c^{n-2}s^2 +$ &c. and alfo that the general terms are as ftated in the theorem. Q. E. D.

II. THEOREM. 1. The cofine of $na = 2^{n-1}c^n - n.2^{n-3}c^{n-2} + \frac{n.\overline{n-3}}{1.2}2^{n-5}c^{n-4}$ &c. to be continued by fucceffively diminifhing the index of c by 2 till it becomes 1 or 0, and affixing to c^{n-u} the coeff.

$$\pm 2^{n-u-1}\cdot\frac{n.\overline{n-u+1}.\overline{n-u+2}\text{ - - to }\frac{u}{2}\text{ terms}}{1.\quad 2.\quad 3\text{ - - - }\frac{u}{2}}$$ of which the fign is $+$ when $\frac{u}{2}$ is even, and — when odd.

2. The fine of $na = \overline{2^{n-1}c^{n-1} - \overline{n-2}.2^{n-3}c^{n-3} + \text{\&c.}} : \sqrt{1-c^2}$ continued by diminifhing the index of c by 2 till it becomes 1 or 0, and affixing to c^{n-u} the coefficient

$$+ 2^{n-u}\times\frac{\overline{n-u+1}.\overline{n-u+2}.\text{ - - }(\frac{u-1}{2}\text{ terms})}{1.\quad 2.\text{ - - }\frac{u-1}{2}}$$ of which the fign is $+$ when $\frac{u+1}{2}$ is odd and — when even.

Demonstr. By fubftituting in the values of the fine and cofine of $n\,a$ found by the laft theorem, for n fucceffively 2, 3, 4, &c. and exterminating s it may be conjectured that the general terms of the fine and cofine will be as here ftated. That this conjecture is true appears in the following manner:

Let $B\,c^{n-1-u}$ be a term in the cofine of $\overline{n-1}\,a$, and $C\,c^{n-u}\,\overline{\sqrt{1-c^2}}$, and $D\,c^{n-u-2}\,\overline{\sqrt{1-c^2}}$ terms in the fine of $\overline{n-1}\,a$: and that the latter terms will be of this form appears from the former theorem. Applying the common theorem for the fine and cofine of the fum of two arcs, it readily appears that the coeff. of c^{n-u} in the cofine of $n\,a = B - C + D$.

Now fuppofing the theorem generally true and fubftituting in the general terms for n, $\overline{n-1}$ and for u fubft. u, $u-1$ and $u+1$ fucceffively, the refult is

$$B = \pm\,2^{n-u-2} \times \frac{\overline{n-1}\cdot\overline{n-u}.\ \overline{n-u+1}\ \text{- - - - to}\ \frac{u}{2}\ \text{terms}}{1.\quad 2.\quad 3\quad \text{- - - to}\ \frac{u}{2}\ \text{terms}}$$

$$-C = \pm\,2^{n-u} \times \frac{\overline{n-u+1}.\ \overline{n-u+2}\ \text{- - - - to}\ \frac{u}{2}-1\ \text{terms}}{1.\ 2.\ 3.\ \text{- - - - - - to}\ \frac{u}{2}-1\ \text{terms}}$$

D=

$$D = \pm 2^{n-u-2} \times \frac{\overline{n-u-1}.\ \overline{n-u}\ ----\ \text{to } \frac{u}{2} \text{ terms}}{1.\quad 2.\quad 3.\ --\ \text{to } \frac{u}{2} \text{ terms}}$$

$$\therefore\ B-C+D = \left\{ \begin{array}{l} \pm 2^{n-u-1} \times \dfrac{\overline{n-1}.\ \overline{n-u}}{u} \\[2ex] \pm 2^{n-u-1} \times 2.\ \overline{n-\dfrac{u}{2}-1} \\[2ex] \pm 2^{n-u-1} \times \dfrac{\overline{n-u-1}.\ \overline{n-u}}{u} \end{array} \right\} \times \frac{\overline{n-u+1}.}{1.\quad 2.\ -}$$

$$\frac{\overline{n-u+2}\ -\ \text{to } \overline{\frac{u}{2}-2} \text{ terms}}{-\ -\ -\ -\ -\ \overline{\frac{u}{2}-1}} =$$

$$\pm 2^{n-u-1} \times \frac{n.\ \overline{n-u+1}.\ \overline{n-u+2}\ --\ \text{to } \frac{u}{2} \text{ terms.}}{1.\ 2.\quad 3.\ ---\ \frac{u}{2}}$$

Let alſo G $c^{n-u-1} s$ be a term in the ſine of $\overline{n-1}.\ a$, and let H c^{n-u} be a term in the coſine of $\overline{n-1}\ a$, and it readily appears that G + H = coeff. of the term $c^{n-u} s$ in the ſine of $n\ a$. Now ſuppoſing the general term of the ſine truly expreſſed.

$$G = \pm 2^{n-u-1} \times \frac{\overline{n-u} \cdot \overline{n-u+1} \; - \; - \; - \text{ to } \frac{u-1}{2} \text{ terms}}{1.\ 2.\ 3. \; - \; - \; - \; - \; - \; - \text{ to } \frac{u-1}{2} \text{ terms}}$$

$$H = \pm 2^{n-u-1} \times \frac{\overline{n-1} \cdot \overline{n-u+1} \; - \; - \; - \text{ to } \frac{u-1}{2} \text{ terms}}{1.\ 2.\ 3 \; - \; - \; - \; - \; - \; - \; \frac{u-1}{2} \text{ terms}}$$

$$G + H = + 2^{n-u} \times \frac{\frac{1}{2} \times \overline{2\,n-u-1} \cdot \overline{n-u \mp 1} \text{ - to } \frac{u-1}{2} \text{ terms}}{1.\ 2.\ 3 \; - \; - \; \frac{u-1}{2} \text{ terms}}$$

$$= \pm 2^{n-u} \times \frac{\overline{n-u+1} \cdot \overline{n-u+2} \; - \; - \text{ to } \frac{u-1}{2} \text{ terms}}{1.\ 2.\ 3 \; - \; - \text{ to } \frac{u-1}{2} \text{ terms.}}$$

Hence it appears that if the general terms are rightly expreſſed for the ſine and coſine of $\overline{n-1}\,a$, they are alſo rightly expreſſed for the ſine and coſine of $n a$, conſequently if they are true in the inferior values of n they are true in the ſuperior, but they are true in the inferior ∵ &c. &c.

III. Cor. If the ſeries be arranged in a contrary order:

1. When

1. When n is *even* the cofine of $na = \pm 1 \mp \frac{n^2 c^2}{1.2} \pm \frac{n^2 . \overline{n^2 - 2^2}}{1.2.3.4.} c^4$ $\mp$ &c. and the general term is $\pm \frac{n^2 . \overline{n^2 - 2^2} . \overline{n^2 - 4^2} \text{ - to } \frac{v}{2} \text{ terms}}{1. \; 2. \; 3 \; - \; - \; v} c^v$ where v is always even. When n is of the form $2p$, (p being any *odd* number the fign will be + or — according as $\frac{v}{2}$ is odd or even and when n is of the form $4p$, (p being any number) it will be + or — according as $\frac{v}{2}$ is *even* or *odd*.

2. When n is *odd*, the cofine of $na = \pm nc \mp \frac{n . \overline{n^2 - 1} \; c^3}{1. \; 2. \; 3} \pm$ &c. and the general term is $\pm \frac{n . \overline{n^2 - 1} . \overline{n^2 - 3^2} \text{ - to } \frac{v+1}{2} \text{ terms}}{1. \; 2. \; 3 \; - \; - \; v} c^v$ where v is always odd. When n is of the form $4p + 1$ the fign will be + or — according as $\frac{v+1}{2}$ is odd or even, when of the form $4p + 3$ it will be + or — according as $\frac{v+1}{2}$ is even or odd. Each feries is to be continued till the coefficient becomes = o.

Dem. The general term of the cofine of na.

= ±

$$= \pm\, 2^{n-u-1} \times \frac{n.\,\overline{n-u+1}.\,\overline{n-u+2}\ \ -\ -\ \frac{u}{2}\text{ terms}}{1.\ \ 2.\ \ 3\ \ -\ -\ \frac{u}{2}\text{ terms}}\, c^{n-u}$$

or fubftituting for u, $n-v$, the coeff. becomes

$$\frac{\pm\, n.\,\overline{v+1}.\,\overline{v+2}\ \ -\ \ \frac{\overline{n+v-4}}{2}.\,\frac{\overline{n+v-2}}{2}}{1.\ \ 2\ \ 3\ \ -\ -\ \frac{\overline{n-v}}{2}}\, 2^{v-1}$$

$$= \pm \frac{n.\,\frac{\overline{n-v-2}}{2}.\,\frac{\overline{n-v-4}}{2}\ -\ \frac{\overline{n+v-4}}{2}.\,\frac{\overline{n+v-2}}{2}}{1.\ \ 2.\ \ 3\ \ -\ \ v}\, 2^{v-1}$$

$$= \pm \frac{n.\,\overline{n-v-2}.\,\overline{n-v-4}\ \ -\ -\ \overline{n+v-4}.\,\overline{n+v-2}}{1.\ \ 2.\ \ 3\ \ -\ -\ v}$$

1. When n is even and $\because$ v even it is of this form

$$+ \frac{n.\,\overline{n-v-2}\ \ -\ -\ \overline{n-2}.\,n.\,\overline{n+2}\ \ -\ -\ \overline{n+v-2}}{1.\ \ 2.\ \ 3\ -\ -\ v}$$

$$= \pm \frac{n^2.\,\overline{n^2-2^2}.\,\overline{n^2-4^2}\ \ -\ -\ \text{to } \frac{v}{2}\text{ terms}}{1.\ 2.\ \ 3\ \ -\ -\ v}$$

The fign is + or — according as $\frac{u}{2}$ or $\frac{n-v}{2}$ is even or odd.

$\therefore$ If

∴ If n be of the form $2p$, (p being odd) the ſign is $+$ or $-$ according as $\frac{2p-v}{2}$ is even or odd ∴ as $\frac{v}{2}$ is odd or even. If n be of the form $4p$ then it is $+$ or $-$ as $\frac{4p-v}{2}$ is even or odd and ∵ as $\frac{v}{2}$ is even or odd.

2. When n is odd and ∵ v odd the gen. coeff. becomes of this form $\pm \frac{n.\overline{n-v-2} \;\; - \; - \;\; \overline{n-1}.\overline{n+1} \;\; - \; - \;\; n+\overline{v-2}}{1.\ 2.\ 3 \;\; - \; - \; - \;\; v}$

$$\overline{2\quad 2}\quad\overline{2\quad 2}$$

$$= \pm \frac{n.\ n-1\ .\ n-3 \quad - \quad -}{1.\ 2.\ 3 \quad - \quad - \quad - \quad v} \text{ to } \frac{v+1}{2} \text{ terms.}$$

The ſign is $+$ or $-$ according as $\frac{u}{2}$ or $\frac{n-v}{2}$ is even or odd.

∴ If n be of the form $4p+1$ it is $+$ or $-$ as $\frac{4p+1-v}{2}$ or $\frac{4p+2-\overline{v+1}}{2}$ is even or odd or ∴ as $\frac{v+1}{2}$ is odd or even. If n be of the form $4p+3$, it is $+$ or $-$ as $\frac{4p+3-v}{2}$ or $\frac{4p+4-\overline{v+1}}{2}$ or ∴ as $\frac{v+1}{2}$ is even or odd. Whence &c. &c.

IV. Theorem.

IV. THEOREM. 1. When n is any even number. The fine of $na = \pm 2^{n-1} s^{n-1} \mp 2^{n-3}.\overline{n-2}\, s^{n-3} \pm$ &c. : $\sqrt{1-s^2}$ to be continued by diminifhing the index of s by 2 till it becomes unity. The upper figns take place when n is of the form $2p$ (p being odd) and the lower when it is of the form $4p$ (p being any number).

THE general term is $\pm \dfrac{\overline{n-u+1}.\ \overline{n-u+2}\ \text{-}\ \text{to}\ \frac{u-1}{2}\ \text{terms}}{1.\ 2.\ 3\ \text{-}\ \frac{u-1}{2}}$

$\times\ 2^{n-u} s^{n-u} \times \sqrt{1-s^2}$: $+$ when $\frac{u+1}{2}$ is odd, $-$ when $\frac{u+1}{2}$ is even, and n of the form $2p$ (p being odd.)

$+$ when $\frac{u+1}{2}$ is even, $-$ when $\frac{u+1}{2}$ is odd, and n of the form $4p$ (p being any number)

2. WHEN n is any odd number, the fine of $na = \pm 2^{n-1} s^{n} \mp n\, 2^{n-3} s^{n-2} +$ &c. to be continued by diminifhing the index of s by 2 till it becomes unity. The upper figns take place when n is of the form $4p+1$, and the under when of the form $4p+3$.

THE

THE general term is $\pm \dfrac{n.\ \overline{n-u+1}.\ \overline{n-u+2}\ \ -\ \ \text{to}\ \frac{u}{2}\ \text{terms}}{1.\ 2.\ 3\ \ -\ \ -\ \ \frac{u}{2}} \times$ $2^{n-u-1}\ s^{n-u}.$

$$\left.\begin{array}{l} +\ \text{when}\ \frac{u}{2}\ \text{is even} \\ -\ \text{when}\ \frac{u}{2}\ \text{is odd} \end{array}\right\}\ \text{and } n \text{ of the form } 4p+1.$$

$$\left.\begin{array}{l} +\ \text{when}\ \frac{u}{2}\ \text{is odd} \\ -\ \text{when}\ \frac{u}{2}\ \text{is even} \end{array}\right\}\ \text{and } n \text{ of the form } 4p+3.$$

DEMON. The general term of the ſine of $n \times \overline{Q-a} = (\text{II})$

$$= \pm \frac{\overline{n-u+1}.\ \overline{n-u+2}\ \ -\ \ -\ \ \text{to}\ \frac{u-1}{2}\ \text{terms}}{1.\ 2.\ 3\ \ -\ \ -\ \ \frac{u-1}{2}}\ 2^{n-u}\ \overline{cs, Q-a}\,^{n-u}$$

$\times\ s, \overline{Q-a},$ where Q is a quad.

1. LET n be of the form $2p$, p being odd. The ſine of $2p \times \overline{Q-a} = s, \overline{2p-2\,Q + 2\,Q - 2pa} =$ (becauſe $\overline{2p-2}.\ Q$ is a multiple of the circumference) ſine $\overline{2\,Q-2pa} = s, 2pa$ ∴ when n is of the form $2p$, p being odd the general term of the ſine of $na =$

 +

$$\pm \frac{\overline{n-u+1}.\ \overline{n-u+2} \quad - \quad \text{to } \frac{u-1}{2} \text{ terms}}{1.\ 2.\ 3 \quad - \quad - \frac{u-1}{2}} \, 2^{n-u} \, \overline{s,a}^{\,n-u} \times cs\ a,$$

$+$ when $\frac{u+1}{2}$ is odd and $-$ when even.

Let n be of the form $4p$, p being any number.

The fine of $4p \times \overline{Q-a} =$ (becaufe $4p\,Q$ is a multiple of the circumference) $=$ fine of $-4pa = -s,\ 4pa$ $\therefore$ when n is of the form $4p$ the general term of the fine of $na = \pm$

$$\pm \frac{\overline{n-u+1} \quad - \quad - \quad \text{to } \frac{u-1}{2} \text{ terms}}{1.\ 2 \quad - \quad - \quad - \quad \frac{u-1}{2}} \times 2^{n-u} \, \overline{s,a}^{\,n-u} \times cs\ a - \text{when}$$

$\frac{u+1}{2}$ is odd and $+$ when even.

2. When n is odd.

The general term of the cofine of $n \times \overline{Q-a} =$ (II)

$$= \pm \frac{n.\ \overline{n-u+1}.\ \overline{n-u+2} \quad - \quad \text{to } \frac{u}{2} \text{ terms}}{1.\ 2.\ 3 \quad - \quad - \quad \frac{u}{2}} \, 2^{n-u-1} \, cs\ \overline{Q-a}^{\,n-u}$$

Let n be of the form $4p+1$.

The cofine of $\overline{4p+1} \times \overline{Q-a} = cs\ \overline{4p\,Q + Q - \overline{4p+1}\,a} = cs,\ \overline{Q - \overline{4p+1}\,a} =$ fine $\overline{4p+1}\,a$ $\therefore$ when n is of the form $4p+1$.

the

the gen. term of the fine of $n a =$

$$\pm \frac{n . \overline{n-u+1} . \overline{n-u+2} \quad - \quad - \quad \text{to } \frac{u}{2} \text{ terms}}{1 . 2 . 3 \quad - \quad - \quad - \quad - \quad \frac{u}{2}} .$$

$$2^{n-u-1} \, \overline{s, a}^{\,n-u} + \text{ when } \frac{u}{2} \text{ is even and} - \text{when odd.}$$

LET n be of the form $4p+3$.

THE cofine of $\overline{4p+3} \times \overline{Q-a} = cs$ of $\overline{3Q-\overline{4p+3}\,a} =$ (becaufe adding or fubtracting $\frac{1}{2}$ the circumference changes the fign of the cofine) $= - cs$ of $\overline{Q-\overline{4p+3}\,a} = - s.$ of $\overline{4p+3}\, a$.

∴ WHEN n is of the form $\overline{4p+3}$ the general term of the fine of

$$n a = \pm \frac{n . \overline{n-u+1} . \overline{n-u+2} \quad - \quad \text{to } \frac{u}{2} \text{ terms}}{1 . 2 . 3 . \quad - \quad - \quad \frac{u}{2}} \; 2^{n-u-1} \, \overline{s\, a}^{\,n-u} .$$

$-$ when $\frac{u}{2}$ is even and $+$ when odd. Whence the truth of the theorem will eafily appear.

V. COR. If the feries be arranged in a contrary order.

THE fine of n A $= n s - \frac{n . \overline{n^2 - 1^2}}{1 . 2 . 3} s^3 +$ &c. when n is any odd

 number

number; and the ſine of $na = ns - \frac{n.\overline{n^2-2^2}}{1.\ 2.\ 3}s^3 + \&c.$ $\times \sqrt{1-s^2}$ when n is any even number.

In the former caſe the general term is

$$\pm\frac{n.\ \overline{n^2-1}.\ \overline{n^2-3^2}\ \text{ - - to } \frac{v+1}{2} \text{ terms}}{1.\ 2.\ 3\ \text{ - - - to } v \text{ terms}} \times s^v$$

v being always odd, $+$ when $\frac{v+1}{2}$ is odd and $-$ when even. In the latter caſe the general term is $\pm$

$$\frac{n.\ \overline{n^2-2^2}.\ \overline{n^2-3^2}.\ \text{ to } \frac{v+1}{2} \text{ terms}}{1.\ 2.\ 3\ \text{ - - to } v \text{ terms}}\, s^v \times \sqrt{1-s^2},$$

$+$ when $\frac{v}{2}$ is odd and $-$ when even.

This Cor. may be deduced from the theorem in the ſame manner as the Cor. Art. III. was deduced from its theorem.

Theorems for the Powers of the Sines and Coſines.

VI. Theorem. If c be the coſine of the arc a and rad. unity then n being any whole poſitive number.

$c^n = \left(\frac{1}{2}\right)^{n-1} \times : cs\ na + n.\ cs\ \overline{n-2}\ a + \&c.$ cont. to $\frac{n+1}{2}$ terms when n is odd and when n is even to $\frac{1}{2}n+1$ taking only $\frac{1}{2}$ the laſt

laft term. The general term is $\frac{n . \overline{n-1} . \overline{n-2} \text{ - - to } m \text{ terms}}{1 . 2 . 3 \text{ - - } m}$
$cs\ \overline{n-2\,m\,a}$.

Dem. Let a, a', a'', &c. reprefent any arcs
c, c', c'', &c. their cofines

Then by trig. $cs, a \times 2\, cs, a' = cs, \overline{a + a'} + cs, \overline{a - a'}$
and in like manner $cs, a \times 2\, cs, a' \times 2\, cs, a'' = cs, \overline{\overline{a + a'} + cs, \overline{a - a'}} \times 2\, cs, a''$
$= cs, \overline{a + a' + a''} + cs, \overline{a + a' - a''} + cs, \overline{a - a' + a''} + cs, \overline{a - a' - a''}$, &c. &c.
and it is evident that to multiply by twice the cofine of any arc it is only neceffary to encreafe and diminifh each of the former quantities $a + a' +$ &c. $a - a'$, &c. by that arc, and take the fum of the cofines of the arcs fo encreafed and diminifhed : therefore becaufe in the product of the cofines of a, $2\, a'$, $2\, a''$, &c. all the arcs a', a'', &c. muft be involved exactly alike, it follows that $2^{n-1} \times cs, a \times cs, a' \times cs, a'' \times$ &c. $=$ fum of the cofines of all the arcs formed by adding to a each combination of the $\overline{n-1}$ arches a', a'', &c. taken pofitively or negatively. Hence by the theorems for combinations, there will be

1. term the cofine of $a + a' + a'' +$ &c.

$\overline{n-1}$

$\overline{n-1}$ terms the cosine (sum $\overline{n-1}$ arcs — 1 arc) (B)

$\dfrac{\overline{n-1}.\ \overline{n-2}}{1.\ 2.}$ terms the cosine (sum $\overline{n-2}$ arcs — sum 2 arcs) (C)

$\dfrac{\overline{n-1}.\ \overline{n-2}\ \cdot\ n-m}{1\ 2.\ -\ \cdot\ m}$ terms the cosine (sum of $\overline{n-m}$ arcs — sum m arcs) (H)

$\dfrac{\overline{n-1}\ -\ -\ n-m-1}{1.\ 2\ -\ m-1}$ terms the cosine (sum m arcs — sum $\overline{n-m}$ arcs) (H′)

$\overline{n-1}$ terms the cosine (sum 2 arcs — sum $\overline{n-2}$ arcs) (C′)

1. term the cosine (1 arc (a) — Sum $\overline{n-1}$ arcs) B′.

Now if the arcs be all taken equal, all the Bs are equal to each other, all the C^{s}, &c. &c. and also B = — B′, C = — C′ &c. &c. and consequently cs, B = cs, B′, cs, C = cs, C′, &c. &c.

$$\because \frac{\overline{n-1}.\ -\ \overline{n-m}}{1.\ 2.\ -\ m}\, cs\, H + \frac{\overline{n-1}\ \ -\ \ \overline{n-m-1}}{1.\ 2.\ 3\ -\ m-1}\, cs\, H'$$
$$= \frac{n.\ \overline{n-1}\ -\ -\ \overline{n-m-1}}{1.\ 2\ -\ -\ m}\, cs,\ \overline{n-2\, m}\, a.$$

WHENCE

WHENCE $c^{n} = \overline{\tfrac{1}{2}}\rceil^{n-1} \times cs\, na + n.\, cs\, \overline{n-2}\, a + \frac{n.\, \overline{n-1}}{1.\ 2}\, cs\, \overline{n-4}\, a + \&c.$ continued to $\frac{n+1}{2}$ terms when n is odd: but when n is even there will be a middle term $\frac{\overline{n-1}.\ \overline{n-2}\ -\ -\ \frac{n}{2}}{1.\ 2\ -\ -\ \frac{1}{2}n} \times cs,\ \overline{n-2.\ \frac{n}{2}}\, a$

$$= \frac{n.\ \overline{n-1}\ -\ -\ \text{to}\ \frac{n}{2}\ \text{terms}}{2.\ 1.\ 2.\ 3\ -\ -\ \frac{1}{2}n} \times cs\, o\, a$$ $\because$ in this case

$$c^{n} = \overline{\tfrac{1}{2}}\rceil^{n-1} \times cs\ n\ a + n.\ cs\ \overline{n-2}\ a + \&c.\ \text{to}\ \frac{n}{2}\ \text{terms} + \tfrac{1}{2} \times \frac{n.\ \overline{n-1}\ -\ -\ \frac{n}{2}\ \text{terms.}}{1.\ 2.\ -\ -\ \frac{n}{2}}$$

VII. THEOREM. 1. When n is any odd number, and s the fine of any arc. a, rad. being unity, $s^{n} = \overline{\tfrac{1}{2}}\rceil^{n-1} \times \pm s,\ n\, a \mp n.\ s,\ \overline{n-2}\ a \pm \frac{n.\ \overline{n-1}}{1.\ 2.}\ s,\ \overline{n-4}\ a \mp \&c.$ continued to $\frac{n-1}{2}$ terms &c. the upper figns taking place when n is any odd number of the form $4p+1$, and the lower when of the form $4p+3$.

THE

THE general m^{th}. term is $\pm \frac{n.\overline{n-1} \text{ - (to } m \text{ terms}}{1.\ 2. \text{ - } m} s, \overline{n-2m}\, a$

$\left.\begin{array}{l}+ \text{ when } m \text{ is even} \\ - \text{ when } m \text{ is odd}\end{array}\right\}$ and n of the form $4p+1$.

$\left.\begin{array}{l}+ \text{ when } m \text{ is odd} \\ - \text{ when } m \text{ is even}\end{array}\right\}$ and n of the form $4p+3$.

2. WHEN n is any even number.

$\overset{n}{s} = \overline{\tfrac{1}{2}|}^{\,n-1} \times \pm cs\, n\, a \mp n.\ cs\, \overline{n-2}\, a \pm$ &c. (to $\frac{n}{2}$ terms) $\pm$

$\overline{\tfrac{1}{2}|}^{\,n-1} \times \frac{n.\ \overline{n-1}.\ \overline{n-2} \text{ - } \frac{1}{2}n \text{ terms}}{2.\ 1.\ 2.\ 3 \text{ - - } \frac{1}{2}n}$. The upper ſigns take place when n is of the form $4p$, and the lower when of the form $2p$, p being any odd number. The m^{th}. term is

$\pm \frac{n.\ \overline{n-1} \text{ - } (m \text{ terms}}{1.\ 2.\ 3 \text{ - } (m \text{ terms})} cs\, \overline{n-2m}\, a$

$\left.\begin{array}{l}+ \text{ when } m \text{ is odd} \\ - \text{ when } m \text{ is even}\end{array}\right\}$ and n of the form $2p$, p being odd.

$\left.\begin{array}{l}+ \text{ when } m \text{ is even} \\ - \text{ when } m \text{ is odd}\end{array}\right\}$ and n of the form $4p$.

DEM. Let Q = a quadr. then (VI) $\overline{cs.\ Q-a}^{\,n} = \overline{\tfrac{1}{2}|}^{\,n-1} \times cs\, n.\ \overline{Q-a}$ + &c. and the general m^{th}. term is $\frac{n.\ \overline{n-1}. \text{ - to } m \text{ terms}}{1.\ 2.\ 3 \text{ - } m \text{ terms}}$ $cs\, \overline{n-2m}.\ \overline{Q-a}$.

1. Iſt

1. 1ſt. WHEN n isof the form $4p+1$, ſubſt. for n, $4p+1$
$cs\ \overline{n-2\,m}.\ \overline{Q-a} = cs,\ \overline{4p-2\,m\,Q + Q - \overline{n-2\,m}\,a} =$ (becauſe adding or ſubtracting the circumference makes no alteration in the value of the coſine and adding or ſubtracting $\frac{1}{2}$ the circumference changes the ſign of the coſine) $\pm\, cs\ \overline{Q - \overline{n-2\ m}\ a} = \pm\, s,\ \overline{n-2\,m}\,a$ + when m is even and — when odd.

1. 2. WHEN n is of the form $4p+3$, ſubſt. for n, $4p+3$, $cs,\ \overline{n-2\,m}.\ \overline{Q-a} = cs,\ \overline{4p+3-2\,m\,Q - \overline{n-2\,m}\,a} = \pm\, s,\ \overline{n-2\,m}\,a$, + when m is odd-and — when even.

2. 1. WHEN n is even of the form $2p$, p being odd, ſubſt. for n $2p$, $cs\ \overline{n-2\,m}.\ \overline{Q-a} = cs\ \overline{2p-2\,m\,Q - \overline{n-2\,m}\,a} = \pm\, cs\ \overline{n-2\,m}\,a$, + when m is odd and — when even.

2. 2. WHEN n is of the form, $4p$; ſubſtituting for n, $4p$, $cs\ \overline{n-2\,m}.\ \overline{Q-a} = cs\ \overline{4p-2\,m,\ Q - \overline{n-2\,m},\ a} = \pm\, cs\ \overline{n-2\,m},\ a$ + when m is even and — when odd.

WHENCE ſubſtituting in the general term for the $cs,\ \overline{Q-a}$, the $s,\ a$ and for $cs,\ \overline{n-2\,m}\,a$, the values above found, the truth of the theorem is evident.

 THEOREM

VIII. THEOREM. Let a, a', a'' reprefent abfciffas meafured from the centre on the axis of an equilateral hyperbola, and o, o', o'' correfponding ordinates: let alfo the hyperbolic area contained by the femi axis (= unity), diftance from the centre to the extremity of the arc, and the arc, the abfciffa of which is a'' and ordinate o'', be equal to the fum of the areas contained in the fame manner by the femi axis, dift. and arcs the abfciffas and ordinates of which are a, a' and o, o': then will $a'' = a\,a' + o\,o'$ and $o'' = a\,o' + a'\,o$.

DEM. Let the area ACV (fee fig.) = ECV + BCV, let the double ordinates FEe, bGB, aHA be produced to meet the affymptote C$w'x'y'$NYXmnWp, and let fall the perp^s. aw', bx', ey', VN, EY, BX, AW. Becaufe ACV = EVC + BCV and becaufe (by prop. hyperb.) CVN = ECY = BCX = ACW ∵ VNEY + VNBX = VNAW or VNEY = BAWX: and it has been proved by many writers on conics that when thefe areas are equal

CN : CY :: CX : CW

or VN : EY :: BX : AW

Whence it follows that

CV : Em :: Bn : Ap

or $1 : a - o :: a' - o' : a'' - o''$

in

in like manner it may be ſhewn
that C V : : em : : bn : ap
or 1 : $a+o$: : $a'+o'$: $a''+o''$
hence $a''-o''=aa'-ao'-a'o+oo'$
and $a''+o''=aa'+ao'+a'o+oo'$
and ∴ $a''=aa'+oo'$ and $o''=ao'+a'o$. Q. E. D.

FROM the ſimilitude between theſe theorems and thoſe for the ſine and coſine of the ſum of two circular arcs, it is unneceſſary to point out how every thing may be deduced for multiple hyperbolic areas in the ſame manner as was done for multiple circular arcs.

REMARKS *on the* VELOCITY *with which* FLUIDS *iſſue from* APERTURES *in the* VESSELS *which contain them.* *By the* *Rev.* MATTHEW YOUNG, *D. D. S. F. T. C. D. & M. R. I. A.*

WHEN water iſſues from a ſmall aperture in the bottom or ſide of a veſſel, which is kept conſtantly full, it has been ſuppoſed, that the force accelerating the loweſt plate of water, of indefinitely little altitude, immediately over the orifice, is the weight of the incumbent water only; and therefore, that after the motion of the plate has once commenced, the preſſure of the incumbent column will be diminiſhed, and of conſequence, the force accelerating the plate, during its deſcent through its own altitude, will not be conſtant. Read Jan. 20th, 1798.

BUT, in fact, it is not the preſſure of the incumbent water, which accelerates the loweſt plate; for every plate of water immediately incumbent over the hole, abſtracting from all lateral preſſure, begins to be accelerated equally at the ſame moment; and therefore the incumbent column, excluſive of any lateral preſſure, could produce no increaſe of velocity, in proportion to its increaſed height. The force which really

accelerates

accelerates the iſſuing plate, is the preſſure of the ambient water, which ſurrounds the cylinder immediately over the aperture; and this lateral preſſure being communicated to the upper ſurface of the plate, muſt be as much encreaſed by the velocity of the ſuperior deſcending plate, as it is diminiſhed by that of the inferior iſſuing plate, ſo as to remain conſtantly of the ſame magnitude.

On this principle it can be eaſily demonſtrated, that the velocity with which water ſpouts from an aperture in the bottom or ſide of a veſſel, is equal to that which a heavy body would acquire in falling through the height of the fluid above the orifice.

This demonſtration, however, as Mr. Atwood obſerves, is true only on hypotheſis that the water ſuffers no reſiſtance, but iſſues in a cylindrical or p iſmatic form correſponding to the hole. But, in fact, the velocity of the water according to theory will be diminiſhed by the friction of the particles againſt the edges of the orifice; from their mutual attraction, by which the iſſuing particles are retarded by thoſe which are ſtill in the veſſel, and have not acquired the velocity of thoſe which precede them; but principally from the obliquity of their motions.

For, as Chev. Du Buat obſerves, when water iſſues from an orifice, the particles will flow from all ſides, towa ds the orifice, with

with an accelerated motion, and in all directions. If the orifice be horizontal, that filament of particles, which anſwers to the centre of the hole, will deſcend in a vertical line, and will ſuffer no other reſiſtance than that of the friction cauſed by the exceſs of its velocity above that of the collateral filaments, or by the retardation which ariſes from the attraction ſubſiſting between them. The other filaments, after they have deſcended vertically for ſome time, are compelled to turn from their vertical courſe, and to approach the orifice in different curves; and when they arrive at it, their directions become more or leſs horizontal, according as they paſs nearer to or farther from the edge of the orifice. The motion therefore is decompoſed according to two directions, the one horizontal, which is deſtroyed by the equal and contrary reſiſtance of the filaments which are diametrically oppoſite; the other vertical, in proportion to which the quantity of water diſcharged is to be eſtimated. Hence we ſee, that the vertical velocity of the filaments decreaſes from the centre of the orifice to its circumference; and that the total diſcharge is leſs, than if all the filaments had iſſued vertically, in the ſame manner with that which anſwers to the centre of the aperture. It alſo follows, that the filaments which are nearer to the centre, moving faſter than thoſe which are nearer to the edges, the vein of the fluid, after it has iſſued from the orifice, will form a cone whoſe baſe is the orifice; that is to ſay, that its diameter will diminiſh, at leaſt, to a certain diſtance, becauſe the exterior filaments are gradually drawn on, in conſequence of their

mutual

mutual attraction, by the interior filaments whofe velocity is greater; whence there follows a diminution in the diameter of the vein.

THIS manner of accounting for the contraction of the vein feems more reafonable than that which is given by Newton; as there appears to be no adequate caufe for the acceleration of the water, after it has been difcharged from the orifice.

THE diminution of the mean velocity of the water, caufed folely by the obliquity of the motions of the iffuing particles, exclufive of any other impediment, may be thus determined:

LET *m n* (fig. 1.) be the diameter of the aperture in the veffel ABDC filled with water: in whatever direction the water iffues, its velocity in that direction will, in all cafes, be the fame, becaufe the preffure of fluids is the fame in all directions; thus, whether a fluid fpouts perpendicularly upwards or downwards, horizontally or obliquely, the fpace through which it is projected, in a given time, is the fame. Now to determine this direction, fince the horizontal and vertical preffures are equal, the iffuing particles will affume the intermediate direction, which will therefore form an angle of 45°. with the plane of the orifice: its vertical velocity therefore will be lefs than its direct or total velocity in the proportion of the diagonal of a fquare to its fide,

or

or as 7 to 5 nearly; but the particles of the central filament iſſue with the full velocity, due to the entire height of the water; therefore the velocity of the central particles will be to the mean velocity, as 7 to the mean between 7 and 5, or as 7 to 6. This is the diminution, as has been ſaid, which takes place in conſequence ſolely of the obliquity of the motions with which the particles iſſue from the orifice: if the other cauſes of retardation be taken into the account, we may conclude, that the velocity ſhould be diminiſhed perhaps in the ratio of 8 or even 9 to 6; which accords very well with experiments. Thus Polenus makes the ratio of the diameters of the contracted vein and aperture, which is the ſame with that of the mean and greateſt velocity, to be as $5\frac{1}{2}$ to $6\frac{1}{2}$; Bernouilli 5 to 7; Chev. Du Buat 6 to 9. When the orifice is infinitely little, the cylinder of iſſuing water becomes a ſingle filament, which is therefore diſcharged without any obliquity, and there will be no diminution of velocity, except ſuch as ariſes from friction and the tenacity of the particles. If the aperture be encreaſed ſo as to become equal to the baſe of the veſſel, the column of water will then deſcend like a falling body, and therefore the velocity will be the ſame as before; but it will not acquire this velocity until the uppermoſt plate of water has been diſcharged. At the beginning of the motion, the firſt or loweſt plate will flow out with a velocity indefinitely little; the next plate with a greater velocity; and ſo on, until the upper plate ſhall have deſcended to the orifice which will then iſſue with the greateſt velocity. But if the veſſel be ſuppoſed to be kept conſtantly full, the velocity of the effluent water will encreaſe ſo as at length to become equal to that which a heavy body would acquire in falling from an infinite height.

Since the middle filament of particles is diſcharged with the full velocity due to the entire altitude of the fluid above the orifice, experiments made on the diſtance or height to which fluids ſpout, will be found to agree very well with theory, but it by no means follows, that all the filaments ſhould be diſcharged with the ſame velocity: the quantity of the fluid therefore diſcharged in a given time, may be leſs than that which would be diſcharged, if all the filaments were diſcharged with the velocity due to the entire altitude; becauſe this quantity depends on the mean velocity of all the filaments. Hence therefore it cannot be inferred from theſe experiments, compared with thoſe which relate to the height or diſtance to which the fluid ſpouts, that the velocity of the water in the orifice is leſs than that which is due to the entire altitude; and that it is accelerated immediately after it gets out of it: becauſe the diſtance to which the fluid ſpouts, depends on the central filament only, but the quantity diſcharged on the mean velocity of the whole.

To bring this queſtion to the teſt of experiment, if all the particles were equally accelerated at their diſcharge from the orifice, and immediately after they leave it, they ought all to be projected

projected horizontally to the very ſame diſtance upon an horizontal plane; but on experiment I found, that when the fluid ſpouted through an orifice of ,08 of an inch diameter, and was kept conſtantly at the ſame height, the greateſt and leaſt diſtances at which it ſtruck the horizontal plane were nearly 15 and 12 inches; but theſe diſtances are proportional to the velocities with which they are diſcharged. It follows therefore, that all the particles are not projected with the ſame velocity. It is to be obſerved, that the particles which are diſcharged with the greateſt and leaſt velocities are few in compariſon of thoſe which are diſcharged with intermediate velocities, for while the entire ſhower extended from 15 to 12 inches on the horizontal plane, the denſer part was found to occupy only the ſpace between $14\frac{1}{4}$ and $12\frac{3}{4}$ inches; ſo that the limits of the velocities of the parts of the denſer ſhower were as 7 and 6,26; but the limits of the whole were 15 and 12, or as 7 and 5,6; and the limits by theory are as 7 and 5. But we may perceive, that when the fluid ſpouts horizontally, the particles which iſſue from the upper part of the aperture, and which therefore ought to move with the leaſt velocity, muſt encounter thoſe below them moving with a greater velocity, which will encreaſe the diſtance to which they are projected on an horizontal plane. Likewiſe, the particles which iſſue from the loweſt part of the orifice, and which ought to move with a leſs velocity, than that with which thoſe in the axis move, in the ratio of 5 to 7, will have their velocity encreaſed by their being at a greater depth. The limit therefore of the ratio of the diſtances to which the particles are projected

 on

on an horizontal plane, muſt be leſs than that which reſults from the theory of water iſſuing through an horizontal aperture. But it is obvious that the greater depth of the lower particles, when the orifice is vertical, cannot account for the entire difference of diſtance to which the particles are projected; for the depth of the orifice being 8,55 inches, and the diameter of the orifice ,08 of an inch, the velocities on account of the difference of depth would be only as $\sqrt{8,55}$ to $\sqrt{8,63}$, or as 14,6 to 15 nearly. Perhaps it might be ſaid, that this difference of diſtance was cauſed, not by the different velocities, but by the different directions in which the particles are diſcharged; ſo that thoſe which are projected in the axis of the vein, will ſtrike the horizontal plane at a greater diſtance than thoſe which are projected from the edges of the orifice with the ſame velocity, but in a different direction. But this cannot be the cauſe; for when the aperture is horizontal, the particles which iſſue from the oppoſite ſides *m*, *n* of the orifice (fig. 2.) meeting each other, deſtroy their convergence, and afterwards proceed in the direction of the axis of the vein, and therefore the vein will continue nearly of the ſame diameter: whereas, if the particles croſſed each other, with the ſame velocity, in different directions, they would deſcribe interſecting parabolas *n s*, *m t*, and the diameter of the vein would continually encreaſe. In order to determine whether this were the caſe, I cauſed the fluid to iſſue through an aperture in the bottom of the veſſel, and at the diſtance of 12 inches I found the diameter of the vein a little encreaſed, when the velocity of the efflux was conſiderable;

ſiderable; but not ſenſibly augmented, when the velocity was much diminiſhed. Since the dilatation of the vein in this caſe depends on the velocity with which the water iſſues from the aperture, it is to be inferred, that it is cauſed by the reſiſtance of the air, which producing a retardation of the preceding particles, thoſe which follow impinge againſt them, and the thickneſs of the vein is encreaſed; for the ſame reaſon as when the jette is made perpendicularly upwards, a broad head is formed in conſequence of the retardation of the uppermoſt particles. Now ſince it appears, that the dilatation of the vein which ariſes either from the different directions of the particles, or the reſiſtance which they undergo from the air, or both together, cannot account for the difference of diſtance to which the particles are projected on an horizontal plane, we muſt conclude that this difference is cauſed by the different velocities with which they eſcape from the orifice.

When a tube *m n r s* (fig. 3) is inſerted into the veſſel A B C D, it is found, that the velocity is increaſed nearly in the ſub-duplicate ratio of the length of the pipe, when the tubes are ſhort; and that it approaches nearer to that ſub-duplicate ratio, according as the length of the pipe is increaſed. To account for this increaſe of velocity has appeared a matter of ſome difficulty, ſince the water cannot iſſue at *r s* with a greater velocity than it enters at *m n*; and it does not appear how the velocity at *m n* can be encreaſed by inſerting a tube beneath it. In order to explain the

cauſe

cause of this effect, we are to consider, that the whole force with which the plate *m n* is pressed down, is the weight of a column of water equal to *e m n f*, together with the weight of a column of air of the same base, reaching to the top of the atmosphere; and the whole force with which it is pressed up, is the weight of an equal column of air, diminished by the weight of a column of water equal to *m n r s*; therefore the actual force with which the plate *m n* is pressed down, is the weight of a column of water equal to *e f r s*; the velocity therefore with which the plate *m n* will issue through the orifice *m n*, will be the same as through the orifice *r s* in the vessel A*bc*D; that is, equal to the velocity which a heavy body would acquire in falling through the altitude *e r*; and all the plates of water in the tube *m n r s* will descend with the same velocity; for they cannot descend faster, because otherwise there would be a vacuum left in the tube, which is prevented by the upward pressure of the atmosphere. And the velocity of the effluent water will be the same, whatever be the pressure of the atmosphere, provided the weight of a column of air of the same base with *r s*, and whose height is equal to that of the atmosphere, be either greater than or equal to the weight of the pillar of water *m n r s*. This might be proved experimentally by a vessel of water with a pipe inserted in the bottom, placed under an exhausted receiver. But as the operation of exhaustion is obstructed more by the evaporation of water than of mercury, it will be better to use mercury in these experiments. Now if D be the defect of the gage from the standard altitude, it will

will meaſure the preſſure of the air on the ſurface of the mercury in the veſſel; let A be the altitude of the mercury in the veſſel above the upper orifice of the pipe, and P the length of the pipe; then the whole force preſſing downwards the plate of mercury which is immediately in the upper orifice of the pipe, will be $= D + A$; and the whole force preſſing the ſame plate upwards will be $D - P$; and the difference between theſe forces will be the abſolute force preſſing the ſame plate of mercury downwards; while D is greater than P, this abſolute force will conſequently be equal to $A + P$; when $D = P$, $D - P$ vaniſhes, and the force preſſing the plate downwards is $= D + A = P + A$; hence therefore no variation in the time of the efflux will be perceived, while the altitude of the mercury in the gage is equal to or leſs than the difference between the length of the pipe and the ſtandard altitude. When D is leſs than P, the force upwards is alſo nothing; and therefore, as before, the whole force preſſing the plate downwards is $= D + A$; and A being given, it decreaſes according as D decreaſes; and when D vaniſhes, that is, when the receiver is abſolutely exhauſted, the force becomes equal to A, and the time of the efflux will be the ſame, as if the pipe had not been inſerted in the bottom of the veſſel. To try the truth of theſe things by experiment, I inſerted a tube 7,8 inches long in a cylindrical veſſel, and cloſing the orifice of the pipe, I filled the veſſel with mercury to the height of 6 inches; then placing the apparatus under the receiver of an air-pump, when the barometer was at 30 inches, and the gage at 28,5, the time of the efflux

was

was 26 feconds; when the experiment was repeated precifely in the fame manner, but in the open air, the time of the efflux was only 19 feconds. Now as the gage ftood at 28,5, the defect D was $30—28,5 = 1,5$, and the preffure on the plate of mercury was $= 6 + 1\frac{1}{2} = 7\frac{1}{2}$; in the open air the preffure was $= 6 + 7,8 = 13,8$; therefore the ratio of the velocity of the efflux in both cafes, which is the fame with the reciprocal ratio of the times, was $\sqrt{7\frac{1}{2}}$ to $\sqrt{13,8}$, or as 2,73 to 3,7; but 2,73 is to 3,7 as 19 to 26 very nearly. No difference was obferved in the times of the efflux, when in the open air and exhaufted receiver, unlefs the gage ftood higher than $22\frac{1}{2}$ inches; that is, unlefs the height of the mercury in the gage was greater than the difference between the length of the pipe and the ftandard altitude. In another experiment, when the gage ftood at 27,9, the height of the barometer was 29,9; the defect therefore was $= 2$, and the preffure $= 8$. But $\sqrt{8} = 2,828$, and $\sqrt{13,8} = 3,7$ but $2,828 : 3,7 :: 19 : 24$, and by experiment the time of the efflux appeared to be 23 feconds. When the efflux is made in vacuo, it is obvious to obferve, that the pipe is not filled during the efflux, as it is while the difcharge is made in the open air.

Since the column of water in the pipe *mnrs* adds to the preffure which the plate *mn* fuftains, by diminifhing the upward preffure of the air through the pipe, it appears that it produces this increafe of preffure in the plate *mn* alone, without producing any

any lateral preſſure in the water which is on a level with mn; for it is manifeſt, that if an aperture were made in mB or nC, the velocity of the water iſſuing through it would not be affected by the inſertion of the pipe; and conſequently that the plate mn, which is immediately in the orifice of the pipe, is the only one, on the ſame level, whoſe tendency downwards is increaſed by the inſertion of the pipe. Hence, the particles of water at the edge of the aperture, having their perpendicular preſſure encreaſed by the weight of the column $mnrs$, without any increaſe of their lateral preſſure, they will iſſue through the orifice mn more perpendicularly; the ſides alſo of the tube will obſtruct the converging motion of the particles, and conſequently, on both theſe accounts, the quantity of water diſcharged through a pipe thus inſerted, will exceed that diſcharged through a ſimple orifice, in a greater ratio than the ſub-duplicate of the height of the water. And according as the length of the pipe encreaſes, the ratio of the quantity of water actually diſcharged by experiment, to that which ſhould be diſcharged according to theory, will increaſe; becauſe the ratio of the perpendicular to the horizontal preſſure increaſes, in the ratio of the ſum of the depth of the veſſel and length of the pipe, to the depth of the veſſel. It follows therefore, that experiments made in this manner, will approach nearer to coincidence with theory, than when made with a ſimple orifice; except either when the tube is ſo long as that the friction of the fluid againſt the ſides of the tube ſhall produce a ſenſible effect, or

 when

it is so short, as not to be sufficient to give the particles a vertical direction. All which agrees very well with the experiments made by the ingenious Mr. Vince, of which he has given us an account in the Phil. Transf. for the year 1795. Thus he tells us, that having inserted a tube, a quarter of an inch in length, into a cylindrical vessel 12 inches deep, he found that the velocity did not sensibly differ from that through the orifice; the cause of which he discovered to be this, that the stream did not fill the pipe, but that the fluid was contracted, as when it flowed through the simple orifice. When the pipe was half an inch long, inserted into a vessel of the same depth as before, the velocity of the water from the pipe and from the orifice, which ought by theory to have been as $\sqrt{12,5}$ to $\sqrt{12}$, or as 49 to 48, was by experiment found to be nearly in the proportion of 4 to 3. Now if the ratio of 49 to 48 be increased in the ratio of 7 to 6, (because this is the ratio of the diminution of the velocity on account of the contraction of the vein, and this contraction either nearly or entirely vanishes in a pipe,) we shall have the ratio of 3,57 to 3. When the pipe was an inch long, the velocity from the pipe and from the orifice, which, according to theory, ought to have been as $\sqrt{13}$ to $\sqrt{12}$, or as 26 to 25, appeared by experiment, very nearly in the ratio of 4 to 3; now if the ratio of 26 to 25 be encreased in the ratio of 7 to 6, we shall have the ratio of 3,64 to 3. When he made use of longer pipes, the velocity of the effluent water by experiment approached nearer to that which ought to have been

been difcharged according to theory; fo that in long pipes, the difference between theory and experiment, he fays, was not greater than what might be expected from the friction of the pipes, and other caufes which may be fuppofed to retard the velocity. When he inferted a pipe of the fame diameter with the aperture, which terminated in a truncated cone fixed in the orifice, (fig. 4.) he expected, that the quantity of water difcharged in a given time would have been diminifhed, becaufe the water, iffuing through the orifice *m n*, would have room to form the *vena contracta* in the enlarging cone; but he found, that the fame quantity of water was difcharged, as if the pipe had continued throughout of the fame diameter with the orifice. The reafon of this is manifeft from what has been faid, for the preffure of the air will not fuffer the truncated cone to remain partly empty, as it would be if the *vena contracta* were formed; it will therefore continue full, and confequently the water will pafs through it in the fame manner as if the water in the cone, furrounding the pipe *m a b n*, were congealed.

Mr. Vince likewife inferted into the bottom of the veffel a perpendicular pipe, in form of a truncated cone, the narrower part being fixed in the orifice; by which he found the efflux to be encreafed more than if he had inferted a cylindrical pipe of the fame length, whofe diameter was equal to that of the narroweft part of the conical pipe. This effect may be explained on the fame principle by which we accounted for the augmentation of the diameter of a vertical vein of water, through a ſimple orifice, when the velocity of the efflux is conſiderable. For when a perpendicular pipe is inſerted, the velocity of the diſcharge being conſiderably encreaſed, the reſiſtance from the air will be ſo likewiſe; and thus the diameter of the vein has a tendency to enlarge itſelf; now in the widening cone, the pipe admits of this augmentation, at the ſame time that it encreaſes the velocity; but the cylindrical pipe, though it equally encreaſes the velocity, yet it does not permit the vein to enlarge itſelf, and by thus confining it, the efflux is obſtructed, and the quantity diſcharged in a given time is diminiſhed. Accordingly, under the receiver of an air-pump, even in a moderate degree of exhauſtion, there is no difference perceived between the velocities with which a fluid is diſcharged through a conical or cylindrical pipe.

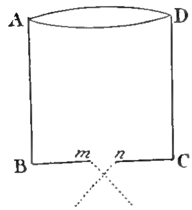


Fig. 1.

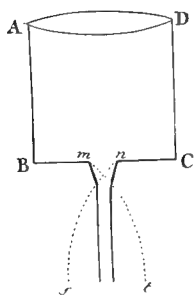


Fig. 2.

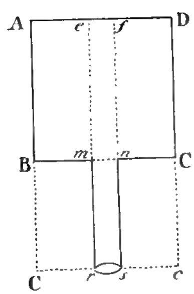
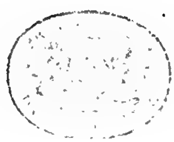


Fig. 3.



A new METHOD *of resolving* CUBIC EQUATIONS.

By THOMAS MEREDITH, *A. B. Trinity College, Dublin.*

THE roots of a cubic equation of this form, $x^3 + 3c.x^2 + 3c^2.x + c^3 - a = o$ which differs from a power only in its laſt term, can be found, by tranſpoſing, a, and extracting the cubic root on each ſide, provided, a, is not an impoſſible binomial. Read June 10th, 1797.

PROBLEM. To reduce any cubic equation to this form, $x^3 + 3c.x^2 + 3c^2.x + c^3 - a = o$, that is, to reduce it to an equation, in which, the ſquare of the co-efficient of the ſecond term is triple the co-efficient of the third.

IF the roots of a cubic equation, $x^3 + px^2 + qx + r = o$, are encreaſed or diminiſhed by any quantity, p^2 and $3q$, will be encreaſed or diminiſhed by an equal quantity, if multiplied, will be multiplied by an equal quantity, therefore their equality or inequality, not affected by thoſe transformations.

$x =$

$$x^3 + px^2 + qx + r = o$$

$$x = \overline{y + a} \qquad \begin{aligned} x^3 &= y^3 + 3ay^2 + 3a^2y + a^3 \\ px^2 &= py^2 + 2pay + pa^2 \\ qx &= qy + qa \\ r &= r \end{aligned}$$

$p^2 = 9a^2 + 6ap + p^2$ and $3q. = 9a^2 + 6ap + 3q$ therefore p^2 and $3q$. encreafed by the fame quantity, viz. $9a^2 + 6ap$

$$x^3 + px^2 + qx + r = o$$

$$x = \frac{y}{a} \qquad y^3 + pay^2 + qa^2y + a^3r = o$$

$p^2 = p^2a^2$ and $3q = 3qa^2$ therefore both multiplied by the fame quantity, viz. a^2.

Hence it appears that the problem cannot be effected by thofe transformations.

But the equation, $x^3 + px^2 + qx + r = o$ by transforming the roots into their reciprocals, and freeing the firft term from a coefficient becomes, $x^3 + qx^2 + prx + r^2 = o$ therefore if in the propofed equation $q^2 = 3pr$, then by transforming the roots into their reciprocals, and freeing the firft term from a co-efficient the equation will be reduced to the required form.

Any cubic equation being propofed, there is a quantity, by which if the roots are encreafed (or diminifhed) q^2 will become equal

equal $3pr$, the value of this quantity may be inveſtigated by ſolving a quadratic equation.

THUS let the equation be,

$$x^3+px^2+qx+r=0$$

$$x=\overline{y+e} \qquad \begin{array}{r} y^3+3ey^2+3e^2y+e^3 \\ py^2+2pey+pe^2 \\ qy+qe \\ +r \\ \hline \end{array}$$

$$\overline{3e^2+2pe+q}^{\,2}= \quad 9e^4+12pe^3 \begin{array}{c}+6q\\+4p^2\end{array}\cdot e^2+4pqe+q^2$$

$$3\times\overline{e^3+pe^2+qe+r}\times\overline{3e+p}=9e^4+12pe^3\begin{array}{c}+9q\\+3p^2\end{array}\cdot e^2\begin{array}{c}+9r\\+3pq\end{array}\cdot e+3pr$$

$$9e^4+12pe^3\begin{array}{c}+6q\\+4p^2\end{array}\cdot e^2+4pqe+q^2=9e^4+12pe^3\begin{array}{c}+9q\\+3p^2\end{array}\cdot e^2\begin{array}{c}+9r\\+3pq\end{array}\cdot e+3pr$$

$$\begin{array}{c}p^2\\-3q\end{array}\cdot e^2\begin{array}{c}+pq\\-9r\end{array}\cdot e\begin{array}{c}+q^2\\-3pr\end{array}=0$$

LET it be required to find the roots of this equation, $x^3+6x^2+3x+2=0$. Subſtituting in the formula

$$\begin{array}{c}36\\-9\end{array}\cdot e^2\begin{array}{c}+18\\-18\end{array}\cdot e\begin{array}{c}+9\\-36\end{array}=0 \because 27e^2=27 \because e^2=1 \because e=1.$$

x^3

$$x^3 + 6x^2 + 3x + 2 = 0$$

$$x = \overline{y + 1} \qquad \begin{aligned} x^3 &= y^3 + 3y^2 + 3y + 1 \\ 6x^2 &= 6y^2 + 12y + 6 \\ 3x &= 3y + 3 \\ 2 &= 2 \end{aligned}$$

$$y = \frac{1}{v} \qquad y^3 + 9y^2 + 18y + 12 = 0$$

$$v = \frac{z}{12} \qquad 12v^3 + 18v^2 + 9v + 1 = 0$$

$$z^3 + 18z^2 + 108z + 144 = 0$$

Extract the cubic root on each side $\}$ $z^3 + 18z^2 + 108z + 216 = 72$

$$z + 6 = \sqrt[3]{72} \therefore z = -6 + 2\sqrt[3]{9}$$

$$v = \frac{-6 + 2\sqrt[3]{9}}{12} \therefore y = \frac{12}{-6 + 2\sqrt[3]{9}} \therefore x = \frac{12}{-6 + 2\sqrt[3]{9}} + 1$$

substituting then for $2\sqrt[3]{9}$ its 3 values, $2\sqrt[3]{9}$, $\overline{-1 + \sqrt{-3}} \times \sqrt[3]{9}$, $\overline{-1 - \sqrt{-3}} \times \sqrt[3]{9}$ the roots of the proposed equation will be

$$\frac{12}{-6 + 2\sqrt[3]{9}} + 1, \quad \frac{12}{-6\, \overline{-1 + \sqrt{-3}} \cdot \sqrt[3]{9}} + 1, \quad \frac{12}{-6\, \overline{-1 - \sqrt{-3}} \cdot \sqrt[3]{9}} + 1$$

But the roots of the given equation may be found after one transformation, for the roots of the final equation are expressed in the co-efficients of the first transformed equation, and the root

root of the firſt transformed is its abſolute quantity divided by the root of the final equation.

Let the equation when its roots are encreaſed by e, be

$$y=\frac{1}{v} \qquad y^3+py^2+qy+r=0$$

$$v=\frac{z}{r} \qquad rv^3+qv^2+pv+1=0$$

$$z^3+qz^2+rpz+r^2=0$$

$$^*z=-\frac{q}{3}+\sqrt[3]{\frac{q^3}{27}-r^2} \qquad v=\frac{-\frac{q}{3}+\sqrt[3]{\frac{q^3}{27}-r^2}}{r} \qquad \therefore y=\frac{r}{-\frac{q}{3}+\sqrt[3]{\frac{q^3}{27}-r^2}}$$

When the value of e, by which the roots are to be encreaſed (or diminiſhed) is impoſſible the coefficients of the transformed equation will be impoſſible binomials $\because a=\overline{\frac{q^3}{27}-r^2}$ an impoſſible binome (unleſs in particular caſes the coeff. of the impoſſible part vaniſhes) hence it appears that a, and e, will be poſſible or impoſſible in the ſame caſes.

 If

* Since the equation $z^3+qz^2+rpz+r^2$ may be thus expreſſed $z^3+3\cdot\frac{q}{3}z^2+3\cdot\frac{q^2}{9}z+\frac{q^3}{27}=\frac{q^3}{27}-r^2$.

IF the roots of a cubic equation are encreafed by $-\frac{p}{3}$ the fecond term will vanifh, if by $-\frac{p}{3} \mp \sqrt{\frac{p^2}{9} - \frac{q}{3}}$ the third term will vanifh, therefore if $p^2 = 3q$ the fecond and third terms may be exterminated together, therefore the equation will have two impoffible roots; hence it appears that the equation of the required form has two impoffible roots,* confequently the value of e, by which the roots are to be encreafed will be impoffible when all the roots of the propofed equation are real ∵ a, whofe cubic root muft be computed, will be an impoffible binomial, unlefs in the particular cafes where the coefficient of the impoffible part vanifhes.

IT remains to be proved that when the propofed equation has but one poffible root, the value of e, by which the roots are to be encreafed (or diminifhed) will be poffible and confequently a,

LET the roots be $\overline{-m+\surd-n}$, $\overline{-m-\surd-n}$, $-b$

$p = \overline{2m+b}$, $\quad q = \overline{m^2+n+2bm}$, $\quad r = \overline{bm^2+bn}$

$$\begin{array}{ccccc} \overline{p^2} & & \overline{+pq} & & \overline{+q^2} \\ & \cdot e^2 & & \cdot e & & = o \\ -3q & & -9r & & -3pr \end{array}$$

m^2.

* That the equation of the required form has two impoffible roots, appears alfo from this, that two of the cubic roots of a, are impoffible.

$$\begin{array}{lll}
m^2.\ e^2 & +\ 2m^3.\ e & +\ m^4 \\
-2bm & -4bm^2 & -2bm^3 \\
-3n & +2mn & +b^2m^2 \\
b^2 & +2b^2m & -2bmn \\
 & -8bn & +2nm^2 \\
 & & -3b^2n \\
 & & +n^2
\end{array}$$

Call the coefficients β, γ, δ, $e = -\frac{\gamma}{2\beta} \mp \sqrt{\frac{\gamma^2}{4\beta^2} - \frac{\beta\delta}{\beta^2}}$

therefore it is to be proved that $\overline{\frac{\gamma^2}{4} - \beta\delta}$ is affirmative, and confequently the fquare root poffible.

$$\frac{\gamma^2}{4} = \overline{m^3 - 2bm^2 + mn + b^2m - 4bn}^{\,2} =$$

$$\begin{array}{lllllll}
m^6 - 4bm^5 & +2n.\ m^4 & -12bn.\ m^3 & +18b^2n.\ m^2 & -8bn^2.\ m & +16b^2n^2 \\
 & +6b^2 & -4b^3 & +\ n^2 & -8b^3n & \\
 & & & +\ b^4 & &
\end{array}$$

$$\beta\delta = \overline{m^4 - 2bm^3 + b^2m^2 - 2bmn + 2nm^2 - 3b^2n + n^2} \times \overline{m^2 - 2bm - 3n + b^2} =$$

$$\begin{array}{lllllll}
m^6 - 4bm^5 & +6b^2.m^4 & -4b^3.m^3 & -5n^2.\ m^2 & +4bn^2.\ m & +10b^2n^2 \\
 & -n & & +\ b^4. & +4b^3n & -3n^3 \\
 & & & & & -3b^4n
\end{array}$$

$$\frac{\gamma^2}{4} - \beta\delta = \begin{array}{lllll}
3n.\ m^4 & -12bn.\ m^3 & +18b^2n.\ m^2 & -12bn^2.\ m & +6b^2n^2 \\
 & & +\ 6n^2 & -12b^3n & +3n^3 \\
 & & & & +3b^4n
\end{array}$$

but this remainder may be refolved into thofe parts.

$$\overline{m^4 - 4bm^3 + 6b^2m^2 - 4b^3m + b^4} \times 3n = \overline{m-b}.^4\ 3n$$
$$\overline{m^2 - 2bm + b^2} \times 6n^2 = \overline{m-b}^2.\ 6n^2$$
$$+ 3n^3 = 3n^3$$

n, is affirmative, and $\overline{m-b}^4$ and $\overline{m-b}^2$ alfo affirmative therefore the remainder affirmative.

Let the roots be $-m\ -m-b$

$$\begin{array}{llll} m^2. & e^2 + m^2.\ 2m.\ e + & m^2\ .\ m^2 & \\ -2bm & -2bm & -2bm & =o \\ +b^2 & +\ b^2 & +\ b^2 & \end{array}$$
$$e^2 + 2m.\ e + m^2 = o$$
$$e = -m \mp \sqrt{m^2 - m^2}$$

therefore when two roots of the propofed equation are equal, the value e, by which the roots are to be encreafed will be one of the equal roots, therefore the two laft terms of the transformed equation will vanifh, therefore reducible to a fimple equation, which will give the remaining root *e.g.* $x^3 + 7x^2 + 16x + 12 = o$

$$\begin{array}{l} 49 \\ -48 \end{array} . e^2 \begin{array}{l} +112 \\ -108 \end{array} . e \begin{array}{l} +256 \\ -252 \end{array} = o \because e^2 + 4e + 4 = o \because e = -2 \mp \sqrt{4-4}$$

$x^3 =$

$$
\begin{array}{lll}
 & x^3 = & y^3 - 6y^2 + 12y - 8 \\
x = y + 2 & 7x^2 & \phantom{y^3 - {}} 7y^2 - 28y + 28 \\
 & 16x & \phantom{y^3 - 6y^2 + {}} 16y - 32 \\
 & 12 & \phantom{y^3 - 6y^2 + 12y - {}} + 12 \\
\hline
 & & y^3 + y^2 \quad . \quad . = 0
\end{array}
$$

$$\because y = -1 \because x = -3, -2, -2.$$

On the FORCE of TESTIMONY in establishing FACTS contrary to ANALOGY. By the Rev. MATTHEW YOUNG, D. D. S. F. T. C. D. & M. R. I. A.

Il n'est pas si glorieux a l'esprit de Geometrie de regner dans la Physique que dans les choses de Morale, si casuelles, si compliquées, si changeantes. Plus une matiere lui est opposé et rebelle, plus il a d'honneur a la dompter.

FONTENELLE.

ARISTOTLE observes, that the chief characteristic of quantity is, that it is that by which any thing may be denominated *equal and unequal.* Præd. p. 34. Ed. Sylb. Every thing therefore is said to admit of quantity, which is capable of more and less. Hence quantities are reduced to two classes, those which consist of parts, and those which are estimated by degrees: accurately speaking, the former alone are quantities, the latter are so only metaphorically. " Propter similitudinem dicuntur quantitates, quantitas perfec- " tionis, quantitas virtutis, intensionis, valoris, & similium. In " his enim est similitudo quædam quantitatis, quæ in eo posita " est, quod sicut quantitas molis dicit extensionem quandam par- " tium

Read, Feb. 3d 1798.

"tium extra partes, ita & dictæ quantitates suo modo quandam "extensionem partium habent. Smigl. p. 294." This latter species of quantity is therefore called "*Quantitas Intensa & Virtutis*," Aldrich p. 43; for the essential perfections and virtues of things are composed of different degrees, in the same manner as quantity, properly so called, is composed of parts. Burgesd. p. 21. Quantities which consists of parts are alone capable of measure, and therefore of mathematical comparison; while the others, though they admit of more and less, yet not being measurable, cannot be mathematically compared. Thus different areas, which consist of parts, are measurable; but pleasure and pain, heat and cold, probability and improbability, virtue and vice, which are estimated by degrees, are not measurable. Crakanthorp therefore describes quantity, by saying, that "it is an absolute accident, by which things are "measured primarily and *per se*," p. 81. Now to make quantities which consist of degrees, and therefore are not measurable, the subject of mathematical comparison, an arbitrary measure is assigned, by referring them to some measurable quantity to which they are related. Thus, in the graduation of the thermometer, an arbitrary measure is established for heat and cold, for the degrees of heat are referred to the expansion of the fluid contained in the thermometer, which is measurable, and to which heat is related. In the same manner, probability has no measure in itself; but an arbitrary measure is assigned to it, by referring it to the ratio of the number of chances by which the event may happen or fail; and

and thus it becomes the ſubject of mathematical calculation, in the ſame manner as the degrees of heat.

The ratio of thoſe quantities which conſiſt of parts, cannot always be accurately aſſigned; nevertheleſs, ſince the quantities are finite, they muſt have ſome finite, determinate ratio to each other. Thus the area of a circle to the circumſcribed ſquare cannot be accurately exhibited: in theſe caſes we can, in general, proceed by continual approximations, and aſſign limits within which the true ratio muſt ſubſiſt.

If there be two things, one of which is greater or leſs than the other, they are quantities of the ſame ſpecies: thus when a cannon ball is ſaid to be greater than an orange, the abſtract magnitudes of both are quantities of the ſame ſpecies.

If two things be of the ſame ſpecies, and one of them can be repreſented by an exponent of a given kind, the other is in its nature capable of being expreſſed by an exponent of the ſame kind. Thus if the area of a ſquare be repreſented by a given right line, the area of the inſcribed circle is capable of being repreſented by another right line, though no mathematician has yet been able to ſhew what that line is, by any geometrical conſtruction.

If the velocity of a ray of light incident on a piece of cryſtal be expreſſed by a given number, there is a number which will alſo expreſs its velocity within the cryſtal.

 The

The *active*, *efficient causes* of events are thus enumerated in the Ethics of Aristotle, " the several causes appear to be nature, ne-" cessity, and chance, and besides these, mind or intellect, and " whatever operates by or through man." L. 3. c. 3. Chance therefore is an active, efficient cause; but it is also *an accidental cause*, " ad causam per accidens revocatur fortuna et casus," Burgesd. Chance therefore is an efficient, accidental cause of an event.

The probability of an event, according to De Moivre and Simpson, is greater or less according to the number of chances by which it may happen, compared with the whole number of chances by which it may either happen or fail.

As, supposing it were required to express the probability of throwing either an ace or duce at the first throw with a single die; then there being in all 6 different chances or ways that the die may fall, and only 2 of them for the ace or duce to come upward, the probability of the happening of one of these will be $\frac{2}{6}$ or $\frac{1}{3}$.

Wherefore if we constitute a fraction, whereof the numerator shall be the number of chances whereby an event may happen, and the denominator the number of chances whereby it may either happen or fail, that fraction will be a proper exponent of the probability of happening.

For

For the fame reafon, the probability of its failing will be equal to the number of chances for its failing, divided by the fum of the number of chances of happening and failing together.

The probability therefore either of the happening or failing of an event is always expreffed by a proper fraction.

If the number of chances of happening be = o, that is, if the event be impoffible, the numerator, and therefore the fraction will be = o; o therefore denotes impoffibility.

If the number of chances of failing be = o, that is, if the event be certain, the numerator will be equal to the denominator, and the fraction = 1; unity therefore expreffes certainty.

Probability therefore extends, as Mr. Locke obferves, from certainty to impoffibility.

When the chances for the happening of an event are equal to the chances of its failing, the fraction, expreffing the probability, is $= \frac{1}{2}$, which is the mean between impoffibility and certainty.

One event therefore is faid to be more probable than another when its probability is expreffed by a greater fraction; though, in the common acceptation of the word, that only is faid to be probable, whofe probability exceeds half certainty; for if the probability be equal to half certainty, it is called *doubtful*; and if the probability be lefs than half certainty it is faid to be *improbable.*

SINCE the chances for happening or failing are equal to the whole number of chances, the probabilities of the happening and failing of the event are together = 1, that is, equal to certainty.

THEREFORE the probability of happening is equal to the difference between certainty and the probability of failing; and the probabity of failing equal to the difference between certainty and the probability of happening.

FROM what has been faid it follows, that the probability that a witnefs tells truth, in a given inftance, will be expreffed by a fraction whofe numerator is the number of chances for his telling truth, and the denominator the fum of the number of chances for his telling truth, and for his telling falfhood together.

IN like manner, the probability that an argument is true, is to be eftimated by the ratio of the number of chances for its truth to the number of chances for its truth and falfhood together.

IT is true, that in neither of thefe latter cafes can we, in general, determine the actual number of chances; neverthelefs in all cafes where a perfon perceives the probability of an event, he muft at the fame time perceive, that there muft be fome finite, determinate

determinate ratio between the chances for its happening and failing, though he cannot assign that ratio; for if there were no finite ratio, either the number of chances for its happening must be infinitely greater, or infinitely less than the chances for its failing; in the former case, the event would appear certain, in the latter impossible, therefore probable in neither.

It may perhaps be objected, that if we cannot determine the actual number of chances, all consideration of the manner of expressing mathematically the probability of events is nugatory. But it is by no means so; because though we cannot determine the exact degree of credit, which we ought to give to each witness, yet we can determine according to what law our belief ought to vary in the case of concurring witnesses, each of equal credibility. Things that are quite unknown, says Hartley, have often fixed relations to one another, and sometimes relations to things known; and as, in Algebra, it is impossible to express the relation of the unknown quantity to other quantities known or unknown, 'till it has a symbol assigned to it, of the same kind with those that denote the others; so in philosophy, we must give names to unknown quantities, qualities, causes, &c not in order to rest in them, as the Aristotelians did, but to have a fixed expression, under which to treasure up all that can be known of the unknown cause, &c. in the imagination, and memory; or in writing, for future enquirers. Vol: I. p. 348.

We

We can alſo from theſe principles ſhew why after a certain number of witneſſes have atteſted a fact, any farther evidence is ſuperfluous.

These principles likewiſe, as Dr. Waring obſerves, may be applied to the inveſtigation of the probability of the truth of the deciſion by any number of voters, and many other caſes; the probability of each voter voting truly being ſuppoſed given. But, as he alſo obſerves, it is impoſſible to determine the knowledge, integrity, and various influences which actuate each perſon, and conſequently to determine the probability of their voting truly.

But though we cannot determine the actual probability, yet ſince the voters are to be ſuppoſed of equal integrity, knowledge, &c. we can determine the relative probabilities of the truth of the deciſions by different majorities; and on theſe principles Mons. Condorcet has enquired into the laws according to which the majorities, which decide queſtions in deliberative aſſemblies, ought to be regulated.

Thus ſuppoſe the enacting of a new law were propoſed to a deliberative aſſembly, ſuch a majority ſhould be required as would give a very great probability of the juſtice of their deciſion; for it is much better that no law ſhould be enacted than a bad one. A majority of more than one ſingle voice ſeems alſo requiſite in ſome queſtions of a civil nature, as for inſtance in long continued poſſeſſion

poſſeſſion; for though length of poſſeſſion ſhould not ſuperſede right, yet conſiderable regard ſhould be paid to it, not only for the ſake of the public tranquillity, but likewiſe becauſe in the progreſs of time, there, in many caſes, ariſes a greater difficulty of producing the original titles of property. So that perhaps it would be wiſe to increaſe the majority, requiſite to decide the queſtion, according to the duration of the poſſeſſion. On the other hand, all queſtions which require immediate determination, ſhould be decided even by the leaſt poſſible majority.

It follows therefore, that although we cannot actually aſſign the fraction which expreſſes the credibility of a given witneſs, yet our reaſonings on teſtimony will be rendered more clear, determinate, and extenſive by this notation. And accordingly Dr. Waring, after he lays down the principles for determining the probabilities of events obſerves that they may be applied to human teſtimony. See his Eſſay on the Principles of Human Knowledge, § 17.

That probability may juſtly be expreſſed by a fraction, certainty being denoted by unity, and impoſſibility by a cypher, will likewiſe appear from the following conſiderations:

If upon the happening of an event, ſays De Moivre, I be entitled to a ſum of money, my expectation of obtaining that ſum has a determinate value before the happening of the event.

If

If a perſon therefore tells me, that an event has happened, by which I am to receive a ſum of money, my expectation of receiving that ſum has a determinate value, before I certainly know whether that event has actually happened or not.

In all caſes, the expectation of obtaining any ſum is eſtimated by multiplying the value of the ſum expected by the fraction which repreſents the probability of obtaining it. Thus if my probability of obtaining £100 be $\frac{3}{5}$, my expectation will be $= \frac{3}{5} \times$ £100 = £60.

Therefore it neceſſarily follows, that the probability of obtaining the ſum is equal to the value of the expectation, divided by the value of the thing expected. And ſince the expectation is neceſſarily determinate, ſo likewiſe is the probability. Now my expectation, derived from the report of the witneſs, muſt be either equal to, greater, or leſs than the expectation derived from an equal chance; the probability will therefore be either equal to, greater, or leſs than an equal chance; therefore the probability in the former caſe is homogeneous with the probability in the latter; but the latter is capable of being expreſſed fractionally, therefore ſo alſo is the former.

Suppose a perſon of good character tells me, that an event has happened by which I am to receive £100; there will hence ariſe an expectation in my mind, which muſt be of ſome determinate

terminate value: for there is a ſum leſs than £100, for which I would ſell my chance, otherwiſe I muſt conſider the report of the witneſs as abſolutely certain; alſo, that there is a ſum for which I would not ſell my chance, is likewiſe evident, for if not, I muſt have no reliance whatſoever on the witneſs. We can therefore aſſign limits, within which the meaſure of my expectation ſubſiſts; and therefore there muſt be ſome intermediate, determinate ſum, which is the meaſure of my expectation. Let this expectation be $= \frac{1}{n} \times £100$; then $\frac{1}{n} \times \frac{£100}{100} = \frac{1}{n}$ expreſſes the probability that the witneſs tells truth; or rather is the meaſure of my belief in his veracity.

This expectation is to be reſolved, as Hume and Waring obſerve, into the conſtitution of our nature; the Supreme Being having impreſſed on our minds a faculty for the ſource of all our knowledge reſpecting exiſtence, namely, a neceſſary or impulſive belief of the future from the paſt, viz. that what has, for the time paſt of our lives, been joined together or conſtantly ſucceeded each other, will for the future be joined together, or be found in the ſame order to ſucceed each other. So that having obſerved, that in certain circumſtances men tell truth, there ariſes, by the conſtitution of our nature, or as ſome hold, by aſſociation, an expectation, that, in like circumſtances, other men will likewiſe tell truth.

But the expectation, in the fame circumftances of an event, will be different according to the conftitution of the expectant; for, according to his antecedent experience, knowledge, prejudices, and paffions, the arguments for or againft the probability of the event will appear more or lefs numerous, more or lefs cogent; fo that in given circumftances of an expected event, or of a propofed argument, the apparent probability will very much depend on the conftitution of the individual, which therefore muft be confidered as a principal element in the computation.

In like manner, in the courfe of nature, we conclude, by experience, from things paft to the future; and when the analogy is properly inftituted, the events feldom or never differ; the more the preceding qualities are which agree, the greater on that account is the probability that the events will be the fame: and from greater experience we gradually conclude a greater degree of probability, though, in general, we cannot affign a reafon for it. Deinde nec illud quenquam latere poteft, fays Bernouilli, quod ad judicandum hoc modo (nempe empirico) de quopiam eventu, non fufficiat fumpfiffe unum alterumque experimentum, fed quod magna experimentorum requiratur copia; quando & ftupidiffimus quifque, *nefcio quo naturæ inftinctu, per fe & nullâ prævià inftitutione (quod fanè mirabile eft)* compertum habet, quo plures ejufmodi captæ fuerint obfervationes, eò minus a fcopo aberrandi periculum fore. Ars Conjectandi, pag. 225.

From

From having obferved, that iron has floated ten thoufand times on quickfilver, there arifes an expectation, that it will likwife float on it in the next trial; but this expectation is not certainty. It does not follow, fays Hartley, that becaufe a thing has happened a thoufand or ten thoufand times, that it never has failed, nor ever can fail. Vol. II. p. 142.

The fources therefore of probability are of two fpecies; the firft comprehends thofe probabilities, which are derived from confidering the number of caufes, which may influence the truth of the propofition: the other is founded folely on experience, from which we conclude, that the future will be like the paft; at leaft when we are affured, that the fame caufes, which produced the paft, ftill exift and are efficient. Thus, firft, let us fuppofe, that 30,000 flips of paper are contained in a wheel, of which 10,000 are black, and 20,000 are white; and that it is required to determine what are the odds, that I fhall at random draw a white paper. In this cafe, from the nature of things we perceive, that the number of chances for drawing a white paper is 20,000, and the whole number of chances is 30,000, therefore the probability of drawing a white paper is $\frac{20,000}{30,000}$ or $\frac{2}{3}$ of certainty; and the probability of drawing a black paper is $\frac{1}{3}$ of certainty. But fuppofe that I am ignorant of the contents of the wheel, and know only in general, that it contains feveral white and ſeveral black papers, in this caſe if it be required to determine the probability of drawing a white paper, we muſt proceed by trials, that is, we are to draw out a paper and obſerve its colour, then replacing it, we are to draw out a ſecond, which is to be replaced likewiſe, then a third, a fourth, and ſo on. It is clear, ſays Diderot, that the firſt drawn paper, being white, gives but a very low degree of probability that the number of the white exceeds that of the black; a ſecond white one being drawn would encreaſe this probability, a third would augment it. At length, if a great number of white papers ſhould be drawn out, without interruption, we would conclude that they were all white; and that with ſo much the more probability as we ſhould have drawn out more papers. But if, of the three firſt ſlips of paper, two ſhould appear to be white and one black, we would infer, that there was ſome low degree of probability, that there was twice as many white papers as black. If of the ſix firſt drawn papers, four ſhould be white and two black, this probability would encreaſe; and it would encreaſe ſo much the more in proportion, as the number of trials ſhould continually confirm the ſame proportion of the white papers to the black.

THIS manner of determining, probably, the ratio of the chances for the happening of an event to thoſe of its failing, is applicable to every thing which is contingent in nature.

IT may be aſked, ſays Diderot, whether this probability, admitting of infinite increaſe by a ſeries of repeated experiments, can arrive

arrive at length at certainty; or whether theſe increments are ſo limited, that diminiſhing gradually, they can at length produce only a determinate degree of probability.

M. Bernouilli has anſwered this queſtion in his Treatiſe De Arte Conjectandi, Part the Fourth. He there ſhews, that the probability which ariſes from repeated experiments, encreaſes continually in ſuch a manner, as to approach without limit towards certainty. His calculation ſhews us, provided the queſtion relates only to a particular caſe, how many times an experiment muſt be repeated in order to arrive at an aſſigned degree of probability. Thus in the caſe of a wheel which contains an unknown number of white and black ſlips of paper, ſuppoſe it were required to determine the ratio of the number of the white to the black; M. Bernouilli finds, that in order that it may be a thouſand times more probable, that there are two black papers for three white, rather than any other ratio, it would be neceſſary to make 25,550 experiments; and in order that it might be 10,000 times more probable, it would be neceſſary to make 31,258 trials; and in order that it ſhould be 100,000 times more probable, 36,966 trials would be requiſite; and ſo on ad infinitum, continually adding 5,708 experiments, according as the probability encreaſes in a decuple proportion. So that the number of experiments is the logarithm of the degree of probability produced. And ſince in high numbers the logarithms

logarithms encreafe nearly in the fame proportion with the abfolute numbers, it follows, that the probability that a future event will happen in the fame manner as in previous trials, will be nearly proportional to the number of thefe previous experiments.

By this it is demonftrable, that the experience of the paft is a principle of probability for the future; and that the more frequently we have experienced an event to happen, the greater reafon have we to expect, that it will happen in the next trial. Now fince in order that we fhould have a given degree of probability for the events happening in a particular way, a certain number of experiments is requifite, it follows converfely, that a given number of experiments will produce fome determinate degree of probability. This probability, we may perceive, depends merely on the number of experiments; fo that, this number being the fame, the degree of probability will be the fame whatever be the fpecific nature of the event.

Hence therefore, fince the inference which we make with refpect to the mechanical phænomena of nature, as well as with refpect to the veracity of human teftimony, are both equally derived from experience of the paft; if the teftimony be of fuch a nature as has never deceived us, the probabilities in both cafes will bear to each other fome determinate ratio, which, however ignorant we may be of the pr ıciples of the calculation, depends

on

on the number of trials which we have reſpectively made with reſpect to them.

It is true that Mr. Price, in his Eſſay on Miracles, p. 391, ſeems to think, that our expectation of the future from the paſt, is not to be reſolved into the conſtitution of our nature, but to *knowledge*; and this knowledge he ſeems to think is *intuitive*. " If," ſays he, " out of a wheel, the particular contents of which " I am ignorant of, I ſhould draw a white paper a hundred " times together, I ſhould *ſee* that it was *probable*, that it had " more white papers than black, and therefore ſhould expect to " draw a white paper the next trial." But here Mr. Price ſeems, unknowingly, to maintain the principle which he controverts; for we perceive the truth of axioms intuitively, either by the conſtitution of our nature, or by aſſociation which is reſolvable into it; and ſince we perceive the probability of propoſitions reſpecting exiſtence, as he aſſerts, in the ſame manner, it follows that we muſt perceive this probability likewiſe, either by the conſtitution of our nature, or by an aſſociation which is reſolvable into it. In fact, all probable propoſitions muſt be ſo either becauſe they are the concluſions of ſyllogiſms, one of whoſe premiſes at leaſt is probable, or becauſe they are primitive probable propoſitions. That there muſt be ſuch primitive, probable propoſitions, is evident, when we conſider that if a propoſition is probable only becauſe it is deduced from premiſes, one of which at leaſt is probable, then this premiſe muſt likewiſe be probable

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for the ſame reaſon; and thus there would be a proceſs ad infinitum, which is abſurd. Theſe primitive probable propoſitions are thoſe only which are the inferences we make of future events from the paſt. That theſe inferences are not certain is admitted; that they are alſo primitive inferences is manifeſt, becauſe there is no medium by which the inference is made out. There are therefore original and primitive probable propoſitions, in the ſame manner as there are original and primitive certain propoſitions, which are called axioms. But to aſcribe the former to *intuitive knowledge* ſeems an abuſe of language, intuition having been univerſally confined to the perception of axiomatical truths.

I KNOW it has alſo been maintained by ſome Metaphyſicians, that teſtimony does not derive its evidence from experience; but that it has a natural and original influence on belief, antecedent to experience. Let us then proceed to examine the arguments by which they endeavour to eſtabliſh this poſition.

FIRST, it is to be remarked, ſays Mr. Campbell, that the earlieſt aſſent, which is given to teſtimony by children, and which is previous to all experience, is in fact the moſt unlimited; that by a gradual experience of mankind, it is gradually contracted, and reduced to narrower bounds. To ſay therefore, that our *diffidence* in teſtimony is the reſult of experience, is more philoſophical,

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becaufe more confonant to truth, than to fay, that our *faith* in teftimony has this foundation. In reply to this arguing, let us confider the progrefs of the human mind; the firft part of our education confifts in leffons of caution againft danger; take care of this fire, fays the affectionate nurfe, it will burn you; of that knife, it will cut you; if you fall, you will be bruifed, &c. Thefe cautions are daily and hourly verified by experience; the child puts its hand towards the candle or the fire, and it is foon warned by pain to withdraw it; and fo in other cafes. Our earlieft affent therefore is the moft unlimited, becaufe derived from an experience that has never failed to confirm the truth of the witnefs. Soon however this uniform veracity of the witnefs begins to fail; the parent gives medicine to the fick child; in the next inftance, artifice is ufed to induce him to take the bitter draught; it is faid to be fweet and pleafant to the tafte, the child is deceived and drinks. Here begins diftruft; diftruft therefore and confidence have both the fame origin, to wit experience: nor can any metaphyfician produce an inftance, in which belief in teftimony has preceded all experience.

2dly, Dr. Reid is of opinion, that there is an inftinctive principle in the human mind to fpeak truth; and that there is another inftinctive principle, the counterpart of the former, which he calls the principle of credulity, or a difpofition to confide in the veracity of others, and to believe what they tell us.

That ſuch a principle exiſts in our own minds, can be determined only by our conſciouſneſs of it; that it reſides in others can be diſcovered only by their words and actions, that is, by experience.

In confirmation of what is here advanced, I ſhall tranſcribe the more concluſive argument of Dr. Prieſtley.

That any man, ſays Dr. Prieſtley, ſhould imagine, that a peculiar inſtinctive principle was neceſſary to explain our giving credit to the relations of others, appears to me, who have been uſed to ſee things in a different light, very extraordinary; and yet this doctrine is advanced by Dr. Reid, and adopted by Dr. Beattie. But really what our author ſays in favour of it, is hardly deſerving the ſlighteſt notice.

"If credulity," ſays he, "were the effect of reaſoning and "experience, it muſt grow up and gather ſtrength in the ſame "proportion as reaſon and experience do. But if it is the gift "of nature, it will be ſtrongeſt in childhood, and limited and re- "ſtrained by experience; and the moſt ſuperficial view of human "life ſhews, that this laſt is really the caſe, and not the firſt."

This reaſoning, continues Dr. Prieſtley, is exceedingly fallacious. It is a long time before a child hears any thing but truth, and therefore it can *expect* nothing elſe. The contrary would be abſolutely

absolutely miraculous. Falshood is *a new circumstance*, which he likewise comes to expect, in proportion as he has been taught by experience to expect it. What evidence can we possibly have of any thing being necessarily connected with experience, and derived from it, besides its never being prior to it, always consequent upon it, and exactly in proportion to it?—Priestley's Examination, &c p. 82.

3dly, Mr. Price argues, that experience is not the ground of the regard we pay to human testimony, for were it so, this regard would be in proportion to the number of instances, in which we have found, that it has given us right information, compared with those in which it has deceived us. But this is by no means the truth. One action, says he, or one conversation with a man may convince us of his integrity, and induce us to believe in his testimony, though we had never, in a single instance, experienced his veracity. His manner of telling the story, its being corroborated by other testimony, and various particulars in the nature and circumstances of it, may satisfy us, that it must be true. See his Essay on Miracles, page 399.

But is not all this confidence the result of experience? Why should the *manner* of telling a story induce us to believe it, unless we had previously learned by experience, that this *manner* was an indication of veracity. 2dly, In like manner, the *circumstances* of a story induce us to believe it, because we have found by experience, that these circumstances discover the integrity or skill

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of the witnefs. 3dly, Concurrent teftimony is not juftly introduced into this argument by Mr. Price, becaufe the foundation of the evidence of difcrete teftimony muft be afcertained, before we can proceed to the eftimation of concurrent teftimony; and alfo, more particularly, becaufe the greater ftrength of concurrent teftimony is equally admitted both by thofe who deny the dependence of the regard we pay to teftimony on experience, and thofe who affert it. 4thly, In our experience of a courfe of nature, our conviction is not always in proportion to the number of experiments *in a given inftance*, though it is in proportion to the whole number of experiments on which our belief is founded: thus if a new metal be difcovered which is fpecifically heavier than lead, we conclude from that fingle experiment that it will fink in water, with a confidence as great as that lead itfelf, on which we have made fo many experiments, will fink in water. And the reafon of this is, becaufe we transfer to this particular inftance the fum of our experiments on other fubftances fpecifically heavier than water, which have always been obferved to fink in it. And in this way it is, that the regard we pay to the report of a witnefs, is not always in proportion to the number of inftances in which we have found that *he* has told truth, namely becaufe we apply to him the fum of all thofe indications of veracity, which in previous inftances we have obferved in others.

4thly, Mr. Price obferves, that we feel in ourfelves that a regard to truth is one principle in human nature; and we know, that there muft be fuch a principle in every reafonable being.

But

But how do we know, that there muſt be ſuch a principle in every reaſonable being?—This implies, that other reaſonable beings are like us; and that they are ſo is to be diſcovered only by experience. When by experience we have diſcovered, that in ſimilar caſes they act in the ſame manner that we do ourſelves, we then infer, that they have the ſame tendencies, the ſame paſſions, the ſame regard to truth that we ourſelves have. So that this inference is preciſely of the ſame nature with that, which we make reſpecting the phænomena of nature. We have found that a piece of lead ſinks in water; another piece of metal occurs, which is found by experience or obſervation to reſemble lead; whence we infer, that it likewiſe will ſink in water. So that our inference in this caſe is founded on that conſtitution of our nature, by which we have a confidence in the future from our experience of the paſt: and our confidence in teſtimony has no other origin.

Having now ſhewn, that our belief in a courſe of nature and in human teſtimony is equally derived from experience, that the degree of probability is proportional to the number of previous experiments when they are very numerous, and that any given degree of probability is juſtly expreſſed by a fraction which denotes the value of our expectation; it follows, that theſe probabilities derived from our experience reſpecting any ſpecies of natural phænomena, and the veracity of human teſtimony are homogeneous quantities; and therefore may be juſtly compared with each other.

But the conviction produced by teſtimony is capable of being carried much higher than the conviction produced by other experience; and the reaſon is this, becauſe there may be concurrent teſtimonies with reſpect to the truth of the ſame individual fact, whereas there can be no concurrent experiments with reſpect to an individual experiment. There may indeed be analogous experiments, in the ſame manner as there may be analogous teſtimonies; but in a courſe of nature there is but one continued ſeries of events, whereas in teſtimony, ſince the ſame event may be obſerved by different witneſſes, their concurrence is capable of producing a conviction more cogent than any which is derived from any other ſpecies of events in the courſe of nature. In material phænomena, the probability of an expected event depends ſolely on analogous experiments, which have been made previous to the event; and this probability admits of indefinite encreaſe from the unlimited encreaſe of the number of theſe precedent experiments. The credibility of a witneſs ariſes likewiſe from our experience of the veracity of previous witneſſes, and admits of unlimited encreaſe, according to their number; and the law of its encreaſe is, of courſe, the ſame with that derived from phyſical events. There is however another ſource of the encreaſe of teſtimony, which is likewiſe unlimited, derived from the number of concurrent witneſſes, and its encreaſe on this account follows a law different from the former. The evidence of teſtimony therefore admitting of an unlimited encreaſe on two different accounts, and the probability of the happening of any

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ſpecific event admitting only of one of them, the former is capable of indefinitely ſurpaſſing the latter.

In order to prove this, we muſt conſider the law which the evidence of concurring witneſſes follows, according to the number of the witneſſes.

Let there be two dies, of the ſame kind, in each of which the number of white faces is m, each alſo having but one black face; and ſuppoſe, that theſe dies being thrown together, it be required to determine, what is the proportion of the number of chances that two white faces will turn up to the number of chances that two black faces will turn up together. The number of combinations of two white faces is the ſquare of m; and the number of combinations of black faces is unity. Therefore the odds that two white faces will turn up rather than two black faces, is as m^2 to 1. The caſes where a black and a white face turn up together are excluded by the nature of the queſtion, becauſe the witneſſes are ſuppoſed to be concurrent, that is, that the faces of the dies are of the ſame colour. In like manner, if there be three dies of the ſame kind as before, the odds that three white faces will turn up together rather than three black faces, will be m^3 to 1; and ſo on, the index of m being always equal to the number of dies.

Now if the number of chances that any witneſſes reſpectively tell truth, to the number of chances of their telling falſhood be as m to 1; the odds that they tell truth rather than falſhood, on ſup-

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pofition that they are concurrent, will be determined in the fame manner, that is, will be as that power of the number of chances of their telling truth, whofe index is the number of witneffes, to unity.

The feries of antecedents whofe common confequent is unity, which exprefs the ratio of the probability of the truth and falfhood of the concurring reporters, being the fucceffive powers of a given number greater than unity, encreafe in geometrical progreffion, and therefore will at length exceed any number however great. And if concurring reporters be all of equal credibility, their number may be fo far encreafed as to produce a probability greater than any that can be affigned.

For let any propofed degree of probability be $= \frac{a}{a+1}$; and let the probability that a given witnefs tells truth be expreffed by the fraction $\frac{b}{b+1}$, b being lefs than a; take fuch a power b^n of b as that it fhall exceed a, and let n be the number of witneffes, then will the probability of the veracity of the concurrent witneffes be expreffed by the fraction $\frac{b^n}{b^n+1}$, which is greater than the fraction $\frac{a}{a+1}$; becaufe unity, the given difference of the numerators and denominators bears a lefs proportion to the greater quantity b^n, and therefore

therefore the quantities b^n and $b^n + 1$ are more nearly equal than the numerator and denominator of the fraction $\frac{a}{a+1}$.

It is manifest, that where the credibility of each witness is very great, a very few witnesses will be sufficient to overcome the probability derived from the nature of the fact. Thus suppose the latter probability $= \frac{6560}{6561}$; and let us suppose that each witness tells truth only nine times for once that he tells falshood; that is, let the probability of the truth of his report be equal only to $\frac{9}{10}$; then four such concurring witnesses will be sufficient to produce belief.

After a certain number of concurring witnesses have given their testimony in confirmation of the truth of a fact, any farther encrease of their number is superfluous; because the difference between unity and the fraction expressing the probability, which is the result of their concurrent testimony, is indefinitely little; and all that an indefinite encrease of the number of witnesses could do, would be to diminish that indefinitely little defect.

Yet that probability, in cases of testimony, admits of an unlimited encrease, is evident; because the limit of probability is certainty, but the denominator of the fraction, which expresses probability, always exceeds the numerator by unity; therefore the fraction can never be equal to unity; that is, no finite number of concurrent reporters can produce abſolute certainty.

By this unlimited encreaſe is to be underſtood the actual, not ſenſible probability; for the indefinitely little defect from certainty is capable of mathematical computation, as well as the greateſt quantity, though it be imperceptible by the human mind. We are therefore juſtified in concluding, that the evidence of human teſtimony effectually attains its maximum, becauſe it arrives at ſuch a degree, as that any further increaſe of it is imperceptible. And the like takes place in extenſion; ſuppoſe a yard to be encreaſed by the hundred thouſandth part of an inch, and by half that quantity, and by the $\frac{1}{8}$, and $\frac{1}{16}$ &c. ad infinitum; the increment of this line would be imperceptible, and yet the line would never attain its maximum.

If the chances for the truth and falſhood of the report of each of any concurrent witneſſes be equal, no number whatever of ſuch witneſſes can render an event probable, by their teſtimony. Becauſe the number of chances of their coincidence in falſhood encreaſes in the ſame proportion with the number of chances for their telling truth. Let their number $= n$, ſince the probability that each witneſs tells truth is $= \frac{1}{2}$, the meaſure of the probability of the concurrent witneſſes will be $= \frac{n}{2n} = \frac{1}{2}$.

If it be improbable that each witneſs tells truth, that is, if the number of chances that each tells falſhood, be to the number

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of chances of his telling truth, in any ratio greater than the ratio of equality, the greater the number of concurrent witneſſes, the leſs will be the probability of the truth of their report; becauſe the greater will be the number of their combinations in falſe report in proportion to the number of their coincidences in truth. Thus if there be three witneſſes, each of whoſe credibility is meaſured by $\frac{1}{5}$, that is, if there be one chance only for the veracity, and four chances for the falſhood of each, then will the improbability of the truth of their report be meaſured by $\frac{1}{65}$.

This concluſion, as Mons. Condorcet obſerves, leads us to a very important remark, which ſhews how unfit numerous popular aſſemblies are for deliberation; for ſince in ſuch aſſemblies, when we conſider the ignorance and prejudices of the voters, we muſt eſtimate the probability that each will vote right at leſs than an even chance, it follows, that the more numerous the aſſembly, the greater will be the probability that their deciſions will be falſe. And hence we perceive, what political evils muſt follow from the determinations of an ignorant democracy. But in a well informed and impartial aſſembly, the more numerous the voters, the greater will be the probability of the rectitude of their deciſions.

Hence, by the way we may remark, that Dr. Halley's mode of computing the probability of the report of concurrent witneſſes is erroneous. According to him, the calculation is to be made in following manner; if the firſt witneſs gives $\frac{a}{a+c}$ of certainty, and there is wanting of it $\frac{c}{a+c}$, the ſecond atteſter will add $\frac{a}{a+c}$ of that $\frac{c}{a+c}$; and conſequently leave wanting only $\frac{c}{a+c}$ of that $\frac{a}{a+c} = \frac{c^2}{\overline{a+c}|^2}$. And in like manner, the third atteſter adds his $\frac{a}{a+c}$ of that $\frac{c^2}{\overline{a+c}|^2}$, and leaves wanting only $\frac{c^3}{\overline{a+c}|^3}$, &c.

HENCE, he obſerves, it follows, that if a ſingle witneſs ſhould be only ſo far credible as to give me the half of full certainty; a ſecond of the ſame credibility, joined with the firſt, would give me $\frac{3}{4}$ths, a third $\frac{7}{8}$ths, &c. which appears to be falſe; for we have ſhewn above, that no number of ſuch reporters could produce an aſſurance greater than that of an even chance, for the truth or falſhood of the fact.

THE fallacy of his argument lies in this, that he ſuppoſes all the individual concurrent witneſſes to produce unequal degrees of aſſurance, which is evidently a falſe poſition; ſince they are all of equal credibility and equally concurrent, and therefore contribute equally in producing our aſſurance.

DR. WARING, whoſe ſolution is eſſentially the ſame with Halley's, ſays, if there be two different arguments (or witneſſes) entirely independent of each other, in ſupport of a fact, whoſe probabilities

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let be $\frac{p}{a}$ and $\frac{q}{a}$; then will the probability in fupport of the fact, refulting from both arguments (or witneffes) be $1-\frac{(a-p)(a-q)}{a_2}$: for if the probabilities in fupport of it are refpectively $\frac{p}{a}$ and $\frac{q}{a}$, then will the refpective probabilities of its failing be $1-\frac{p}{a}=\frac{a-p}{a}$, and $1-\frac{q}{a}=\frac{a-q}{a}$; and confequently the probability of failing from both will be $\frac{a-p}{a}\times\frac{a-q}{a}$; whence the probability of the fact refulting from both will be $1-\frac{a-p}{a}\times\frac{a-q}{a}$.

In this argument there is one ftep, which appears inadmiffible; it is affumed, that if the probability of failing from both, or rather of both failing, be $=\frac{a-p}{a}\times\frac{a-q}{a}$, then $1-\frac{a-p}{a}\times\frac{a-q}{a}=$ the probability of happening from both, which does not appear to be true; becaufe $1-\frac{a-p}{a}\times\frac{a-q}{a}$ is equal to the probability of both happening, together with the probability of one happening and the other failing. Thus if there be an even chance for both, $\frac{a-p}{a}=\frac{a-q}{a}=\frac{2-1}{2}=\frac{1}{2}$; then $\frac{1}{4}=$ the probability that both will fail; alfo $\frac{1}{4}=$ the probability that both will happen, and $\frac{2}{4}=$ the probability that one will happen and the other fail; therefore $1-\frac{1}{4}=\frac{1}{4}+\frac{2}{4}=$ the fum of the probabilities that both will happen, and

and that one will happen and the other fail. This mode of calculation adopted by Dr. Waring, however it may hold in joint annuities, where the desired end is equally answered, whether one or all of the lives attain the proposed period, will not equally apply to the conjoint probability of arguments, or concurring witnesses, where the evidence fails either when the arguments are all false, or are opposed to each other.

If the witnesses that attest a fact, or the voters that decide on a question, contradict each other, and it be required to determine what is the resulting probability of the truth of the fact or of the decision upon the whole, we are to proceed thus: first compute the odds that the affirmative witnesses are right, or the ratio of the number of chances of their being right to the number of chances of their being in errror; proceed in the same manner with the negative witnesses; then the product of the number of chances that the affirmative witnesses are right, into the number of chances that the negative witnesses are mistaken, will be the number of chances for the truth of the fact; and the product of the number of chances that the affirmative witnesses are mistaken, into the number of chances that the negative witnesses are right, will be the number of chances for the falshood of the fact; and consequently the probability of the truth of the fact resulting upon the whole, will be equal to the former product divided by the sum of the two products. For example, let there be seven voters, of which let four be affirmative and three negative; and let the chance

chance that each votes rightly be the ratio of a to b, then the ratio of a^4 to b^4 will be the odds that the affirmative voters are right; and the ratio of a^3 to b^3 will be the odds that the negative voters are right; and the ratio of $a^4\ b^3$ to $b^4\ a^3$, or a to b, will be odds resulting that the affirmative voters are right.

If there were eight voters, the lowest majority must be five and three; and the odds that the affirmative voters were right would be a^5 to b^5; and the odds that the negative voters were right would be a^3 to b^3; and the resulting odds that the question was justly decided would be $a^5\ b^3$ to $a^3\ b^5$ or a^2 to b^2.

In general therefore it appears, that the odds for the truth of the decision, will be that power of the odds that each person votes justly, whose index is the difference between the number of affirmative and negative voters.

And hence we may correct the error of those who imagine, that the probability, *cæteris paribus*, is the same, if the proportion of the number of affirmative witnesses to the number of negative witnesses be the same; whereas the probability is to be estimated by the difference of these numbers.

We have already remarked, that in the enacting of a new law, we ought to have at least that probability for the expediency of the law, below which a person cannot act without imprudence. As the manner of determining this degree of probability is extremely

tremely ingenious, I cannot avoid mentioning it. The object to be attained is equivalent to this, that in the enacting of a law, the rifk of error fhould not be greater than what we difregard, even where our own life is in queftion. Buffon and Bernouilli have endeavoured to eftimate the value of this rifk, but the following method adopted by Condorcet, feems to be the beft. It is obferved, that from the age of thirty-feven years to forty-feven, and from eighteen to thirty-three, the rifk men run of dying by accident or difeafes of fhorter duration than a week, encreafes continually in nearly a regular manner; and it is alfo obferved, that a man of thirty-three years is not more apprehenfive of fuch kind of death than a man of eighteen, nor a man of forty-feven than a man of thirty-feven years; the difference of rifk therefore in thefe cafes is difregarded: now, from the tables of mortality, it appears, that, in the firft period, the difference of rifk is = $\frac{1}{301115}$, and in the fecond = $\frac{1}{144768}$; let us then take the latter, which is the greater, as the limit of that rifk which may be difregarded, and confequently $\frac{144767}{144768}$ will be the limit of the affurance, which we ought to have in the enacting of a new law.

If we fuppofe that the odds that each legiflator votes juftly, is ten to one, then will a majority of fix be requifite to give the affurance required; which in an affembly of three hundred is only a majority of one in fifty.

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These principles, which we have laid down above, may be likewise applied, as is manifeſt, to determine the probability of the deciſions in courts of appeal; where the ſame queſtion is ſucceſſively tried before different tribunals.

And here I cannot avoid obſerving, that Dr. Waring's method of determining the reſulting probability, where different arguments are contradictory, is erroneous. Let P, ſays he, be = the probability reſulting from the arguments in ſupport of the fact, and Q = the probability reſulting from all the arguments againſt the fact; then the probability of all the arguments for the fact will be P—Q, if P be greater than Q; or againſt it = Q—P, if Q be greater than P. See Principles of Human Knowledge, § 10. Now, according to theſe principles, if two witneſſes of equal veracity ſhould contradict each other, the difference between the probabilities for and againſt the fact would be = *o*, that is, the fact would be impoſſible; which evidently cannot be a true inference. But in reality, in this caſe, there would be an equal chance for the truth and falſhood of the fact; for let the odds that each witneſs tells truth be the ratio of a to b, then the odds reſulting that the fact is true, will be the ratio of ab to ba, and the reſulting probability = $\frac{ab}{ab+ab} = \frac{1}{2}$.

Again, if againſt a propoſition which is abſolutely certain, there ſhould occur an argument for the truth of which there was an even chance; the probability refulting upon the whole, according to Dr. Waring, would be no more than an even chance, for $1-\frac{1}{2}=\frac{1}{2}$; which is manifeftly a falfe inference. In fact, fince the odds that the propofition is true are infinite, or as 1 to 0, the refulting odds muft always be as fome finite number to 0, that is, infinite, that is, the propofition will ftill be certain.

I have here mentioned fome circumftances relative to the nature of the evidence refulting from concurring and contradictory reporters, not tending directly, it is true,, to the eftablifhment of the point I propofed to myfelf, but nearly connected with it; my principal, and I may almoft fay, my fole object being to fhew, that the evidence of teftimony can overcome any degree of improbability however great, which can be derived from the nature of the fact.

Our expectation that a phyfical event, in the courfe of nature, will happen in a particular manner, is founded on previous experience; which experience may be both perfonal and derived; that is our expectation may be deduced both from our own actual experience, and the reports of others vouching their experience, of the like events in fimilar cafes. Since this expectation muft neceffarily be of fome determinate value, depending in fome manner

manner on the number of experiments either actually made by ourſelves or reported by others, we will ſuppoſe it $= \frac{e}{e+1}$. This argument is founded on an analogy which has never deceived us, and is called, by Mr. Hume, a proof. On the other hand, there is a direct and poſitive teſtimony of a ſingle witneſs, that the contradictory of this event did actually happen; and this is ſuch a teſtimony as both perſonal and derived experience aſſures us has never deceived; the probability of the truth of this teſtimony we will call $\frac{t}{t+1}$; this argument Mr. Hume likewiſe calls a proof, and he ſuppoſes, that it is equal to the former, that is, $\frac{t}{t+1} = \frac{e}{e+1}$. This however is a mere hypotheſis; for they are both probable inferences only, deduced from experience; but it is by no means ſhewn, that the number of experiments made in both caſes are the ſame, or the circumſtances exactly parallel; t therefore may be either equal to, or greater, or leſs than e, in any aſſigned proportion. The evidence of a ſingle witneſs is to be compared with that probability of an event in phyſical phœnomena, which is derived from a ſeries of ſimilar experiments only; becauſe the veracity of human teſtimony conſtitutes one ſpecies of events in the courſe of nature, in the ſame manner as the ſinking of lead in water, or the diſſolution of gold in aqua regia; and therefore is deduced, in the ſame manner as any other ſpecific phœnomenon, from experience, and appears to ariſe, in the ſame manner, from an eſtabliſhed law. This veracity therefore is confirmed by the analogy of other phœnomena, in the ſame manner as any given ſpecies of phyſical phœnomena; inaſmuch as theſe other phœnomena contribute to eſtabliſh the general principle, that *all things* are conducted according to eſtabliſhed laws. If now we conſider the numerous experiments we make every day on the veracity of human teſtimony in certain circumſtances, ſo that our analogy in this caſe is founded on an indefinitely greater number of inſtances than in any other ſpecies of events in the courſe of nature, we may perceive, how the evidence even of a ſingle witneſs may be ſo circumſtanced, as to eſtabliſh an individual phyſical phœnomenon, however contradictory it may appear to our previous experience of ſimilar facts. Let us however ſuppoſe, that the evidence of the ſingle witneſs is leſs than the evidence of experience in any aſſigned proportion, or that t is leſs than e in the proportion of 1 to m; then $mt = e$, and $\frac{e}{e+1} = \frac{mt}{mt+1}$. Take now ſuch a power t^n of t, as that it ſhall be greater that mt, and $\frac{t^n}{t^n+1}$ will be greater than $\frac{e}{e+1}$; that is, if n be the number of witneſſes, each of whoſe veracity is $= \frac{t}{t+1}$, their concurrent teſtimony will be ſufficient to overcome the probability $\frac{e}{e+1}$ derived from the nature of the fact. Hence

therefore

therefore it follows, that the evidence of teſtimony can approach indefinitely near to certainty; and can at length exceed the evidence of any inference, however cogent, which can poſſibly be deduced from perſonal experience, or from perſonal and derived experience conjointly.

It is to be obſerved, however, that the calculation here ſtated, applies only to the teſtimony of different witneſſes, who ſimply give their evidence as to the truth or falſhood of a propoſed fact; or of witneſſes each of whom has an opportunity of knowing what teſtimony the others have given. This, without doubt, is to take the force of concurrent teſtimony at the greateſt diſadvantage; nevertheleſs, even in this caſe we find, that it has no limit. But there are other caſes in which the leaſt number of concurrent witneſſes, let the degree of their veracity be however ſmall, can afford a probability which ſhall exceed any given degree of probability however great; namely, where the witneſſes have had no means of knowing each others teſtimony, and the fact is attended with contingent circumſtances, which make a part of their depoſition: becauſe the chances of their not concurring in theſe circumſtances, may exceed any given chance. In theſe caſes we obſerve, that even witneſſes who have been obſerved to tell falſhood oftener than truth, may yet produce belief; becauſe here the probability of the truth of their report is not derived from the chances of their coinciding, abſtractedly, in truth or falſhood, but from the chances of their coinciding in circumſtances contingent in their nature, and which have no apparent connection with each other. As for inſtance, if each witneſs ſhould declare, that a celeſtial phænomenon, even ſuch as we had never ſeen, had appeared in a certain region of the heavens, on a certain day, hour, and ſecond; had run over a particular tract, and laſtly diſappeared with circumſtances peculiar and minutely detailed; we muſt perceive, that our belief would not be founded on an enquiry into the characters of the witneſſes, but ſolely into the chances of their concurrence in theſe contingent circumſtances.

On the NUMBER of the PRIMITIVE COLORIFIC RAYS in SOLAR LIGHT. By the Rev. MATTHEW YOUNG, D. D. S. F. T. C. D. & M. R. I. A.

THE opinion that there are but three primitive colours has been maintained by M. du Fay, and after him by Father Caſtell. See Montucla, Vol. I. p. 630.; but they and all others who hold the ſame doctrine, defend it merely on the principles of a painter, who ſhews how with theſe three colours on his pallet, he can compound all others; for with red and yellow he can form an orange colour; with blue and yellow he forms green; and with blue and red he forms indigo and violet; and thus having compounded the ſeven priſmatic colours, it is manifeſt that all other colours, with their different gradations, can be formed from them likewiſe. But this *pharmaceutical* argument is by no means ſufficient to ſatisfy us as to the real compoſition of ſolar light. Read April 7th, 1798.

" LIGHT, in refracting, is decompoſed into ſeven rays, red, " orange, yellow, green, blue, indigo and violet. It has been " ſuppoſed,"

"supposed," say Fourcroy, "that three of these colours, the "red, yellow and blue, were simple; and that the other four "were formed each of its two neighbours; that is, the orange "from the red and yellow, the green from the yellow and blue, "the indigo from the blue and violet, and the violet from the "red and indigo. But this supposition has never been proved." See his Philosophy of Chem. ch. 1. § 3. Besides that this is a mere hypothesis, unsupported by any fact, as Fourcroy observes, we remark, that it is in itself inadequate; 1st, because in the solar spectrum, the red and indigo are not *neighbouring* colours but are almost at the greatest possible distance from each other. 2dly, According to this hypothesis, indigo is composed of blue and violet; but violet is composed of red and indigo; indigo therefore is composed of red, blue and indigo, that is, indigo itself is one of its own essential ingredients, which is absurd.

The experiments of the prism seem to establish, in a very clear manner, the existence of seven original and uncompounded colours; and though green, for instance, may be compounded of blue and yellow, yet it does not directly follow from thence, that it always is so actually compounded. Accordingly Newton tells us, that green may be exhibited in two different ways, either by primitive, green-making rays, which are simple and not resolvable by any reflection or refraction into different rays; or by a composition of blue and yellow rays, which are differently refrangible, and which therefore after their union, may again be separated

rated by refraction, and exhibit their proper colours of blue and yellow.

On this doctrine of the two-fold generation of green, we may in the first place remark, that the antient, received axiom "Deus "nil agit fuftra" ought not to be too haftily abandoned, as it muft appear to be, if this doctrine be maintained: for if green may be produced by blue and yellow, then blue and yellow being already exiftent, green is a confequence; and therefore peculiar rays formed for the production of green are fuperfluous. Though I acknowledge, that this maxim is not fo cogent or felf-evident, as to preclude all objection, yet fince the general obfervation of nature feems to fhew, that this wafte of power or multiplicity of means is not adopted by the Supreme Artift, it certainly feems juftly entitled to our attention, at leaft fo far as this, that we fhould be careful in fhewing, that we are led to thefe different caufes of the fame effect, by a legitimate and cautious analyfis.

In defence of the doctrine of three primitive colours only, F. Caftelli contents himfelf with faying, that the colours of the prifm are immaterial, accidental, artificial, and therefore unworthy the regard of a philofopher; whereas the colours of painters are fubftantial, natural, palpable. From them, of confequence, the theory of chromatics fhould be deduced; but they

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tell us, that there are but three parent colours, which give birth to all others.

In reply to this we need only obferve, that Sir I. Newton has proved, that the colours of natural bodies depend on the colorific qualities of the rays of light; and therefore that our theory of colours muft be derived from an enquiry into the conftitution of folar light, for according to that conftitution the colours of bodies will vary: and he farther fhews, that if folar light confifted of but one fort of rays, all bodies in the world would be of the fame colour. However true therefore F. Caftelli's theory may be, the manner in which he deduces it from phænomena is unqueftionably falfe.

I fhall therefore proceed to enquire fcrupuloufly into the compofition of the folar fpectrum, from which, without doubt, the true doctrine of the origin of colours is to be derived.

If the folar light confifted of feven primitive, homogeneal coloured rays, and that thefe homogeneal rays were equally refrangible, the fpectrum would confift of feven circles of different colours, fince the homogeneal rays of each colour would paint a circular image of the fun. But it is manifeft, that feven circles could not compofe an oblong fpectrum, with rectilineal fides. Therefore the rays of the fame denomination of colour muft be differently refrangible. Which is alfo made ftill farther evident

evident by obſervation of the ſpectrum, ſince in it we perceive, that the priſmatic colours are diffuſed over ſpaces, which are, on the ſides, terminated by right lines, and therefore the centers of the circles of the ſame denomination of colour are diffuſed over lines equal to theſe ſegments of the rectilineal ſides of the ſpectrum. Newton has ſhewn, prop. 4. B. 1. Optics, how to ſeparate from one another theheterogeneous rays of compound light, by diminiſhing the breadth of the ſpectrum, its length remaining unchanged; and when the length of the ſpectrum is to its breadth, as 72 to 1, the light of the image is ſeventy-one times leſs compound than the ſun's direct light. In the middle of a black paper he made a round hole, about a fifth or a ſixth part of an inch in diameter, upon which he cauſed this ſpectrum ſo to fall, that ſome part of the light might paſs through the hole of the paper; this tranſmitted part of the light he refracted with a priſm placed behind the paper, and letting the light fall perpendicularly upon a white paper, he found that the ſpectrum formed by it was perfectly circular. Hence, therefore, it follows, that the equally refrangible rays occupy a ſpace on the rectilineal ſides of the ſpectrum equal at leaſt to the fifth or ſixth part of an inch; that is, the rays of the ſame colour are differently refrangible.

The different quantity of the homogeneous rays of different colours will not account for the different ſpaces they occupy in the ſpectrum; for this difference in quantity would affect only the intenſity of the colour, not the magnitude of the ſpace which it would occupy. All the red light therefore is not homogeneous; but confifts of rays of innumerable, different degrees of refrangibility; and fo of the other colours.

Now fince the rays which are of the fame denomination of colour are differently refrangible, they will either form oblong fpectrums detached from each other; or they will in part lap over, and fall on each other. The former pofition is manifeftly falfe: therefore the original prifmatic colours will partly lap over and fall on each other, and therefore neceffarily generate the intermediate colours. And fo Sir I. Newton obferves, where he fays, that the original, prifmatic colours will not be difturbed by the intermixture of the conterminous rays, which are intermixed together. This overlapping however, which Newton fpeaks of, arifes only from the fun's having a fenfible diameter, and does not neceffarily imply an equal refrangibility in any differently coloured rays. If there be but three original prifmatic colours, red, yellow and blue, and that the red and yellow lap over, fo as that there fhall be a certain fpace in the fides of the fpectrum equally occupied by yellow and red circles, then will thefe circles by their intermixture compound an orange colour; and this colour as to refrangibility will be homogeneous, becaufe the coincident rays of different colours are equally refrangible. In like manner green may be compounded by the mixture of blue and yellow circles, equally refrangible. Now this is fimple, and conformable to the other phænomena of the fpectrum; for if rays of the

the ſame denomination of colour be differently refrangible, it is not unreaſonable to ſuppoſe, that rays of a different denomination of colour may be equally refrangible; and therefore ſince the red rays are unequally refrangible, and likewiſe the yellow, there is nothing incongruous in ſuppoſing that ſome of the leſs refrangible of the yellow may be equally refrangible with ſome of the more refrangible of the red; and if ſo, they will conſequently be intermixed with them: and the ſame may be ſaid of the green. This hypotheſis likewiſe receives conſiderable ſtrength from this conſideration, that the orange, green, indigo and violet occupy thoſe places which they ought to do, in caſe there were but three primitive colours, red, yellow and blue: thus the orange lies between the red and yellow, becauſe it is formed by ſome of the extreme rays of red and yellow, which are equally refrangible; in like manner the green lies between the blue and yellow, becauſe it is formed by the mixture of blue and yellow. The indigo and violet muſt alſo occupy the extreme part of the ſpectrum, where the moſt refrangible red and blue rays are united, and gradually becoming more and more dilute, fade away, and at length entirely vaniſh. But if the orange, green, indigo and violet be primitive colours, there is no apparent reaſon why they ſhould have had ſuch degrees of refrangibility aſſigned them, as that they ſhould occupy the places they do, rather than any other.

Moreover, if theſe three colours red, yellow and blue be the primitive colours, they cannot themſelves be generated; and accordingly

cordingly we find, that yellow cannot be generated by the mixture of the adjacent prifmatic colours, orange and green; and the reafon of this is evident, becaufe orange is compounded of red and yellow; and green is compounded of yellow and blue; but red and blue compofe purple; which added to the yellow will generate a new compound colour, viz. a fickly green, differing manifeftly from yellow, the colour which ought to refult according to the analogy of the other primitive colours, in which the extremes, by their mixture, generate that which is intermediate. In the fame manner, blue cannot be generated by the mixture of green and indigo, becaufe green is compofed of yellow and blue, and indigo of blue and violet; therefore the refulting colour is compofed of blue, yellow and violet; but yellow and violet do not compofe blue, therefore neither will blue, yellow and violet compofe a blue colour. Now if orange and green be primitive colours, in the fame manner as red, yellow and blue, we can affign no reafon why blue fhould not be generated by the mixture of the adjacent colours, as well as green and orange. But it is a received principle, that an hypothefis fhould folve all the phænomena; of the two hypothefes therefore, namely, that there are feven primitive colours, differently refrangible; or that there are but three, fome of which, of each fpecies, are equally refrangible; the latter alone folves all the phænomena of the folar fpectrum, and therefore is to be preferred.

If it be faid, that thofe rays which are equally refrangible muft excite the fame fenfation on the retina, becaufe they muft have

have the ſame momentum; it is replied, 1ſt, That it has not yet been proved, that the ſenſation of different colours depends on the different momentum of the rays. 2dly, The rays may have different momentums, and yet be equally refrangible; for ſince refraction is ſuppoſed to depend on the attractive force of the denſer medium, we muſt ſuppoſe it analogous to the attractive force of gravity, which is proportional to the quantity of matter; and therefore the greater or leſs quantity of matter in a particle of light would produce no alteration in its refraction. Neither can the different refrangibility depend on the different velocity of the rays; becauſe the difference of refrangibility of the red and violet rays is much greater in flint glaſs than in crown glaſs; and this would require a proportionably greater difference in the original velocities, which cannot be. And this ſame argument holds equally againſt the former hypotheſis, that the difference of refrangibility depends on the different magnitude or denſity of the particles of light. 3dly, Refraction ſeems to ariſe from a ſpecies of elective attraction, ſince different mediums which act on the mean rays equally, act on the extreme rays unequally: hence rays of the ſame quantity of matter and velocity, and therefore of the ſame momentnm, may be diverſely refracted; and rays of different momentums equally refracted.

Nor is it to be wondered at that the rays of light ſhould be differently refrangible, independent of any regard to their momentum, when we conſider, that the different coloured rays appear

pear to be combined with combuſtible bodies, with different degrees of attractive force. For in combuſtion we find, that different bodies are diſpoſed to part with different rays with greater facility; but when the combuſtion is ſufficiently rapid, they part with all the different coloured rays together, and the flame is therefore white; and this is what is called a white heat. Dr. Fordyce in the Phil. Tranſ. for 1776, tells us, that when the heated ſubſtances are colourleſs, they firſt emit a red light; then a red mixed with yellow, and laſtly, with a great degree of heat, a pure white. All this is wonderfully conformable to the refraction of light by tranſparent ſubſtances, which refract, and therefore attract the red light leſs, and conſequently in combuſtion part with it more eaſily. On the other hand I know it is generally believed, that the light in combuſtion proceeds from the air, but this circumſtance of the different colour of the light in different caſes, ſeems to overturn this opinion; for if vital air were oxygen diſſolved in caloric and light, then the oxygen being abſorbed by the burning body, the light extricated would in all caſes be of the ſame nature; the greater or leſs rapidity of the combuſtion would only produce an extrication of a greater or leſs quantity of light, but could not produce any variation in its nature, it being neceſſarily the ſame in all caſes, to wit, that in which vital air is diſſolved. But the truth or falſhood of this reaſoning will not affect the validity of the poſition, that the refrangibility of the rays of light cannot depend on the different magnitude, denſity or velocity of the particles.

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But though ſpeculation ſeems thus to render it probable, that there are but three parent colours; our theory muſt ever remain unſatisfactory, unleſs it receives the ſanction of direct experiment. In this however there is no ſmall difficulty; for ſince the rays of light which compoſe any given individual point of the colours of orange, green, violet, and indigo are equally refrangible, they will be alſo equally reflexible; and therefore cannot be ſeparated either by refraction or reflection, ſo as to exhibit the different coloured rays of which they are compoſed. It ſeems therefore, that the only way remaining, by which we can experimentally aſcertain the compoſition of theſe colours, if they be indeed compound, is tranſmiſſion. For ſince tranſparent coloured bodies are ſuch merely by their letting paſs through them either ſolely, or more copiouſly, rays of a certain colour, and intercepting all others, ſuch tranſparent bodies, applied to compound colours, will aſcertain that compoſition, by extinguiſhing, in a great meaſure, all rays except ſuch as are ſo adapted to its conformation, as to paſs through it, and give it its peculiar denomination of colour.

In order to try the truth of the hypotheſis of ſeven colours by this teſt, I looked through a blue glaſs at the red end of the ſpectrum: now we are to conſider, that if that part of the ſpectrum was compoſed of red rays, and none other, the only effect of the blue glaſs would either be a total or partial ſuffocation of the red rays; and therefore that part of the ſpectrum, when looked at

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through the glaſs, would either totally diſappear, or become a faint and diluted red. But, on experiment it appeared of a purple colour. The purple in this caſe could not be a primitive and original colour, as is manifeſt, becauſe it did not proceed from the purple part of the ſpectrum; we muſt therefore conclude, that it was a compound colour. But purple, when compound, is made up of blue and red, therefore it follows, that ſome blue rays did actually exiſt in the red part of the ſpectrum; which combined with the few, ſtraggling red rays which penetrated the blue glaſs, compoſed that purple colour, which the red extremity of the ſpectrum aſſumed, when viewed by the light tranſmitted through the blue medium.

To try, on the other hand, whether any red rays lay hid amongſt the blue, I proceeded in the ſame manner, and looking at the blueſt part of the ſpectrum through a red glaſs, it appeared of a purple colour; ſome red rays therefore are equally refrangible with the blue; and if the red extends as far as the blue, there is no reaſon why we may not ſuppoſe that it extends ſomewhat farther, ſo as to compound, with a diluted blue, the extreme colours of the ſpectrum, indigo and violet.

But it may be ſaid, that if blue rays exiſted amongſt the red, that part of the ſpectrum could not appear ſo extremely brilliant as it really does; but would put on a purpliſh appearance in the ſpectrum itſelf, even to the naked eye. In anſwer to this objection

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we may obſerve, that the moſt intenſe and vivid, natural red bodies do, in fact, reflect a very great proportion of blue rays, becauſe they appear of a ſtrong blue colour when placed in the blue part of the ſpectrum; and therefore they reflect juſt as many when the direct, white ſolar light falls on them, in which all that blue is involved; though by the predominance of the red rays, they appear of that colour, without any viſible tincture of blue.

In order to determine whether the purple appearance of the red extremity of the ſpectrum, when viewed through a blue glaſs, was cauſed by any of the white ſolar light, which might perhaps be reflected from the air, or ſurrounding objects to the ſpectrum, and thus throw on that part ſuch a quantity of blue as might produce a ſenſible effect; I cauſed the middle and moſt intenſe part of the red to paſs through a hole in a blackened paper, and then fall on an optical ſcreen; by which I was ſure that I had as pure and uncompounded a red as could be deſired; which alſo underwent the uſual teſt of purity by ſubſequent refraction, without any change in the form of the ſpectrum; I then looked at the body which was illuminated with this red, through the ſame blue glaſs, and the effect was the ſame as before.

To try this doctrine of three parent colours ſtill farther, I conſidered, that if the orange were really compounded of the red and yellow rays, then by looking at the orange through a red glaſs, the orange would in a great meaſure vaniſh, and the red would appear to extend much farther than in the original ſpectrum; becauſe the yellow rays being conſiderably obſtructed, the red would become more predominant; and that part of the ſpectrum, which before appeared orange, in conſequence of a certain mixture of yellow and red, would now, by the failure of ſo conſiderable a part of the yellow, loſe its orange appearance, and put on that of red: and, on experiment, I found the caſe to be ſo really in fact; for while an aſſiſtant looked at the ſpectrum through the red glaſs, I moved an obſtacle from the red towards the other end of the ſpectrum, deſiring him to ſtop me, when the obſtacle ſhould arrive at the confines of red and orange; but when he did ſo, the obſtacle had attained the middle of the orange, or rather had paſſed beyond it. Now if the orange were really a primitive colour, I ſhould ſuppoſe, that when looked at through the red glaſs, it would either appear diluted, without any change of dimenſions; or that if the weak part of the orange, next the red, ſhould vaniſh, by the obſtruction of the glaſs, a dark interval would appear between the orange and the red; in neither caſe can we account for the apparent extenſion of the red into the region of the orange; nor by any other hypotheſis, as appears to me, than that ſome of the red rays are equally refrangible with ſome of the orange.

There is another argument derived from the ocular ſpectra of Dr. Darwin, which ſtill further corroborates the doctrine of three primogenial

primogenial colours. Place a piece of coloured ſilk, about an inch in diameter, on a ſheet of white paper, about half a yard from your eyes; look ſteadily upon it for a minute; then remove your eyes upon another part of the white paper, and a ſpectrum will be ſeen of the form of the ſilk thus inſpected, but of a different colour, thus

Red ſilk produced a green ſpectrum,		
Green	-	red,
Orange	-	blue,
Blue	-	orange,
Yellow	-	violet,
Violet	-	yellow.

The reaſon of theſe phænomena is very ingeniouſly aſſigned by Dr. Darwin; he ſays, that the retina being excited into a violent and long continued action by the red rays, in the firſt experiment, at length is ſo fatigued as to become inſenſible to them; but that it ſtill remains ſenſible, that is, liable to be excited into action by any other colours at the ſame time; and therefore the ſpectrum aſſumes a green appearance, becauſe if all the red rays be taken out of the ſolar light, the remaining rays will compoſe green. See Phil. Tranſ. Vol. LXXVI. Converſely, a green object produces a red ocular ſpectrum. Now we may obſerve, that if all the green rays be taken out of the ſolar ſpectrum of ſeven colours, the remaining colours will not compound red. If indeed green be not a primitive colour, but a compoſition of blue and yellow, then

will

will the eye, in looking on a green objeſt, be at once affeſted by blue and yellow rays; and therefore become inſenſible to them both; and conſequently the ſpeſtrum will appear red. But if green be a primitive, original colour, generated by its own peculiar green-making rays, the eye in contemplating a green objeſt, will become inſenſible only to the green rays; and therefore the other ſix priſmatic colours, which are ſpecifically different from the green, ought to be ſenſible, and produce their proper compound effeſt; but this would not be the ſenſation of red. In like manner, if the objeſt be yellow, the eye will at length become inſenſible to the yellow-making rays, and the ſpeſtrum will be violet. Now ſince on the hypotheſis of ſeven original colours, the orange and green are primitive, though the eye be rendered inſenſible to the yellow rays, it will not be ſo to the orange and green, which therefore, together with the red, blue, violet and indigo will produce their compound effeſt; but the colour reſulting from this joint aſtion is not violet, which nevertheleſs is the colour of the ocular ſpeſtrum. On the other hand, if there be but three primitive colours, red, yellow and blue, when the eye is inſenſible to the yellow-making rays, the ſpeſtrum muſt neceſſarily be violet, which is the colour that reſults from the mixture of red and blue. If it be objeſted, that the eye is not only inſenſible to the unmixed yellow rays, but likewiſe to the yellow of the orange and the green, then it is admitted that orange and green are compound colours. Beſides, ſince the colour which would reſult from the mixture of red, orange, green, blue,

blue, indigo and violet is not yellow, the eye ought not to be insensible to this colour; and consequently, since by the exemption of the yellow rays from the white solar light, that colour does not result, but a distinct purple, it follows, that the orange and green are not primitive colours inherent in solar light.

It remains now only for us to shew, that the three colours of red, yellow and blue are adequate to the solution of all the phænomena of chromatics. But in order to shew this, few words will be sufficient, for having seen, that the seven prismatic colours can be generated by these three, it follows that all others can be generated from them, as Sir I. Newton has proved at large. However I think it will not be superfluous to observe, that white may be directly produced by these three colours, without the previous generation of the other four prismatic colours, in the same manner as it is usually generated with seven. "I could "never yet," says Newton, "by mixing only two primary co-"lours, produce a perfect white. Whether it may be composed "of a mixture of three, taken at equal distances in the circum-"ference, I do not know." Now to shew that white may be thus generated, let an annulus of about four inches diameter be divided into three parts by lines tending towards the centre, and let these three divisions be respectively painted red, yellow and blue, in proportions to be ascertained by trial; then if the annulus be turned swiftly round its centre, it will appear white. That white may be generated by the mixture of only the three

colours

colours red, yellow and blue might alfo appear from the rule which Newton himfelf has given us, for determining the colour of the compound which refults from the mixture of any primary colours, the quantity and quality of each being given.

The rule is this, the circumference of a circle is diftinguifhed into feven arches proportional to the feven mufical intervals in an octave, that is, proportional to the numbers 45, 27, 48, 60, 60, 40, 80: the firft part is to reprefent a red colour, the fecond orange, the third yellow, the fourth green, the fifth blue, the fixth indigo, and the feventh violet. Thefe are to be confidered to be all the colours of uncompounded light gradually paffing into one another, as they do when made by prifms, the circumference reprefenting the whole feries of colours from one end of the fun's coloured image to the other. Round the centers of gravity of thefe arches let circles proportional to the number of rays of each colour in the given mixture be defcribed. Find the common centre of gravity of all thefe circles, and if this common centre of gravity coincide with the centre of the circle, Newton fays that the compound will be white. Join therefore the centers of gravity of the blue and yellow circles, and from the centre of the red circle draw a right line through the centre of the principal circle; from the conftruction it will cut the line which joins the centers of the blue and yellow circles; if therefore the number of the blue and yellow rays be to each other inverfely as their diftances from the point where the line which joins their centers is cut by the line drawn from the centre

centre of the red circle; and if the number of red rays be to the ſum of the yellow and blue rays inverſely as the diſtances of the centre of the red circle, and the common centre of the yellow and blue from the centre of the principal circle, the common centre of gravity of the red, blue and yellow circles will coincide with the centre of the principal circle, and therefore the reſulting compound will be white.

But it is manifeſt that this conſtruction cannot be relied on, becauſe the quantities of the rays of any given colour in ſolar light, do not appear to be proportional to the ſpaces which they occupy in the rectilineal ſides of the ſpectrum. Thus it is known that the yellow making rays are predominant in ſolar light, yet the ſpace they occupy in the ſpectrum is to the ſpace occupied either by green or blue as four to five, and to the ſpace occupied by the violet only as three to five.

OBSERVATIONS *on the* THEORY *of* ELECTRIC ATTRACTION *and* REPULSION. *By the Rev.* GEORGE MILLER, *F. T. C. D.*

BEFORE that the theory of a ſingle electric fluid was propoſed, no difficulty occurred in the explanation of the attractions and repulſions obſerved to ariſe from electricity. If we admit that there are two diſtinct electric fluids, each of which ſtrongly attracts the other, but conſiſts of particles mutually repulſive; it becomes eaſy to account for the attraction ſubſiſting between bodies in different ſtates of electricity, and the repulſion between thoſe in the ſame. But when Dr. Franklin*, obſerving that a man, ſtanding upon a non-conductor, could not electrify himſelf, but that he could electrify another perſon alſo ſtanding upon a non-conductor, was induced to regard the operation of exciting electricity only as a transfer of one and the ſame fluid from one body to another; it was found to be difficult to reconcile to the new theory the mutual repulſion of bodies in that ſtate which is, according to this theory, denominated

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* Dr. Prieſtley's Hiſtory of Electricity, p. 161.

denominated negative electricity. Doctor Franklin* acknowledged that he could not assign a satisfactory reason for it; and Doctor Priestley † has proposed it, as one of the queries remaining to be solved for completing the science of electricity. Many attempts have been made to obviate this apparent objection to the simple theory of a single fluid; but the difficulty seems still to be as great as it was in the time of Franklin.

‡ ÆPINUS has applied a very elaborate system of mathematical reasoning to the solution of electrical phænomena, and has adopted as the basis of his theory, the same opinion which Franklin had entertained concerning the nature of the electric fluid; but he has combined with this opinion other principles so inadmissible, that his reasonings cannot be regarded as just explications of the phænomena. He has assumed, apparently without any other reason than its importance to his conclusions, that the particles of all other substances repel each other. His system must therefore be considered, not as a physical solution agreeable to the known laws of natural operations, but merely as an ingenious exercise of mathematical ability.

M. DE LUC, who rejected the solutions of Æpinus has endeavoured to supply the deficiency. § Having remarked that the divergence

* Dr. Priestley's History of Electricity, p. 165. † Ibid. p. 492.

‡ Journal de Physique, Dec. 1787. § Journal de Physique, Juin 1790.

divergence of the balls of an electrometer, included in the receiver of an air-pump, is continually diminiſhed during the progreſs of exhauſtion; he conſiders it as proved, that the cauſe of all electrical movements, whether of attraction or of repulſion, is the action of the air. This principle he applies in the following manner. When two bodies are in ſimilar ſtates of electricity, either poſitive or negative, they will conſpire to modify, either by giving or receiving the electric fluid, the ſtate of the intermediate air, whilſt that of the exterior air is only modified by either of them ſingly; and therefore the ſtate of the exterior air will differ more from that of the electrified bodies, than the ſtate of the intermediate air. In this caſe he contends that a repulſion muſt take place, becauſe each body muſt move towards that part of the ſurrounding medium, whoſe electrical ſtate is moſt different from its own. On the other hand, when bodies are in different ſtates of electricity, they will mutually counteract the changes, which they might ſeparately produce in the ſtate of the intermediate air; but each will operate on the exterior air without any compenſation. In this caſe the ſtate of the intermediate air will continue to differ from that of each body as much as at the firſt inſtant, whilſt the ſtate of the exterior air is ſeparately modified by each body according to its reſpective ſtate of electricity. The two bodies therefore, moving towards that part of the ſurrounding medium, whoſe electrical ſtate is moſt different from their own, will at the ſame time move towards each other.

This theory very ingeniously avoids the difficulty of explaining the case of electrical repulsion, by resolving it into an attraction towards the surrounding medium. It seems however to be liable to two objections. In the first place, instead of assuming unauthorized principles with the preceding theory, it omits the consideration of one whose existence seems to be ascertained by experiments. If a body be in either state of electricity, it will induce in an adjacent body the contrary state, until it shall have come within a certain distance. This property, which has been ascertained by various experiments, indicates a repulsive force subsisting between the portions of the electric fluid that belong to the adjacent bodies; and this theory makes no allowance for such a repulsion. The fundamental principle of it is merely a diffusion of the electric fluid, and is * thus stated by M. De Luc: "the electric matter tends towards all substances, and the more strongly in the same proportion in which they possess a smaller quantity." In the second place, it does not appear, when carefully considered, to afford any assistance towards the removal of the grand difficulty, the mutual repulsion of bodies negatively electrified. If two bodies negatively electrified be placed at a small distance, they will both, according to M. De Luc's explanation, receive the electric fluid from the intermediate air, which will consequently retain a smaller portion than the surrounding atmosphere From the law above-mentioned it should follow, that the redundant fluid of the exterior air should by diffusion

* "La loi suivante suffit seule: La matière électrique tend vers toutes les sub-"stances, d'autant plus fortement, qu'elles en possèdent moins." Journal de Physique, Juin 1790.

diffufion be communicated both to the bodies and to the intermediate fpace; but no reafon appears, which would induce us to fuppofe that the bodies themfelves fhould recede to a greater diftance. M. De Luc does indeed endeavour to prove that fuch a motion fhould take place, but by an experiment whofe folution contradicts his own theory. He fufpended by a filk thread a large, but light, metallic ball, and prefented it in a ftate of pofitive electricity to a body negatively electrified. The former was attracted towards the latter until it arrived at a certain diftance, at which it difcharged its electricity. Hence he concluded, in general, that when a body has more of the electric fluid than the neighbouring bodies, and is lefs difpofed to refift its own motion than to abandon the excefs of its electric matter, it will move towards that place which contains lefs of this matter. But in this experiment he confiders the two bodies as acting on each other at a diftance without any reference to the intermediate air.

Mr. Cavallo*, in the laft edition of his treatife on electricity, has obferved, that the mutual repulfion of two bodies negatively electrified is ftill fuppofed to contradict the theory of Franklin; and has therefore deemed it neceffary to obviate the objection by a very particular detail. For this purpofe he has premifed the following propofitions. Prop. 1. No electricity can appear on the furface of a body, or no body can be electrified either pofitively or negatively,

* Vol. III. p. 192.

negatively, unlefs the contrary electricity can take place on other bodies contiguous to it. Prop. 2. There is fomething on the furface of bodies, which prevents the fudden incorporation of the two electricities, viz. of that poffeffed by the electrified body with the contrary electricity poffeffed by the contiguous air, or other furrounding bodies. Prop. 3. Suppofing that every particle of a fluid has an attraction towards every particle of a folid; if the folid be left at liberty in a certain quantity of that fluid, it will be attracted towards the common centre of attraction of all the particles of the fluid. To this laft propofition he has fubjoined the two following corollaries: 1.* the fame thing muft happen, when the quantity of fluid is fmaller than the bulk of the body; 2. if the attraction of the particles of the fluid be exerted only towards the furface of the folid, the effect will be the fame when the body is of a regular fhape; but the difference will in any cafe be inconfiderable.

With regard to the folution founded upon thefe principles it muft be remarked, that it is not derived fimply from a confideration of the fuppofed nature of the electric fluid; but from a mixed ftatement of that nature and of properties affumed merely from experiments as matters of fact. The firft and fecond propofitions exprefs thofe properties, and, though the experiments to which the former refers, may be explained by afcribing the phænomena to the repulfive nature of the fluid, yet the latter is affumed "without

* Of this corollary Mr. Cavallo does not appear to make any diftinct application.

without any ſuch reference. "Without examining," ſays Mr. Cavallo, "the nature, the extent, and the laws of this property in bodies, "it will be ſufficient for the preſent purpoſe to obſerve, that the "fact is certainly ſo; for otherwiſe a body could not poſſibly be "electrified, or it would not remain electrified for a ſingle moment." From theſe principles thus aſſumed, Mr. Cavallo deduces the exiſtence of atmoſpheres of contrary electricity exiſting in the air contiguous to the bodies; and from the attractions which are thereby occaſioned he infers the apparent repulſion of the electrified bodies.

If theſe atmoſpheres be conceived to be formed by the repulſive nature of the fluid, ſome allowance ſhould be made for the mutual repulſion of the two redundant portions belonging to bodies poſitively electrified. This however ſeems to be neglected for the purpoſe of explaining the repulſion of bodies negatively electrified. But the difficulty ſeems to be only changed. If the negative atmoſphere adjacent to a body poſitively electrified be cauſed by the repulſion of the redundant fluid of the body, it will be neceſſary to ſhew that this repulſion is overpowered by the attraction ſubſiſting between that redundant fluid and the portion of air thus deprived of a part of its electric fluid.

But the reality of theſe atmoſpheres of contrary electricity may well be queſtioned. It ſeems to require, that we ſhould conceive a portion of air contiguous to each body to be permanently, during the mutual repulſion of the bodies, in a ſtate of electricity oppoſite to that of the bodies. But * it is aſcertained experimentally, that the air ſurrounding any electrified body acquires the ſame electricity which had been poſſeſſed by the body, and retains it even after the removal of the body. This muſt be ſuppoſed, agreeably to the known laws of electricity, to be communicated by the alternate attraction and repulſion of the adjacent particles of air. Each particle muſt be firſt attracted towards the body, and, when by contact it has acquired the electricity of the body, repelled from it. Inſtead therefore of a permanent ſtate of contrary electricity conſtituting theſe ſuppoſed atmoſpheres, each adjacent ſpace muſt be occupied by particles, ſome of which are attracted and others repelled. The time requiſite for thus reducing the electricity of the body to an equilibrium with that of the ſurrounding air, is ſufficient for explaining the continuance of the electricity of the bodies, without the aid of the ſecond propoſition; and the firſt propoſition is deduced only from a conſideration of bodies in a ſolid ſtate.

Possibly a more diſtinct application of a principle, already in ſome degree adopted both by Doctor Prieſtley and Mr. Cavallo, may remove all the difficulties of this inquiry. At leaſt I will hope, that it may lead to ſuch a conſideration of the queſtion, as may ſubject the merits of the theory itſelf to a fair and deciſive diſcuſſion.

* Cavallo's Complete Treatiſe on Electricity, Vol. I. p. 326.

discussion. This principle is saturation. * Doctor Priestley has explained the communication of the redundant fluid of a body positively electrified to another, a part of whose fluid had been previously expelled, by supposing that it was more strongly attracted by the other body, than by its own which had more than its natural share; and † Mr. Cavallo has in the same manner accounted for the mutual attraction of bodies in different states of electricity.

I applying this principle to the solution of electric phænomena three forces must be considered: 1st, the attraction subsisting between each body and its own portion of the electric fluid; 2dly, the attraction which may subsist between each body and the portion of fluid belonging to the other; and 3dly, the repulsion subsisting between the two portions of the electric fluid.

THAT the attraction subsisting between two bodies in opposite states of electricity may be explained, it is necessary to consider previously the case of two bodies in their natural or ordinary state. In this case the force subsisting between each body and its own portion of the electric fluid is not in a state of saturation, because it must be sufficiently strong to counterbalance the elasticity of the fluid. Each body is therefore still capable of being attracted by the fluid belonging to the other, and each portion of the fluid is also capable of such attraction. This force, if it should operate alone,

* History of Electricity, p. 253. † Vol. I. p. 109.

alone, would draw the bodies together; but the mutual repulſion of the two portions of the fluid tends to produce the oppoſite effect. The quieſcence of the bodies proves the equality of theſe forces.

If two bodies in oppoſite ſtates of electricity be brought together, the body poſitively electrified cannot be attracted towards the remaining electric fluid belonging to the other, becauſe this body may be conſidered as ſaturated with the fluid, and that portion of the fluid as ſaturated with ſolid matter. For the oppoſite reaſons an attraction will take place between the body negatively electrified and the fluid belonging to the former. It remains to be ſhewn, that this attractive force may exceed the mutual repulſion of the two portions of fluid. It muſt be obſerved, that the repulſion remains the ſame, becauſe the ſum of the two quantities of fluid is not altered; whereas the attraction is augmented by the unequal diſtribution of the fluid. The one body is charged with more fluid than that which its own attracting force is capable of retaining, and the redundant fluid will conſequently be ſtrongly impelled towards the other body, whoſe attractive power is at the ſame time increaſed by the deficiency of its own portion of fluid.

In the caſe of two bodies ſimilarly electrified the bodies may be either both poſitively, or both negatively electrified. When they are both poſitively electrified, they are both ſaturated with the electric fluid; and when they are both negatively electrified, both

both remaining portions of the electric fluid are reciprocally ſaturated with ſolid matter. In neither caſe therefore can any attraction take place between either body and the fluid belonging to the other. Conſequently, the repulſion exiſting between the two portions of the fluid muſt operate without reſiſtance, and the two bodies be repelled from each other.

Should this ſolution of electric attraction and repulſion be admitted, it will perhaps alſo remove the difficulty of magnetic repulſion. In this part of philoſophy it has been found difficult to explain the repulſion of the correſponding poles agreeably to the theory of a magnetic fluid. In every magnetical body the equilibrium of this fluid is ſuppoſed to be diſturbed, and one part of the body is conceived to be overcharged with the fluid, whilſt the other is undercharged. The difficulty was to explain the repulſion of the undercharged poles, as in electricity to explain the repulſion of bodies negatively electrified. Mr. Kirwan has indeed, in a Memoir contained in the Sixth Volume of the Tranſactions of the Academy, referred the phænomena of magnetiſm to cryſtallization; but his mention of the term *ſaturated* in that Memoir ſeems to imply, that he does not mean to exclude the ſuppoſition of a magnetic fluid. If this be adopted, the preceding ſolution may be applied to the phænomena of magnetiſm, in the ſame manner in which it has been already applied to thoſe of electricity.

The theory, according to which the preceding ſolution has been propoſed, ſuppoſes the electric fluid a *ſingle* fluid; but it is not neceſſary

ceſſary that it ſhould be conceived to be abſolutely *ſimple.* We know, for inſtance, that atmoſpheric air is a combination of at leaſt two diſtinct fluids; and yet explain the phænomena of the barometer, air-pump, and condenſer, as depending merely on its preſence or abſence, without any reference to the compoſition of its nature. In the ſame manner ſome electric phænomena may be juſtly explained by conſidering them as the effects of the different diſtribution of the ſame fluid; whilſt its phoſphoric ſmell, its power of changing blue vegetable colours to red, and its combuſtion may poſſibly be derived from its decompoſition.

A GENERAL DEMONSTRATION *of the* PROPERTY *of the* CIRCLE *difcovered by* MR. COTES *deduced from the* CIRCLE *only*. *By the Rev.* J. BRINKLEY, *A. M. ANDREWS' Profeffor of Aftronomy, and M. R. I. A.*

THE very elegant property of the circle difcovered by the celebrated Cotes has for its extenfive ufes always been juftly efteemed among mathematicians. The inventor left no demonftration of it; and although it immediately excited the attention of the moft eminent cultivators of the fcience, yet no general inveftigation has been hitherto given, if we except one derived from the hyperbola and impoffible expreffions, which was firft given by De Moivre, afterwards by Maclaurin and other authors. But the elegance of the theorem and the ftrictnefs of mathematical reafoning feem to require a very different kind of demonftration. The author of "Epiftola ad Amicum de inventis Cotefii," has indeed attempted a demonftration from the circle only; however it will readily appear on examination that it is not general, even conceding the demonftration

Read Nov. 4th 1797.

ſtration of the theorem for expreſſing the coſine of a multiple arc in terms of the coſine of the ſimple arc. No author before Dr. Waring has given a general demonſtration of this latter theorem, and conſequently all demonſtrations of Cotes's property by the circle alone previous to his, cannot be general ſo far as that theorem is concerned, and it will be found that in another circumſtance not leſs important they are all defective. Dr. Waring in his letter to Dr. Powell has from his theorem for the chords of the ſupplement of a multiple arc ſhewn the truth of Cotes's property in particular inſtances, and in his "Propr. Algebr. Curv. Prob. 32," has given the heads of a general ſolution. But it appears one of the ſteps there omitted is the only difficult part of the demonſtration after conceding the theorem for the coſine of a multiple arc.

The demonſtration here given is general and probably as direct and ſimple as the propoſition will admit of. The proof of the lemma which it was neceſſary to premiſe is much the moſt difficult part of the whole, and it is in that ſtep of the demonſtration where the Lemma is applied that all demonſtrations heretofore have been defective and only applicable to particular inſtances.

Lemma.

Lemma.

If m and n reprefent any affirmative whole numbers: then

$$\left.\begin{array}{l} +:\overline{n-1}.\,\overline{n-2}.\,\overline{n-3}.\ \text{- - - - -}\ \overline{n-m-1} \times 1 \\ -:\overline{n-2}.\,\overline{n-3}.\,\overline{n-4}.\ \text{- - - - -}\ \overline{n-m} \times m \\ +:\overline{n-3}.\,\overline{n-4}.\,\overline{n-5}.\ \text{- - - - -}\ \overline{n-m+1} \times m.\,\dfrac{\overline{m-1}}{2} \\ \text{- - - - - - - - -} \\ \text{- - - - - - -} \\ \text{- - - - - - -} \\ \pm:\overline{n-m+1}.\,\overline{n-m+2}\ \text{- - - -}\ \overline{n-2m-1} \times 1 \end{array}\right\} = 0.$$

Where 1, m, $m.\frac{\overline{m-1}}{2}$, &c. are formed by the law of the coefficients of a binomial raifed to the m^{th}. power. The number of terms $= m+1$.

Demonstration. Let the terms of the annexed table reprefent the different expreffions for the above quantity, according to the different values of m and n.

TABLE E.

Values of m	Values of n: 1	2	3	..	..	$n-2$	$n-1$	n
2	A	B	C	..	..	..	..	..
3	A′	B′	C′	..	..	..	..	..
.	..	..	..	..	..	..	..	..
.	..	..	..	..	..	..	..	..
$m-1$	..	..	..	..	..	H	K	
m.	..	..	..	..	..	H′	K′	L

Then

THEN 1. By substituting $n-1$ instead of n in the above expression

$$\text{we have } K' = \left\{\begin{array}{l} +\overline{n-2}.\ \overline{n-3}\ \ -\ -\ -\ \ \overline{n-m}\times 1 \\ -\overline{n-3}.\ \overline{n-4}\ \ -\ -\ \ \overline{n-m+1}\times m \\ +\overline{n-4}.\ \overline{n-5}\ \ -\ -\ -\ \ \overline{n-m+2}\times\dfrac{m.\overline{m-1}}{2} \\ \quad\&c.\ -\ \&c. \end{array}\right.$$

therefore

$$L'-K' = \overline{m-1}\times\left\{\begin{array}{l} +\overline{n-2}.\ \overline{n-3}\ \ -\ -\ \times 1 \\ -\overline{n-3}.\ \overline{n-4}\ \ -\ -\ \times m \\ +\overline{n-4}.\ \overline{n-5}\ \ -\ -\ \times m.\dfrac{\overline{m-1}}{2} \\ \quad\&c.\qquad\&c. \end{array}\right.$$

But by substituting $n-2$ and $n-1$ respectively instead of n, and $m-1$ instead of m we have

$$H = \left\{\begin{array}{l} +\overline{n-3}.\overline{n-4}\ -\times 1 \\ -\overline{n-4}.\overline{n-5}\ -\times m-1 \\ \quad\&c.\qquad\&c. \end{array}\right. \text{ and } K = \left\{\begin{array}{l} +\overline{n-2}.\overline{n-3}\ -\times 1 \\ -\overline{n-3}.\overline{n-4}\ -\times m-1 \\ \quad\&c.\quad\&c. \end{array}\right.$$

or $\overline{H+K}\times\overline{m-1} = L'-K'$ or $L' = \overline{m-1}\times\overline{H+K}+K'$.

2. Taking $m=n$ the expression becomes

$$\left.\begin{array}{l} +\overline{n-1}.\ \overline{n-2}\ \ -\ -\ -\ -\ -\ \ 2\times 1\times:1 \\ -\overline{n-2}.\ \overline{n-3}\ \ -\ -\ -\ -\ -\ \ 1\times 0\times:m \\ -\ -\ -\ -\ -\ -\ - \\ -\ -\ -\ -\ -\ -\ -\ - \\ \pm\ \ 0.\ \ 1\ \ -\ -\ -\ \ -\overline{n-3}.-\overline{n-2}\times:m \\ \mp -1.-2\ \ -\ -\ -\ \ -\overline{n-2}.-\overline{n-1}\times:1 \end{array}\right\}$$

WHICH

Which will be $= 0$, becauſe the firſt and laſt terms are the ſame with contrary ſigns, and becauſe o will be a factor in each of the other terms. That the firſt and laſt terms will have contrary ſigns appears from conſidering that in the laſt term there are $n-1$ negative factors, and conſequently when n is even the product will be negative and the ſign of the term itſelf will be poſitive becauſe $m+1$ $(n+1)$ is odd, and when n is odd the product will be poſitive and the ſign of the term negative.

3. Substituting for m, 2, the general term of the firſt horizontal rank $=$ $\left.\begin{array}{l} +\, n-1 \\ -\, \overline{n-2}\,.\, 2 \\ +\, n-3 \end{array}\right\} = 0.$

From theſe different concluſions we collect: 1ſt, that (becauſe $'L = \overline{m-1}\,.\,H + K + K'$) if each of the terms in any horizontal rank $= 0$ the terms in the rank below are equal: 2dly, therefore it follows becauſe a term in each rank $= 0$ (when $m = n$) that if each of the terms in any horizontal rank are equal to o, that the terms of the rank beneath are each $= 0$, and 3dly, becauſe thoſe of the firſt horizontal rank are each $= 0$, it follows therefore that each term of the table $= 0$. Q. E. D.

Theorem.

1. Let the circumference of a circle be divided into n equal parts OO', $O'O''$, &c. and from a point P in the radius OC or

 the

the radius produced without the circle draw PO, PO′, &c. then $\overset{n}{PC} - \overset{n}{OC} = PO \times PO' \times PO'' \times$ &c. when P is without the circle and $\overset{n}{OC} - \overset{n}{PC} = PO \times PO' \times PO'' \times$ &c. when P is within the circle.

2. Let the circumference be divided into $2n$ equal parts OS, SO′, O′S′, &c. then $\overset{n}{PC} + \overset{n}{OC} = PS \times PS' \times$ &c.

Demonstration.

1. Let OC be unity, $PC = x$; a, a', a'', &c. the cosines of o, OO′, OO″, &c.

Then will $PO^2 = x^2 + 1 - 2ax$
$PO'^2 = x^2 + 1 - 2a'x$
&c. &c.

or $PO^2 \times PO'^2 \times$ &c. $= \overline{x^2 + 1 - 2ax} \times \overline{x^2 + 1 - 2a'x} \times$ &c. $=$

$$\overline{x^2+1}\big|^{n} - \left.\begin{matrix} a \\ a' \\ a'' \\ \text{\&c.} \end{matrix}\right\} 2x.\overline{x^2+1}\big|^{n-1} + \left.\begin{matrix} aa' \\ aa'' \\ \text{\&c.} \end{matrix}\right\} 2^2x^2.\overline{x^2+1}\big|^{n-2} \quad - \quad -$$

$$\pm \overset{n}{2}\, aa'a'' \text{ \&c.} \times \overset{n}{x}.$$

Now if c be the cosine of any arc, the cosine of n times that arc will be $\overset{n-1}{2}\overset{n}{c} - n.\overset{n-3}{2}.\overset{n-2}{c} + \frac{n.\overline{n-3}}{1.\ 2}\overset{n-5}{2}\overset{n-4}{c} -$ &c. continued by successively diminishing the index of c by 2 until it becomes o or 1, and

and affixing to c^{n-u} the coefficient

$$\pm \frac{n.\overline{n-\frac{u}{2}-1}.\overline{n-\frac{u}{2}-2} \times \&c. \text{ to } \frac{u}{2} \text{ terms}}{1.\ 2.\ 3 - - \frac{u}{2}}\, 2^{n-u-1},$$ + when $\frac{u}{2}$ is even and — when odd. Hence becaufe unity is the cofine of o, P (Periphery), 2 P, 3 P, &c. it follows that if $2^{n-1}c^{n} - n.\,2^{n-3}c^{n-2} + \&c. = 1$ the different values of c will be a, a', a'', &c. the cofines of o, $\frac{P}{n}$, $\frac{2P}{n}$, &c. or that the roots of the equation

$$c^{n} - \frac{nc^{n-2}}{2^2} + \frac{n.\overline{n-3}\,c^{n-4}}{1.\ 2.\ 2^4} - - - \frac{1}{2^{n-1}} = 0$$ will be a, a', a'', &c.

Therefore by the nature of equations

$$a + a' + a'' + \&c. = 0$$

$$aa' + aa'' + \&c. = -\frac{n}{2^2}$$

$$aa'a'' + aa'a'' + \&c. = 0$$

$$aa'a''a''' + \&c. = +\frac{n.\overline{n-3}}{1.\ 2.\ 2^4}$$

&c. &c.

or generally the fum of the products of u values a, a', a'', &c. u being even $= \pm \frac{n.\overline{n-\frac{u}{2}-1}.\overline{n-\frac{u}{2}-2}. - (\text{to } \frac{u}{2} \text{ terms}}{1.\ 2.\ 3 - - \frac{u}{2}.\ 2^{u}}$ + when

$\frac{u}{2}$

$\frac{n}{2}$ is odd and — when even: alſo the product of all the values when n is odd $= \frac{1}{2^{\frac{n-1}{2}}}$ and when even $= \pm$

$$\frac{n.\overline{\frac{n}{2}-1}.\overline{\frac{n}{2}-2} \;-\;-\; (\text{to } \frac{n}{2} \text{ terms})}{1.2.3 \;-\;-\;-\; \frac{n}{2}.\,2^{n}} - \frac{1}{2^{n-1}}$$

Whence the value of $PO^2 \times PO'^2 \times$ &c. above found becomes

$$\overline{x^2+1}^{\,n} - nx^2.\,\overline{x^2+1}^{\,n-2} + \frac{n.\overline{n-3}}{1.2}x^4 \times \overline{x^2+1}^{\,n-4} \;-\; + 2x^{n}$$

or expanding theſe terms

$$\left\{\begin{array}{l}
\overline{x^2+1}^{\,n} = x^{2n} + nx^{2n-2} + \frac{n.\overline{n-1}}{1.2}x^{2n-4} + \frac{n.\overline{n-1}.\overline{n-2}}{1.2.3}x^{2n-6} + \;-\;-\;-\; + nx^2 + 1 \\
-nx^2 \times \overline{x^2+1}^{\,n-2} = -nx^{2n-2} - n.\overline{n-2}\,x^{2n-4} - \frac{n.\overline{n-2}.\overline{n-3}}{1.2}x^{2n-6} \;-\;-\;-\;-\;-\; - nx^2 \\
+\frac{n.\overline{n-3}}{1.2}x^4.\overline{x^2+1}^{\,n-4} = \frac{n.\overline{n-3}}{1.2}x^{2n-4} + \frac{n.\overline{n-3}.\overline{n-4}}{1.2}x^{2n-6} + \&c. \\
-\frac{n.\overline{n-4}.\overline{n-5}}{1.2.3}x^6.\overline{x^2+1}^{\,n-6} = -\frac{n.\overline{n-4}.\overline{n-5}}{1.2.3}x^{2n-6} \;\&c. \\
\&c. \qquad\qquad\qquad\qquad \&c. *
\end{array}\right\}$$

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* Mr. Simpſon in his Eſſays, page 115, has arrived by a different proceſs at a ſimilar concluſion, and aſſerts without any demonſtration that the co-efficients deſtroy each other. This however is the only difficult ſtep in the whole propoſition.

Hence collecting the co-efficients it readily appears by considering the general value of the ſum of u products ſtated above, that the coefficient of the term x^{2n-2m} the ſame as the coeff. of the term $x^{2m} =$

$$\left.\begin{array}{l}
+\dfrac{n.\,\overline{n-1}.\,\overline{n-2}\ \text{- - -}\ \overline{n-m-1}}{1.\ 2.\ 3\ \text{- - - -}\ m} \\[2ex]
-\dfrac{n.\,\overline{n-2}.\,\overline{n-3}\ \text{- - -}\ \overline{n-m}}{1.\ 1.\ 2\ \text{- - - -}\ \overline{m-1}} \\[2ex]
\text{- - - - - -} \\
\text{- - - - - -} \\[1ex]
\pm\dfrac{n.\,\overline{n-m}.\,\overline{n-m+1}\ \text{- -}\ \overline{n-2m}}{1.\ 2.\ 3\ \text{- -}\ \overline{m-1}\times 1.\ 2} \\[2ex]
\pm\dfrac{n.\,\overline{n-m+1}.\,\overline{n-m+2}\ \text{-}\ \overline{n-2m+1}}{1.\ 2\ 3\ \text{- - -}\ m}
\end{array}\right\}$$

or reducing theſe fractions to a common denominator the numerator becomes $n \times$ into the expreſſion in the Lemma which therefore $= 0$ hence

$PO^2 \times PO'^2 \times$ &c. $= x^{2n} - 2x^{n} + 1$ or $PO \times PO' \times$ &c. $= x^{n} \backsim 1$. Q. E. D.

2. Because $PO \times PS \times PO' \times PS' \times$ &c. $= x^{2n} \backsim 1$ and $PO \times PO' \times$ &c. $= x^{n} \backsim 1$ $\therefore PS \times PS' \times$ &c. $= x^{n} + 1$. Q. E. D.

ADDITIONAL OBSERVATIONS *on the* PROPORTION *of* REAL ACID *in the* THREE ANTIENT KNOWN MINERAL ACIDS, *and on the* INGREDIENTS *in various* NEUTRAL SALTS *and other* COMPOUNDS. *By* RICHARD KIRWAN, *Esq.* *L.L.D. F.R.S. and M.R.I.A.*

Read 16th Dec. 1797.

THE fundamental experiments on which the proportion of real acid in the three mineral acids antiently known, and alſo the proprortion of ingredients in many neutral ſalts, were determined, I have already ſet forth in a paper to be found in the IVth Vol. of the Tranſactions of this Academy. In that paper I have inſerted tables of the quantity of ſtandard acid exiſting in 100 parts of each of the acid liquors, of given ſpecific gravities, and alſo in each of the neutral ſalts therein mentioned; the mode of expreſſing the quantity of acid I had then adopted I ſince diſcovered to be very inconvenient, as in ſome of theſe neutral ſalts an acid ſtill ſtronger than the aſſumed ſtandard was found to exiſt. But I have there alſo noticed that the ſtrongeſt vitriolic acid now known, exiſted in *vitriolated tartarin*, the ſtrongeſt nitrous acid in *nitrated ſoda*, and the ſtrongeſt muriatic acid in *muriated tartarin*;

Acids of ſuch ſtrength I have therefore denominated *real*, as either containing no water or containing only as much as is neceſſary to their eſſential compoſition, as far as this is at preſent known. The method of transforming the expreſſion *ſtandard* into that of *real*, I have there alſo given p. 67, and by it have formed the table I here preſent; this latter expreſſion I therefore now employ in every caſe inſtead of that of *ſtandard*, together with the ſubſtitution of a more commodious expreſſion of the ſtrength of acids: The deſign of this paper is alſo to exhibit an illuſtration or amendment of ſeveral of the determinations contained in my laſt, which being for the moſt part ſingle, required confirmation by ſhewing their agreement with the experiments of ſeveral of the moſt eminent chymiſts made ſince the publication of mine, that is ſince the year 1791, with a few made nearly at the ſame time. In my former paper I compared my reſults with thoſe of Bergman and Wenzel, they being almoſt the only perſons who had made this ſubject the principal object of their enquiry, and had purſued it to a conſiderable extent; in each particular inſtance I have traced the reaſon of the difference of their reſults from my own when it was ſuch as to deſerve notice, and I ſhall not here repeat what I have there ſaid; but I cannot avoid again mentioning one general ſource of error attending the mode of inveſtigation adopted by both and yet noticed by neither, namely the loſs that many neutral ſalts undergo dering evaporation, a loſs whoſe diſcovery is of conſiderable importance, not only to the preſent inquiry, but alſo to the

the conduct of ſeveral manufactures, particularly to that of ſaltpetre, and hence noticed by Mr. Lavoſier, 15 An. Chy. 254. On this head however I hope the Academy will ſoon receive the fulleſt information, as our worthy member, Mr. Higgins, has at my requeſt undertaken to examine its reality and extent with reſpect to a conſiderable number of the moſt known among theſe ſalts.

THOUGH Bergman and Wenzel ſhould have conducted their experiments nearly in the ſame manner, as far as we can judge from the mode preſcribed by Mr. Bergman in his notes on Scheffer, publiſhed in 1779, yet his reſults differ conſiderably in many inſtances from thoſe of Wenzel, and appear to me far more faulty, the cauſe of which ſeems to me to be, that he has in moſt caſes departed from the method he had originally propoſed to follow, and ſuppoſed quantities of water of cryſtallization to exiſt in various ſubſtances without ſufficient reaſon, or at leaſt without aſſigning any ſuch. Thus he tells us that pellucid calcareous ſpars loſe only 34 per cent. of fixed air by ſolution in acids, whereas the daily experience of all chymiſts ſhews them to loſe from 43 to 44 per cent. but 11 of theſe he ſuppoſes to be water, becauſe by diſtillation he could not obtain more than 34 per cent. of fixed air, a method now well known to be defective, as from the poroſity of earthen retorts, the inefficacy of lutes, and the inſufficiency of the heat applicable to thoſe of glaſs, the true quantity of fixed air can never be thus obtained. Mr. Cavendiſh could obtain from 311 grains

 of

of Carrara marble only 1 grain of water *, and Florian de Bellevue, who lately has particularly enquired into this matter, ſays, marbles contain no water, or ſcarce any; and it is of the granularly cryſtallized that he ſpeaks ‡. Dr. Watſon alſo makes the ſame remark.

To tartar vitriolate Bergman has alſo aſſigned 8 grains of water of cryſtallization, whereas when dried even in a heat of 70 degrees only, except it contains an exceſs of acid, it retains not even 1 per cent. of water. To nitre he aſſigns even 18 per cent. a quantity ſo great that he can ſcarce be ſuppoſed to have meant water of cryſtallization. Lavoſier, who by profeſſion muſt have been well acquainted with a property ſo obvious, tells us on the contrary that it contains little or none, 15 An. Chy. 256. Mr. Keir allows it when not well dried about 2,5 per cent. Wenzel, on the other hand, took but little notice of the water of cryſtallization, and his miſtakes are not ſo conſiderable, moſt of them independently of the ſource of error already mentioned originated from the ſuppoſition of a fictitious ſubſtance which he called *Cauſticum*, the unheeded decompoſition of nitre when ſtrongly ignited, and the ſuppoſition that acids, when the compounds into which they enter are heated to redneſs, either retain no water or at leaſt a conſtant and not a variable quantity of it; this is indeed an error inherent in the method purſued by him, Bergman

* Phil. Tranſ. 1766, p. 167.

‡ 41 Roz. 94.

Bergman and myſelf in my firſt eſſays. But he alſo followed another method, which preſerved him from many miſtakes, which was to eſtimate the quantity of the ſtrongeſt acid in a given quantity of vitriolic acid, v: z: 240 grains by the quantity of it retained during ignition in tartar of vitriolate, and in 240 grains muriatic acid by the quantity retained in muriated tartarin, for in effect theſe acids, as I have found, contain leaſt water in theſe compounds; this advantage however he ſometimes loſt by the decompoſitions ariſing from ignition, particularly in his experiments on metallic ſubſtances.

To render this paper ſtill more uſeful, I ſhall lay before the Academy ſome important determinations of the proportion of ingredients in compounds of which I had not myſelf treated, and are either not generally known, or ſcattered in divers treatiſes not eaſily collected, to moſt of which however I have added my own experiments.

When alkalies or earths combined with fixed air are diſſolved in acids, though far the greater part of the fixed air is expelled during the ſolution, yet ſome portion of it is often retained, and may in ſome degree alter the ſp. grav. of the ſolution; this circumſtance I did not recollect till lately; it was firſt noticed by Mr. Cavendiſh, Phil. Tranſ. 1766, p. 172, and afterwards by Bergman in his notes on Scheffer, §. 51, but more explicitly by Scheele, Chy. An. 1786,

p. 13,

p. 13, and by Butini on Magnefia, p. 149. As to the ufe refulting from refearches of this nature it were fuperfluous to attempt to prove it at this day, the recourfe which the moft eminent analyfts have been obliged to have to it in particular inftances, as will prefently appear, fufficiently evinces it. "Inquiries of this kind (fays "Mr. Fourcroy) are more difficult and delicate than thofe which "have hitherto been made on falts; whatever requires a precife "knowledge of quantities and proportions, prefents difficulties fo "great as often to appear infurmountable, yet without this know-"ledge no progrefs can now be made in chymiftry," 10 An. Chy. 325; and according to Bergman, "Ufus cognitæ proportionis prin-"cipiorum ingredientium egregius eft et multifarius." 1 Bergm. 137. chap. 1. § 1.

T A B L E

OF THE

QUANTITY OF REAL ACID

In 100 Parts of Vitriolic, Nitrous and Marine Acid Liquors of different Densities, at the Temperature of 60°.

In *Vitriolic Acid* at different Densities, at the Temperature of 60°.				In *Nitrous Acid* at different Densities, at the Temperature of 60°.				In *Marine Acid* of different Densities, at the Temperature of 60°.	
100 Parts Sp. Gravity.	Real Acid.	100 Parts Sp. Gravity.	Real Acid.	100 Parts Sp. Gravity.	Real Acid.	100 Parts Sp. Gravity.	Real Acid.	100 Parts Sp. Gravity.	Real Acid.
2,0000	89,29	1,4666	44,64	1,5543	73,54	1,3364	41,91	1,196	25,28
1,9859	88,39	1,4427	43,75	1,5295	69,86	1,3315	41,18	1,191	24,76
1,9719	87,50	1,4189	42,86+	1,5183	69,12	1,3264	40,44	1,187	24,25
1,9579	86,61	1,4099	41,96	1,5070	68,39	1,3212	39,71	1,183	23,73
1,9439	85,71	1,4010	41, 7	1,4957	67,65	1,3160	38,97	1,179	23,22
1,9299	84,82	1,3875	40,18	1,4844	66,92	1,3108	38,34	1,175	22,70
1,9163	83,93	1,3708*	39,28	1,4731	66,18	1,3056	37,50	1,171	22,18
1,9041	83,04+	1,3663	38,39	1,4719	65,45	1,3004	36,77	1,167	21,67
1,8914	82,14	1,3586	37,50	1,4707	64,71	1,2911	36,03	1,163	21,15
1,8787	81,25	1,3473	36,60	1,4695	63,98+	1,2812	35,30+	1,159	20,64
1,8660	80,36	1,3360	35,71	1,4683	63,24	1,2795	34,56	1,155	20,12
1,8542	79,46	1,3254	34,82	1,4671	62,51	1,2779	33,82	1,151	19,60
1,8424	78,57	1,3149	33,93	1,4640	61,77	1,2687	33,09	1,147	19,09
1,8306	77,68	1,3102	33,03	1,4611	61,03	1,2586	32,35	1,1414	18,57
1,8188	76,79+	1,3056	32,14	1,4582	60,30	1,2500	31,62	1,1396	18,06
1,8070	75,89	1,2951	31,25	1,4553	59,56	1,2464	30,88	1,1358	17,54
1,7959	75,—	1,2847	30,35	1,4524	58,83	1,2419	30,15	1,1320	17,02
1,7849	74,11	1,2757	29,46	1,4471	58,09	1,2374	29,41	1,1282	16,51
1,7738	73,22	1,2668	28,57	1,4422	57,36	1,2291	28,68	1,1244	15,99
1,7629	72,32	1,2589	27,68+	1,4373	56,62	1,2209	27,94	1,1206	15,48
1,7519	71,43	1,2510	26,78	1,4324	55,89	1,2180	27,21+	1,1168	14,96
1,7416	70,54+	1,2415	25,89	1,4275	55,15	1,2152	26,47	1,1120	14,44
1,7312	69,64	1,2320	25,—	1,4222	54,12+	1,2033	25,74+	1,1078	13,93
1,7208	68,75	1,2210	24,10	1,4171	53,68	1,2015	25,00	1,1036	13,41
1,7104	67,86	1,2101	23,21	1,4120	52,94	1,1963	24,26	1,0984	12,90
1,7000	66,96	1,2009	22,32	1,4069	52,21	1,1911	23,53	1,0942	12,38
1,6899	66,07	1,1918	21,43+	1,4018	51,47	1,1845	22,79	1,0910	11,86
1,6800	65,18	1,1836	20,53	1,3975	50,74	1,1779	22,06	1,0868	11,35
1,6701	64,28	1,1746	19,64	1,3925	50,00	1,1704	21,32	1,0826	10,83
1,6602	63,39	1,1678	18,75	1,3875	49,27	1,1639	20,59	1,0784	10,32
1,6503	62,50	1,1614	17,85	1,3825	48,53	1,1581	19,85	1,0742	9,80
1,6407	61,61	1,1531	16,96	1,3775	47,80	1,1524	19,12	1,0630	8,25
1,6312	60,71	1,1398	16,07	1,3721	47,06	1,1421	18,48	1,0345	5,16
1,6217	59,82	1,1309	15,18+	1,3671	46,33	1,1319	17,65+	1,0169	2,58
1,6122	58,93	1,1208	14,28	1,3621	45,59	1,1284	16,91		
1,6027	58,03	1,1129	13,39	1,3571	44,86+	1,1241	16,17		
1,5932	57,14	1,1011	12,50	1,3521	44,12	1,1165	15,44		
1,5840	56,25	1,0955	11,60	1,3468	43,38	1,1111	14,70		
1,5748	55,36+	1,0896	10,71	1,3417	42,65	1,1040	13,27		
1,5656	54,46	1,0833	9,80						
1,5564	53,57	1,0780	8,93+						
1,5473	52,68	1,0725	8,03						
1,5385	51,78	1,0666	7,14						
1,5292	50,89	1,0610	6,25						
1,5202	50,00	1,0555	5,35						
1,5112	49,11+	1,0492	4,46						
1,5022	48,21	1,0450	3,57						
1,4933	47,32	1,0396	2,67						
1,4844	46,43	1,0343	1,78						
1,4755	45,53								

* The Sp. Gravity was 1,3741 in the former Table.

The Numbers above the Lines drawn acroſs the Tables of vitriolic and nitrous Acids were found by Experiments; thoſe under the Lines only by Analogy.

The Affinity of vitriolic Acid to Water decreaſes in the Ratio of the Square of the Quantity of Water united to it. 23 Ann. Chy. 196 and 197.

And ſo I believe it does to all other Subſtances; it is the *mean* Affinity that is commonly given.

Note. The ſtandard Quantities of *Vitriolic* Acid were reduced to *Real* by multiplying them into 0,8929. of the *Nitrous*, by multiplying them into 0,7354, and the *Marine* by multiplying them into 0,516, for the Reaſons mentioned in my laſt Paper.

Of the Alteration arising from Difference of Temperature.

To difcover this alteration by experiment in each individual inftance would be an endlefs tafk, hence I have felected only 3 cafes with refpect to the vitriolic acid, and 2 of the nitrous, and obferved the changes in each at every 5 degrees above 60° unto temperature 70°, and at every 5 degrees below 60° unto temperature 50° nearly, thefe being the temperatures at which experiments are ufually made.

Of the Vitriolic Acid.

Vitriolic acid	1,8360	at	temperature	60°
Becomes	1,8292	at	-	70°
	1,8317	at	-	65°
	1,8382	at	-	55°
	1,8403	at	-	50°
	1,8403	at	-	49°

hence we fee that vitriolic acid, whofe denfity at 60° is 1,8360, lofes by *afcending* and gains by *defcending* 0,00c68 for every degree of temperature between 60° and 70° and 0,00043 nearly by each degree between 60° and 49°.

 Again

Again, vitriolic acid	1,7005	at	-	60°
Becomes	1,6969	at	-	70°
	1,6983	at	-	65°
	1,7037	at	-	55°
	1,7062	at	-	50°

hence vitriolic acid, which at 60° is 1,7005 gains or loſes 0,00036 nearly for every degree between 60° and 70°, and 0,00051 by every degree between 60° and 50°.

Laſtly, vitriolic acid	1,3888	at	-	60°
Becomes	1,3845	at	-	70°
	1,3866	at	-	65°
	1,3898	at	-	55°
	1,3926	at	-	49°

hence vitriolic acid, which at 60° is 1,3888 gains or loſes 0,00043 nearly by every degree between 60° and 70°, and 0,00034, nearly by every degree between 49° and 60°, between 49° and 50°, I perceived no difference.

Of the Alteration of Denſity from Difference of Temperature in Nitrous Acid.

Nitrous acid, which was	1,4279	at	-	60°
Became - -	1,4178	at	-	70°
	1,4225	at	-	65°

1,4304

1,4304 at - 55°
1,4336 at - 50°
1,4357 at - 45°

hence nitrous acid, which at 60° is 1,4279, gains or loſes 0,00101 nearly by every degree between 60° and 70°; and 0,00052 by every degree between 45° and 60°.

I formerly found that the ſtrongeſt *ſpirit of nitre* is moſt expanded by heat or contracted by cold.

Also, that nitrous acid, whoſe ſp. grav. at 34° was 1,4750, was expanded by heat as follows:

1,4750 at 34 } then it gains or loſes 0,0097 by 15°
became 1,4653 at 49 } between 34° and 49° incluſively.

Again I found that colourleſs nitrous acid whoſe ſp. grav. was

1,4650 at - 30°
became - 1,4587 at - 46°
1,4302 at - 86°

hence by the firſt 16° from 30° to 46° it gained 0,0063, and by 40°, that is from 46 to 86°, it gained 0,0285.

Again, nitrous acid whoſe denſity was

1,2363 at - 60°
became - 1,2320 at - 70°
1,2342 at - 65°

 1,2384

1,2384	at	-	55°
1,2406	at	-	50°
1,2417	at	-	45°

hence nitrous acid, which at 60° is 1,2363, gains or loſes by every degree between 60° and 70°, 0,00043 and 0,00036 by every degree between 60° and 45°; and we may aſſume 0,0005 as the variation incident to every degree between 60° and 70° in nitrous acid, whoſe denſity at 60° is between 1,3 and 1,4 and 0,0004 for the variation between 44° and 60°

Of Marine Acid.

I formerly found that this acid of the denſity 1,196 at 33° became of the denſity 1,1820 at 66°, the alterations of acids of lower ſp. grav. I have not examined, but I found that in general its dilatability is greater than that of nitrous acid of the ſame denſity.

OF THE USE OF THESE TABLES.

PROBLEM 1ſt.

AN *extratabular* ſpecific gravity being given, but intermediate between ſome of thoſe in the table, to *find the quantity of real acid* in 100 parts of ſuch acid liquor.

1ſt.

1ſt. FIND the difference betwixt the next higher and lower tabular denſities = D, and alſo the difference betwixt their acid contents = D′.

2d. FIND the difference betwixt the *extratabular* ſp. gravity and the next upper or next lower, which ever it is neareſt to = *d*, and let the difference betwixt its acid contents (or quantity of real acid) and thoſe of the next upper or lower = *d′*, which is the quantity ſought; then as D. D′ : : *d*. *d′* then $d' = \frac{D'd}{D}$ conſequently *d′* added to the acid contents of the lower tabular ſp. grav. or ſubſtracted from the upper, *is the quantity ſought.*

Note. IN general when *d*, that is the difference between the extratabular ſp. grav. and any tabular ſp. grav. does not exceed $\frac{9}{1000}$ it is inſenſible, and the acid contents of the lower or upper, which ever is neareſt, may be aſcribed to it.

PROBLEM 2d.

THE *quantity of real acid* in 100 parts of an acid liquor being given but *extratabular*, being intermediate between ſome of the quantities in the tables, *to find the ſp. grav. of* ſuch acid liquor.

FIND D, D′ and *d′* as in the foregoing problem. then $d = \frac{D\,d'}{D'}$ then *d* added to the lower tabular ſp. grav. or ſubſtracted from the upper, gives the ſp. grav. ſought.

BUT

But with regard to the *marine acid* its ſp. grav. is to be inveſtigated according to the ordinary mathematical rules.

Problem 3d.

To find how much water muſt be added to 100 parts of an acid liquor of a given ſp. grav. to bring it down to another lower given ſp. grav.

1ſt. Find by the table the quantities of acid and water in 100 parts of each of the acid liquors reſpectively, each being ſuppoſed to be in the table, let the quantity of water in the denſer be W, and the quantity of acid = A, let the quantity of water in the leſs denſe = w, and the quantity of acid = a, and the quantity of water to be added to 100 parts of the denſer = m

then $W + m$ muſt be to A as w to a

And $W a + a m = A w$. And $a m = A w - W a$.

And $m = \frac{A w - W a}{a}$

Problem 4th.

Given weights of 2 *or more acid liquors* of different ſp. gravities being mixed, to find the quantity of real acid in 100 parts of the mixt liquor and its ſp. grav.

Find

Find the ſum of the quantities of real acid in 100 parts of the mixture, then find the reſulting ſp. grav. by the 2d problem, if the given ſp. gravities be extratabular, the operation muſt be more tedious, as the acid contents of each muſt be found.

Problem 5th.

The quantity of an acid liquor requiſite to ſaturate 100 *parts* of *any baſis* being found, to find the ſp. grav. of that acid liquor.

1ſt. Find by the 4th table the quantity of real acid requiſite to ſaturate 100 *parts* of the given baſis, it is then plain that the *given* quantity of acid liquor contains the requiſite quantity of *real acid*, ſince it is ſuppoſed to ſaturate 100 parts of the baſis and hence we may ſee how much 100 parts of ſuch acid liquor contains of *real acid*, and if this laſt found quantity be in the table, its ſp. grav. will be ſeen, but if extratabular, its ſp. grav. muſt be ſought by the 2d problem.

Problem 6th.

The quantity of real acid requiſite to ſaturate 100 parts of any baſis being known, to find how much of one acid liquor of any given ſp. grav. is requiſite to ſaturate that, and conſequently any other given quantity of ſuch baſis.

If

If the given ſp. grav. of the acid liquor be *tabular* the quantity of real acid in 100 parts of it is apparent, and conſequently the quantity of ſuch acid liquor containing the required quantity of real acid, is eaſily found by the rule of proportion. But if the given ſp. grav. is *extratabular* the quantity of real acid in 100 parts of the acid liquor muſt be ſought by the firſt problem.

Problem 7th.

The quantity of real acid, in a given quantity of an acid liquor being known, and alſo the quantity requiſite to ſaturate 100 parts of any given baſis. To diſcover the quantity of ſuch baſis contained in any ſolution, or in any powder, by which the given quantity of acid liquor is ſaturated.

If the baſis be ſingle (that is unmixed with any other baſis to which the acid may unite) or combined only with fixed air the ſolution is eaſy, but if the given baſes be mixed with other baſes combinable with the ſame acid, the ſolution is more complex and varies according to the variety of caſes.

Problem 8th.

To find how much of an acid liquor of *one ſort* will hold as much *real acid*, as is held by a given weight of an acid liquor of *another ſort* whoſe ſp. grav. is alſo given:—For inſtance, how much vitriolic acid will contain the ſame quantity of real acid as is contained in 100 grains nitric acid whoſe ſp. grav. is 1,3925.

1ſt.

1ft. Firft find by the table the quantity of real acid contained in the given quantity of the fecond acid, whofe fp. gr. is given, or if not in the table it muft be found by Problem 1ft.

2d. It is apparent that the quantity of the firft acid liquor muft vary with its fp. gr. thus, in the inftance given, as 100 parts nitrous acid of the fp. grav. 1392 contains 50 parts real nitrous acid, fo 100 parts vitriolic acid whofe fp. grav. is 1,5202 contains by the table the fame quantity of real acid, *v. z.* 50 parts, but of the vitriolic acid whofe fp. grav. is 1,800 only 64 parts are requifite to contain 50 parts of real acid, whereas 200 grains are requifite of the vitriolic acid whofe fp. grav. is 1,2320.

Note, The folution of this problem may hereafter be found of ufe in comparing the quantities and affinities of oxygen in different acids.

Problem 9th.

To find the fp. grav. of fuch vitriolic acid as that 100 parts of it fhall contain the fame quantity of real acid as 100 parts of the nitrous.

This can be found only by *infpection* on confulting the tables; an example has been feen in the laft problem, fo alfo 100 parts vitriolic acid 1,3102 contain the fame quantity of real acid as 100 parts nitrous acid whofe fp. gr. is 1,2687. And 100 grains vitriolic acid whofe fp. gr. is 1,1746 contains the ſame quantity of real acid as 100 grains ſp. ſalt whoſe ſp. gr. is 1,159.

AND 100 grains nitrous acid 1,1963 contains the ſame quantity of real acid as 100 grains ſpirit of ſalt whoſe ſp. grav. is 1,187.

HENCE it ſhould ſeem that the ſp. grav. of the *real marine acid* is ſmaller than that of the *real nitrous*, and that of the real nitrous ſmaller than that of the *real vitriolic*, ſince when the weight of each acid, and alſo the weight of real acid in each is equal, the vitriolic acid is ſpecifically heavier than the nitrous, and the nitrous than the marine, but this perhaps may ariſe from penetration.

PROBLEM 10.

To find how much of a neutral ſalt of one ſort holds as much real acid or baſis as a given weight of the *ſame* neutral ſalt in another ſtate, or as a given weight of *another* ſalt in any given ſtate.

THESE queſtions are reſolved by the 4th and 5th tables, thus if it be aſked, how much nitre contains as much acid as 20 grains of vitriolated tartarin? By the 4th table I ſee that 221,48 parts of vitriolated tartarin and 227,22 parts nitre contain equal quantities of acid ſince both contain 100 parts, then as 221,48. 227,22 : : 20.20,5.

AGAIN,

Again, How much deficcated foda will hold as much alkali as 30 parts cryftallized foda? In the 5th table I fee that 541,1 parts of the cryftallized hold as much alkali as 227,4 parts of the deficcated, then as 541,1 . 227,4 : : 30 . 12,6.

PROBLEM 11th.

How much of a given bafis will be requifite to faturate the acid contained in a given quantity of a given neutral falt, thus how much deficcated foda will be requifite to faturate the acid contained in 50 parts cryftallized Epfom?

By the 4th table I fee that 100 parts real vitriolic acid are contained in 340 parts cryftallized Epfom. Then if 340.100 : : 50, 14,7, then by the 3d table I fee that 100 grains of real vitriolic acid faturate 78,32 of foda. Confequently if 100 faturate 78,32 : : 14,7 would faturate 11,51 of foda.

Laftly, In the 6th table I find that 100 grains deficcated foda contains 60 of foda. Then if 100 : 60 : : x. 11,51, then $x = 19,1$ parts deficcated foda. Then 19,1 parts deficcated foda will faturate the acid contained in 50 parts cryftallized Epfom.

Note 1ft. This problem is of ufe in determining the quantity of any precipitating fubftances to be employed in decompofitions, operated either by a fingle or double affinity. But in moft cafes *more* of the precipitant muft be employed than the exact quantity neceffary for faturation, and particularly when decompofitions are attempted in the dry way, as otherwife a complete contact with the fubftance to be decompofed will not be attained, or if volatile it may be fublimed before the decompofition takes place.

PROBLEM 12th.

SOME analyfts have denoted the ftrength of their acids by expreffing the quantities of each neceffary to faturate a *certain quantity* of alkaline liquor (and fometimes of another bafis) without even telling whether the alkali was mild or cauftic, or the quantity of it contained in the alkaline liquor. This problem is confequently indeterminate. However a method of giving fome folutions of it may be underftood from the following example; and circumftances will generally fhew whether the application to particular cafes be juft.

Link tells us that 240 grains of a vitriolic acid which he employed, faturated 6,5 times its weight of tartarin (he muft mean in a liquid ftate, as no vitriolic acid will faturate fix times its weight of real alkali) and that 240 grains of the nitrous acid he employed faturated 2,5 times its weight of the fame alkali. *Quere* the fpec. gravity of both acids?

1ft IT is plain, that fince 240 grs. of the nitrous acid faturated 2,5 times its weight of the alkali, 624 grs. of that acid would faturate

turate 6,5 times its weight of the alkali; and ſince 624 grs. of the nitrous acid would ſaturate as much alkali as 240 of the vitriolic acid, then 260 grs. of it would ſaturate as much alkali as 100 grs. of the vitriolic acid could ſaturate. Therefore ſuppoſing 100 of the vitriolic acid to contain 75 of real acid, ſince more real nitrous acid is required to ſaturate a given quantity of tartarin than of vitriolic acid, in the inverſe ratio of 1214 to 1177 (as appears by the third table,) then denoting the quantity of real nitrous acid in 260 grs. of the nitrous liquor, by x we have the following equation as 1214. 1177 : : x 75. and $x = 77{,}55$. Then 260 grs. of the nitrous acid contain 77,55 of real nitrous acid, conſequently 100 grs. of it contained 29,82 real acid. And therefore its ſp. grav. was nearly 1,234, and that of the vitriolic about 1,800.——The quantity of alkali in the alkaline liquor might alſo on this ſuppoſition be determined.

So if it be required to know how much common ſalt is requiſite to decompoſe a ſolution of nitrated ſilver containing 176,25 grs. of ſilver:

1ſt. I find by the 6th table that 75 grs. ſilver take 16,54 of marine acid, conſequently 176,25 gr. ſilver take up 38,87.

2d. By the 4th table, I find that 100 grs. muriatic acid are contained in 257,2 of common ſalt, conſequently 38,87 are contained in 99,973, that is 100 grs. common ſalt, then 100 grs. of it are neceſſary to precipitate the ſilver.

ILLUSTRATION

ILLUSTRATION OF THE TABLES.

Few chymifts have made experiments appofite to my prefent purpofe, and thofe that have made any relative to it, have generally neglected marking the temperature, and thus prevented an exact comparifon of the refults they obtained with thofe that fhould be expected from the quantities of real acid and water fet forth in my tables.

The moft accurate of thefe experimenters was Hahn, who has inftituted a confiderable number, of which an account is given in his Differtation *De Efficacia Mixtionis in Mutandis Corporum Voluminibus*; of thefe I fhall felect a few, which I think by their coincidence with the refults to be obtained, calculating from my tables, furnifh a full proof of their accuracy, at leaft to as great a degree as can be expected in fubjects of this nature.

Of the Table of Vitriolic Acid.

1ft Experiment.

Hahn, to 800 grs. of vitriolic acid whofe fp. grav. was 1,8489 at the temperature of 44°, added 400 grs. of water in a veffel that confined the vapours, and when the mixture was cooled down to the temperature of the air he found its fp. grav. 1,545.—p. 48 and 49.

Application.

Application.

Vitriolic acid of this denſity loſes, as we have ſeen, ,00043 in denſity, by each degree between 44° and 60°; hence its ſp. grav. at 60° ſhould be 1,8489 —,00043 × 16 = 0,0068 = 1,8421, which differs inſenſibly from the next lower tabular ſp. grav. 1,8424, and therefore this may be taken for it.

The quantity of real acid in 100 grs. of the acid liquor, whoſe ſp. grav. is 1,8424 amounts to 78,57 per cent. *per table*, then 800 grs. of that acid liquor contains 78,57 × 8 = 628,56 of real acid, and conſequently the 1200 gr. of the mixture contain that quantity of real acid, and therefore 100 grs. of the mixed liquor contain 52,38 of real acid, which we ſee differs but little from the *tabular real acid*, 52,68 which indicates the ſp. grav. to be 1,5473, and the difference between this and the ſp. grav. found by Hahn is inconſiderable.

However, to obtain a cloſer approximation, and to give an example of the mode of ſolving the 2d problem, I ſhall deduce the ſp. grav. from the rules laid down for the ſolution of that problem.

1ſt. The next higher ſp. grav. is 1,5473, and the next lower is 1,5385, and the difference between them is 0,0088 = D.
Their acid contents are 52,68 and 51,78, and their difference 0,9 = D′.

2d.

2d. THE difference betwixt the given extratabular acid contents, 52,38, and the next lower tabular acid contents 51,78 is $0,6 = d'$ then d, the quantity to be added to the lower ſp. grav. is found by the formula $d = \frac{D d'}{D'} = \frac{0{,}0088 \times 0{,}6}{0{,}9} = \frac{0{,}0528}{0{,}9} = 0{,}0058$

$$\begin{array}{rl} & 1{,}5385 \\ \text{Now} + & 0{,}0058 \\ \hline = & 1{,}5443 \end{array}$$

and that found by Hahn is 1,545.

THIS it is true would be the ſp. grav. at 60°, and after 3 days reſt (the time I allowed for the penetration of the mixtures mentioned in my tables,) and it does not appear what the temperature of Hahn's mixture was when he took its ſp. grav. if it was 44° (the temperature of his oil of vitriol) it is poſſible that the cold without exact penetration might produce an effect equivalent to that which time would produce by penetration.

2d Experiment.

IN this Hahn added 400 grs. of water to the 1200 grs. of the foregoing mixture, and conſequently the new mixture weighed 1600 grs. and contained the ſame quantity of real acid as the foregoing, that is 628,56 grs. he found its ſp. grav. when cold to be 1,38,40.

Application.

Application.

SINCE 1600 grs. of the mixture contained 628,56 real acid, 100 grs. of it ſhould contain 39,28; now this quantity of real acid is exactly in the table, and correſponds with the ſp. grav. 1,3768. Then the difference between Hahn's reſult and that of my determinations is $\frac{72}{10000}$.

3d Experiment.

To the 1600 grs. of the laſt mixture Hahn added 800 of water, and when the whole was cooled down to the temperature of the air he found the ſp. grav. of the mixture 1,2439. Ibid. p. 50.

Application.

THIS mixture weighed 2400 grs. and contained the ſame quantity of real acid as the laſt, namely, 628,56 grs. conſequently 100 grs. of it contained 26,19; this quantity of real acid is extratabular; the neareſt tabular quantity of real acid is 25,89, which correſponds with the ſp. grav. 1,2415; though this ſeems ſufficiently near to Hahn's reſult, yet I have found it more exactly by the 2d problem. Here $D = 0{,}0095$ and $D' = 0{,}89$ and $d' = 0{,}3$, then by the formula $d = \frac{D\,d'}{D'}$ we have $\frac{0{,}0095 \times 0{,}3}{0{,}89} = 0{,}0032$, and the lower ſp. grav. $1{,}2415 + 0{,}0032 = 1{,}2448$, which differs from Hahn's reſult by only $\frac{9}{10000}$.

THE 3 firſt experiments of Hahn not perfectly agreeing with each other, and not having been made with equal accuracy, I omit.

Morveau's Experiment on the Quantity of Real Acid in Vitriolic Acid, whoſe Sp. Grav. was 1,841. 1 *Encyclop.* 592.

HE took 58 grs. vitriolic acid, whoſe ſp. grav. at 8°,5 Reaum. (= 51° Fahr.) was 1,841, and poured into it a ſolution of acetited barytes until a precipitate ceaſed to appear. The precipitate waſhed and dried (by ignition as it would ſeem by what he adds in the 2d column of the above page) weighed 110,3 grs.

Application.

VITRIOLIC acid, whoſe ſp. grav. at 51° of Fahr. is 1,841, would have its ſp. grav. lowered to 1,838 at 60° of Fahr. the degree for which my tables were formed, as I have ſhewn in my remarks on the alteration by temperature.

Now the ſp. grav. 1,838 is intermediate between the tabular denſities 1,8306 and 1,8424, but nearer to this; then by the firſt problem its acid contents will be found to be 78,24 per cent. then if 100 grs. of vitriolic acid of this ſp. grav. contain 78,24 per cent. real acid, 58 ſhould contain 45,37 of real acid. But 110,3 grs. of ignited barytes contain 36,76 real acid, allowing 100 grs. of ſuch barytes to contain 33,33 per cent. the difference then between

tween Mr. Morveau's reſult and that of my calculation is 8 61 grs.; the reaſon, however, is obvious; Morveau employed acetited barytes, this acid rendered part of the acid ſulphureous, as is well known; the ſulphureous acid does not decompoſe acetited barytes per Bergman's table, his other experiments on the ſulphureous acid cannot therefore apply.

OF THE TABLE OF NITROUS ACID.

THOUGH this acid was not exactly oxyginated and colourleſs, yet it was far from being fully de-oxyginated, but in that pale red ſtate in which it commonly appears; what changes the variety of oxyginations may produce I have not experienced; the reſults are not quite ſo accurate as moſt of thoſe in the table of vitriolic acid, partly from the eruption of vapour during the weighing, and partly from the diſorder the fumes cauſe at long run in the ſcales; but the error in the quantity of real acid in 100 parts of the acid liquor,* no where, as far as I have had occaſion to examine, amounts to 1 per cent. or at leaſt does not exceed that amount; the lower part of the table I found moſt faulty, and have rectified the errors to a great degree.

Experiment 1ſt.

To 400 grs. of nitrous acid, whoſe ſp. grav. at 63° was 1,4995, Hahn added 200 of water, and when the whole was cooled down to 64° he found the ſp. grav. to be 1,3157.

Application.

The ſp. grav. 1,4995 at 63° would be (by the table of variation already ſeen) $1,4995 + 00101 \times 3 = 1,5025$, which ſcarcely differs from 1,5070, a tabular number, which denotes the acid contents 68,39—and if 100 grs. of this acid liquor contain 68,39 real acid, 400 grs. contain $68,39 \times 4 = 273,56$, and when 200 grs. of water were added, then 600 grs. contained 273,56, and conſequently 100 grs. of the mixture contained 45,59, which indicates the tabular ſp. grav. 1,3621, which at the temperature of 64° would be 1,3581.

THIS denſity differs much from that found by Hahn, being $\frac{42}{1000}$, but that the error proceeds from his not having allowed ſufficient time for the penetration of the water and acid, and from the loſs of acid by the heat excited will be ſeen in the examination of the 2d experiment.

Experiment 2d.

To the 600 grs. of the mixture of the laſt experiment, whoſe ſp. grav. was by him 1,3157, and at 60° would be 1,317, he added 200 grs. of water, and found the ſp. grav. of this laſt mixture at 64°, 1,2561, which at 60° would be 1,2578, the heat excited amounted to 80°.

Application.

Application.

The ſp. grav. 1,317 differs inſenſibly from 1,316, which indicates the acidity 38,97 per cent. and if 100 grains contain 38,97 : : 600 ſhould contain 233,82 (whereas we have already ſeen that 600 contains 273,56) and when 200 grains more of water were added, then 800 ſhould contain 233,82, and conſequently 100 ſhould contain 29,22 real acid, which indicates very nearly the ſp. grav. of this 2d mixture to be 1,237, which differs from Hahn's reſult by $\frac{\begin{array}{r}1,257\\ -1,237\end{array}}{0,020}$ by $\frac{20}{1000}$, a difference which, though conſiderable, is by the half ſmaller than that of the 1ſt experiment, as by the interval of time between the 1ſt and 2d experiment the penetration of the 200 grains of water firſt added had increaſed.

This calculation is grounded on Hahn's reſults, which are erroneous from want of reſt and the eſcape of vapours. We ſhall now ſee what the ſp. grav. of this laſt mixture ſhould be, if both this and the former experiment were more accurately conducted, and the water ſo gradually added that little or no heat would be generated, on which principle my former calculation proceeded. This experiment may be conſidered as a mixture of 600 grains of an acid liquor, whoſe ſp. grav. ſhould, by my table, be 1,3621, and whoſe acid contents are 273,54 grains with 200 grains of water, and then

800

800 grains (the quantity of this 2d mixture) muft contain 273,54 grains of real acid, and confequently 100 grains of this new mixture contains 34,19 grains real acid, which indicates very near the fp. grav. 1,2779, which differs from Hahn's refult by $\frac{20}{10000}$, being fo much higher.

But this fame experiment may alfo be confidered as a mixture of 400 grains of the ftrong acid 1,5025 with 400 grains of water, then as the 400 grains acid liquor contains 273,56 grains real acid as already faid, 800 grains of the mixture fhould contain the fame quantity of real acid, and the fame fp. grav. would be found to refult as above.

Experiment 3d.

In this experiment he added 2 parts water (fuppofe 200 grains) to 1 part of the fp. of nitre 1,5025, much heat and copious red vapours were produced, infomuch that a few grains of the weight of the whole were loft (about 3 per cent.) and the fp. grav. was 1,1723, the temperature is not mentioned, but it feems probable it was 64°, the temperature at which, he fays, the mixture was made, then at 60° it would be 1,1740.

Application.

Here the 300 grains of mixed acid liquor contained 68,39 real acid, then 100 grains of it would contain 22,79, which is in the table, and indicates the fp. grav. 1,1845, which exceeds Hahn's refult

refult by $\frac{10}{1000}$, a difference which evidently arifes partly from the efcape of the red vapours and partly from want of fufficient time for penetration; it fhould however be remarked, that in large veffels there may fometimes be an increafe of weight from the abforption of oxygen by the nitrous air expelled by the generated heat.

Experiment 4th.

Mr. Richter (Stochymetrie, 3 theile, p. 9.) mixed fpirit of nitre, whofe fp. grav. was 1,5304 with water, in the proportion of 100 parts of the acid with 342 of water, and found the fp. grav. of the mixture 1,123; the temperature is not mentioned.

Application.

100 grains nitrous acid 1,530 contains by my table about 70 grains real acid, and when mixed with 342 of water, 442 grains will then contain 70 real acid, and confequently 100 grains of the mixture will contain 15,83 of real acid, this quantity lies between the tabular acidities 16,17, and 15,44, and by the 2d problem it will be found to correfpond with the fp. grav. 1,120.

Of

OF THE MARINE ACID.

THE mixtures of this acid and water are attended with little or no heat, and the ſp. gravities are ſuch as may be found by calculation. See 33 Roz. 242. Mr. Berthollet, among his experiments on oxyginated muriatic acid, Mem. Par. 1785, relates that having precipitated a ſolution of nitrated ſilver with 500 grains of common muriatic acid, whoſe ſp. grav. was 1,141, he obtained 547 grains of muriated ſilver, conſequently 100 grains of this acid would have afforded 109,4 of muriated ſilver. Now, as we ſhall hereafter ſee, 100 grains of muriated ſilver contain 16,54 of real marine acid, therefore 109,4 grains of muriated ſilver ſhould contain 18,02; and by my table 100 grains of the muriatic acid 1,1414 contains 18,57 of real acid.

CHAP. II.

ILLUSTRATION OF THE PROPORTION OF INGREDIENTS IN VITRIOLIC NEUTRAL SALTS.

BEFORE I treat of theſe ſalts it will be proper to notice the ſtate of each of their baſes.

OF VEGETABLE ALKALI OR TARTARIN.

THIS alkali may be obtained in three ſtates, the fully aerated and cryſtallized, the imperfectly aerated or common mild tartarin, and the cauſtic, which may alſo by particular proceſſes be cryſtallized.

THE fully aerated and cryſtallized contains, by Mr. Pelletier, 41 per cent. of alkali, 43 fixed air, and 16 water, 15 An. Chym. note, however, that even the cryſtallized is not always fully aerated, 1. Bergman, 16, 17.

COMMON dry ſalt of tartar contains about 60 per cent. of alkali, 28 or 30 of fixed air, with a few grains of Silex, vitriolated tartarin and argill; common pot-aſh generally contains alſo ſome grains of vitriolated and muriated tartarin.

Section 1ſt.

VITRIOLATED TARTARIN.

BY my determinations, 86 grains purified and dry tartarin * were ſaturated by 130 grains of vitriolic acid, whoſe ſp. grav. at 60° was 1,565.

Now this ſp. grav. indicates by the table 54,46 real acid; conſequently 130 grains of it contained 70,79 real acid, and 86 tartarin + 70,79 real acid = 156,79 vitriolated tartarin.

HENCE 100 parts tartarin take up 82,48 of real vitriolic acid. And 100 parts real vitriolic acid take up 121,48 of tartarin. And 100 grains tartar vitriolate contain 54,8 tartarin, and 45,2 of real vitriolic acid; or in round numbers 55 tartarin, and 45 real acid; or in the proportion of 11 to 9.

Experiment of Dr. Black.

SINCE the publication of the above mentioned determinations, the highly delicate and accurate experiments of Dr. Black, undertaken with the view of aſcertaining the contents of the Geyſer waters have appeared, with one of which I have compared the foregoing

* By tartarin the mere cauſtic ſtate is indicated; when it contains fixed air I call it mild; or fully aerated, if it be ſaturated therewith.

foregoing determinations, and had the pleasure of finding an almost perfect coincidence. See 3 Edinb. Transf. p. 101, 102. Dr. Black to vitriolic acid whose sp. grav. at 60° was 1,798, added 100 times its weight of water, and found that 112 grs. of this dilute acid saturated exactly 1 gr. of tartarin.

Application.

To exclude fractions I shall multiply Dr. Black's quantities by 100; then if 200 grains of vitriolic acid 1,798 were diluted with 20000 grains of water, his dilute acid would consist of 20200 grains, 11200 of such dilute acid would saturate 100 grains of tartarin. Now vitriolic acid 1,798 differs insensibly from 1,7959 which by my table contains 75 grains per cent. real acid. Therefore 11200 grains of such acid so diluted would contain 83,16 real acid, which differs from my determinations only by $\frac{68}{100}$ of a grain. $83{,}16 - 82{,}48 = 0{,}68$.

Hence we may find the sp. gravity and quantity of real acid in the sp. of vitriol employed by Wenzel, which it will be useful to know as he made several interesting experiments; and thus also the accuracy of the table of vitriolic acid will be still farther confirmed.

For this investigation he has furnished us with two *data*; 1st, he tells us that his sp. of vitriol was formed of two parts of highly concentrated vitriolic acid and three parts water, and 2dly, that 240 grains of this ſp. contained 75,75 of ſuch acid as is found in ignited tartar vitriolate which is what I call real acid.

Whence I deduce that $\frac{2}{5}$ of his ſp. of vitriol conſiſted of the highly concentrated acid, and $\frac{3}{5}$ of water. Now $240 \times \frac{2}{5} = 96$, therefore 96 grains of the concentrated acid contained 75,75 of real acid, then 100 grs. of it would contain 78,9, which quantity belongs to a ſp. grav. intermediate between the tabular denſities 1,8542 and 1,8424, and by the ſecond problem will be found to be 1,8467, therefore when one part of it is mixed with $1\frac{1}{2}$ of water, or for inſtance, when 100 grains of it are mixed with 150 of water, (which is the ſame as mixing two parts with three) the compound amounting to 250 grains contain 78,9 real acid, and 100 grains of this dilute acid contain 31,56 of real acid, a quantity which is extratabular, but belongs to a ſp grav. which by the ſecond problem will be found to be 1,2987.

Therefore the ſp. grav. of Wenzel's oil of vitriol is 1,8467 containing 78,9 real acid per cent and the ſp. grav. of his ſpirit of vitriol was 1,2987, containing 31,56 per cent real acid.—261,976 (262 grs.) of his ſpirit of vitriol would ſaturate 100 grs. of tartarin.

1000 grains of Dr. Black's dilute vitriolic acid contained 7,425 real acid. As I found it has lately been denied that vitriolated tartarin

tartarin contained 45 per cent. real vitriolic acid, I diſſolved 100 grains of it in ſix ounces of water, and precipitated the acid by muriated barytes, the reſulting baroſelenite weighed after ignition 135,25, which proves as we ſhall ſhall preſently ſee that the vitriolated tartarin contained 45,078 grains of real acid.

Section 3d.

Of Soda and Vitriolated Soda, or Glauber.

As ſoda may be had either chryſtallized, effloreſced or deſiccated, it will be neceſſary to examine the proportion of real alkali in each, in order to find the proportion in neutralized compounds.

1ſt. In its cryſtalized ſtate even when recently formed, I found the proportion of its ingredients ſomewhat variable, but in the greater number of experiments the cryſtals being dried in filtering paper in a temperature not above 66°, and the air not much diſpoſed to give out moiſture. I found 100 parts of the cryſtals to contain 64 of water, 21,58 of real ſoda, and 14,42 of fixed air. 36 Grains therefore of aerated but deſiccated ſoda are equal to 100 grains of the cryſtallized, that is, contain the ſame quantity of alkali.

2dly

2dly, In its ſimply effloreſced ſtate the quantities are variable according to the more or leſs perfect effloreſcence, the ſtate and temperature of the air.

3dly, 100 parts *ſoda fully aerated but thoroughly deſiccated* in a heat ſomewhat below ignition contains 59,86 alkali or mere ſoda, and 40,05 of fixed air per cent. or nearly 60 of alkali, and 40 of fixed air. In the experiments in my former paper the ſoda was heated to ignition, and thus part of the fixed air was probably expelled, for I found only 36 per cent. of fixed air.

Of Glauber.*

By my determinations, 100 *parts of ſoda* (that is, mere ſoda, dry and free from fixed air) are ſaturated by 127,68 of real vitriolic acid. And 100 parts of real vitriolic acid are ſaturated by 78,32 of ſoda. Hence if Glauber contained *no water*, 100 parts of it would contain 43 92 of ſoda and 56,08 of real vitriolic acid, or nearly 44 *ſoda* and 56 *real acid*; and this is the ſtate of glauber thoroughly deſiccated.

But *cryſtallized Glauber* contains a large proportion of water, for 100 parts of it *loſe* 58 by a heat ſomewhat below ignition, therefore 42 parts only remain which contain alkali and acid in the proportion above mentioned of 44 to 56, that is, 18,48 of alkali and 23,52 of acid.

* This ſalt being long known by the name of Glauber's ſalt, I ſhall ſimply call Glauber, this being ſhorter, and ſerving as a memorial of the antient denomination. It claims by the ſame (but a much elder) title, as *Scheelium* and *Witherite*.

Hence 1st. 42 grains of deficcated Glauber are equivalent to 100 of the cryftallized.

Hence 2dly, 100 grains deficcated Glauber fhould give, or are equivalent to, 238 of the cryftallized, that is, they contain the fame quantity of alkali and acid as 238 of the cryftallized.

Hence 3dly, 100 grains of foda faturated with vitriolic acid fhould give 541 + of cryftallized Glauber, or 227 of deficcated Glauber, and 100 parts cryftallized foda fhould give 116,77 of cryftallized Glauber; or 49 deficcated Glauber, and 100 grains real vitriolic acid fhould give when faturated with foda (whether cryftallized or not) 425 + of cryftallized Glauber or 178,5 of deficcated.

But thefe quantities of deficcated or cryftallized falt are never *exactly* obtained, on account of the lofs by evaporation, and of what remains in the mother liquor.

Application.

On the Proportions in aerated Soda.

Experiment 1ft. Dr. Black's. 3 Edinb. Tranf. 106.

His quantities being fractionary to render the calculation clearer, I multiply all into 1000.

He

He found 2380 grains cryſtallized ſoda to contain 514 of mere cauſtic alkali, then 100 ſhould contain 21,17 of alkali, which differs from my reſult by leſs than ½ a grain. Hence 21,17 grains of mere ſoda are equivalent to 100 grains of the cryſtallized.

Again, he found that 2380 grains of cryſtallized ſoda loſe by thorough deſiccation 1523 grains, and conſequently are reduced to 857 grains, therefore 100 grains of the cryſtallized are reduced to, and are equivalent, as to real alkaline contents, to 36 grains, loſing therefore 64 grains of mere water.

And, laſtly, he found that 857 grains of deſiccated ſoda contain 514 of mere cauſtic ſoda, and conſequently 100 grains of the deſiccated contain nearly 60 of mere ſoda, and conſequently 40 of fixed air. All theſe reſults agree almoſt exactly with mine.

Experiment 2d. 1 *Klaproth*, 333.

In his experiment 1000 grains of dry cryſtallized ſoda loſt when thoroughly deſiccated in a ſand-heat, 637 grains of water, conſequently 100 ſhould loſe 63,7, which ſcarcely differs from Dr. Black's reſult, then the dry reſiduum amounts to 36,3 grains.

Of

Of the Proportions in Glauber.

1st Experiment. Dr. Black's, 3 Ed. Transf. 106.

514 grains of mere foda were faturated by 88180 of the dilute vitriolic acid before mentioned in treating of vitriolated tartarin.

Application.

Since 514 grains of mere cauftic foda require 88180 of the dilute acid, 100 grains of foda would require 17155, and fince 1000 grains of this acid contains 7,425 of real acid, 17155 contains 127,37 grains. A refult which differs from mine by lefs than $\frac{1}{3}$ of a grain.

2d Experiment. 1 *Klapr.* 333.

100 grains of the thoroughly deficcated foda above mentioned require for their faturation 382 of a dilute vitriolic acid, formed of a mixture of 1 part vitriolic acid, whofe fp. grav. was 1,850 and 3 parts water, and the refulting neutral falt weighed 132,5 grains.

He alfo found that 1000 parts newly cryftallized Glauber, dried betwixt filtering paper, afforded by thorough deficcation in a fand-heat only 420 grains, and therefore loft 580.

Application.

The ſp. grav. 1,850 is extratabular, lying between the tabular ſp. gravities 1,8542 and 1,8424, but nearer to the former; its acid contents are 79,14 per cent. then if 200 grains of this acid be diluted with 3 times that weight of water, we ſhall have 800 grains of a dilute acid, which will contain $79{,}14 \times 2 = 158{,}28$ grains of real acid; then 382 grains (the quantity employed by Klaproth) contain 75,57 of real acid, now 100 grains of deſiccated ſoda contain, as we have ſeen, 60 of mere cauſtic ſoda, and ſince 100 grains of mere ſoda require 127,68 for their ſaturation, 60 grains of ſuch ſoda ſhould require 76,60; the difference then between Klaproth's reſult and my determination is only 1,03 grains.

Lastly, it may naturally be expected, that the reſulting neutral ſalt ſhould amount to the joint weight of the real acid and mere alkali, and conſequently ſhould in this caſe weigh $75{,}57 + 60 = 135{,}57$ grains, which differs from Klaproth's reſult only by 3,07 grains, a loſs which may well be imputed to that which the ſalt ſuffers by evaporation.

These concordant experiments fully prove the accuracy of the table of vitriolic acid to a large extent, and of the proportion of ingredients I have aſſigned to ſoda, vitriolate tartarin and Glauber; for as Klaproth's experiments were made with an acid whoſe ſp.

grav.

grav. was 1,850, Dr. Black's with an acid whofe fp. grav. was 1,798, and mine with an acid whofe fp. grav. was 1,565, we may be affured that to that extent (which includes 25 determinations) no material error exifts.

We muft not however imagine, that all mineral alkali contains the fame proportion of ingredients as foda; for the natural mineral alkali found in Africa, and called *trona*, contains a fomewhat larger proportion of fixed air and a much fmaller of water. 195,6 grains of trona, which Dr. Black had the goodnefs to fend me, were faturated by 260,5 of vitriolic acid 1,383, and gave out 66,5 grains of fixed air, therefore 100 grains of trona would require 133,18 of this acid for their faturation, and would lofe 34 grains of fixed air.

Now this acid differs infenfibly from the tabular, whofe fp. grav. is 1,387, which contains 40,18 grains per cent. real acid, and therefore 133,18 grains of it contains 53,5 of real acid. But we have fhewn that 100 parts real vitriolic acid faturate 78,32 of mere mineral alkali, therefore 53,5 grains of this acid faturate 41,9; this therefore is the quantity contained in 100 grains of trona, then 41,9 + 34 of fixed air = 75,9 and 1,8 grains of reddifh earth, confequently the remainder, that is 22,3 grains, were water.

Here we fee the alkali takes up more fixed air than ufual, for fince ufually 60 of the alkali take up 40 of fixed air, 41,9 of the

 pure

pure trona ſhould take up but 27,91, or nearly 28, whereas here it takes up 34, which is owing to its retaining but a ſmall portion of water during its cryſtallization.

HENCE alſo we find the proportions in Mr. Keir's effloreſced but dry ſoda, for he tells us, that 100 grains of ſuch ſoda were ſaturated by 90 grains of vitriolic acid, whoſe ſp. grav. was 1,800; now this acid differs inſenſibly from the tabular, whoſe ſp. grav. is 1,807, which contains 75,89 real acid, conſequently 90 grains of this acid contains 71,9 real acid; and ſince 127,68 real vitriolic acid take up 100 of mere ſoda, 71,9 ſhould take up 56,31; and as 60 of mere ſoda take up 40 of fixed air, 56,31 ſhould take up 37,53, the ſum of both is 93,84, then the remainder of the 100, that is 6,16 parts, are water, which remained as it was not dried by ignition.

MR. KEIR alſo found that 100 parts of an impure Indian foſſil alkali contained as much real alkali as 58,8 grains of the above effloreſced and dry ſoda, and were ſaturated by 53 grains of the vitriolic acid 1,800. Now, from the proportions above ſtated, it will be ſeen that 58,8 of his ſoda contain 33,11 of mere alkali, and that 53 grains of the acid contained 40,22 of real acid, and that theſe ſhould ſaturate, and thereby indicate 31,5 of mere alkali, which differs from his reſult by 1,61 grains *.

LASTLY, the proportions aſſigned to cryſtallized tartarin and to cryſtallized ſoda, and alſo the proportions of mere tartarin and mere

* Tranſ. of the Society of Arts and Manufactures. Vol. 6, p. 130, &c.

mere ſoda taken up by a given weight of real vitriolic acid, are confirmed by an experiment of Mr. Fourcroy's, 2 Ann. Chy. 289. for he found that the *ſame quantity* of vitriolic acid which ſaturated 193 grains of the cryſtallized ſoda alſo ſaturated 188 of cryſtallized tartarin. Now I have aſſigned 21,58 per cent. of mere alkali to cryſtallized ſoda, therefore 193 grains of it contain 41,6, and as the cryſtallized tartarin contains 41 per cent. of alkali, 188 grains of it contain 77. I have alſo ſhewn that 100 grains real vitriolic acid take up 78,32 of mere ſoda, and 121,48 of mere tartarin, then the harmony of theſe proportions with Fourcroy's experiments will thus appear: as 78,32. 121,48 :: 41.76,36; the difference between us is only 0,64 of a grain.

Section 3d.

BAROLITE AND BAROSELENITE.

100 parts barytic earth precipitated from its ſolution in acids by a mild alkali, whether fixed or volatile, and heated to gentle ignition, contains 78 or 79 parts of earth, 21 or 22 of fixed air, about 1,5 are ſtronthian earth, a quantity which in this caſe deſerves little attention. See 1 Klapr. 271. 2 Klapr. 82 and 86. 2 Chy. Ann. 1793, 196. 1 Chy. Ann. 1795, 111. Hence 100 grains barytic lime take up 28,2 of fixed air, and 100 grains of fixed air would precipitate 354,5 of barytic earth, and probably more, as the earth may not be ſaturated.

BAROSELENITE

Baroselenite or Vitriolated Barytes.

Barytic Solutions being the moſt delicate teſt of vitriolic acid as yet known, the determination of the proportion of real vitriolic acid taken up in the artificial compound of both is of the greateſt importance, and its agreement with the foregoing determinations will tend to their mutual eſtabliſhment.

I have already mentioned that by real vitriolic acid I mean acid of ſuch ſtrength or concentration as exiſts in well-dried and neutralized vitriolated tartarin. If therefore I can ſhew in what proportion the acid contained in a given weight of this ſalt enters into the compoſition of a given weight of thoroughly dried baroſelenite, the proportion of real acid in this laſt will of courſe be demonſtrated. Now this may very nearly be aſcertained by the experiment of Dr. Withering, Phil. Tranſ. 1784, p. 304. But firſt I muſt premiſe that by the experiments of the moſt accurate analyſts, 100 parts baroſelenite when ſufficiently dried contain very nearly 33 of vitriolic acid.

The reſults obtained by Dr. Withering were as follow:

1ſt. 480 grains of baroſelenite being fuſed with 960 of ſalt of tartar, 428 grains of the baroſelenite were decompoſed, and 52 remained undecompoſed.

2dly.

2dly. The decompofed part neverthelefs weighed after decompofition only 360 grains.

3dly. 300 grains of vitriolated tartarin were alfo obtained.

In this experiment we are only to attend to the 428 grs. which were decompofed, as the 52 that efcaped decompofition were no way altered.

In the firft place it is plain that the 428 grains that were decompofed contained at leaft as much vitriolic acid as they imparted to the alkali in forming 300 grains of vitriolated tartarin: Now 300 grains of vitriolated tartarin contain $45 \times 3 = 135$ of real vitriolic acid therefore 428 grains of barofelenite contain at leaft 135 of real vitriolic acid, that is, 31,5 per cent. a quantity that already approaches pretty nearly to the direct refults of moft analyfts. But in the next place it is equally evident, that the quantity of acid was greater than here ftated, and the quantity of mere barytic earth much below 360 grains, the quantity expreffed in the 2d refult; for if this quantity were juft, the fum of its weight and of that of the acid would furpafs the weight of the decompofed part. As inftead of 428 the fum would amount to $360 + 135 = 495$, which is impoffible. The truth then is, that thofe 360 grains of refiduary earth comprehend the weight not only of the mere earth, but alfo of the fixed air, which it had taken from the alkali in exchange for the acid it had imparted

imparted to it; consequently to find the true quantity of acid we must find out how much of the residuary 360 grains were mere earth, for by deducting this quantity from 428, the remainder will express the quantity of acid in the 428 grains.

THEN let the quantity of earth in the 360 grains $= x$, and the quantity of fixed air $= y$, then $x + y = 360$. and $x : y :: 78 : 21$ * nearly; and therefore $21\, x = 78\, y$, and $x = \frac{78\, y}{21}$, then $y + \frac{78\, y}{21} = \overline{360 \times 21} = 7560$, and $21\, y + 78\, y = 7560$ or $99\, y = 7560$, and $y = \frac{7560}{99} = 76{,}36$ grains of fixed air; and deducting this from 360, we have the quantity of mere earth $= 360 - 76{,}36 = 283{,}64$; and deducting this quantity from 428, we have the quantity of vitriolic acid $= 144{,}36$ grains; and lastly, if 428 baroselenite contain 144,36 of vitriolic acid, 100 grains baroselenite should contain 33,64.

THIS last quantity of acid somewhat exceeds the usual centenary proportion obtained by chymists, yet I believe the saturating proportion of acid to be still higher, for the following reasons:—— There are three ways of adding to each other an acid and earth or metal, the one by dropping the acid to be combined into the solution of the earth in an acid to which the earth hath a weaker affinity, and the other by inserting the earth immediately into the

* I say 21 rather than 22, as Dr. Withering himself states it at 20,8.

the acid with which it is to be combined, or by dropping its ſo-lution in a weaker acid into the acid with which it is intended to be combined. In the 1ſt mode of combination the ſaturation is ſcarce ever complete, becauſe the new compound in many caſes precipitates before it is fully ſaturated, and even though there ſhould be an exceſs of the acid to be combined in the liquor, yet the inferior part of the precipitate ſeldom receives it, being ſhel-tered by the ſuperior, and becauſe its affinity to its laſt comple-ment of acid is much weaker than that to its *mean* proportion of acid.

But in the 2d or 3d mode of addition, the earth being ſur-rounded by the acid with which it is to be combined, and thus expoſing a greater ſurface, takes up more of it and even fre-quently an exceſs, as I have often experienced.

This explains the difference which may be obſerved in the ex-periments I ſhall now ſtate:

1ſt. Dr. Withering having made a ſolution of 100 parts native aerated barytes in muriatic acid, dropped vitriolic acid into it until a precipitation ceaſed to appear; this artificial baroſelenite weighed 117 grs. Phil. Tranſ. ibid. 405. Now this native barytes con-tained but 78, 6 of pure barytic earth, as he had proved in a former experiment; therefore 78,6 of barytic earth took up as much real vitriolic acid as raiſed its weight to 117 grs. namely 38,4 grs.; and if 117 grs. baroſelenite contain 38,4 grs. of vitriolic acid, 100 parts baroſelenite muſt contain 32,8.

So alſo Klaproth tells us, that a baroſelenite which he had formed by dropping vitriolic acid into a muriated ſolution of aerated barytes contained barytic earth and acid nearly in the proportion of 2 to 1, conſequently 100 parts of it contained 66,66 of earth and 33,33 of real vitriolic acid. 2 Klapr. 72.* And p. 97 he tells us, he found the ſame proportion in another experiment, as 126 baroſelenite contained 42 of real acid.

On the other hand, Fourcroy having diſſolved 100 grs. native aerated barytes with the aſſiſtance of heat in very dilute vitriolic acid, found it to afford 138 grs. of baroſelenite, (inſtead of 117 which Dr. Withering had found by the 1ſt method) and that the barytic earth had taken up 48 parts vitriolic acid. Now if 138 parts broſelenite contain 48 of acid, 100 muſt contain 34,78. 4 Ann. Chym. 65.

Klaproth found that 85,5 grs. vitriolic acid whoſe ſp. grav. was 1,850, entered into the compoſition of 194 grs. baroſelenite, and by his own rule $\frac{1}{3}$ of theſe 194 grs. were real acid = 64 66; therefore 100 grs. of baroſelenite ſhould contain 33,33 grs. real acid. 1 Klapr. 153. By my table, as already ſaid, 100 grs. vitriolic acid, whoſe ſp. grav. is 1,850, contains 79,14 grs. real acid, therefore 85,5 grs. of this acid ſhould contain 67,7 real acid, and if

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* The baroſelenite in all theſe caſes was ignited, and he found that 185 grs. merely dried weighed after ignition 180, conſequently 100 parts of the merely dry loſe about 2,7 or 2,8 by ignition.

194 grs. barofelenite contain 67,7, 100 grs. of the barofelenite fhould contain 34,92, which differs from Klaproth's refults by 1,59 grs.

Again, Dr. Black, in the analyfis of Geyfer Waters, p. 117, tells us that 170 grs. barofelenite contain as much acid as 100 of fully deficcated Glauber. Now I have already fhewn by my own experiments that 100 grs. of deficcated Glauber contain 56,08 of real vitriolic acid, therefore 170 of barofelenite contain the fame quantity, and if fo, 100 grs. barofelenite muft contain 32,98, very nearly 33, which we fee fcarcely differs from Klaproth's proportion, the quantity of real acid being computed from my table.

Lastly, Klaproth found that 100 grs. of deficcated Glauber decompofed by acetited barytes gave 168 grs. of barofelenite. 1 Klapr. 333. Then 168 grs. barofelenite contain 56,08 of real vitriolic acid, and 100 fhould contain 33,38 of this acid. The confonance of thefe refults with my table may hereby be eafily difcerned. In general then the quantity of real acid in any quantity of ignited barofelenite may be difcovered by dividing it by 3, it being $\frac{1}{3}$ of the whole weight.

Hence 100 parts barytic earth take up 50 of real vitriolic acid, and would give 150 of barofelenite. And 100 grs. real vitriolic acid take up 200 of barytic earth, and afford 300 of barofelenite.

Section 4th.

Aerated Stronthian.

100 parts native aerated ſtronthian, or of the artificial ſufficiently dried, contain 31 of fixed air and 69 of earth.

Hence the quantity of air in any given quantity is found by multiplying this quantity into 0,31, and the quantity of earth by multiplying it into 0,69; then 100 parts of this earth are ſaturated by 45 of fixed air, and 100 parts of fixed air by 222,5 of this earth.

According to Dr. Hope, 100 grs. of cryſtallized ſtronthian lime contain 32 of earth and 68 of water.

Lowitz found 100 grs. of the artificially aerated ſtronthian to contain, when dried in heat, 32,5 per cent. of fixed air. 1 Chy. Ann. 1796. 128.

Per Pellitier, 100 grs. native aerated ſtronthian calcined with 10 grs. charcoal loſt only 28 grs. 21 Ann. Chy. 124.

Vitriolated Stronthian.

100 Parts vitriolated ſtronthian contain 42 of real vitriolic acid, for this is the quantity which muriated barytes ſeparated from the

tartarin

tartarin which decompoſed 100 parts of vitriolated ſtronthian, conſequently the earthy part amounts to 58 grs. 2 Klap. 96, 97. Then 100 of this earth take up 72,41 of real vitriolic acid; and 100 grs. real vitriolic acid would take up 138 of ſtronthian earth.

Section 5th.

AERATED LIME AND SELENITE.

IN artificial aerated lime formed by precipitation, by ſoda or even by common tartarin, if added to exceſs, the proportion of lime to fixed air is conſtant, being as 55 of the lime to 45 of the air, that is, as 11 to 9, ſo that the quantity of air being given that of the lime in the compound may be known; and if the compound be free from any other ingredient, and heated to redneſs to free it from water, then its weight being given the quantity of lime it contains is found by multiplying it into 0,55, and the quantity of air by multiplying it into 0,45; but whether dry or not, the weight of the air being found, the weight of lime is found by the 1ſt analogy.

MR. BERGMAN repeatedly aſſerts, that 100 grs. of calcareous ſpar contain 55 of mere lime, 11 of water, and only 34 of fixed air. It always gives me concern to find my reſults different from thoſe of this great man, but on this occaſion I am happily able to detect the cauſes of this difference.

1ſt.

1ſt. He tells us that by ſlow ſolution of 100 grs. of the ſpar in acids he found the loſs of weight to amount to 34 grs. only, though by applying a ſtrong heat he found it to amount to 45. Hence he concludes that in ſolution the fixed air ſingly was expelled, but that both fixed air and water were expelled by heat. Now to obtain a ſlow ſolution in acid he muſt have uſed a very dilute acid, and have employed a very narrow mouthed veſſel. In this caſe much of the fixed air is reabſorbed by the ſolution, as daily experience ſhews, and thus muſt have prevented his perceiving the real quantity of the air expelled from its combination with the earth.

Again Lavoiſier computes 100 grs. of chalk to have loſt about 34 grs. of air by ſolution in nitrous acid; but this loſs he inferred not from a direct trial, but from the weight of the volume of air found by compariſon with that of common air, calculated according to Mr. De Luc's rules. This concurrence muſt undoubtedly have confirmed Mr. Bergman in this erroneous eſtimation.

So alſo in natural lime-ſtones, the quantity of fixed air being found that of the lime is in the above proportion, except in a few caſes where magneſia exiſts in them or the lime not ſaturated. Hence 100 grains lime take up 81,81 of fixed air, but 100 grains of it are precipitated by ſomewhat leſs. Klaproth eſtimates the proportion in this at 4 of fixed air to 5 of lime. And 100 grains fixed air ſaturate 122,24 of lime, but would precipitate 125.

Section

Section 6th.

SELENITE.

THERE are two ways of combining vitriolic acid with lime and and ſome other ſubſtances; one by *direct ſolution* or addition of the baſis unto perfect ſaturation, ſo as no longer to diſcolour the uſual teſts, the other by precipitation from another menſtruum; in this laſt method the baſis takes up an exceſs of acid, which as it is waſhed off in other purſuits occaſions no miſtake, though it does in this.——The 1ſt experiment in my laſt paper I followed the 1ſt method, in it I found that 439 grains of a mixture of 225 grains of vitriolic acid whoſe ſp. grav. at 60° was 1,5654, with 225 of water, ſaturated 152 grains of marble which contained (at the rate of 55 per cent.) 83,6 of lime. By the table it appears, that the vitriolic acid before dilution contained 54,46 real acid, then 439 of the dilute acid contained 119,5 of real acid, conſequently 83,6 of lime took up 119,5 of real vitriolic acid, therefore if the compound of both were free from water, we ſhould have its weight equal 83,6 + 119,54 = 203,14 nearly, and 100 parts of it would contain 41,15 lime and 58,84 of real acid.——But unleſs the compound be expoſed to a high heat this weight cannot be expected; the reſulting ſelenite will always retain a proportion of water, varying with the degree of heat to which it was expoſed, and it is this that occaſions the variety of determinations of the pro-

portion

portion of ingredients in ſelenite, when, as in this experiment, its ingredients are directly combined; yet the proportion taken from the quantity of precipitate is more fallacious, as will preſently be ſhewn.

In this direct experiment the quantity of ſelenite obtained after deſiccation in a heat not exceeding 170°, amounted to 237.25 grains, of theſe 203,14 were lime and real acid; the remainder then *v: z* 34,11 were water; then by the rule of proportion 100 grains of this ſelenite ſhould contain 14,38 of water, conſequently 85,62 were lime and acid. And if 100 parts of ſuch compound contain 41,15 of lime, as already ſeen, 85,62 ſhould contain 35,23, and deducting this from 85,62, we have 50,39 for the acid part, conſequently the centenary proportion of ingredients in ſelenite dried at about 165° is as follows:

Real vitriolic acid -	50,39
Lime - - - -	55,23
Water - -	14,38
	100,00

If the ſelenite were dried by mere expoſure to the air, the quantity of water in 100 parts of the ſelenite would be greater, and that of the lime and acid ſmaller, and if it were ignited the proportion of theſe laſt would be greater, and that of the water ſmaller,

ſmaller, as is evident; but by expoſing any quantity of it to a ſtrong red heat the water will, for the moſt part at leaſt, be expelled, and the proportion of the other ingredients may be determined very nearly by the above analogy, if the ſelenite be ſaturate and free from foreign ingredients.

THE experiments I made in precipitating lime from the nitrous and marine acids by the vitriolic, and alſo by vitriolated tartarin, I found to be fallacious, as much of the ſelenite remains in ſolution in theſe acids, and conſequently it is not poſſible to limit or diſcover the proper addition of the precipitant. Hence 100 *grains lime* take up 143 nearly of real vitriolic acid, and afford about 284 of ſelenite thus dried and formed. And 100 grains real vitriolic acid take up nearly 70 of lime, and afford 198 of ſelenite thus dried and formed.

100 grains lime precipitated by vitriolic acid take about 15,8 per cent. exceſs of real acid, and vitriolated tartarin does not precipitate the whole of it without repeated evaporations and additions.

Experiment 1ſt. 1 *Klaproth*, 195.

ACCORDING to Klaproth 100 grs. of vitriolic acid, whoſe ſp. grav. was 1,850 (neglecting he ſays inſignificant fractions) were ſaturated by 55 of lime or 100 of aerated lime, and afforded 160 grs. ſelenite.

Application.

100 grs. of the vitriolic acid 1,850 contain 79,14 grs. per cent. of real acid as already ſeen; then if there were no water in the compound its weight would be 79,14 + 55 = 134,14 grs. but he found the weight to be 160, then the difference or 160—134 muſt be water = 26 grs.

Then the centenary proportion was
- 49,47 real vitriolic acid
- 34,37 lime
- 16,25 water
- ———
- 100,00

This proportion we ſee ſcarcely differs from mine and therefore his ſelenite was probably expoſed to nearly the ſame heat.

Experiment 2d. 2 *Klaproth*, 124.

In this experiment he tells us the ſelenite was heated to *ignition*, and conſequently we may ſee the difference of proportion produced by that heat. 38 grs. of it contained 14,75 of lime; the quantity of acid is not mentioned.

Application.

Application.

By his own proportion 14,75 lime ſhould take up 21,22 of real acid, for as 55 : 79,14 : : 14,75 : 21,22 nearly, then 1745, + 21,22 = 35 97, conſequently the remainder *v. z.* 2 grs. were water, then the centenary proportion ſhould be

as 38 : 14,75 : : 100 to	38,81 lime
38 : 21,22 : : 100 to	55,84 real acid
then the remainder is	5,35 water
	100,00

Experiment 3d. 1 *Bergm.* 135.

Bergman's ſelenite is cryſtallized and contains ſo much water that it is plain he ſuppoſes it dried by mere expoſure to the heat of the atmoſphere.

according to him 100 grs. contain	46 vitriolic acid
	32 lime
	22 water
	100

Hence 1ſt, we have the proportion of ſelenite dried in 3 different degrees of heat, that of ignition, that of the atmoſphere, and that between 130° and 170°.

 2dly. We

2dly. We may now explain and do juſtice to the *firſt* experiment of Mr. Wenzel on this ſubject. He diſſolved 240 grs. of clean oyſter ſhells in his ſpirit of nitre, and precipitated the lime contained in them by dilute vitriolic acid, he then evaporated the whole, firſt to dryneſs and afterwards by gentle ignition to expel the exceſs of acid, and laſtly expoſed the ſelenitic maſs to a more intenſe heat for one hour, then weighing it in the ſame veſſel found the ſelenite to weigh 309,75 grs.

Application.

The 240 grs. of purified oyſter ſhells contained 126,72 grs. of lime, which I prove thus, he tells us p. 101, that 81 grs. of the ſame oyſter ſhells gave out during ſolution 35 grs. of fixed air, conſequently 100 grs. would give out 43,2 ; now we have already ſeen that 45 grains of fixed air denote the preſence of 55 of real calcareous earth in 100 parts aerated lime, therefore 43 2 denote 52,8 of lime and therefore 240 parts of theſe ſhells contained 126,72.

Now as to the acid, ſince 100 parts lime take up 143 126,72 ſhould take up 181,20 and the ſelenite being ſo ſtrongly heated ſhould weigh only the ſum of both *v. z*: 181,20 + 126.72 = 307 92 or 308, grains which wants only 1,75 of the weight found by Wenzel, this increaſe found by him I impute to ſome phoſporated lime originally in the ſhells, the acid of which was not expelled in the above experiment.

Wenzel

Wenzel was ſet aſtray by his 2d experiment; for having calcined 240 grains of his ſhells for 4 hours he concluded they were wholly converted into lime and he found their weight 133,5, but here lay the miſtake, he had no proof that 3 or 4 grains did not remain uncalcined, and the preſence of phoſphorated lime he did not ſuſpect. In a third experiment he came very near the truth for he concluded the quantity of lime to be 125 grains, but the difference between this and 133,5 he attributed to the *cauſticum.*

Section 7.

Of Magnesia and Epsom.

This earth may be obtained in three ſtates, either *fully aerated and cryſtallized*, and then from its great ſolubility in water it may be called a ſalt; or *imperfectly aerated*, ſuch as *common magneſia;* or *perfectly deaerated* and freed from water by a white heat.

The proportion of ingredients even in cryſtallized magneſia are differently ſtated, probably from having undergone ſome unperceived effloreſence; according to Fourcroy who ſeems to have beſtowed moſt attention to this object, 100 parts cryſtallized magneſia contain

magneſia contain	50	fixed air
	25	magneſia
	25	water
	100	

hence

hence they loſe 75 per cent. of their weight when ſtrongly heated 2 An. Chy. 297, 298.

But according to 1 Bergm. 29 and 373, the chryſtallized contains but 30 per cent. of fixed air, with whom Butini agrees, but he afterwards found even common magneſia to contain a larger proportion, ſee p. 23 and 146 of his treatiſe; of the other ingredients neither mentions the proportion.

In *common* magneſia the proportions are, according to Bergman

45 of earth
25 fixed air
30 of water
——
100

And according to Butini p. 146, 40,62 earth
37,5 fixed air
21,88 water
——
100,00

According to Fourcroy 100 parts of common magneſia contain

40 earth
48 fixed air
12 water
——
100

conſequently they ſhould loſe 60 per cent. in a white heat.

Consequently per Bergman 100 grs. common magnesia should lose 55 per cent. in a strong heat, and per Butini it should lose nearly 60, and with this determination two experiments of Klaproth's agree. See 2 Klapr. 9 and 20; yet in another experiment the loss was but $\frac{46}{100}$, 2 Klap. 174.

Hence we see the proportions of air and water are variable, but the sum of both generally amounts to 55 per cent. at the least, and and hence I rate the mere earthy part in common magnesia at 45 per cent. when by a strong heat less is found I believe the difference to have been volatilized. The various proportions of fixed air arise from the various proportions of it contained in the different precipitants used in obtaining magnesia.

Note however, the water may gradually be expelled from common magnesia in a heat much below ignition.

Epsom.

In my former paper I have stated that 35 grains of common magnesia, containing 15,75 of mere earth, were saturated by 50 grains of vitriolic acid, whose sp. grav. was 1,5654, diluted with a large proportion of water, but containing, as appears by the table, 27,23 real acid, and from this and a comparative experiment, I deduced that 100 parts of crystallized Epsom contained 17 of mere earth,

earth, 29,35 of real vitriolic acid, and 53,65 of water, it was ſtandard acid that I had before mentioned, but its quantity of real acid is as I now ſtate it, as may be ſeen by calculation.

Hence 100 parts perfectly deſiccated Epſom ſhould contain 36,68 nearly of earth, and 63,32 nearly of real acid.

And 100 grains mere magneſia take 172,64 real vitriolic acid, and ſhould afford 590 nearly of cryſtallized Epſom.

And 100 parts real vitriolic acid ſhould take up 57,92 of magneſia, and afford 340 nearly of cryſtallized Epſom.

According to Bergman 100 grains of magneſia take up 173 of real vitriolic acid.

According to Wenzel 100 grains magneſia take up 181,8 real acid, this ariſes from his rating the mere earthy part of common magneſia at 41,2 per cent. which, as I think, ariſes from volatilization of part of the magneſia.

Experiment 1ſt. Fourcroy, 2 *An. Chy.* 285,282.

He found that cryſtallized tartarin taken in the proportion of 80 parts to 100 of cryſtallized Epſom operate an *almoſt total* decompoſition of the Epſom.

Application.

Application.

As 100 parts cryſtallized tartarin contain, as already ſaid, 41 of vegetable alkali, 80 parts of it muſt contain 32,8, but as 100 parts of this alkali take 82,48 of real vitriolic acid, 32,8 ſhould take 27,05, which is nearly the whole of what 100 parts of Epſom contain.

Experiment 2d. Fourcroy, 2 An. Chy. 288.

CRYSTALLIZED ſoda, applied in the proportion of 108,8 to 100 of cryſtallized Epſom, perfectly decompoſed the Epſom.

Application.

100 parts cryſtallized ſoda contain, as already ſhewn, 21,58 of mere alkali, conſequently 108,8 parts contain 23,479. Now 100 parts of this alkali take up 127,68 of real vitriolic acid; therefore 23,479 ſhould take 29,97, which differs from the quantity of acid I have aſſigned to 100 parts cryſtallized Epſom only by 0,62 of a grain.

Section 8th.

Alum.

The combinations of argill with vitriolic acid are ſo diverſified, as Mr. Vauquelin has lately ſhewn in a ſeries of curious and intereſting experiments, that to aſcertain the limits of each would require a particular examination which the generality of the preſent inquiry does not at preſent permit to enter into.

The reſult of my former eſſay was, that 100 parts alum contain 31,34 of earth dried at 465°; 17,66 of real vitriolic acid, and 51 of water; but the acid contained in vitriolated tartarin, of which alum may contain 6 or 7 per cent. is not noticed; but being counted the whole amounts to 20 per cent.

The earth heated to *whiteneſs* may be reduced to 18, or ſtill fewer parts. Wenzel and others ſay 11,7, and I believe this to be moſt exact.

Hence 100 grains burnt alum, that is alum from which its water was expelled, ſhould contain 35,4 of real acid. But the alums of different countries differ much. See Vauquelin in An. Chy. & Pref. to 1 Laborant. VII.

Section 9th.

Vitriol of Iron.

100 parts of this vitriol newly cryſtallized contain by my determination 28 calx of iron in the ſtate of œthiops, equal nearly 22 of metallic iron, 26 real vitriolic acid, 38 water of cryſtallization, and 8 water of compoſition, that is which adheres to the acid. This determination I lately confirmed; for from a ſolution of 100 grains of this vitriol decompoſed by muriated barytes I obtained 77,25 of ignited baroſelenite, which at the rate of 33,3 per cent. contained 25,747 of real vitriolic acid.

Hence 100 parts vitriol of iron calcined to redneſs contain 41,93 of real acid, and 12,9 of water; but the calx of iron will weigh more than 45, as it attracts oxygen during the calcination.

The water of compoſition is for the moſt part expelled with the acid during diſtillation. Then 22 grains metallic iron ſhould afford 100 grains of cryſtallized vitriol; and 100 grains of the beſt iron would give 454,54 of vitriol.

The vitriol above examined was of a full *graſs green colour.* I have met with another which is of a pale ſea green colour, and contains much leſs acid, for 100 grains of it treated as above afforded only 56,7 of baroſelenite, and conſequently contained but 18,99 of real acid.

Section 10th.

Vitriol of Lead.

By the experiments of Klaproth, 100 grains vitriol of lead ſhould contain about 71 of metallic lead;* by thoſe of Bergman and Wenzel nearly 70; but as the lead, being precipitated from the nitrous acid, is in a calcined ſtate, we may add 4 of oxygen.

Again, Wolfe found 120 grains vitriol of lead decompoſed by tartarin to afford 65 of vitriolated tartarin; therefore 100 grains of this vitriol would afford 54,16 of vitriolated tartarin. Now this quantity of vitriolated tartarin contains 23,37 of real vitriolic acid; therefore juſt ſo much is contained in 100 grains of vitriol of lead. Phil. Tranſ. 1779. and 10 Roz. 368.

Hence the quantity of ingredients *in* 100 *parts of this ſalt* are 75 calx of lead, (= 71 of metallic lead) 23,37 real vitriolic acid, and 1,63 water.

Hence 100 *grains metallic lead* (with the addition of oxygen) take up 32,91 of real vitriolic acid, and afford 140 of vitriolated lead.

And 100 *parts real vitriolic acid*, unite to 303,8 of metallic lead, (when calcined) and afford 425,49 of vitriol of lead.

According

* See 1 Klapr. p. 169, and 173; and 2 Klapr. p. 219.

According to Bergman, 100 grains metallic lead would afford 143 of vitriol of lead.

According to Wenzel 143,33.

According to Wolfe 137,5. He precipitated the nitrous solution of lead by vitriolated tartarin, and probably did not apply enough, or this salt did not disengage the last portions of the nitrated lead, or some part of the vitriol of lead remained in the nitrous acid. This last supposition is highly probable.

Vitriol of Copper.

100 grains of this salt, perfectly crystallized, lost 28,5 by exposure to a heat of 370°.

By precipitation with muriated barytes they afforded 91 of ignited baroselenite, and hence contain 28,5 of water of crystallization and 30,33 of real vitriolic acid; and consequently about 40 of calx of copper = 32 of metallic copper.

Vitriol of Zinc.

100 grains of vitriol of zinc, crystallized in needles, lost in a heat of 375° 39 grains; and 100 grains of the same crystals, being dissolved and treated with muriated barytes, afforded 61,24 of ignited baroselenite, and hence contain 20,414 grains of real vitriolic acid.

CHAP.

CHAP. III.

OF NITRO NEUTRAL SALTS.

Section 1st.

Of Nitre.

From the different refults of various experiments, I am led to think that 100 parts of cryftallized, but dry nitre, contain 51,8 parts mere alkali, 44 of acid and 4 of water of compofition.

Hence 100 parts tartarin take up 84,96 of real acid with 8,1 of water, and would afford 193 + of dry nitre.

And 100 grains real nitrous acid take up 117,7 of tartarin, and would afford 227,24 of nitre, by reafon of 9,54 of water of compofition, which in this cafe accompanies the acid.

This is the beft account I am at prefent enabled to give of nitre, the inveftigation of the proportion of the acid contained in fp. of nitre, being attended with peculiar difficulties, as much as the acid efcapes, when, in its concentrated ftate, water is added to it, and fo much the more as it is more highly mephitized and the temperature

temperature higher. The more it is mephitized the more alkali it appears to ſaturate, but afterwards the ſalt extracts oxygen from the air; when melted it alſo loſes part of its oxygen and of its water of compoſition, but in time ſeems to regain them.

According to Wenzel, 83,5 parts tartarin were ſaturated by 90 of his ſtrongeſt acid, and the compound heated to redneſs weighed 173,5.

Then 100 grains tartarin ſhould take up 107,78 of his ſtrongeſt acid (= 87,51 of my real acid) and afford 207,78 of dry nitre.

And 100 parts nitre contain, by his account, 48,13 of alkali, and 51,87 of his ſtrongeſt acid, = 42,118 of my real acid.

According to Bergman, 49 parts tartarin afford, when ſaturated with nitrous acid, 100 parts nitre, conſequently 100 parts tartarin would afford 204.

Lavosier * allows nearly 49 per cent. of alkali and 51 of acid (including water) to 100 grains of nitre.

Berthollet, in the Memoirs of the Royal Academy for 1781, obſerves, that 480 parts of nitre afford, by diſtillation, 714 cubic inches of ſomewhat impure oxygen air, then 100 parts of nitre would

* P. 157 of the Engliſh Edition of his Elements of Chymiſtry.

would afford 148,7 (English measure), these at the rate of 33 per cent. would weigh 49,07 grains, including water lost, which differs but little from my account.

Kier found 22,5 grains dry nitre were saturated by 12,54 grains vitriolic acid, 1,844. Phil. Transf. by my determination 22,5 grains nitre contain 11,655 grains tartarin (for if 100 grains nitre contain 51,8 : : 22,5, 11,655) and 11,655 grains tartarin require 9,61 of real vitriolic acid for their saturation.

Now the sp. grav. 1,844 is intermediate between the tabular gravities 1,8542 and 1,8424, but nearer to the latter; then by the 1st problem its centenary acidity will be found to be 78,58, and if 100 grains of this acid contains 78,58 real acid, 12,54 should contain 9,85, the difference is not quite $\frac{1}{4}$ of a grain. Mr. Kier required 12,54 of this acid, by my determination 12,23 are sufficient, the difference is not $\frac{1}{3}$ of a grain. Phil. Transf.

Klaproth found 200 grains of Leucite, treated with marine acid, to afford 70 of muriated tartarin; and that 300 of that stone, treated with nitrous acid, afforded 123 of nitre. Now by Bergman's calculation 100 grains muriated tartarin contains 61 of alkali, therefore 70 grains should contain 42,7.

By my calculation 100 grains muriated tartarin contains 64 of alkali, therefore 70 should contain 44,8 of alkali.

Then

Then 100 grains Leucite ſhould contain, per Bergman, 21,35, and by my calculation, 22,4.

By Bergman's calculation there is a *deficit* of 0,27 of a grain, and by mine an exceſs of 0,77 of a grain. See 2 Klaproth, 50.

But with reſpect to nitre, my calculation has the advantage both over his and Wenzel's, for ſince 300 grains Leucite afford 123 of nitre, 100 grains of this ſtone ſhould afford 41. Then by Bergman's account, 41 grains ſhould contain 20,09 of alkali, which leaves a deficit of 2,16 grains; by my determination 41 grains of nitre contain 21,238, which leaves a deficit of only 1,012 grains:

For the calculation ſtands thus:

Silex	53,50	Silex	53,50
Argil	24,25	Argill	24,25
Tartarin	20,09	Tartarin	21,238
	97,84		98,988

Section 2d.

Nitrated Soda.

In my former experiment 36,05 grains of mere ſoda were ſaturated by 145 of nitrous acid, whoſe ſp. grav. at 60° was 1,2574; this denſity is intermediate between the tabular ſp. gravities

gravities 1,2779 and 1,2687, but nearer to the former, and by the solution of the 1st problem will be found to denote 33,8 grains real acid; consequently 145 grains of this liquor contained 49 grains of real acid. The quantity of nitrated soda formed was found by a standard experiment to be 85,142 grains, which is very nearly the sum of the weights of the real acid and mere alkali, as 36,05 + 49 = 85,05; this trifling difference may be water.

HENCE 100 parts desiccated nitrated soda should contain 57,57 real acid and 42,34 soda.

THEN 100 parts soda should take up 135,71 of real nitrous acid. And 100 parts nitrous acid should take up 73,43 of soda.

SURPRISED at finding no water in this neutral salt, I lately examined its composition by my antient method. I dissolved 200 grains of pure and well desiccated soda, and saturated the solution with 1225 grains of dilute nitrous acid, of which $\frac{1}{4}$ consisted of the concentrated acid 1.416, of which consequently 306,2 were employed; the loss of fixed air was 75 grains, and consequently the quantity of real alkali was 125 grains.

THE sp. grav. 1,416 lies between the tabular gravities 1,417 and 1,412, and by the solution of the 1st problem its centenary quantity of acid will be found to be 53,53; and 100 grains of this

this acid liquor being diluted with 300 of water, then 400 grains of the dilute acid contain 53,53 real acid; and consequently 1225 grains of it contained 163,9 grains; this quantity therefore was taken up by 125 of soda; if therefore the salt thus formed contained no water the sum of both these quantities should express its weight, namely, 163,9 + 125 = 288,9 grains; but having very gently evaporated the solution, namely, in a heat not exceeding 120°, and then drying the residuum in a heat of 400° for six hours, I found it to weigh in the evaporating dish (from which I could not separate it without loss) 308 grains, consequently these 308 grains contained 19,1 of water, however it is evident that in a greater heat even these would be evaporated.

And then very nearly the same proportion of acid and alkali would be found as in the preceding experiment, for 308 — 19,1 = 288,9, and if 288,9 grains contain 125 of alkali, 100 grains of the nitrated soda should contain 43,27, and consequently 56,73 of acid; and allowing 19 grains of water in 308 of this salt dried at 400°, then 100 grains nitrated soda should contain 40,58 of soda, 53,21 of real nitrous acid, and 6,21 of water.

And 100 grains mere soda saturated with nitrous acid should afford 246,42 of nitrated soda dried at that heat. And 100 grains real nitrous acid saturated by soda should give 188 nearly of nitrated soda dried as above.

Bergman, Vol. I. p. 20, allows to 100 parts ſoda very nearly the ſame quantity of real nitrous acid as I do, namely, 135,5 parts.

Having re-diſſolved the above 308 grains and expoſed the ſolution to ſpontaneous evaporation, I found the cryſtals dried at 70° to weigh 317 grains; hence this ſalt contains 2,8 per cent. of water of cryſtallization, but in a ſtrong heat it would loſe much more.

Though Wenzel's determinations ſeemingly differ conſiderably from the foregoing, yet on a cloſer inſpection the difference will be found not greater than the uſual imperfection of weights and weighing, and the varying nature of the acid may admit.

He found 71,5 grains mere ſoda ſaturated with nitrous acid to afford 190,75 of thoroughly deſiccated nitrated ſoda, and hence concluded that it contained no water, and conſequently 190,75 grains of this ſalt to contain 71,75 of alkali and 119,25 of real acid.

Hence 100 *grains of this ſalt* ſhould contain 37,48 of alkali and 62,52 of acid; it is plain then that this acid contained the 6,21 grains of water which I found in 100 parts of this ſalt, for if we add 6,21 to the quantity of acid I aſcribe to 100 parts of this ſalt, we ſhall find very nearly Wenzel's weight of acid, for $53{,}21 + 6{,}21 = 59{,}42$.

According

According to Wenzel, then, 100 grains mere ſoda take up 166,7 of this aqueous acid, and ſhould afford 266 of nitrated ſoda thoroughly deſiccated; and 100 grains of the aqueous acid ſhould take up 59,9 of ſoda.

From the experiment on nitrated ſoda Wenzel deduces the ſtrength of his ſp. of nitre, which being the ſame as he employed in his ſubſequent numerous experiments it is important to diſcover.

As he ſaturated 71,5 grains ſoda with 347 grains of this ſp. of nitre and found the ſoda to take up 119,25 of what he thought the ſtrongeſt nitrous acid, he concluded that 240 grains of it contained 82,5 of the ſtrongeſt acid, and conſequently 100 grains of it ſhould contain 34,375 of his ſtrongeſt acid. Now to compare the quantity of his real acid in his ſp. of nitre with that which I judge his to poſſeſs, I muſt obſerve that to ſaturate 71,5 grains mere ſoda, 96,933 grains of my real acid would be requiſite, and conſequently that 347 grains of his ſpirit of nitre contained no more; therefore 240 grains of his ſpirit of nitre contained but 67,04 of my real acid, and 100 grains of it contained 28 of my real acid; the difference is water contained in his ſtrongeſt acid. Then 1000 grains of his ſtrongeſt acid is only equal to 812,6 of my real acid; the remainder *v. z.* 187,4 being water contained in his ſtrongeſt acid.

Morveau.

Morveau faturated 485 grains of cryftallized foda (= 104,66 of mere foda) with 545 grains of fpirit of nitre, whofe fp. grav. at 4° of Reaumur (= 41° of Fahrenheit) was 1,2247, which at 60° of Fahren. would be 1,219, and this by my calculation contains about 27,29 per cent. real acid; confequently 545 grains contained 148,73; then 100 grains mere foda fhould take up 142 of real nitrous acid. 2d Old Mem. Dijon 184.

Lavosier alfo faturated a given quantity of foda with nitrous acid, but as there was an excefs of acid no ftrefs can be laid on his experiment.

I found nitrated foda to attract moifture in a moderate degree.

Nitrated Barytes.

As barytic earth cannot well be diffolved in nitrous acid without the affiftance of heat, I was obliged to attempt the analyfis of this falt by indirect methods, namely, precipitation by cryftallized foda and vitriolated tartarin.

The foda I employed contained 15 per cent. of fixed air, and 21,5 of mere alkali; and 100 grains of aerated barytes ignited contains about 21,5 of fixed air.

Now I found that 100 grains of cryftallized nitrated barytes were precipitated by 105 nearly of this foda, and that the earth after

after edulcoration, deficcation and ignition, weighed 70,25 grains nearly; but at the rate above-mentioned thefe 70,25 grains are reducible, deducting the fixed air to 55,10 of pure barytic earth.

On the other hand 108 grains of cryftallized foda contain 23,22 grains of mere foda, and thefe we have already feen are capable of taking up 31,41 of real nitrous acid, therefore by this experiment 100 grains of cryftallized nitrated barytes contain 31,41 of acid and 55,10 of earth; the remainder then is water of cryftallization, = 13,49 grains.

Again, 100 grains of cryftallized nitrated barytes were diffolved in 2400 of water, and precipitated by the gradual addition of a folution of vitriolated tartarin; the precipitate which was flowly and difficultly formed and collected weighed after ignition about 88 grains; thefe (at the rate of 33,33 per cent.) contained 29,33 of real vitriolic acid, and confequently 58,67 of mere earth.

Taking a mean, then, of thefe two experiments, 100 grains of nitrated barytes contain 56,88 grains of mere earth.

Lastly, 308 grains of native aerated barytes diffolved in 240 of nitrous acid, whofe fp. grav. was 1,451, diluted with 5 times its weight of water in a gentle heat afforded 384 grains of cryftallized nitrated barytes, befides a fmall refiduum.

Now

Now this acid contains 58 per cent. real acid, and confequently 240 grains of it contained 139,2 of real acid; and if 384 grains of the cryftals contain 139,2, then 100 grains fhould contain 36,25. But it muft be confidered that fome was contained in the mother liquor, fome in the cryftals that were not wafhed, but dried on filtering paper, and fome was difperfed by the heat applied.

This experiment alfo gives fome, though not an accurate information of the proportion of earth in nitrated barytes, for 308 grains of aerated barytes (at the rate of 21,5 per cent.) contain 66,22 of fixed air, and confequently 241,78 of mere earth. Suppofing then 384 grains of nitrated barytes to contain this quantity of earth, 100 grains of this falt fhould contain 62. But this fuppofition is inadmiffible by reafon of the loffes juft mentioned.

Upon the whole we may ftate the centenary proportion of this falt at 57 of earth, 32 real acid, and 11 of water.

Hence 100 grains barytic earth take up 56 of real nitrous acid, and fhould afford 175,43 of nitrated barytes. And 100 grains real nitrous acid fhould take up 178,12 grains of barytic earth, and fhould afford 312,5 of nitrated barytes.

100 grains of this falt loft only $\frac{1}{2}$ a grain of its weight by expofure to a heat of 300° for half an hour. It is alfo difficultly foluble. Its folution when faturated does not redden Litmus.

Nitrated

Nitrated Stronthian.

100 grains of perfectly cryftallized nitrated ftronthian, diffolved in 480 of water, were precipitated by about 107 of cryftallized foda, containing 16 per cent. of fixed air and 21,5 of mere alkali, the precipitate, after ignition, weighed 53,25 grains, and contained 17,04 of fixed air, and confequently 36,21 of mere earth.—Alfo the 107 grains of foda (at the rate of 21,5 per cent.) contained 22,9 of mere alkali, which (at the rate of 135,71 per cent.) took up 31,07 of real nitrous acid; then by this experiment 100 grains cryftallized nitrated ftronthian contain 36,21 of earth, 31,07 of acid, and 32,72 of water.

Then 100 grains of pure ftronthian earth take up 86 nearly of real nitrous acid, and fhould afford 276 of cryftallized, or about 92 of thoroughly deficcated nitrated ftronthian.

And 100 grains real nitrous acid fhould take up 116,5 of mere ftronthian earth, and afford 321 of cryftallized, or 107 of thoroughly deficcated nitrated ftronthian.

Section 3d.

Nitrated Lime.

In my experiment 136 grains Carrara marble were faturated by 400 of nitrous acid, whofe fp. grav. was 1,2754, and which confequently contained (at the rate of 33,59 per cent.) 134,36 real acid. The 136 grains Carrara marble contained (at the rate of 55 per cent.) 74,8 of lime.

Consequently 100 parts lime take up 179,5 of real nitrous acid, and 100 parts real nitrous acid take up 55,7 of lime.

Lavosier diſſolved 972 grains of ſlacked lime, dried in a heat of about 600°, in 3456 grains of nitrous acid, whoſe ſp. grav. was 1,2989, and conſequently contained (at the rate of 36,7 per cent.) 1268 grains real acid; from the 972 grains lime we muſt deduct (at the rate of 28,7 per cent. water abſorbed in the ſlacking) 268,9 of water, and alſo 35 grains of fixed air, abſorbed while ſlacking and drying, there remain then 668 of mere lime, and theſe took up 1268 of real acid, then 100 grains of lime would take up 190 of real acid. 1 Lavoſier, 198. Perhaps the difference ariſes from my computing the quantity of real acid from a ſpecific gravity taken at 60°, whereas his might have been taken at a higher degree.

Bergman found 100 grains of nitrated lime, *well dried* (that is dried in air) to contain 32 of lime; by the above analogy the proportion of the other two ingredients may be found, for ſince 100 parts lime take 179,5 of real acid, 32 ſhould take 57,44, conſequently the remainder, viz. 10,56 are water; if the nitrated lime could be perfectly dried, it would contain about 36 per cent. of lime and 64 of real acid.

According

According to Wenzel 122,66 grains of lime take up 240 of his ſtrongeſt acid, conſequently 100 of lime would take up 195,64 of ſuch acid, but this quantity is equivalent to only 159 of my real acid, this difference I cannot account for.

Section 4th.

Nitrated Magnesia.

By my experiment 100 parts mere magneſia require 210 of real nitrous acid for their ſaturation.

And 100 grains real nitrous acid take up 47,64 of mere magneſia. 100 grains cryſtallized nitrated magneſia contain 46 real acid, 22 magneſia, and 32 of water, as I found.

According to Wenzel 77 grains of the magneſia he employed contained but 32,13 mere earth, and yet required 240 of his ſp. of nitre for their ſaturation, which ſp. of nitre, by my calculation, contained but 67,2 real acid, and conſequently 100 grains mere magneſia would require 209 real nitrous acid ; by his own calculation 240 of his ſp. of nitre contained 82,5 of his ſtrongeſt nitrous acid, and conſequently 100 grains mere magneſia ſhould take up 256 of ſuch acid, = 207,87 of my real acid.

According to Fourcroy, 4 An. Chy. 214, 150 grains aerated magnesia, containing 48,66 per cent. mere magnesia, and consequently in all 73 grains, were saturated by 222 grains of nitrous acid, whose sp. grav. appears to have been 1,5298, of which 100, by my table, contain 69,88 real acid, and consequently 222 contain 155; and if 73 grains mere magnesia take up 155 real acid, 100 grains mere magnesia should take up 212.

CHAP.

CHAP. IV.

OF MURIATIC NEUTRAL SALTS.

Section 1ſt.

OF MURIATED TARTARIN.

IN my laſt paper I have ſtated that 86 grains of mere tartarin were ſaturated by 254 grains of muriatic acid, whoſe ſp. grav. at 60° was 1,1466; this is extratabular, but intermediate between the tabulated ſpecific gravities 1,147 and 1,1414, but nearer to the higher, and its centenary acid contents will be found by the 1ſt problem to be 19,06; conſequently 254 grains of this acid liquor contained 48,412 real acid; the ſum of the acid and alkaline parts then amounts to 48,412 + 86, = 134,412 of muriated tartarin; and ſince 134,412 of this ſalt contained 86 of alkali, 100 parts of the dry ſalt ſhould contain 64 nearly of tartarin, and the remainder or 36 parts are real marine acid.

HENCE 100 grains tartarin take up 56,3 of real marine acid, and ſhould afford 156,3 of well dried muriated tartarin. And 100 grains real marine acid ſhould take up 177,6 of mere tartarin, and afford 277,6 of deſiccated muriated tartarin.

WENZEL

Wenzel found 83,5 grains of tartarin to afford him 129 of muriated tartarin, consequently 100 parts of this salt should contain 64,7 of alkali, and 35,3 of acid, and 100 parts tartarin should take up 54,491 of real acid, and afford 154,491 of muriated tartarin, all which determinations differ very little from mine, and afford no inconsiderable proof of the accuracy of the table.

Hence we may deduce the quantity of real acid in Wenzel's sp. of salt and its sp. gravity.

By his own account 202 grains of his sp. of salt contained 45,5 of his strongest acid, consequently 100 grains of it should contain 22,52, and 240 grains of it 54, and its sp. gravity about 1,174.

By my calculation 202 grains of his sp. of salt contained 46,44 of my real acid, and 100 grains of it contained nearly 23 of my real acid, and 240 of it contained 55,17, and its sp. gravity should be about 1,176.

Another proof of the accuracy of my determinations will be found in the 2d §.

Klaproth's determination agrees fully with mine, for to 116 grs. of sylvian he ascribes 42 of concentrated muriatic acid, consequently 100 grains of sylvian should contain 36,2. 1 Klapr. 134.

Section

Section 2d.

Common Salt.

It has been seen in my last paper that 30,05 grains of mere soda were saturated by 129 grains of muriatic acid or sp. of salt, whose sp. gravity in the temperature of 60° was 1,1355; this by the table contains about 17,5 real acid per cent. consequently 129 grains of it contained 22,07 grains real acid, if therefore the neutral salt here formed contained nothing else but mere soda and real acid, its weight should be 30,05 + 22,07 = 52,12. Yet by the last experiment it appeared that the weight of the salt thus formed amounted to 56,74 gr. the surplus 4,62 grains must therefore have been water, and since 56,74 grains of common salt contain alkali, real acid and water in the above proportions, 100 grains of common salt (well dried and deprived of the water interspersed between its pores) must contain 52,96 soda, 38,88 real acid, and 8,16 water of composition that always accompanies the acid when this salt is formed, and therefore must in all other ways of examining the composition of this salt, have been confounded with it. In this sense therefore I may say that 100 parts common salt contain in round numbers 53 parts alkali and 47 of acid.

Hence 100 parts mere soda take up 73,41 of real marine acid, or 88,74 of the aqueous acid, and then afford 188,74 of common salt. And 100 parts of the aqueous acid should take up 112,688 of of ſoda, and afford 212,688 of common ſalt, and 100 grains real marine acid ſhould take up 136,31 of ſoda, and afford 257,2 of common ſalt.

100 grains of the aqueous acid contain 15,33 of water.

According to Wenzel 131,5 of ignited common ſalt contain 71,5 of alkali, and 60 of his ſtrongeſt marine acid; conſequently 100 grains common ſalt ſhould contain 54,3 of alkali, and 45,7 of that acid. And 100 parts mere ſoda ſhould afford 184 nearly of ignited common ſalt. This ſtatement differs very little from mine, and from Weigleb's ſtill leſs, for he found 100 parts common ſalt to contain 53,5 of alkali, and 46,5 of acid, and 100 parts ſoda ſhould take up 87,5 of acid, and afford 187,5 of common ſalt.

But Mr. Bergman's ſtatement differs widely from the foregoing, both Wenzel and I have found the alkaline part to exceed the acid, he on the contrary found the acid to exceed by much the alkaline, for to 100 parts common ſalt he aſſigns 52 of acid, 6 of water and only 42 of alkali. From the great reſpect I have ever entertained for this excellent man, this circumſtance always gave me much uneaſineſs. To inveſtigate the truth by direct experiment otherwiſe than was always done appeared difficult. I therefore endeavoured to diſcover it by an *indirect* experiment, namely, by finding how much cauſtic ſoda might be obtained from the decompoſition of a given quantity of common ſalt this

decompoſition

decompofition I effected by tartarin, but the exact feparation of the foda from the fylvian was fo difficult that I defpaired of obtaining fatisfaction in that w y: luckily, however, a more patient and fkilful experimenter, Mr. Hahneman has fince performed this experiment, and found that 11 parts mere tartarin were requi te to feparate 7 of *mere* foda from common falt.* We may the e-fore now examine with which of the two oppofite ftatements this proportion is beft fuited.

By my determination 7 grains foda enter into the compofition of 13,21 of common falt, and this quantity of common falt contains alfo 5,13 grains real acid, which muft be taken up by the tartarin to fet the 7 grains of foda free. Now fince 100 parts tartarin take up 56,3 of real marine acid, 9,12 of tartarin fhould take up 5,12 of this acid, which falls fhort of Hahneman's refult by 1,88 grains. But it is well known that fomewhat more of any divellent agent muft be applied to effect an *intire feparation* of any principle than would be neceffary to faturate that principle if it were in a free difengaged ftate.

By Bergman's determination 7 grains of foda enter into the compofition of 16,66 of common falt, and this quantity of common falt contains alfo 8 of real marine acid, now, as according to him 100 parts tartarin take up 51,5 of the ftrongeft or real marine acid, 15,53 would be requifite to take up 8 of that acid, which exceeds Hahneman's refult by 4,53 grains, whereas by the above reafon it fhould rather fall fhort of it.

* 2 Chy. An. 1797, p. 396.

But there are two other experiments which fet the inaccuracy of his determination in a ftill clearer light, the one executed by Mr. Wolfe, aud the other by Dr. Black †.

Mr. Wolfe found that 120 parts muriated filver or luna cornua, when decompofed by tartarin, afforded 55 grains of fylvian or muriated tartarin; thefe 55 grains therefore contained all the acid that exifted in 120 of muriated filver. Now Dr. Black found that 235 grains of muriated filver contain all the acid that exifts in 100 grains of common falt, and confequently 120 grains of the muriated filver contain all the acid that exifts in 51,06 of common falt, whence it follows that 55 grains of fylvian and 51,06 of common falt contain the *fame quantity of acid*, fince the firft received and the latter gave out all the acid that exifts in 120 parts muriated filver.

We may now fee in which of the 2 different ftatements this equality is found, or whether in neither or in both.

1ft. According to Bergman 100 grains of muriated tartarin contain 31 of real acid, then 55 grains of that falt fhould contain 17,05.

Again, 100 grains of common falt contain by his ftatement 52 of real acid, then 51,06 of this falt fhould contain 27,55; thefe quantities are evidently very diftant from an equality.

2d.

† Phil. Tranf. 1776, p. 611. 3 Edinb. Tranf. 116.

2d. By my ſtatements 100 parts ſylvian contain 36 of real acid, then 55 parts of this ſalt ſhould contain 19,8; alſo 100 parts common ſalt contain 38,88 real acid, then 51,06 parts of this ſalt ſhould contain 19,85.

Section 3d.

MURIATED BARYTES.

The proportion of ingredients in this ſalt may be inveſtigated from the following facts:

1ſt. Klaproth found that 73 grains of aerated native barytes (which contained an inconſiderable proportion of ſtronthian) ſaturate 100 grains of muriatic acid, whoſe ſp. grav. was 1,140 diluted with 200 grains of water, and that 100 grains of aerated barytes contain 22 of fixed air, 2 Chy. An. 1793, p. 195 and 196, and 1 Klapr. 269, therefore 73 grains of aerated barytes contain 56,94 of barytic lime.

2dly, He found that 56,59 pure aerated barytes diſſolved in this acid afforded 6850 of cryſtallized muriated barytes. 2 Klapr. 84.

Then 100 grains of aerated barytes, or 78 of mere barytes, would give 21,04 of muriated barytes. And 100 grains of mere barytic earth ſhould give 155 nearly of cryſtallized muriated barytes.

According to Fourcroy, 4 An. Chy. 71, 100 grains native barytes afford but 112 of deſiccated muriated barytes; yet Pelletier tells us, that 100 grains native aerated barytes afforded him 138 of cryſtallized muriated barytes, but moſt probably it retained ſome of the mother liquor.

Hence I deduce, 1ſt, that as 100 grains muriatic acid, 1,140 contain 18,11 real acid, 56,94 of barytes took up that quantity.

Consequently 100 *grains mere barytic earth* take up 31,8 of real marine acid, and afford 155 of cryſtallized muriated barytes.

And 100 *grains real marine acid* ſhould take up 314,46 of barytes.

We may alſo remark, that the muriatic acid whoſe denſity is 1,140, being mixed with twice its weight of water, will have its ſp. grav. 1,0427 which is nearly the ſame as that which Fourcroy found beſt adapted to ſuch ſolution, namely, 1,0347; and perhaps if the temperature were equal would approach each other ſtill more nearly. It appears then that the real acid ſhould be accompanied with 16 times its weight of water.

2dly, It follows, that 121,04 parts cryſtallized muriated barytes contain 78 earth, 24.8 acid, and 18,24 water, conſequently 100 *parts of the cryſtallized* ſalt contain 64,44 earth, 20.45 acid, and 15,06 water.

And 100 *grains of the deſiccated* contain about 70 of earth, 22 of acid, and 8 of water.

(Per

(Per Crawford, quoted by Schmeiſſer in Phil. Tranſ. 179, 421, muriated barytes is nearly as ſoluble in hot as in cold water, and three times leſs ſoluble than muriated ſtronthian.)

To confirm this concluſion I muſt add, that having precipitated a ſolution of 100 grains of cryſtallized muriated barytes by a ſolution of nitrated ſilver, I found the precipitate duly dried to weigh 118 grains, which as we ſhall preſently ſee argues the preſence of 19,51 of real marine acid. I alſo found that 100 parts muriated barytes expoſed to a heat of 300° for two hours, loſt 16 grains of water of cryſtallization, hence we may rate in round numbers the proportion of ingredients in this ſalt, at 64 of earth, 20 of acid, and 16 water of cryſtallization.

Section 4th.

Muriated Stronthian.

Klaproth obſerved, that 55 grains of native mild ſtronthian ſaturated 100 of marine acid, whoſe ſp. grav. was 1,140, this being diluted with 50 grains of water, 100 grains marine acid of this ſp. grav. contain, computing from my table 18,11 grains of real acid, and 55 grains mild ſtronthian, (at the rate of 69 per cent.) contain 37,95 of mere earth.

Hence

Hence I conclude, that 100 grains mere ſtronthian earth take up 47,79 of real acid (ſince 37,95 take 18,11 of real acid) and would afford, as we ſhall preſently ſee, 254,84 of cryſtallized muriated ſtronthian, or 147,79 of deſiccated ſtronthian*.

And 100 grains real marine acid enter into the compoſition of 209 grains of deſiccated ſtronthian, or of 360 of the cryſtallized.

Again, Dr. Hope found, that 100 *grains cryſtallized muriated ſtronthian* contain 42 *of water of cryſtallization*, and conſequently 58 of deſiccated which contain earth and acid in the proportion above mentioned (or 100 earth to 47,79 acid) that is, 39,24 of earth and 18.76 of acid, this proportion agrees very exactly with that obſerved by Pellitier †, for he found 100 grains of native aerated ſtronthian (which contain 69 of earth) to afford 176 of cryſtallized muriated ſtronthian.

And ſince, in Dr. Hope's experiment, 39,24 of this earth afforded 100 grains of muriated ſtronthian, 69 ſhould afford 175,8. Some experiments however of Mr. Lowitz vary conſiderably from the above ſtatements, it appeared to him that in muriated ſtronthian the quantity of acid exceeded that of earth in the proportion of 54 to 46 ‡; if ſo, 100 grains of muriatic acid of the ſp. grav. 1,140 ſhould contain 44,54 of real acid, for it took up 37,95 of earth

* 2 Chy. An. 1793, p. 194. † 21 An. Chy. p. 128. ‡ 1ſt. Chy. An. 1796, p. 128, 129.

earth in Klaproth's experiment already quoted, which is inconſiſtent with the proportion of real acid. I have found in muriatic acid in a multitude of experiments, and contrary to all analogy, as we ſee that by barytes and fixed alkalis betwixt which this earth undoubtedly ſtands, take up leſs than their own weight of real marine acid: it is alſo contradicted by Pellitier's experiment, for ſince 100 grains native aerated ſtronthian contain 69 of earth, theſe at the rate of 46 to 54 ſhould take up 80 grains of real muriatic acid, and the ſum of both would be 149 grains: and ſince by Dr. Hope's experiment 58 grains of united earth and acid take 42 grains of water of cryſtallization, 149 grains ſhould take 107; and hence inſtead of 176 grains of cryſtallized muriated ſtronthian we ſhould have 256 grains from 100 of aerated ſtronthian.

Klaproth informs us, that from a ſolution of 100 grains of aerated ſtronthian in muriatic, precipitated by the addition of concentrated vitriolic acid, as long as any precipitate appeared, he obtained no more than 114 grains of vitriolated ſtronthian, and that dried only in air*; whereas the precipitate ſhould amount, if the whole of it were obtained, to 118 grains; for ſince 58 grains of this earth, as he elſewhere relates, † afford 100 of vitriolated ſtronthian, 69 ſhould afford 118; it is plain therefore that the ma-

rine

* 2 Chy. An. 1793, p. 200. † 2 Klapr. p. 97.

rine acid retained ſome, or that a ſufficiency of the vitriolic acid was not added. This earth is not therefore a proper teſt of vitriolic acid, at leaſt not as proper as the barytic.

To obtain a leſs circuitous proof of the proportion of ingredients in 100 parts of this ſalt, I precipitated a ſolution of 100 grs. of cryſtallized muriated ſtronthian by mild ſoda; the precipitate after ignition weighed 56,75 grains, but theſe being diſſolved in marine acid gave out 17 grains of fixed air, and therefore contained only 39,75 of mere earth.

2dly. I precipitated a ſolution of another 100 grains of this cryſtallized ſalt by a ſolution of nitrated ſilver, and found the precipitate duly dried to weigh 110 grains, a weight which indicates the preſence of 18,19 grains real marine acid. The weight of the 3d ingredient, namely water, muſt therefore amount to 42,06 grains nearly, as Dr. Hope has ſtated.

HENCE we may rate the proportion of ingredients in 100 parts of this ſalt at 40 of earth, 18 of acid, and 42 of water. And to 100 parts of the deſiccated ſalt we may allow about 69 of earth and 31 of acid.

HENCE 100 *parts ſtronthian earth* take up 45 or more, exactly 46 of real marine acid, and ſhould *afford* 250 of cryſtallized, or 145 of deſiccated muriated ſtronthian. And 100 *parts real marine acid*

acid ſhould take 222, or more exactly 216,21 of ſtronthian earth, and *afford* 540 of cryſtallized, or 313,5 deſiccated muriated ſtronthian.

Section 5th.

MURIATED LIME.

IN my experiment already mentioned 158 grains of powdered Carrara marble were ſaturated by 402 of muriatic acid, whoſe ſp. grav. was 1,1355, which contained 17,5 per cent. real acid; therefore 402 grains of it contained 70,55 real acid. The 158 grains marble (at the rate of 53 per cent.) contained 83,74 of lime. Then 83,74 grains lime took up 70,55 of real marine acid. To effect a ſaturation a heat of 160° was employed towards the end of the ſolution.

HENCE 100 *grains of lime* would ſaturate 84,488 of real marine acid. And 100 *grains real marine acid* would ſaturate 118,3 of lime.

IN Wenzel's experiment the acid was not ſaturated, and hence the reſult differs from that of mine. To 240 grains of his ſp. of ſalt he added 120 grains of fragments of purified oyſter-ſhells (which, as we have already ſeen in treating of ſelenite, contained 52,8 per cent. of lime,) and at the latter end expoſed them

 to

to a gentle heat, and when no fenfible folution appeared he feparated what remained undiffolved, and found that after wafhing and drying it, it weighed 19,625 grains; hence he concluded that 100,375 grains of thefe fhells were diffolved; but then he had no reafon to think the acid was faturated, or that in a longer time it would not take up more, efpecially as the fhells were not in a fine powder, nor did he apply any teft as I did. Having evaporated the folution to drynefs and heated the dry mafs to fufion, he found it to weigh whilft ftill red hot 106,125 grains.

This fhews the folution not to have been faturated, for 100,375 grains of the fhells contained 53 of lime, and the 240 grains fp. of falt contain 54 of real acid by his own account; therefore, as faturated muriated lime lofes no acid in a melting heat, the falt fhould weigh even by his eftimation 107 grains, and by my calculation 112 grains; the remainder therefore of the unfaturated acid was expelled by the heat of fufion.

According to him 100 grains lime fhould take up 102 grains of the ftrongeft marine acid.

It muft be remarked, alfo, that this falt though in a melting heat ftill retains fome water, and Wenzel's experiment fhews how much; for by my determination 53 grains lime take up only 44,75 of real acid; and the fum of the ingredients in Wenzel's experiment amounts only to 97,75 grains; yet he found the weight 106,125; then 8,375 grains were water.

Then

THEN 100 grains muriated lime, weighed red hot, contain nearly 50 of lime, 42 of acid and 8 water.

BERGMAN agrees with me ſo far as ſtating the proportion of lime in this ſalt to be ſuperior to that of acid; to 100 parts of this ſalt he aſſigns 44 of lime and 31 of acid, but the proportion of acid is higher, for to 44 of lime 37 of acid appertains, by the proportion above ſtated then that of water is 19.

Note. His ſalt was weighed at far a lower temperature than Wenzel's, and hence the quantities but not the proportions in 100 grains of it are altered, as it powerfully attracts water.

Section 6th.

MURIATED MAGNESIA.

THE proportions of acid and baſis in this ſalt are difficultly determined, as it powerfully attracts moiſture and eaſily loſes its acid if ſtrongly heated, and without ſuch heat will retain much water.

IN my experiments it appeared that 100 grains mere magneſia took up 215,8 of ſtandard, or 111,35 of real marine acid.

AND 100 grains real marine acid take up 89,8 of mere magneſia.

Klaproth * found 420 grains of muriated magnefia evaporated to drynefs to contain 290 of magnefia; as it was precipitated by foda he probably meant mild magnefia, which generally contains but 0,45 of earth; if fo, 290 contained but 130,5 of mere magnefia; confequently 100 grains of muriated magnefia gently but fenfibly dried fhould contain 31,07 mere magnefia, and this by my computation fhould take up 34,59 of real acid. The remainder is therefore water.

Wenzel's experiments accord with mine with refpect to the fuperiority of the proportion of earth to that of acid in a given weight of muriated magnefia. According to him 100 grains of mere magnefia take up 122 of real marine acid; but by my computation of the quantity of real acid in his fp. of falt, *v. z.* 23 per cent. allowing his mild magnefia 45 per cent. of earth, 100 grains of it fhould take up 115,8 real muriatic acid.

Bergman's refults differ from thefe very widely, for according to him 41 grains mere magnefia take up only 34 of the ftrongeft marine acid.

Section

* 1 Klapr. 369.

Section 7th.

Muriated Silver.

It is now well known from the experiments of Margraff, Bergman, Klaproth, Wolfe, Wenzel, &c. to which I need not add my own, that 100 grains of muriated ſilver contain very nearly 75 of ſilver when dried in a heat of 80°, or 75,235 when heated more but not fuſed, as in Wenzel's and Wolfe's experiments; but it muſt not be inferred that the remaining 25 grains are mere marine acid, for ſilver diſſolved in nitrous acid takes up 10,8 per cent. of oxygen; therefore 75 grains of it take up 8,1, which ſubſtracted from 25, leaves the quantity of acid 16,9; or if the muriated ſilver were much heated, the acid and oxygen would amount only to 24,76; and deducting the oxygen, the acid ſingly would be 16,6 grains; this agrees exactly with Wolfe's experiment, for he found as already ſaid that 120 grains of this metallic ſalt decompoſed by tartarin afford 55 of muriated tartarin. Now 120 grains contain by this computation 19,92 of real acid; and as 100 grains muriated tartarin contain 36 of real acid, 55 grains of it ſhould contain 19,8; the difference is inſignificant.

Hence 100 grains ſilver take up 22,133 of real marine acid, and afford 133 of muriated ſilver by the addition of oxygen.

And 100 grains real marine acid unite to 451,87 of ſilver, and afford 602,4 of muriated ſilver.

100 grains pure cryſtallized common ſalt precipitate from a ſolution of nitrated ſilver 233,5 grains of muriated ſilver by Klaproth's, 235 by Dr. Black's, and 237 by Arrhenius's experiments *; Dr. Black's is a medium between both; the difference ariſes only from the degree of deſiccation.

100 grains of muriated tartarin ſhould produce 216,86 of muriated ſilver.

Section 8th.

MURIATED LEAD.

THIS ſalt may be obtained in two ſtates, either in acicular cryſtals or thoroughly deſiccated. The proportion of ingredients in each I deduce from the following facts:

1ſt. KLAPROTH having diſſolved 100 grains lead in dilute nitrous acid, and precipitated the lead by cauſtic tartarin, found the precipitate ſharply dried until it began to grow yellow, to weigh 115 grains. 1 Klaproth, 274.

2d. HAVING precipitated a ſolution of 100 grains of lead in nitrous acid by dropping muriatic acid as long as any precipitate appeared, and evaporated the whole to dryneſs in a ſand heat, he found the muriated lead to weigh 133 grains. Ibid.

3d.

* Mem. Stock. 1785.

3d. He alſo found that 22,5 grains of cryſtallized acicular muriated lead, well drained and dried by expoſure to the air, contained 16 grains of metallic lead, therefore 100 grains of ſuch cryſtals ſhould afford 71,11 of metallic lead.

First, to theſe facts I muſt farther add, that in muriated lead, whether cryſtallized or deſiccated, the lead is in a calcined ſtate.

Hence I infer, that ſince 100 grains of metallic lead give 133 of calx of lead, the 71,11 grains of metallic lead in 100 parts cryſtallized muriated lead amount to 81,77 of calx of lead. The calx, including not only the metallic lead, but alſo oxygen and water, as we ſhall preſently ſee; the remainder therefore is real marine acid, amounting to 18,23 grains.

Again, as 133 grains of the thoroughly deſiccated muriated lead contain 100 of metallic lead, 100 grains of this muriated lead muſt contain 75,12, but 75,12 metallic lead form 83 of calx; the remainder therefore muſt be real marine acid = 17 grains.

These concluſions are farther confirmed by the experiment of Mr. Wolfe. Phil. Tranſ. and 10 Roz. 370. Having decompoſed 120 grains of muriated lead dried by expoſure to the air by a ſufficient quantity of tartarin, he found them to produce 61 grains of muriated tartarin. Therefore both the 120 grains muriated

riated lead and the 61 grains of muriated tartarin ſhould contain the ſame quantity of real marine acid. Now if 100 grains muriated lead dried in air contain 18,23 real acid, 120 grains of it ſhould contain 21,87 real acid.

And ſince 100 grains muriated tartarin contain by my former determination 36 grains real acid, 61 grains of this ſalt ſhould contain 21,96; the difference is only 0,09 of a grain.

As to the 115 grains calx of lead produced in the precipitation of a ſolution of 100 grains of lead in nitrous acid by cauſtic tartarin, I have already ſhewn in the 2d vol. of my Mineralogy, p. 497, that 100 parts lead, when diſſolved in nitrous acid, take up 5,8 of oxygen*, therefore the remainder is water, = 9,2 grains.

Hence 100 parts metallic lead take up about 25,63 of real marine acid, and afford 140,62 of cryſtallized muriated lead, or 133,12 of the deſiccated.

And 100 grains real marine acid unite to 394,06 of metallic lead, and afford 548,64 of cryſtallized muriated lead.

And

* Fourcroy, 2 An. Chy. 213, ſtates the quantity of oxygen at 12,5 in 100 of muriated lead, but this is contradicted by the experiment of Mr. Wolfe, &c. He moſt probably means the muriated lead formed in the ſolution of a calciform ore.

And 100 parts cryſtallized muriated lead contain 81,77 calx of lead (= 71,11 metallic lead,) and 18,23 of real marine acid.

And 100 grains thoroughly deſiccated muriated lead contain 83 calx of lead (= 75,12 metallic lead,) and 17 of real marine acid.

According to Wenzel, 100 grains metallic lead ſhould afford 137,5 of deſiccated muriated lead; he probably dried it ſomewhat leſs than Klaproth had done. The proportions of lead and acid he could not well determine, the exiſtence and proportion of oxygen not being known when he wrote.

Note. The quantity of metallic lead obtained from 100 parts cryſtallized muriated lead by fuſion with black flux is much ſmaller than that above ſtated. (ſee 1 Klapr. 171,) as much is retained by that flux. Yet ſee 3 Weſtrumb. Phyſical and Chem. Abhandl. 383.

Of Aerated Vol-Alkali and Ammoniacal Salts.

The former experiments which I made with a view of aſcertaining the proportion of ingredients in theſe ſalts were defective in ſeveral reſpects:

1ſt. For want of a due eſtimate of the quantity of mere vol-alkali in a given quantity of aerated alkali, the ſubſtance to be ſaturated with the three other mineral acids. Dr. Prieſtly's experiments, the baſis of the eſtimate I then formed, not exhibiting the temperature and preſſure of the atmoſphere when the volumes of fixed and alkaline airs were combined, afforded an opportunity for forming rather an approximation than an accurate determination of their ſeveral weights.

2d. I was not then aware of the difficulty of finding the exact point of ſaturation of the aerated vol-alkali with the mineral acids; a difficulty however mentioned by Macquer *, and ſo great that Du Hamel judged it impoſſible to vanquiſh it †. Wenzel very ſagaciouſly abſorbed the exceſs of acid by oyſter ſhells, but in my mode of experimenting this teſt could not be applied; hence there was an exceſs of acid in all of them. Theſe errors induced me to analyze rather than compoſe theſe ſalts.

Of Aerated Vol-Alkali.

By diſtilling 100 grains of aerated vol-alkali with 300 of dry ſlacked lime in a pneumatic apparatus and a ſand heat I obtained 129 cubic inches of alkaline air, barometer 30,2, and thermometer at

* Macquer's Elem. 389, Engliſh. † Mem. Par. 1735, p. 664, in 8vo.

at a medium 62,5. 100 grains of alkaline air weigh 18,16 grains, as I have ſhewn in a former treatiſe, barometer 30, thermometer 61. Then at that barometrical height 129 cubic inches would become 130; but as the heat in the preſent experiment exceeded 61, the expanſion reſulting from it muſt be ſubſtracted; and according to Mr. Morveau, 2 An. Chy. a volume of this air at 32° being taken as 1 becomes at 77° 1,2791, and conſequently gains 0,0062 by each intermediate degree, conſequently the volume of this would at 61 be only 129,1; its weight therefore is nearly 24 grains. This ſalt contained 52 per cent. of fixed air, conſequently its ingredients were 52 grains fixed air, 24 of mere alkali, and 24 of water.

The proportion of vol-alkali in aerated vol-alkalis vary, increaſing or decreaſing with the proportion of fixed air they contain.

Mr. Cavendish in the Philoſophical Tranſ. for 1766, p. 169. found that 1643 grains of aerated vol-alkali, containing 53,8 per cent. of fixed air, ſaturated the ſame quantity of marine acid as 1680 of another parcel, which contained but 52,8 per cent. of fixed air.

Hence the quantities of mere alkali in each were reciprocally as 1680 to 1643, and theſe are nearly to each other as 53,8 to 52,8; and as the aerated vol-alkali that contained 52,8 per cent. of fixed air contained 24 per cent. of mere vol-alkali; that which contained 53,8 per cent. of fixed air ſhould have contained 24,83 per cent.

HENCE the *proportion of fixed air in aerated vol-alkalis is* to that of mere alkali in thoſe ſalts *as* 13 *to* 6, and the remainder is water of compoſition.

WENZEL, p. 100, alſo perceived that the proportion of mere alkali in aerated vol-alkali was very ſmall, and ſtates it nearly as low as I do; for to 240 grains of this ſalt, containing 53,75 per cent. of fixed air he aſcribes 129 of fixed air, 31,125 of water, and conſequently 79,875 of mere alkali. Hence 100 grains ſhould contain 53,75 fixed air, 33,28 of alkali and 12,97 of water.

COMMON SAL AMMONIAC.

BY diſtilling in a pneumatic apparatus and a ſand heat, 100 grains of ſublimed ſal ammoniac and 300 grains of quick lime, I found it to yield as much alkaline air as amounted to 25 grains, with ſome few drops of water; the remainder of the water being probably detained by the lime or by the muriated lime which is known to retain water moſt obſtinately.

BY treating 100 parts of this ſalt in ſolution with a ſolution of nitrated ſilver, I found it to afford 258,5 of muriated ſilver heated to fuſion, and conſequently to contain 42,75 of real marine acid.

HENCE

Hence 100 parts of this ſalt contain 42,75 of real marine acid, 25, or making allowance for loſſes, 28 of mere vol-alkali, and 29,25 of water of cryſtallization and compoſition.

Hence 100 parts mere vol-alkali take up 152,68 of real marine acid, and ſhould afford, if there were no loſs, 357,14 parts of ſublimed ſal ammoniac. And 100 parts marine acid take up 65,4 nearly of mere volalkali, and ſhould afford 233,9 parts of ſublimed ſal ammoniac; but in ſubliming ſal ammoniac there is always ſome loſs.

Mr. Cavendiſh, in the Philoſophical Tranſactions for 1766 tells us, that 168 parts aerated vol-alkali, containing 52,8 per cent. of fixed air, ſaturated as much marine acid as 100 grains of marble, which contained 40,7 per cent. of fixed air; now 100 grains of this marble contain, by the analogy formerly given, (45 of fixed air to 55 of lime) 50 grains of lime, by the 2d table, take up 42,2 of real marine acid, and 100 grains of the aerated vol-alkali there mentioned, contain 24 per cent. of mere alkali, and conſequently 168 grains of it ſhould contain 40 of mere alkali, which by the above ſtatement would require for ſaturation 61 of real marine acid. This experiment would have made me doubt of the propriety of the above concluſions, had not Mr. Cavendiſh expreſsly ſtated that his ſolution of marble was ſaturate, (and conſequently as a ſaturate ſolution cannot be obtained without heat, which he did not apply, he muſt have added an exceſs of marble, and judged the ſolution

ſolution ſaturate when no more air was expelled) and on the other hand he tells us, that the alkaline ſolution contained an exceſs of acid, and this exceſs exiſting in every particle of a large ſolution muſt be conſiderable.

In the experiment related in my laſt paper, I ſtated that 100 grains of aerated vol-alkali were ſaturated by 246 of marine acid, whoſe ſp. grav. was 1,1355, which appears by the firſt table to contain 17,5 per cent. real acid, and conſequently the quantity in 246 grains was 43 grains; on the other hand, the vol-alkali, containing but 43 per cent. of fixed air, contained, by my actual experiments, only 19,85 grains of mere alkali, and this quantity ſhould take up but 30 of real marine acid. Hence in my former experiments there was an exceſs of 13 grains of acid, which made the ſp. grav. equal to that of the teſt ſolution, and thus induced me to think the quantity of ſal ammoniac formed greater than it really was.

Wenzel found 168,4 grains of vol-alkali, containing 53,75 per cent. of fixed air, to require 240 grains of his ſp. of ſalt to ſaturate them, and this quantity of his marine acid we have already ſeen to contain 55,17 of real acid, and 168,4 of the aerated alkali contained, by the analogy already ſtated, 41,71 of mere vol-alkali, the ſum of both was 96,88; yet having evaporated the ſolution to dryneſs, and expoſing the reſiduum to a heat of 212° for four hours, he found

found it to weigh 110,125 grains, as he knew 55 of theſe to be acid (or according to him 54), he naturally ſuppoſed the remainder to be vol-alkali; hence according to him 100 parts of ſal ammoniac thus dried contain 49 parts of acid and 51 of vol-alkali. The difference between us ſeems to ariſe from the loſs always experienced during evaporation, and if this had not happened, the dry reſiduum would have amounted to 128 grains; as to the quantity of vol-alkali he had no method of eſtimating it.

Cornette perfectly decompoſed 2304 grains of ſal ammoniac by an equal quantity of lime, which he ſlacked after weighing it, examining the reſiduum, he threw it on a filter, and edulcorated it with repeated effuſions of water, and what remained undiſſolved he found to weigh, when dry, 756 grains, and hence he judged the remainder, viz. 1548 grains to have been diſſolved by the acid of the ſal ammoniac, and to confirm this concluſion, he precipitated the ſolution which had paſſed the filter with a fixed alkali, and drying the precipitate, found it to weigh 1542 grains*; whence it ſeems to follow, that the acid contained in 2034 of ſal ammoniac had diſſolved 1542 of lime, whereas, by my calculation, it ſhould diſſolve but 1272,46 of lime, for ſince 100 grains of ſal ammoniac contain 42,75 of real marine acid, 2304 ſhould contain 1008; and ſince by the third table 100 grains real marine acid take up 118,3 of lime, 1008 ſhould take up but 1272,46 of lime.

But

* Mem. Par. 1786, p. 533.

But the lime I ufed was pure and perfectly free from fixed air; can that be faid of the common lime of Marly, which he employed and does not fay he had prepared? Befides, by his edulcorations, much pure lime muft have been diffolved, and have mixed with the folution of muriated lime, and if his alkali were not cauftic, the quantity of lime precipitated by it muft have been at leaft partially aerated, and confequently the mere earthy part apparently greater than it would have been if pure. However, as this experiment forms a cumulative proof both of the proportion of acid contained in fal ammoniac, and of the quantity of it taken up by a given weight of lime, I thought it incumbent upon me to repeat it, hence I mixed 50 grains of fal ammoniac with 150 of flacked lime, and heated the mixture in a large glafs phial until all the alkali was driven off and the mixture ceafed to fmell, I then added a fufficient proportion of water, and digefted the whole in a gentle heat for fome hours, then filtered and edulcorated the mafs on the filter, as I judged the folution to contain lime as well as muriated lime, I paffed a ftream of fixed air into it, which inftantly turned it milky, and then filtered it off; the folution now free from lime I precipitated by a folution of an aerated foda, which contained 17 per cent. of fixed air, as much of the folution was requifite as contained 123 grains of foda. The precipitate collected, edulcorated and dried for fome hours on the filter, in a heat of 150°, weighed 46,75 grains, though no more could be feparated than 41,62, thefe after ignition weighed 35 grains, fome ftuck to the glafs glaſs, and 5,25 remained in the filter; 123 grains of the ſoda gave out 20,91 of fixed air, and, as I afterwards found, kept about a a grain of the lime in ſolution, now 21 grains of fixed air are abſorbed by 23,44 of lime, this then was the quantity of lime taken up by the acid contained in 50 grains of ſal ammoniac, that is, 21,37 real marine acid, whereas by my calculation, ſince 100 grains marine acid take 118,3 of lime, 21,37 ſhould take up 25,28, the difference is 1,84 grains, and even this I believe to proceed from the whole of the ſal ammoniac not having been decompoſed, 19,8 grains of the acid appear to have been taken up by the lime, and about 3,6 of the ammoniac eſcaped decompoſition, this alſo clearly appears by the action of the ſoda, for 100 grains of this ſoda contain 22 of mere alkali, then 123 grains of it contains 27; as 100 grains mere mineral alkali take up 73,41 of marine acid, then 27 ſhould take up 19,82.

Hence we ſee that in Rigour 100 parts ſal ammoniac may be decompoſed by 100 parts chalk, for 100 parts chalk generally afford 42 of fixed air, and conſequently contain 51,3 of lime, and 100 parts ſal ammoniac contains 42,75 real acid, and ſince 100 grains real marine acid are ſaturated by 118,3 of lime, 42,75 of this acid require but 49,57 of earth; but in all ſuch caſes the medium of decompoſition is always taken in greater quantity than is abſolutely requiſite, otherwiſe the mixture would never be perfect, and in this caſe part of the ſalt might ſublime without decompoſition; hence 200 parts chalk are moſt commonly uſed, though 125 are ſaid to be ſufficient. *Doſſie Elab. laid open* 110, 1 *Labor. in Groſs* 68, in note per *Weigleb.* and in effect 125 grains chalk, at the above rate, would furniſh 52 grains of fixed air, which would ſaturate 24 of vol-alkali, and the ammoniac contains a ſufficiency of water.

Hence alſo we ſee how it happens that 100 parts ſal ammoniac decompoſed by 200 parts chalk frequently afford 89, nay, according to Baumé, even 94 parts aerated vol-alkali, for if there were no loſs 125 parts of chalk were ſufficient, but then this large quantity of fixed air is expelled, not by the acid of the ſal ammoniac, but by the heat applied, as Pellitier de la Sale has noticed, 2 Pharmacopie de Londres 427, and on this account magneſia, as it parts with its fixed air much more eaſily, and contains more water, affords a quantity of aerated vol-alkali, when uſed as a medium for decompoſing ſal ammoniac, nearly double that of the ſal ammoniac employed. Thus Weſtrumb from 100 grains of ſublimed ſal ammoniac and 300 of magneſia obtained 193 grains aerated vol-alkali, 2 Chy. An. 1788, p. 15; his magneſia muſt have contained a very large proportion both of fixed air and water, for he ſays that 1920 grains of it being calcined left only 600 of earth, ibid. 17.

Hence alſo, Dolfuz having treated 100 parts ſal ammoniac with 125, and even with 200 of chalk, in a glaſs retort, obtained no more than 50 of aerated vol-alkali; the ſame thing happened when he uſed an earthen retort, as he ſimply heated it to redneſs, whereas a ſtrong white heat is requiſite to expel fixed air from chalk, 2 Crell. Beytr. 199. I believe unpurified ſal ammoniac would yield more aerated vol-alkali than the purified, on account of the oil it contains, which affords fixed air. Another certain proof that 125 grains chalk are not acted upon by the acid contained in the 100 parts ſal ammoniac, but contribute to the increaſed quantity of aerated alkali merely by the fixed air expelled from them by heat, is that the reſiduum contains ſome calcareous earth which the acid had not attacked, as Richter has obſerved, 1 Stock. 2 Theile 98 and 99.

SEVERAL important deductions may be deduced from the knowledge of the compoſition of ſal ammoniac, for inſtance, an eaſy explanation of its great refrigerating power, &c. which being improper for this place, I omit.

VITRIOLIC AMMONIAC.

100 grains of cryſtallized vitriolated vol-alkali and 300 dry ſlacked lime, pneumatically diſtilled in a pneumatic apparatus and a ſtrong ſand heat, Bar. 30,2, Therm. 66°, afforded 78,41 cubic inches of alkaline air, = 14,24 grains.

From a ſolution of vitriolated vol-alkali, precipitated by a ſolution of muriated barytes, 164 grains of ignited baroſelenite were obtained, hence the ſalt contained 54,66 grains real vitriolic acid.

Hence 100 grains vitriolated vol-alkali contain 14,24 of mere vol-alkali, 54,66 of real acid, and 31,1 of water.

In my former paper I ſtated the quantity of vitriolic acid in 100 grains of cryſtallized vitriolated vol-alkali to be 62,47 ſtandard, = 55,7 real acid, the variation is not conſiderable, but of the alkali I could not then form a proper eſtimate.

Hence 100 parts mere vol-alkali take up 383,8 of real vitriolic acid, and *afford* 702,24 of vitriolated volalkali.

2dly, 100 parts real vitriolic acid ſhould take up 26,05 of mere vol-alkali, and *afford* 182,94 of vitriolated vol-alkali.

According to Wenzel, alſo, 100 parts vitriolic ammoniac contain 58,8 of real acid, hence of all cryſtallized ſalts it contains the greateſt proportion of this acid, as Glauber does the leaſt.

Nitrated Vol-Alkali.

From 50 grains of cryſtallized nitrated vol-alkali, mixed with twice its weight of ſlacked lime, I obtained, in a pneumatic apparatus, 40 cubic inches of alkaline air, Bar. 30,06, Therm. 61°, by by the ſimple heat of a candle; ſome water alſo paſſed, which undoubtedly abſorbed ſome air, a greater heat could not be applied without riſking a decompoſition of the alkali itſelf; hence 100 grains of this ſalt would yield 80 of air, which in theſe circumſtances would weigh 14,52 grains. In another experiment I obtained ſtill leſs of this air, for 50 grains of this ſalt afforded only 34,962 cubic inches, the barometer indeed ſtood higher, namely at 30,26, and the thermometer only at 58.

Finding this method inadequate to the diſcovery of the exact quantity of vol-alkali in this ſalt, I tried the effect of ſpontaneous evaporation on a mixture of this ſalt with lime and water, but ſoon found the quantity evaporated ſo great that it was very evident it did not proceed from the mere volatilization of the alkaline part, but in a great meaſure from that of the water alſo, hence I was obliged to content myſelf with detecting the proportion of the acid part.

For this purpoſe I made a ſolution of 400 grains cryſtallized nitrous ammoniac, and to this added a ſmall proportion of a ſolution of tartarin ſlightly aerated; as the point of ſaturation could not be aſcertained by any teſt, I added but little of the tartarin, and ſet the liquor to evaporate in a very gentle heat. The next day I found ſome cryſtals of nitre, which I carefully picked out, waſhed and dried, then added more tartarin to the mother liquor, ſet it to evaporate and cryſtallize as before. Thus I proceeded for

ſeveral

ſeveral days and at laſt obtained 412 grains cryſtallized, well dried nitre. Now 412 grains nitre contain, by my account, 181,28 grains real nitrous acid, this quantity therefore exiſted in 400 grains of the nitrous ammoniac, conſequently 100 grains of this ſalt ſhould contain 45,3 of real nitrous acid.

THERE are however ſtrong reaſons to think that this ſalt contains much larger proportion of acid; for in the firſt place the ſalt volatilizes without decompoſition with the water that holds it in ſolution, as Berthollet obſerved in an experiment I ſhall preſently relate, and conſequently it is reaſonable to ſuppoſe that ſome eſcaped this way in my experiment, and moreover nitre is itſelf in ſome meaſure volatile during the evaporation of its ſolution, and laſtly, both Wenzel, Cornette and myſelf found a larger proportion of acid taken up by vol-alkali during the combination of both.

IN my laſt paper I ſtated the proportion of ingredients in nitrous ammoniac at 24 vol-alkali 78,75 ſtandard, which quantity is equivalent to 57,8 grains real acid, but noticed that there was an exceſs of acid. At preſent all due corrections made from this experiment, I infer that 100 grains cryſtallized nitrous ammoniac contain 57 nitrous acid, 23 of vol-alkali and 20 of water.

HENCE 100 grains vol-alkali take up 247,82 of nitrous acid, and ſhould afford 435 of cryſtallized nitrated vol-alkali, if there were no loſs in evaporation or no decompoſition.

AND

AND 100 grains nitrous acid ſhould take up 40,35 of vol-alkali, and *afford* 175,44 of ammoniac, if no loſs &c.

AN experiment of my own, related in my laſt paper, ſeems to contradict theſe reſults, for I there ſtated that 200 grains aerated vol-alkali, which contained 50 per cent. of fixed air, and conſequently the whole, 46 of vol-alkali, having been ſaturated with nitrous acid, to have afforded 296 of nitrated ammoniac, whereas by calculating from the above ſtatements they ſhould afford but 200: but the reaſon is, that the maſs of ſalt then procured was not wholly cryſtallized, but contained much of the mother liquor and an exceſs of acid which increaſed its weight. The only object I had then in view was to ſhew that the weight obtained was leſs than could be expected from the theory I had then formed; for this purpoſe it was not neceſſary to puſh the deſiccation very far—a decompoſition alſo took place as will preſently be ſeen.

ACCORDING to Wenzel 240 grains of dry uncryſtallized nitrated vol-alkali contain 155,9 of his ſtrongeſt acid, 77,5 mere vol-alkali and 6,6 water: then 100 grains of this ſalt ſhould contain 64,95 acid, 32,29 vol-alkali, and 2,76 water. 123 grains of his aerated vol-alkali which contained 53,75 of fixed air, being ſaturated with nitrous acid, afforded him in one experiment 127 of nitrated vol-alkali, and in another 123; by my calculation, this quantity of vol-alkali ſhould afford 132,6 of the cryſtallized ſalt.

Cornette ſaturated 2304 grains of nitrous acid whoſe ſp. grav. was to that of water as 10 to 8, that is, 1,250 (he does not mention the temperature) with 1152 of an aerated vol-alkali extracted from ſal ammoniac by a fixed alkali (he does not tell how much air it contained), and evaporating to dryneſs obtained 1476 of uncryſtallized nitrated vol-alkali, Mem. Par. 1783, p. 748.

If the ſp. grav. of the acid were taken at 60° it would contain by my table 31,62 per cent. real acid, but if at 10° of Reuamur, as is uſual in France, it would contain 32 per cent. the concrete alkali being extracted by a fixed alkali which yields moſt, cannot be ſuppoſed to contain leſs than 52 per cent. of fixed air, and conſequently 24 per cent. of mere vol-alkali, then 2304 grains of his acid contained 737,28 real nitrous acid, and 1152 of the aerated vol-alkali contained 281,48 of mere vol-alkali; and if 737,28 real nitrous acid take up 281,48 of mere vol-alkali, 100 grains of the acid ſhould take up 38,2 nearly of vol-alkali which approaches nearly to my concluſion.

But as to the quantities of nitrated vol-alkali the difference is far greater; for if 737,28 grains of real acid ſaturated with vol-alkali afford 1476 of nitrated vol-alkali, 100 grains of this acid ſhould afford 200 of this ſalt; whereas by my computation it ſhould afford but 175,44.

These diſcordant reſults evidently ſhew that a decompoſition takes place in evaporating this ſalt in a heat even of 80°; the hydrogen

drogen of the vol-alkali partially decompoſes the nitrous acid, and converts it either into nitrous air, which by contact with the atmoſphere reforms nitrous acid, is reabſorbed, and attracting more moiſture forms the exceſs of acid and increaſe of weight which is ſometimes found; or the acid is ſo far decompoſed as to become *rudimental nitrous air*, which is the ſubſtance Dr. Prieſtly calls *dephlogiſticated nitrous air*, which refuſing all combination, flies off and occaſions a loſs of weight; ſometimes both changes take place.

Berthollet * diſtilled 1152 grains of dry nitrated vol-alkali in a hydro-pneumatic apparatus, conſiſting of a retort, two enfiladed receivers, and a jar to receive air, 1080 grains paſſed out of the retort into the receiver, conſequently 72 grains only remained in the retort.

The enfiladed receivers contained 619 grains of a liquor highly acid, and much *rudimental nitrous air* (what Dr. Prieſtly calls dephlogiſticated nitrous air) was produced, the weight of this or other air and water, produced and loſt, conſequently amounted to 461 grains, for 1080 — 619 = 461.

To diſcover the contents of the 619 grains of acid liquor he diſtilled it in a water bath, there remained in the retort 320 grains

 of

* Mem. Par. 1785, 316.

of ammoniac, which had not been decompofed by the 1ft diftillation, but had paffed with the water into the enfiladed receivers, which proves that much of this falt is volatilized during the evaporation of its folution.

By this 2d diftillation an acid liquor paffed into the receiver, its weight muft have been 619 — 320 = 299 grains, thefe 299 grains he faturated with tartarin, the addition of which produced no fmell of vol-alkali, confequently no undecompofed vol-alkali remained. He then diftilled off the water and found it perfectly pure, there remained in the retort 54 grains of nitre, whence, depending on Bergman's calculation, he fuppofes the 299 grains of the acid liquor to have contained 18 grains of real nitrous acid, and that the remainder, viz. 281 grains muft have been water formed; hence he concludes, 1ft, that 760 grains of nitrated ammoniac were decompofed, for 72 + 320 = 392 efcaped during decompofition, and thefe being fubftracted from 1152, leave 760. 2dly, That from this decompofition 281 grs. of water had been produced, and even more, for fome was loft, p. 318. All thefe changes were effected by the 1ft diftillation.

I shall now examine this curious experiment on the grounds of the foregoing theory.

1ft,

1st, 760 grains of nitrated vol-alkali contain, by my account, 57 per cent. nitrous acid, 23 per cent. vol-alkali, and 20 per cent. water.

Consequently of	acid	433,2
	vol-alkali	174,8
	water	152,00
		760

Again, 54 grains nitre contain, by my account, 23,76 real acid, and these, substracted from 433,2, leave 409,24 to form water and the rudimental nitrous air.

Hence 760 — 23,76 = 736,24 grains form the quantity to be accounted for; we must also assign the reason why rudimental nitrous air, and not mere nitrous air, was left.

2dly, Of the 281 grains of *water*, found by Berthollet, 152 pre-existed by my theory, consequently the formation of 129, and of the additional quantity lost, must be accounted for. To effect this we are to observe,

3dly, That according to Berthollet's analysis 100 grains vol-alkali consist of 19,34 of hydrogen, and 80,66 of mephite, consequently that 178,4 grains of vol-alkali contain 33,8 of hydrogen.

4thly, 100 grains water, by the most exact experiment, require for their composition 14,338 grains of hydrogen, consequently 129 grains of water require 18,497 of hydrogen, 9 Ann.

 Chy.

Chy. p 45; confequently 129 grains water require 18,497 of hydrogen, confequently there remained 15,3 grains of hydrogen for the formation of about 100 grains more of water, which were loft.

5th. LAVOSIER affigns to 100 grains of fully oxyginated nitrous acid about 64 of nitrous air and 36 of acidifying oxygen; but in its common ftate of oxygination we may affign it 25 only of fuperadded oxygen; and confequently 100 grains of the common acid contain 75 of nitrous air and 25 of acidifying oxygen. Nitrous air itfelf contains about $\frac{2}{3}$ of its weight of oxygen, and $\frac{1}{3}$ mephite. 1 Lav. Elem. 235, and Mem. Par. 1781.

Now 100 grains water require for their formation 85,662 of oxygen, therefore 229 grains of water would require 196,16; but 409 grains nitrous acid, fuppofing it even fully oxyginated, contain no more than 147,24 of acidifying oxygen, therefore the remainder *v. z.* 48,92 muft have been extracted from the nitrous air, and much more, if we fuppofe the nitrous acid to contain but 25 per cent. of acidifying oxygen; for then the nitrous acid would fupply but 102,25 and confequently 93,91 fhould be taken from the nitrous air.

Now, according to the experiments of Dr. Prieftly and Dieman, if much oxygen be fubftracted from nitrous air it will be converted into rudimental nitrous air; thus this converfion, and the quantity of water found, are adequately accounted for on the theory above laid down.

THE

The account of the refults of this operation may be rendered ftill clearer by the following table.

1080 grains paffed into the receivers at the firft diftillation, namely,

		Grs.
	undecompofed ammoniac -	320,
	undecompofed nitrous acid -	23,76
	water of compofition -	152,
	water produced - -	229,
	mephite of the vol-alkali - -	141,
Of 409 nitrous acid, from which its acidifying oxygen, namely, 102,25 grains, were extracted, there remained 306,95 grains of nitrous air; and of this, after the extraction of 93,91 of oxygen, there remained 212,84 of rudimental nitrous air.	rudimental nitrous air -	212,84
	Total - -	1078,60
	Lofs unaccounted for -	1,4
		1080,0

Remarks

Remarks on Mr. Richter's Calculation of the Proportion of Ingredients in Neutral Salts.

Since the publication of my laſt paper Mr. Richter, an able German Mathematician and natural philoſopher, has publiſhed an elaborate treatiſe on the ſame ſubject, in which infinite labour and great mathematical ingenuity is diſplayed; his concluſions, however, differ conſiderably from mine; leaſt this difference among ſo many experiments ſhould ſuggeſt a doubt concerning the determinations I have endeavoured to eſtabliſh, I feel myſelf obliged to inveſtigate the ſource of this difference, and to ſhew the inaccuracy of ſeveral of his fundamental inductions.

Section 1ſt.

Stochyometry, 2 Theile.

By his firſt experiment, the foundation of ſeveral of his ſubſequent concluſions, he endeavours to diſcover the real quantity of calcareous earth in chalk, he found 2400 grains of chalk expoſed in an *earthen* veſſel to the greateſt heat of a wind furnace (how long?) to weigh, when cool, only 1342 grains, therefore 1000 grains of this chalk would weigh 559 grains, and this without farther proof he takes to be the true quantity of lime contained in it.

On

On this experiment I remark, that it does not clearly appear that the chalk was thoroughly calcined, but on the contrary there is great reaſon to think it was not, becauſe chalk has never been known to contain ſo large a proportion of lime as $\frac{559}{1000}$, it is true, he ſays, it did not effervefce with acids, but ſurely it heated and bubbled, and ſuch bubbles are not diſtinguiſhable from real effervefcence, where the quantity of fixed air is ſmall, but by weighing before and after the addition of an acid, which he does not ſay he had done.

Dr. Black found it impoſſible to calcine any conſiderable quantity of lime in an *earthen* crucible, but was obliged to uſe one of black-lead to avoid vitrifaction, 2 Ed. Eſſays, 219. Smith found the ſame difficulty to effect the entire expulſion of fixed air, Diſſert. de Aere fixo, p. 40, 43. Chalk in general contains no more than 49 or 50 per cent. of fixed air, and the chalk he uſed, if it was purified, as he mentions in the 2d ſection, muſt have contained abundance of moiſture; it commonly contains but 41 per cent. of fixed air, and the proportion of earth in ſuch caſe is only 50 per cent. or $\frac{500}{1000}$, therefore $\frac{59}{1000}$ grains of fixed air remained unexpelled.

Section 3d.

5760 grains of ſp. of ſalt were ſaturated with 2393 of the aforementioned chalk, and the whole being evaporated to dryneſs and

heated

heated to thin fufion, weighed 2544 grains, now at the rate he had before laid down, the 2393 grains of chalk contained 1337 of lime, and deducting this from the falited mafs, he concludes the remainder, viz. 1207 to have been mere, or what I call real marine acid *. There the error committed in ftating the quantity of lime is important, as from this the proportion of real acid in the fp. of falt is deduced, and applied in calculating its proportion in other muriatic falts. If the chalk contained 50 per cent. of lime, as I ftate it, then 2393 grains of it contained 1196 of lime, and deducting this from the 2544 of falited lime, the remainder, viz. 1348 is the quantity of real muriatic acid contained in that mafs, and confequently that which is contained in 5760 grains of his fp. of falt, and 1000 grains of it contained 234,03 nearly, inftead of 209, as he ftates it.

Section 33*d*.

I PASS to this fection, as it is here that the defect of his determination will more clearly appear. In this he tells us, that he faturated 1760 grains of a folution of mild vegetable alkali with 2740 grains of the above mentioned fp. of falt, evaporated and fufed the neutral falt thus formed, and found it to weigh 1856 grains, whence, as by his ftatement, 2740 grains of that fp. of falt contained

* By an error of the prefs it is ftated in the original that 1207, 2544 : : 1000, 1107.

tained 573 of real acid, and this quantity entered into the 1856 grains of neutral falt, it follows that by fubftracting 573 from 1856 the remainder will exhibit the weight of the alkali, namely, 1283 grains.

It muft be allowed that this is a very indirect and improper method of difcovering the real quantity of mild alkali in the alkaline folution, for it comes loaded with the inaccuracies attending the two previous determinations, that of the real quantity of lime, from which that of the marine acid is inferred, and that of the marine acid, from which this laft determination is deduced; befides, if any muriated tartarin exifted in the alkaline folution, as it often does, it would efcape this method and could not be detected.

But a more apparent objection lies to it; if 1586 grains of muriated tartarin contain only 573 of real acid, then 100 grains of this falt would contain only 30,86; now if any thing be well proved in my effay, it is affuredly the affertion, that 100 grains of this falt contain nearly 36 of real acid, being confirmed by the experiments on falited filver, and the decompofition of common falt, therefore Richter's determination is erroneous, by allowing to this falt too fmall a proportion of acid.

But if we determine the quantity of alkali in the 5760 grains of alkaline folution by the quantity of real marine acid it was able

 to

to ſaturate, calculated as I mentioned in the above experiment on lime, it will be found very exactly; for there I ſtated that 5760 grains of his ſp. of ſalt contained 1348 of real acid, and conſequently 2740 contained 641,25. Now as 36 of acid take up 64 of vegetable alkali, 641,25 take up 1140 of that alkali, and the ſum of both *v. z.* 1781, will be the quantity of muriated tartarin thus formed. It is true he found its weight to be 1856 grains, that is 75 grains more than by my calculation, but this exceſs moſt probably was cauſed by the muriated tartarin previouſly exiſting in his alkaline ſolution. His mode of obtaining what he calls a pure alkaline ſolution renders this highly probable.

To obtain a pure alkali (§ 33) he ſimply pours cold water on common pot-aſh, and leaves them together, frequently agitating them for 24 hours; the ſolution thus obtained he evaporates to dryneſs, and then again treats the ſaline maſs with cold water, but with a quantity of it too ſmall to re-diſſolve the whole; ſuch was the alkaline ſolution he employed. Now though much of the neutral ſalts contained in pot-aſh may thus remain undiſſolved, yet ſome certainly will be taken up, and among the reſt muriated tartarin, which is frequently found in vegetable aſhes * and does not require above three times its weight of water to diſſolve it. To this, then, the exceſs of 75 grains may well be aſcribed.

The juſtneſs of this concluſion is ſtill further confirmed by examining his experiment on vitriolated tartarin. He ſaturated another

* Wiegleb. uber die Alkaliſche Salze 98.

ther pound of the alkaline ſolution with 3647 grains of dilute vitriolic acid, and after evaporation and ignition found the ſalt to weigh 2090 grains, and as he thinks he has proved the quantity of alkali in 5760 grains of the alkaline ſolution to be 1283 grains, hence he concludes the quantity of acid in the 2090 grains to be 2090 — 1283 = 807 grains; if ſo, vitriolated tartarin ſhould contain but 38,6 grains per cent. of acid, whereas it has been proved to contain much more.——But allowing the quantity of alkali in the pound of alkaline ſolution to be, as I ſtated it, 1140 grains, then as 55 parts alkali take up 45 of real vitriolic acid, 1140 will take up 933 of this acid, and the ſum of both will be 2073, which differs from 2090 only by 17 grains, owing probably to the muriated tartarin contained in his alkaline ſolution, which may even have been decompoſed by the vitriolic acid. He determined, it is true, the quantity of vitriolic acid by another operation, § 18, but here a material and evident error occurs, as I ſhall preſently ſhew:

1ſt, To 8460 grains of vitriolic acid, whoſe ſp. grav. was 1,8553, he added 19200 of water, or, which is the ſame thing, to 84,6 of the concentrated acid he added 192 of water, and found the ſp. grav. of the mixture 1,214.

2dly, He ſaturated 9075 grains of this dilute acid with 3215 grains of the chalk above-mentioned, and as by his account 1000 parts of that chalk contained 559 of lime, he concluded that

3215 grains of it contained 1596 of lime. Then having heated the ſelenite thus formed to a degree ſufficient to convert lime-ſtone into lime, he found it to weigh 3600 grains, and deducting from this weight that of the lime, he found the remainder, *v. z.* 2004 grains to be the weight of the vitriolic acid which was contained in 9075 grains of the dilute acid liquor, and conſequently that the 3647 grains of it which he had employed in ſaturating the alkali in the former experiment contained 806 grains.

HERE, not to repeat with reſpect to the chalk what I have already ſuggeſted, I ſhall confine myſelf to a *ſingle* error, becauſe it is manifeſt:

As 1000 parts chalk (he ſays) contain 559 of lime, 3215 grains of it ſhould contain 1596, whereas by the rule of proportion it ſhould be 1797,185; then deducting 1797 from 3600, the remainder, *v. z.* 1803, and not 2004, ſhould be the weight of the acid part of the ſelenite; and 3647 grains of the dilute acid employed in ſaturating the alkali ſhould contain, by his own account, 722, and not 806 grains. It would ill become me to reproach Mr. Richter with this overſight, as many of ſuch have often eſcaped my notice in my own calculations, and occaſioned me infinite labour in rectifying their numerous ſpurious conſequences.

TABLE

TABLE II.

Quantity of Real Acid taken up by mere Alkalis and Earths.

100 Parts.	Vitriolic.	Nitrous.	Marine.	Fixed Air.
Tartarin -	82,48	84,96	56,3	105 almoſt
Soda - -	127,68	135,71	73,41	66,8
Vol-alkali -	383,8	247,82	171	Variable
Barytes -	50	56	31,8	282
Stronthian -	72,41	85,56	46	43,2
Lime -	143	179,5	84,488	81,81
Magneſia -	172,64	210	111,35	200 Fourcroy
Argill - -	150,9			335 nearly Berg.

TABLE III.

Of the Quantity of Alkalis and Earths taken up by 100 Parts of Real Vitriolic, Nitrous, Muriatic and Carbonic Acids, ſaturated.

100 Parts	Tartarin.	Soda.	Vol-Alkali.	Barytes.	Stronthian.	Lime.	Magneſia.
Vitriolic - - - -	121,48	78,32	26,05	200,	138,	70,	57,92
Nitrous - - - -	117,7	73,43	40,35	178,12	116,86	55,7	47,64
Muriatic - - - -	177,6	136,2	58,48	314,46	216,21	118,3	89,8
Carbonic - - - -	95,1	149,6	— —	354,5	231, +	122,	50,

TABLE IV.

Quantity of Neutral Salts afforded by 100 Parts of the above-named Acids when ſaturated with the above-named Baſes.

100 Parts	Tartarin.	Soda.	Vol-Alkali.	Barytes.	Stronthian.	Lime.	Magneſia.
Vitriolic	221,48	{ 425 cryſtallized 178,5 deſiccated }	182,94	300	238	{ 170 in a white heat 198 at 170°	340 cryſtallized 158 deſiccated
Nitrous	227,22	188	175,44			174 well dried, that is, in air	
Marine	277,6	257,2	233,9	{ 487,4 cryſtallized 454,5 deſiccated }	540 cryſtallized 313,5 deſiccated	238 in a red heat	286,2 well dried
Carbonic	232,5	{ 693,5 cryſtallized 150 deſiccated }		454,5	331,7	222,25	200

T A B L E V.

Quantity of Neutral Salt afforded by 100 Parts of different *Bases*, when *combined* with the Vitriolic, Nitrou , Marine, or Carbonic Acid.

100 Parts with	Vitriolic.	Nitrous.	Marine.	Carbonic Acids.
Tartarin - -	182,48	193 +	156,3	244
Soda - -	541,1 cryſtallized 227,4 deſiccated	246,42	188,74	463,3 cryſtallized 167 deſiccated
Vol-alkali -	702,94	435	400	
Barytes -	150		155,16 cryſtallized 142,8 deſiccated	382
Stronthian -	172,41		250 cryſtallized 145 deſiccated	143,16
Lime - -	312 dried at 50° 284 dried at 130° 262,8 ignited 243 incandeſcent	312 dried at 80° 280 fully deſiccated	200 in a red heat	182
Magneſia - -	588,23 cryſtallized 272,62 deſiccated		321,8 gently but ſenſibly dried.	400

TABLE VI.

Of the Proportion of Ingredients in the following Saline Compounds:

100 Parts *Carbonic.*	Basis.	Acid	Water.	State.
Aerated Tartarin	41,	43,	16,	Crystallized.
Common Salt of Tartarin or Pearl Ash	70,	[illegible]	6	Dry.
Aerated Soda	21,58	[illegible]	[illegible]	Fully crystallized.
Do.	59,86	[illegible]	-	Deficcated.
Aerated Barytes	78,	22	-	Natural or ignited.
Aerated Stronthian	69,5	[illegible]	-	Natural or ignited.
Aerated Lime	55,	45,	-	Natural if pure, or artificial ignited.
Aerated Magnesia	25,	50,	25,	Crystallized.
Common Magnesia	45,	34,	21,	Dried at 80°.
Aerated-Vol alkali	In the Ratio of [illegible] Air	to	13 fixed Air.	
Vitriolic.				
Vitriolated Tartarin	54,8	45,2	-	Dry.
Glauber	18,48	23,52	58,	Fully crystallized.
Do.	44,	56,	-	Deficcated at 700°.
Vitriolated Vol-alkali	14,24	54,[illegible]	31,[illegible]	
Barofelenite	66,66	33,[illegible]	-	Natural and pure, artificial ignited.
Vitriolated Stronthian	58,	42,	-	Natural and pure, artificial ignited.
Selenite	32,	46,	22,	Dried at 60°.
Do.	35,23	50,39	14,38	Dried at 170°.
Do.	38,81	55,84	5,35	Ignited.
Do.	41,	59,	-	Incandefcent.
Epsom	17,	29,35	53,65	Fully crystallized.
Do.	36,68	63,32	-	Deficcated.
Allum	12, ignited	17,66	51, of Crystal + 19,24 in the Earth	Crystallized
Do.	63,75	36,25	-	Deficcated at 7[illegible]°
Vitriols				
Of Iron	28, ♆ of ♂ = 12, Metal	26,	38, + 8 of Composition	Crystallized.
Do.	45,	41,93	13,07	Calcined to Redness.
Lead	75, Calx = 71 Metal	23,37	1,63	
Copper	40 Calx = 30 Metal	31,	29,	
Zinc	40 Calx = 30 Metal	20,5	39,	

100 Parts *Nitrous.*	Basis.	Acid.	Water.	State.
Nitre	51,8	44,	4,2 of Composition	Dried at 70°.
Nitrated Soda	40,58	53,21	6,21 of Composition	Dried at 400°.
Do.	42,34	57,55	-	Ignited.
Nitrated Vol alkali	23,	57,	[illegible]	
Nitrated Barytes	57,	32,	11,	Crystallized.
Nitrated Stronthian	36,21	31,07	32,72	Crystallized.
Nitrated Lime	32,	57,44	10,56	Well dried, that is in Air.
Nitrated Magnesia	22,	46,	22,	Crystallized.
Muriatic.				
Muriated Tartarin	64,	36,	-	Dried at 80°.
Common Salt	53,	47, aqueous, 38,88 real	-	Dried at 80°.
Sal Ammoniac	-	-	-	Crystallized.
Do.	25,	42,75	32,25	Sublimed
Muriated Barytes	64,	20,	16,	Crystallized.
Do.	76,2	23,8	-	Deficcated.
Muriated Stronthian	40,	18,	42,	Crystallized.
Do.	69,	31,	-	Deficcated.
Muriated Lime	50,	42,	8,	Red hot.
Muriated Magnesia	31,07	34,59	34,34	Senfibly dry.
Muriated Silver	75,	16,54	8,46 Oxygen	Dried at 130°.
Muriated Lead	81,77 ♆ ♄	18,23	In the Calx	Crystallized.
Do.	83, ♆ of ♄	17,	-	Deficcated.

ESSAY *on* HUMAN LIBERTY. *By* RICHARD KIRWAN, *Esq.* *L. L. D. F. R. S. and M. R. I. A.*

1. *POWER* denotes the principle of action. Action denotes the *exercise* of power. Read July 28th 1798.

2. *Necessity* denotes the conceived *impossibility* of the non-existence of any thing.

3. HENCE necessity is of three kinds, metaphysical, physical and moral.

AN object is said to be *metaphysically* necessary when its absence involves a contradiction; and to be *physically* necessary when its non-existence contradicts the established laws of corporeal nature, or when it cannot fail to exist, or cannot exist otherwise than it does, without a miracle.

Laftly, THAT is faid to be *morally* neceffary whofe non-exiftence is contrary to the laws by which moral agents conftantly and univerfally govern their conduct. On the other hand we call that future object *certain*, which will not fail to come to pafs.

4. HENCE certainty differs from neceffity in this, that what is neceffary *cannot*, and what is certain *will not*, fail to happen. What is neceffary is certain, but not *vice verfa*.

5. A power is faid to be *free* when its exercife in every fenfe is morally poffible.

6. *Will or the power or faculty of willing* is faid to be *free*, when it may act or not act, or *elect*, without the conftraint of moral neceffity; for no other can be applied to the will. The application of this definition requires fome farther obfervations.

7. 1ft, WE muft obferve, that the will can form no volition, but with a view of obtaining fome *good* either real or apparent. For all rational agents *neceffarily* covet happinefs, and efteem that to be good which promotes or conftitutes any degree of happinefs, and confequently purfue it, with an ardour proportioned to the degree it expofes to their view. A volition like every action requires a fufficient reafon for its exiftence, and in this cafe

caſe none can be adduced but the attainment of ſome degree of happineſs. The good or advantage thus held forth to the mind is called the *motive* or final cauſe of its action. But the efficient cauſe of the volition is the mind itſelf; the term *motive* is in ſome degree improper as it conveys the idea of activity, whereas it is in reality paſſive, being the term towards which the mind moves, or from which it recedes.

8. 2d, As the will can never act without a motive, the *connexion* between a volition and ſome motive is metaphyſically neceſſary, it being grounded on the very nature of the mind, or of an intelligent agent, which cannot act but with a view of obtaining happineſs. But with reſpect to *particular motives* the following diſtinctions are to be obſerved:

9. If the good preſented to the mind be apparently *infinite*, its connexion with a correſpondent volition is then morally neceſſary, but if the good preſented be *finite*, the connexion muſt be weaker; but ſtill, as it is no leſs real ſince it exiſts, it is *certain.*

Note—Certainty is an ambiguous term, as it ſometimes denotes the reality of an object, ſometimes the foundation or cauſe of that reality, and ſometimes the firm perſuaſion of the mind of the reality of an object. Here it is employed in the firſt ſenſe, and ſometimes in the ſecond, but never in the laſt. In

 the

the firſt ſenſe it is oppoſed to *unreality*, or *non-exiſtence*, in the third, it is oppoſed to *uncertainty* or mere probability.

10. NECESSITY and contingency are oppoſed to each other, as contingency denotes the mere poſſible exiſtence or non-exiſtence of an object in any future time, but the oppoſite of certainty is *unreality*.

11. HENCE we may obſerve a gradation in the ſtrength of the tendency of the mind towards the motives that are preſented to it from that which is infinitely ſtrong, and therefore produces a moral *neceſſity*, to that which is indefinitely weak, but whoſe connexion with volition is nevertheleſs *certain*. To attribute a purſuit equally ſtrong to motives of apparently unequal appetibility is evidently abſurd, yet this the neceſſitarians are forced to maintain, as neceſſity admits of no degrees. The ſtrength or force of motives, or more properly ſpeaking their appetibility, evidently reſults from the degree of *apparent good* which they preſent.

12. BUT it may be replied that neither can reality admit of different degrees, nor conſequently can certainty. This is true with reſpect to the firſt ſenſe, but not with reſpect to the ſecond ſenſe of that word. For the foundation of certainty is ſo much the ſtronger as it approaches more to neceſſity.

13. IF

13. If ends or motives, *apparently equally* desirable, but suggesting different or opposite volitions, be presented to the mind, and if both present a greater good than that resulting from remaining in its actual state by embracing neither, in that case the mind may tend to either, that is, may form a volition to obtain the good presented by either. For though there is no reason for preferring either, yet the good presented by each is a sufficient reason for pursuing that presented by any of them, and the impossibility of pursuing both is a sufficient reason for pursuing one of them. Yet probably some *extrinsic* reason generally suggests the choice, such as that one of them was first thought of, or last thought of, &c.

14. If motives, *apparently unequally desirable*, be presented to the mind, then if the inequality be *infinite* the mind will *necessarily* pursue the most desirable for the reasons already given.

15. If the inequality be *finite*, it frequently happens that by considering them in different points of view their appetibility may be inverted, the *most* desirable being in some respects the *least so*, and the least desirable appearing in some lights the *most so*. Hence the mind is free to pursue either from the intrinsic good each holds to its view.

16. This

16. This inverſion becomes ſo much the eaſier as the inequality betwixt the propoſed motives is apparently ſmaller, and ſo much the more difficult as the apparent inequality is greater. And hence we perceive the benefit of inſtruction, as by its means the *apparent* inequality approaches indefinitely to the *real.*

17. Motives are preſented to the mind either by ſenſation, imagination, paſſion, ſenſe of duty, fear of remorſe, or moral inſtincts. In general thoſe preſented by the three firſt modes of perception are moſt purſued, becauſe in receiving them the mind is entirely paſſive, and their rejection is attended with a greater or leſſer degree of pain; whereas the comprehenſion of the latter, in their full ſuaſorial view, requires attention and ſelf command, which are oppoſed by the natural indolence of the mind, though the importance of the determination to be taken ſtrongly indicate the propriety of applying them, and though the underſtanding pronounce the purſuit of the object they ſuggeſt to be in ſome reſpects the greater good. Hence the ſaying of Medea, *Video meliora*, &c.

18. The difficulties in which this ſubject has hitherto been involved have ariſen in great meaſure from the improper expreſſions uſed in treating it, moſt of which are in their literal ſenſe applicable only to corporeal nature which is paſſive, and therefore ſuggeſt falſe conceptions when applied to mind, which is eſ-

ſentially

ſentially active. Thus *motives* ſeem to imply ſomething active, whereas they are in reality paſſive, being the ends which the mind purſues or may purſue. They are ſaid to *impel* the mind to action, which again falſely denotes activity, whereas the mind naturally *purſues* them in proportion to the apparent good they preſent. Thus alſo *force* and *ſtrength* are improperly applied to them.

I SHALL now proceed to obviate the objections to human liberty advanced by Dr. Prieſtley, who of all others has ſtated them with moſt clearneſs and preciſion, occaſionally noticing any thing farther relevant to the ſubject that has been advanced by other writers.

THE Doctor, in p. 7 of his Illuſtrations of Philoſophical Neceſſity, tells us, "that the liberty he denies to man is that of do-"ing ſeveral things, when all the previous circumſtances (in-"cluding the *ſtate of his mind* and his *views of things)* are preciſe-"ly the ſame; and aſſerts, that in the ſame preciſe ſtate of mind, "and with the ſame views of things, he would *always* voluntari-"ly make the ſame choice and come to the ſame determina-"tion."

BY views of things the Doctor evidently means motives, and conſequently in ſome caſes, namely, thoſe mentioned in Nos. 9 and

and 14, his affertion is perfectly juft, the motive being there fuppofed to be infinitely defireable, but in moft cafes, as thofe mentioned in Nos. 13 and 15, it may be true, and it may alfo be falfe; for as in thofe cafes the reafons for oppofite determinations are apparently equal, the mind may *at one time* form one choice and at another time another, or it may *always* form the fame, or each time a *different*.

THE Doctor alfo fays, " he allows to man the liberty of *doing* " whatever he pleafes," but the liberty here meant is not the liberty of performing any external action, but the liberty of willing or chufing.

MR. LOCKE feems to think that the will cannot properly be faid to be free, becaufe liberty (he fays) " is but a power belong- " ing to agents, and cannot be an attribute or modification of " will which is alfo a power;" but liberty is not merely a power but a fpecies of power, as power may be exerted either neceffarily or freely.

To eftablifh his conclufion, Dr. Prieftley lays down fome obfervations relative to caufe and effect, which being folely applicable to corporeal nature, I omit. He then tells us, p. 13, " that a " particular determination of the mind could not be otherwife " than it was, if the laws of nature be fuch as that the fame de-

" termination

" termination ſhall conſtantly follow the ſame ſtate of mind and
" the ſame view of things, and it could not be poſſible for the
" ſame determination to have been otherwiſe than it *has been*, *is*,
" or *is to be*, unleſs the laws of nature had been ſuch, as that
" though both the ſtate of the mind and the views of things were
" the ſame, the determination might or might not have taken
" place. But in this caſe the determination muſt have been an
" effect without a cauſe, becauſe in this caſe, as in that of a
" balance, there would have been a change of ſituation without
" any previous change of circumſtances, and there cannot be any
" other definition of an effect without a cauſe."

To this reaſoning I reply, that the laws of nature, with reſpect to intellectual agents, are ſuch, that though the ſtate of mind and the views of things be exactly the ſame, one and the ſame determination might not have taken place in the caſes mentioned Nos. 13 and 15, and yet whether the ſame or a different determination take place it will not be an effect without a cauſe; for as in thoſe caſes different motives or final cauſes, equally attractive, are ſuppoſed to occur, which ever of them the mind purſues, its determination will not want a final cauſe. The compariſon of a balance, which will remain in æquilibrio when the ſcales are loaded with equal weights, is inapplicable, as the balance does not act, but is acted upon, whereas the mind is evidently poſſeſſed of an active power of purſuing a propoſed end.

THE Doctor further adds, in his reply to Mr. Palmer, p. 7. "that certainty or univerſality is the only poſſible ground of "concluding that there is a neceſſity in any caſe whatever," which is true as far as reſpects corporeal nature; but with reſpect to intelligent beings the perceived connexion betwixt their actions and a ſupreme *degree* of apprehended happineſs is the true ground of the neceſſity of their volitions when they are neceſſary, as ſhewn Nos. 9 and 14, which indeed may be indicated by conſtancy and univerſality; and where this ground does not exiſt, certainty (with reſpect to our knowledge) cannot be obtained.

THE next argument in proof of the neceſſity of human actions is derived from divine præſcience. Dr. Prieſtley ſtates it thus: "As it is not in the compaſs of power in the author of any "ſyſtem, that an event ſhould take place without a cauſe, or "that it ſhould be equally poſſible for two events to follow the "ſame circumſtances, ſo neither, ſuppoſing this to be poſſible, "would it be within the compaſs of knowledge to foreſee "ſuch a contingent event; for as nothing can be known to "*exiſt*, but what does exiſt, ſo certainly nothing can be known "to *ariſe from what does exiſt*, but what does ariſe from it, or "depend upon it; but according to the definition of the terms, "a contingent event does not depend upon any previous known "circumſtances, ſince ſome other event might have ariſen in the "ſame

" ſame circumſtances. All that is in the compaſs of knowledge
" in this caſe is, to foreſee all the different events that might
" take place in the ſame circumſtances, but which of them will
" actually take place cannot poſſibly be known." P. 19.

In anſwer to this argument we muſt obſerve, that not only the immenſely complicated ſeries and concatenation of events which we denominate the *actual ſyſtem of the world*, was originally *barely poſſible*, but alſo an infinite number of other ſyſtems differently arranged and equally complicated. In ſome of theſe the contingent act appeared linked with one of the motives with which, in the ſame circumſtances, it might poſſibly be connected, and in another ſyſtem a very different event might ariſe from the equally poſſible connexion with the oppoſite motive, as in the caſes Nos. 13 and 15. Each of theſe events would give room to a totally different ſeries of ſubſequent events, for the greateſt and moſt important ariſe from others ſeemingly the leaſt important. Among theſe different ſyſtems God has choſen the *beſt*, or at leaſt *one of the beſt*, and upon this choice his fore knowledge of that determinate contingent object which is to happen, to which the Doctor alludes, and where apparently unequal motives do not determine it, is grounded.

To this argument Mr. Crombie, in his Treatiſe on Philoſophic Neceſſity, p. 73. farther adds, that ſince the Deity foreſees future events they muſt neceſſarily take place. But as knowledge of

 any

any kind is perfectly extrinsic to the events known, and exerts no sort of influence over them, all that can justly be inferred from the infallibility of divine præscience is, that the event foreseen will *certainly* and infallibly, but not necessarily happen; for to secure the infallibility of divine fore-knowledge, the future existence of the event foreseen, and not the impossibility whether physical or moral of its non-existence, or in other words its certainty, but not its impossibility, must be supposed.

All the objections hitherto made to human liberty seem to me reducible to those I have here noticed. It is needless to adduce any argument in proof of it, as the consciousness of our being ourselves the active principle from which our determinations originate, and the remorse incident to the abuse of this self-determining power impress the fullest conviction of this important truth.

[To face Page 316.

1798.

1798.		Inches.	STORMS.
January - -		2,52222	5, N. to W. & S. to W.
February - -	l Hail	1,57639	5, SW. to W. & NW.
March - -	fell Hail	1,33993	
April - -		1,65520	1, S. to SE.
May - -		0,486047	
June - -		0,577996	
July - -		3,310419	1, W. to W. by N.
Auguft - -		2,574763	1, W. to NW.
September -		0,472917	1, S. to W.
October - -		1,339931	3, S. SW. & W.
November - -		2,837502	8, S. SW. W. & NW.
December - -		1,471290	2, SW. to E.
Mean of the Year -		20,16457 Total of the Year.	27, Total in the Year.

[To face Page 316.

Synoptical View of the State of the Weather in the Year 1798.

By RICHARD KIRWAN, *Esq. L.L.D. F.R.S. and M.R.I.A.*

1798.	BAROMETER.					THERMOMETER.			RAIN.		STORMS.
	Highest.	Day it happened.	Lowest.	Day it happened.	Mean of the Month.	Highest in the Day.	Lowest at Night.	Mean of the Month.	Days.	Inches.	
January	30,62	8th, E. by S.	29,00	18th, W. & N. by W	30,038	51,50	28,50	40,85	18, on 2 of which fell Snow	2,52222	5, N. to W. & S. to W.
February	30,88	7th, N. great * Fog	29,27	22d, NW.	30,194	56,	25,	41,03	12, on 6 of which fell Snow and Hail	1,57639	5, SW. to W. & NW.
March	30,46	24th, E.	29,46	17th, Var. W. to N.	30,124	57,	29,	43,11	13, on 2 of which fell Snow and 6 fell Hail	1,33993	
April	30,48	25th, E.	28,80	4th, S. & SE.	30,299	65,50	32,	50,4[illegible]	1[illegible], on 2 of which fell Hail	1,655[illegible]	1, S. to SE.
May	30,63	21st, S. to SW.	29,25	14th, S. & S. by E.	30,175	73,	42,	[illegible]	11, on 3 of which fell Hail	0,486047	
June	30,65	8th, E. to N[illegible]	29,[illegible]	2[illegible]th, W. to S.	30,266	81,	44,	[illegible]	12	0,577996	
July	30,47	29th, W.	[illegible]	20th, W. to [illegible] & [illegible]	[illegible]	73,	47,	[illegible]	28, on 2 of which Hail	[illegible]310419	1, W. to W. by N.
August	30,63	27th, W. and NW.	29,84	10th, W. & NW.	[illegible]	[illegible]	47,[illegible]	[illegible]	17	[illegible]	1, W. to NW.
September	30,31	18th, SW.	29,30	12th, W. to N.	[illegible]	[illegible]	[illegible]	53,42	17	[illegible]	1, S. to W.
October	[illegible]	2d and 3d, W. to E.	29,22	30th, N. to E	[illegible]	62,50	[illegible]	48,[illegible]	19	[illegible]	3, S. SW. & W.
November	30,44	16th, W.	28,92	8th, W.	[illegible]	5[illegible]	25,	[illegible]	[illegible]1, on 1 of which fell Snow	2,837502	8, S. SW. W. & NW.
December	30,66	24th, 30th, and 31st E.	[illegible]	1st, SW.	[illegible]	5[illegible]	[illegible]	[illegible]	[illegible], on 3 of which fell Snow	1,471290	2, SW. to E.
Mean of the Year	[illegible]		[illegible]		[illegible]			4[illegible],22	191, on 12 of which fell Snow	[illegible] Total [illegible]	27, Total in the Year

* The Fog on this Day was so great that Carriages were by Mistake driven into the Liffey.

An ABSTRACT *of* OBSERVATIONS *of the* WEATHER *of* 1798, *made by* HENRY EDGEWORTH, *Eſq. at Edgeworth-ſtown in the County of Longford in Ireland.*

	BAROMETER.			THERMOMETER.			RAIN.	
	Higheſt.	Loweſt.	Mean.	Higheſt.	Loweſt.	Mean.	Days.	Inches.
January -	29-98	28-24	29-58	49	30	39	14	5-80
February -	30-25	28-75	29-49	49	25	38	10	1-91
March -	29-83	29-26	29-54	54	31	42	7	1-27
April -	29-70	28-10	29-50	64	38	50	8	2-80
May - -	29 98	28-61	29-70	72	46	50	6	0-99
June -	30-06	29-10	29 53	76	51	60	11	2-40
July -	29-76	28-72	29-34	69	50	58	23	6-37
Auguſt -	29 96	29-17	29-62	73	51	61	9	2-27
September -	29-70	28 60	29-48	70	42	50	11	2-67
October -	29-90	28-55	29-42	62	32	46	11	3-63
November -	29-92	28-25	29-46	55	26	39	13	3-62
December -	30 00	28-73	29-44	49	18	38	9	1-83
Mean of the Year -	30-25	28-10	29-50	76	18	48	Total 132	Total 35-56

An Abstract of the Quantity of Wind in 1796, 1797, 1798.

Years.	No. of most windy Days.	The most windy Months.	No. of windy Days in these Months.
1796 -	165	January -	29
1797 -	160	January -	22
1798 -	157	October -	21

Of

Of the Instruments that are used at Edgeworthstown for keeping Diaries of the Weather.

The barometer is placed in the corner of a drawing-room, the windows of which have a south and east aspect. The floor of the room is about three feet above the surface of the earth.

The thermometer is hung at the outside of a NW. by N. window, about twenty feet from the surface of the earth.

The rain-gage is a funnel one foot square at the base, with a lip of an inch and a half deep.

A METHOD *of expreſſing, when poſſible, the* VALUE *of* ONE VARIABLE QUANTITY *in* INTEGRAL POWERS *of* ANOTHER *and* CONSTANT QUANTITIES, *having given* EQUATIONS *expreſſing the* RELATION *of thoſe* VARIABLE QUANTITIES. *In which is contained the* GENERAL DOCTRINE *of* REVERSION *of* SERIES, *of* APPROXIMATING *to the* ROOTS *of* EQUATIONS, *and of the* SOLUTION *of* FLUXIONAL EQUATIONS *by* SERIES. *By the Rev.* J. BRINKLEY, *M. A. ANDREWS' Profeſſor of Aſtronomy, and M.R.I.A.*

THE moſt general and uſeful problem in analytics is, from a given relation between two variable quantities to expreſs one of thoſe quantities in terms of the other and conſtant quantities. The caſes however in which this can be completely performed are few in compariſon of thoſe in which it can be only partially done. Among the partial ſolutions are thoſe by ſeries not terminating. When ſuch ſeries converge they afford the ſolution required. Various methods have been given by authors for obtaining theſe ſeries principally derived from thoſe given by Sir I. Newton. Of theſe the method of aſſuming a ſeries with coefficients to be de- Read Nov. 3. 1798.

termined from a comparifon of homologous terms is, perhaps, the beft where it can be practifed; yet the cafes are very numerous where without other affiftance it is difficult and almoft impoffible to practice it with any advantage. A method, therefore, which befides being in all cafes as fimple as any of the others is as general as can be defired, and is often attended with the fuperior advantage of demonftrating the law of the feries, muft be an object for the confideration of mathematicians. Such a method is attempted in the following pages. Its foundation is built upon a theorem firft given by that excellent mathematician Dr. Brooke Taylor. This theorem, given in Cor. 2. Prop. 7. page 23, of his method of Increments, is well known, and is in purport as follows:

If x and z be two variable quantities, the relation of which is given, then while x by flowing uniformly is increafed by $\dot{x}$, z will be increafed by $\dot{z} + \frac{\ddot{z}}{1.\,2.} + \frac{\overset{3}{\dot{z}}}{1.\,2.\,3.} + \&c.$ In which the values of $\dot{z}$, $\ddot{z}$, &c. are to be determined from the given equation.

It readily occurs that this theorem contains a method of deriving the values of one quantity by a feries afcending by powers of the other: and accordingly fome authors have ufed it in a few fimple cafes, but have not attempted a general ufe. And upon confideration it is obvious that without farther affiftance it cannot be be practised in cases at all complex. For if in an equation expressing the relation of x and z the successive fluxions be derived one from the other generally and without having regard to particular values of x and z, almost insuperable trouble would arise except in the most simple cases, and the method be very far inferior to others. This farther assistance I have endeavoured to give in the following pages, principally by theorems for taking fluxions of different orders per saltum, that is, without finding the fluxions of the inferior orders. These will render the theorem of Taylor of the most extensive utility, as will best be seen by the examples hereafter given.

M. De la Grange is the only author I know of who has attempted to simplify the computation of $\dot{z}$, $\ddot{z}$, &c. This he has done by a most elegant theorem for an equation of a particular form (See Cousin's " Astron. Physique, Art. 20, p. 15.") But no use can be made of this theorem except in equations of that particular form. The theorems for taking fluxions per saltum will enable us to compute the values of $\dot{z}$, $\ddot{z}$, &c. by substituting the values of x and z when they begin to flow, and as in that case it often happens that the problem is such that x and z begin from nothing, the conclusions are then derived in the most simple manner.

The method of assuming a series with undetermined coefficients for the quantity to be found, besides the objections in every particular cafe to the legality of fuch an affumption not being felf-evident, often requires perplexing confiderations to avoid introducing unneceffary terms. Indeed the greateft difficulty often occurs in that part. In this method no feries is to be affumed. The feries derived follows from the nature of the problem. Oftentimes its law even in very complex cafes can be derived, in which by the method of affuming a feries it would be almoft impoffible to demonftrate it. Thus the truth of the law of the Multinomial Theorem, when the power is negative or fractional, is demonftrated by this method. It was done by De Moivre for integral powers, and I know of no author who has generally demonftrated it for all powers. The examples given to illuftrate the method are moft of them fuch as are well known, and may be compared with the fame as done by other methods. Among them are two feries firft given by Mr. James Gregory (See Comm. Epift.) the inveftigation of the latter of which has been confidered by mathematicians as very difficult.

Demonftration of Dr. Brooke Taylor's Theorem*.

THEO. If z and x be cotemporaneous values of two quantities any how related, and $\underset{\cdot}{z}$ and $\dot{x}$ = flux. of x, cotemporaneous increments, of which x is uniformly generated, then will

$$z + \underset{\cdot}{z} = z + \frac{\dot{z}}{1} + \frac{\ddot{z}}{1.\ 2.} + \frac{\dddot{z}}{1.\ 2.\ 3.} + \ \text{-} \quad \text{-}\ \frac{\overset{m}{\dot{z}}}{1.\ 2.\ldots.m} +,\ \&c.$$

when this feries terminates or converges.

DEMONSTRATION.

* Authors who have given this theorem have not been fo attentive to accuracy of demonftration as the importance of the theorem feems to require.

Demonstration.

$$\text{Let}\quad \left.\begin{array}{llllll} x, & x+\dot{x}, & x+2\dot{x} & - \; - & , & x+\dot{x} \\ z, & z', & z'', & \overset{3}{z}, \overset{4}{z} & - & z+\dot{z} \end{array}\right\} \text{be cotemporaneous values of } x \text{ and } z$$

$$\text{Let also}\quad \left.\begin{array}{llll} a, & a', & a'', & \overset{3}{a} \;\&c. \\ b, & b', & b'', & \&c. \\ c, & c'', & \&c. & \\ d, & \&c. & & \end{array}\right\} \text{be differences of the respective orders.}$$

Then by the theorem for differences.

$z+\dot{z} = z + na + \dfrac{n.\overline{n-1}}{1.\;2.}\,b + \dfrac{n.\overline{n-1}.\overline{n-2}}{1.\;2.\;3}\,c +$, &c. where n is the number of successive values from x to $x+\dot{x}$, or from z to $z+\dot{z}$. Now if n be increased *sine limite*, any assigned number of terms of this quantity approaches to the same number of terms in the series,

$z + na + \dfrac{n^2\, b}{1.\;2.} + \dfrac{n^3\, c}{1.\;2.\;3.} +$, &c. as its limit, or because $n = \dfrac{\dot{x}}{\dot{x}}$, to its equal $z + \dfrac{a}{\dot{x}}\dot{x} + \dfrac{b}{\dot{x}^2} \times \dfrac{\dot{x}^2}{1.\;2.} +$, &c. But when n is so increased, the limiting ratio of $a : \dot{x}$ or the limiting ratio of the increments of of z and x is the ratio of the fluxions of z and x, and it follows therefore that when n is increafed *fine limite*, the limiting value of $\frac{a}{\dot{x}} = \frac{\dot{z}}{\dot{x}}$. Alfo for the fame reafon $\frac{b}{\dot{x}} = \frac{\dot{a}}{\dot{x}}$, $\frac{c}{\dot{x}} = \frac{\dot{b}}{\dot{x}}$, &c. Whence the limiting value of $\frac{b}{\dot{x}^2} = \frac{\dot{a}}{\dot{x}\dot{x}} = \frac{\ddot{z}}{\dot{x}^2}$, becaufe $\frac{\dot{a}}{\dot{x}} = \frac{\ddot{z}}{\dot{x}}$

of $\frac{c}{\dot{x}^3} = \frac{\dot{b}}{\dot{x}\dot{x}^2} = \frac{\dddot{z}}{\dot{x}^3}$, becaufe $\frac{\dot{b}}{\dot{x}^2} = \frac{\dddot{z}}{\dot{x}^2}$,

&c. &c.

Whence the limiting value of $z + na + \frac{n.\,\overline{n-1}}{1.\ 2.} b + \frac{n.\,\overline{n-1}.\,\overline{n-2}}{1.\ 2.\ 3} c$ + &c. when n is increafed *fine limite* is $z + \frac{\dot{z}}{1} + \frac{\ddot{z}}{1.\ 2.}$ +, &c. And becaufe when the former feries terminates its value is $z + \dot{z}$: and when it converges its limit is alfo $z + \dot{z}$ $\therefore z + \dot{z} = z + \frac{\dot{z}}{1} + \frac{\ddot{z}}{1.\ 2.}$ + &c. when the feries terminates or converges. When it does not converge, nothing can be afferted of it, becaufe we cannot reafon concerning a limit which does not exift.

Problems

Problems for finding Fluxions *per Saltum.*

PROB. I. To find the n^{th} fluxion of x^m when x does not flow uniformly, and m denotes any whole, fractional or negative number.

SOLUTION. Let $a,\ b,\ c,\ d\ -\ -\ \kappa\ -\ -\ \delta,\ \gamma,\ \beta,\ \alpha$ be the 1^{st}, 2^{d}, 3^{d}, 4^{th}, - - k^{th} - $n-4, n-3, n-2, n-1$ fluxions of x.

THEN the n^{th} fluxion of $x^m =$

$$m x^{m-1} \overset{n}{\dot{x}} + \left.\begin{array}{r} n.a\alpha \\ \frac{n.\overline{n-1}}{1.\ 2} b\beta \\ \frac{n.\overline{n-1}.\overline{n-2}}{1.\ 2.\ 3} c\gamma \\ \&c. \end{array}\right|\ m.\overline{m-1}\, x^{m-2} + \left.\begin{array}{l} \frac{n.\overline{n-1}.\overline{n-2}}{1.\ 2.\ 1} ab\gamma \\ \frac{n.\overline{n-1}.\overline{n-2}.\overline{n-3}}{1.\ 3.\ 2.\ 1} ac\delta \\ \&c. \end{array}\right|\ m.\overline{m-1}.\overline{m-2}\, x^{m-3} +, \&c.$$

THE following are the laws of this ſeries:

1. THE index of x diminiſhes in each term by unity, and is to be continued till it becomes o or $m-n$.

2. THE coefficient of x^{m-v} is the product of $m.\overline{m-1}.$ - - $\overline{m-v-1}$, into the ſum of quantities, with numeral coefficients annexed, deduced from the different fluxions of x.

3. THESE

3. These quantities are formed by multiplying together a number v of the ſeveral fluxions of x, ſo that the ſum of their exponents ſhall be n. Thus if $\overset{p}{a}\overset{q}{b}\overset{r}{c}\overset{s}{d}$ be one of theſe quantities $p + 2q + 3r + 4s = n$ and $p + q + r + s = v$.

Note—By exponent of a fluxion is meant its order. Thus the exponent of d or of the fourth fluxion of x is 4.

4. To the quantity $\overset{p}{a}\overset{q}{b}\overset{r}{c}\overset{s}{d} \; - \; - \; \overset{\sigma}{x}$ is to be annexed, for a coefficient, a fraction the numerator of which is $n.\overline{n-1}.\overline{n-2} \; - \; \overline{k+1}$, and the denominator $p \times \overline{p-1}. \; - \; - \; 1 \times \overline{2}^{q}.q.\overline{q-1} \; - \; 1 \times \overline{3.2}^{r}.r.\overline{r-1} \; - \; - \; 1 \times \overline{4.3.2}^{s} \times s.\overline{s-1} \; - \; - \; 1 \times \overline{k.k-1 - - 1}^{\sigma-1} \times \sigma \times \overline{\sigma-1} \; - \; - \; 1$. The law of continuation of which is evident*.

The Demonſtration, as far as regards the 1ſt, 2d and 3d laws of the ſeries, is readily deduced from conſidering the manner in which the ſucceſſive fluxions of $\overset{m}{x}$ are derived. The demonſtration of the fourth law is ſomewhat more difficult, but may be deduced as follows: A quantity $\overset{p}{a}\overset{q}{b}$ prefixed to a power of x is evidently derived by taken the fluxion of x, $\overline{p+q}$ times, and of a, q times in every

* Since writing the above I find that Dr. Waring, at the end of his "Meditationes Analyticæ," ſpeaking of "methodus deductionis & reductionis," mentions this problem, and gives the three firſt laws, in which indeed there is no difficulty; the fourth, the only one difficult to inveſtigate, he does not give, nor does he mention any uſe to which the problem may be applied.

every different order, with the exception that each a must be taken before the b, which is derived from it. Consequently the coefficient of $a^p b^q$ must be the number of these different orders. This coefficient may therefore be deduced either from the doctrine of permutations or from that of probabilities. The former method is certainly the most natural, and at first sight may appear shorter: but the latter is more readily applicable to general expressions. And from it the coefficient of $a^p b^q$ is deduced by finding the probability of taking $a, a, a,$ - (p) $a_{'}, a_{''}, a_{'''}$ - (p) $b_{'}, b_{''}, b_{'''}$ - (q terms) in the order in which they are written. The marks underneath shewing the as from which the corresponding bs are derived. The inverse of the fraction expressing this probability is the number of different orders, and consequently the coefficient of $a^p b^q$. The prob. that an a, from which a b is not derived is taken first is $\frac{p}{n}$, that another a of the same description is taken next is $\frac{p-1}{n-1}$, &c. so that the probability that all the as of that description are taken previously to any of the as, from whence the bs are derived is $\frac{p}{n} \times \frac{p-1}{n-1} \times \frac{p-2}{n-2} \times$ - - $\frac{1}{n-p-1}$. That an a is taken next is certainty or

$\frac{2q}{n-p}$. The Prob. that another a is taken next is $2 \times \frac{q-1}{n-p-1}$ because each a, besides its own chance, has the chance of the b, which is derived from it, &c. &c. Whence it follows that the probability that all the a^s will be taken before any of the b^s is

$$\frac{p}{n} \times \frac{p-1}{n-1} \times \frac{p-2}{n-2} \quad - \quad - \quad \times \frac{1}{n-p-1} \times \frac{2q}{n-p} \times \frac{2 \times \overline{q-1}}{n-p-1} \times \frac{2 \times \overline{q-2}}{n-p-2} \times \quad - \quad - \quad \frac{2}{n-p+q-1}.$$

The probability that the b derived from the first a is taken next is $\frac{1}{n-p+q}$, &c.

Whence the prob. that the whole will be taken in the order in which they are written is

$$\frac{p \times \overline{p-1} \quad - \quad - \quad 1 \times 2^{q-1} \times q \times \overline{q-1} \times \overline{q-2} \quad - \quad - \quad 1}{n.\,\overline{n-1} \quad - \quad - \quad - \quad \overline{n-p+2q-3}}.$$

The reciprocal of which fraction is the coefficient of $a^p b^q$. And by the same process the general coeff. of $a^p b^q c^r d^s \; - \; - \; x^\sigma$ as given in the 4th law is readily deducible.

The dem. by the method of permutations is concisely as follows. If the quantities a, a, a, (p) a, b, a, b $(2q)$ were all different, the number of orders is $n.\,\overline{n-1}.\; - \; - \; 1$, but as p quantities are the same, the number must be reduced by dividing by $p.\,\overline{p-1} \; - \; - \; 1$, or the number of permutations of of p things, and becauſe the permutations of two things are two without regard to order, when the order is fixed the whole number of permutations muſt be alſo divided by the number of permutations in each order that is fixed, that is by $2 \times 2 \times 2 \times$ &c. $(q) = 2^q$, alſo becauſe q a^r are the ſame, it muſt be divided by $q \times \overline{q-1} \times - - 1$. Whence the number of permutations or the coefficient of $a^p\, b^q$ is as above ſtated, &c. &c.

Example I. The 6^{th} fluxion of $x^m =$

$$= m \overset{6}{\dot{x}}\, x^{m-1} + \left.\begin{matrix} 6\,\dot{x}\,\overset{5}{\dot{x}} \\ 15\,\ddot{x}\,\overset{4}{\dot{x}} \\ 10\,\overset{3}{\dot{x}}{}^{2} \end{matrix}\right| m.\overline{m-1}\, x^{m-2} + \left.\begin{matrix} 15\,\dot{x}^{2}\,\overset{4}{\dot{x}} \\ 15\,\overset{2}{\dot{x}}{}^{3} \\ 60\,\dot{x}\,\ddot{x}\,\overset{3}{\dot{x}} \end{matrix}\right| m\ldots\overline{m-2}\, x^{m-3} + \left.\begin{matrix} 20\,\dot{x}^{3}\,\overset{3}{\dot{x}} \\ 45\,\dot{x}^{2}\,\overset{2}{\dot{x}}{}^{2} \end{matrix}\right| m\ldots\overline{m-3}\, x^{m-4} +$$

$$15\,\dot{x}^{4}\,\ddot{x}\,\big|m\ldots\ldots\overline{m-4}\, x^{m-5} + m. - - \overline{m-5}\, \dot{x}^{6}\, x^{m-6}.$$

Example II. The 8^{th} fluxion of x^2 when $x = o$, and the 1^{st}, 3^d, 5^{th}, and 7^{th}, or the fluxions of the uneven orders are alſo $= o$ is $56\,\overset{2}{\dot{x}}\,\overset{6}{\dot{x}} + 70\,\overset{4}{\dot{x}}{}^{2}$. For in this caſe the coeff. of $\overset{2}{\dot{x}}\,\overset{6}{\dot{x}} = \frac{8.\,7}{2.1.1.} \times 2 =$ 56, and the coeff. of $\overset{4}{\dot{x}}{}^{2} = \frac{8.\,7.\,6.\,5}{\overline{4.\,3.\,2.\,1}|^{2-1} \times 2.\,1} \times 2 = 70.$

Example III. The coeff. of x^5 in the 7^{th} fluxion of x_8, when the even fluxions are $= o$ is

$$\left.\begin{array}{l} \dfrac{7.6.5.4}{1.\overline{3.2.1}\;.\;2}\,.\,\dot{x}\,\overset{3}{\dot{x}}{}^{2} \\ \dfrac{7.6}{2.1.1}\,.\,\dot{x}^{3}\,\overset{5}{\dot{x}} \end{array}\right|\, 8.7.6 = \overline{70\,\dot{x}\,\overset{3}{\dot{x}} + 21\,\dot{x}^{2}\,\overset{5}{\dot{x}}} \times 8.7.6.$$

Cor. If $a' = a$, $b' = \frac{b}{1.2}$, $c' = \frac{c}{1.2.3}$ &c. The denominator of the coefficient to be affixed to $\overset{p}{a'}\,\overset{q}{b'}\,\overset{r}{c'}$ - - $\overset{\sigma}{x'}$ is $p.\overline{p-1}$ - - $1.$ $q.\overline{q-1}.$ - - 1 - - - $\sigma \times \overline{\sigma - 1}$ - - 1 and the numerator $n.\overline{n-1}.\overline{n-2}$ - - - $1,$

Problem 2. To find the n^{th} fluxion of xyz (m quantities.)

Solution. The n^{th} fluxion of xyz (m quantities).

$$\overset{n}{\dot{x}}yz, \text{ \&c.} + x\overset{n}{\dot{y}}z, \text{ \&c.} + xy\overset{n}{\dot{z}}, \text{ \&c.}$$

$$+\, n\,\dot{\overset{n-1}{\dot{x}}}\,yz, \text{ \&c.} + n\,x\dot{\overset{n-1}{\dot{y}}}\,z \text{ \&c.} + \text{\&c.}$$

$+\, n.\overline{n-1}\,\overset{m-2}{\dot{x}}\,\dot{y}\dot{z}$, &c. $+$ &c. To form this quantity the ſum of &c.

all the $\overset{\alpha}{\dot{x}}\overset{\beta}{\dot{y}}\overset{\gamma}{\dot{z}}$, &c. muſt be taken where $\alpha + \beta + \gamma +$, &c. $= n$. Affixing when α or β, or γ, &c. $= o$, inſtead of $\overset{\alpha}{\dot{x}}$, x, inſtead of $\overset{\beta}{\dot{y}}$, of $\overset{\beta}{\dot{y}}, y$, &c. The coefficient of $\overset{\alpha}{\dot{x}}\overset{\beta}{\dot{y}}\overset{\gamma}{\dot{z}}$, &c. is readily deducible by the methods in the former problem, and is =

$$\frac{n\cdot\overline{n-1}\cdot\overline{n-2}\ \cdots\ 1}{\alpha\times\overline{\alpha-1}\ \cdots\ 1\times\beta\times\overline{\beta-1}\ \cdots\ 1\times\gamma\times\overline{\gamma-1}\times\ \cdots\ 1\times,\ \&c.}$$

Problem 3. To find the n^{th} fluxion of the sine of an arch x taken m times when the arch does not flow uniformly.

Solution. Radius being unity. The n^{th} fluxion of the sine of $m x$

$$= m\overset{n}{\dot{x}}\ cs\, mx\ \left.\begin{array}{l} -\,n\,\ddot{x}\,\dot{x}^{n-1} \\ -\,\dfrac{n\cdot\overline{n-1}}{1\cdot 2}\,\overset{2}{\ddot{x}}\,\dot{x}^{n-2} \\ -\,\dfrac{n\cdot\overline{n-1}\cdot\overline{n-2}}{1\cdot 2\cdot 3}\,\overset{3}{\ddot{x}}\,\dot{x}^{m-3} \\ \quad \&c. \end{array}\right|\ m^2\, s,\ mx\ -,\ \&c.$$

The following are the laws of this series:

1. The quantities to which the products of the fluxions and their coefficients are affixed are successively $m\, cs$, mx: $m^2\, s$, mx: m^3 cs, mx: &c. The sign is + or − according as the number of preceding terms of cosines is even or odd.

2. The

2. The number of fluxional factors to be affixed to the r^{th} term is r, and the ſum of their exponents is to be n. Thus if $\dot{x}^p \times \overset{2}{\dot{x}}{}^q \times$ &c. be one of theſe products $p + 2q +$ &c. $= n$, and $p + q +$ &c. $= r$.

3. The coefficient of $\dot{x}^p \times \overset{2}{\dot{x}}{}^q$, &c. is as ſtated in Prob. I.

Example. The fourth fluxion of the ſine of $3x$, when $x = o$ is $3\overset{4}{\dot{x}} - 3 \cdot 2 \cdot \dot{x}^2 \overset{2}{\dot{x}}$.

Cor. The n^{th} fluxion of the coſine of mx is had by ſubſtituting in the above ſeries for the coſine of mx, $-s$, of mx, and for s, mx, cs, mx.

The Application of the preceding Problems.

Problem 5. The relation of two quantities being expreſſed by one or more equations to find the value of one of them in a ſeries aſcending by integral powers of the other.

Solution. Let x and y be the two quantities to find x in a ſeries, aſcending by integral powers of y. Compute from the given equations, by help of the preceding problems, the values of x (A), $\dot{x}$ (B), $\ddot{x}$ (C), &c. when $y =$ a given value as a and $\dot{y} = y - a$ making y flow

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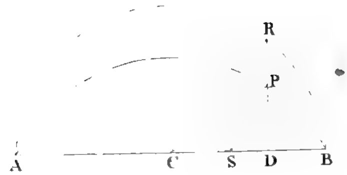

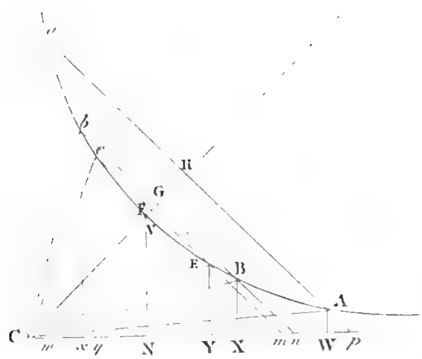

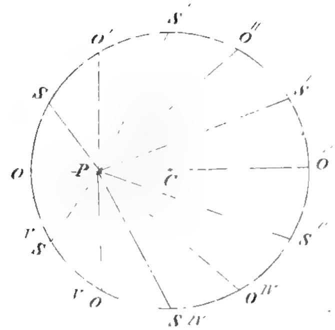

flow uniformly. Then by Taylor's theorem whilſt y changes its value from a to y, x will from A become $A + \frac{B}{1} + \frac{C}{1.2} + \frac{D}{1.2.3}$, &c.

For more readily uſing the preceding problems, it will generally be of uſe to clear the given equations from fractions, ſurds, &c. and ſometimes alſo to take the 2d, &c. fluxions generally, in order to have a more convenient equation, from which the particular fluxions of the higher orders are to be deduced. The particular fluxions of the different orders are to be taken *per ſaltum* by the preceding problems, ſubſtituting at the ſame time whenever convenient the values of x, $\dot{x}$, $\ddot{x}$, &c. previouſly found.

The utility and practice of this method will beſt appear by examples.

Example I. From the cubic equation $x^3 + qx + r = o$, to deduce the values of x in a ſeries aſcending by the powers of r.

Solution. Let the ſucceſſive fluxions of this equation be taken by Cor. Prob. 1. making $a = \dot{x}$, $b = \frac{\ddot{x}}{1.2}$, $c = \frac{\dddot{x}}{1.2.3}$ &c. and $\dot{r}$ conſtant.

1st. $\overline{3x^2+q}\,.\,a + r = 0.$

2d. $\overline{3x^2+q}\,.\,1.\,2.\,b + a^2.\,3.\,2.\,x = 0.$

3d. $\overline{3x^2+q}\,.\,1.\,2.\,3\,c + 3.\,2.\,1.\,ab.\,3.\,2.\,x + a^3.\,3.\,2 = 0.$

4th. $\overline{3x^2+q}\,.\,1.\,2.\,3.\,4\,d + \left.\begin{matrix}4.\,3.\,2.\,1\;ac\\ 4.\,3\quad b^2\end{matrix}\right|3.\,2\,x + 4.\,3.\,a^2b.\,3.\,2 = 0.$

5th. $\overline{3x^2+q}\,.\,1.\,2.\,3.\,4.\,5\,e + \left.\begin{matrix}5.\,4.\,3.\,2.\,1\;ad\\ 5.\,4.\,3.\,2.\,1\;bc\end{matrix}\right|3.\,2\,x + \left.\begin{matrix}5.\,4.\,3\;a^2c\\ 5.\,4.\,3\;ab^2\end{matrix}\right|3.\,2 = 0$

&c. &c. &c.

or $a = -\dfrac{r}{3x^2+q}$, $b = -\dfrac{3a^2x}{3x^2+q}$, $c = -\dfrac{3.\,2\,abx + a^3}{3x^2+q}$, $d = -\dfrac{3.\,2\,acx + 3\,b^2}{3x^2+q}$, $e = -\dfrac{\overline{ad+bc}.\,3.\,2.\,x + 3.\,\overline{a^2c+ab^2}}{3x^2+q}$

&c. &c.

Calling the exponents of the quantities a, b, c, &c. their places in the series, and the exponents of $\overset{p}{a}$, p: of $\overset{p}{b}$, $2p$, &c. the law of continuation is easily had. For the numerator of the quantity, the exponent of which is m, consists of two terms, the first of which is $3.\,2.\,x$ into a coeff. which is the sum of the products of every two quantities, the sum of the exponents of which is m, and when m is even, $\frac{1}{2}$ the square of the quantity

quantity, the exponent of which is $\frac{1}{3}m$ is to be added. The fecond term is the fum of all the quantities $3\alpha^2\beta$, $3.2\gamma\delta\epsilon$, fo that the fum of the exponents of each quantity $= m$: and when m is a multiple of 3, the cube of the term, the exponent of which is $\frac{m}{2}$ is to be added.

Now when $r = o$, $x^3 + qx = o$, and the values of x are o, $+\sqrt{-q}$, fubftituting thefe values in the values of a, b, c, &c. found above, and r for $\dot{r}$, we have the three values of $a + b + c +$ &c. $= \dot{x} + \frac{\ddot{x}}{2} +$ &c. the three increments of x, while r from o becomes r. Let thefe values be A, B, C, and the values of x are A, $\sqrt{-q} +$ B, $-\sqrt{-q} +$ C.

The preceding is given as an example of the method, and not to fhew its fuperiority to others. Since by affuming a feries for x, and making ufe of the multinomial theorem, the fame conclufion will be derived by a procefs equally fhort. Yet it muft be obferved, that the multinomial theorem is only a particular theorem far lefs extenfive indeed in its ufes than the method here given, and not at all more ready in practice.

Example II. Given the fine of an arch (A) to find the fine of n times that arch.

Solution. Let x = fine of A, y = fine of n A. Then $\frac{\dot{y}}{\sqrt{1-y^2}} = \frac{n\dot{x}}{\sqrt{1-x^2}}$ or $\dot{y}^2 \times \overline{1-x^2} = n^2\dot{x}^2 \times \overline{1-y^2}$, taking the fluxions generally making x flow uniformly, and dividing by $\dot{y}$.

$$\ddot{y} \times \overline{1-x^2} - \dot{y}\,x\,\dot{x} = -n^2\dot{x}^2 y.$$

The $\overline{m-2}$ fluxion of this equation being taken by Prob. 1 and 2, when $x = o$ and $y = o$,

$$\overset{m}{\dot{y}} + \frac{\overline{m-2}.\overline{m-3}}{2}\overset{m-2}{\dot{y}} \times -2\dot{x}^2 - \overline{m-2}\,\overset{m-2}{\dot{y}}\,\dot{x}^2 = -n^2\dot{x}^2\overset{m-2}{\dot{y}}$$

or $\overset{m}{\dot{y}} = -\dot{x}^2\overset{m-2}{\dot{y}} \times \overline{n^2 - \overline{m-2}|^2}$.

Now becaufe when x and $y = o$, $\dot{y} = n\dot{x}$ and $\ddot{y} = o$; it follows therefore that all the even fluxions of y are $= o$; and taking for m the odd numbers 3, 5, 7, &c. and x for $\dot{x}$, we have

$y = \dot{y} + \frac{\ddot{y}}{1.2} + \&c. = nx - \frac{n^2-1}{2.3}ax^2 + \frac{n^2-3^2}{4.5}b\dot{x} - \&c.$ a, b, &c. being the preceding terms. Alfo if k and l be the $p-1$, and p terms $l =$ (becaufe $m = \overline{2p-1}$) $= -kx^2 \times \frac{n^2 - \overline{2p-3}|^2}{\overline{2p-2} \times \overline{2p-1}}$.

The

The above ſolution affords a conſpicuous inſtance of the advantage of this method, in the ready manner in which the general law of the ſeries is derived. This ſeries has been inveſtigated by ſeveral authors ſince Sir I. Newton, who firſt invented it. But all have only deduced a few of the firſt terms, without any proof whatever of the law of the ſeries. Indeed to have deduced by any of their methods even the 10^{th} term would have been an almoſt inſuperable labour.

Example III. To expreſs the hyperbolic logarithmic ſecant by a ſeries aſcending by powers of the arch.

Solution. Let a, s, and l be the arc, ſecant, and logarithmic ſecant, rad. being unity. By the nature of the circle

$\dot{a} = \frac{\dot{s}}{s\sqrt{s^2 - 1}}$, and alſo $\dot{l} = \frac{\dot{s}}{s}$

$\therefore \dot{l}^2 = \dot{a}^2 \times \overline{s^2 - 1}$, or taking the fluxions and making $\dot{a}$ conſtant,

$2\dot{l}\ddot{l} = \dot{a}^2 \times 2s\dot{s} = \dot{a}^2 \times 2s^2 \times \dot{l}$ or $\ddot{l} = \dot{a}^2 s^2 = \dot{l}^2 + \dot{a}^2$ (A).

But when $a = o$, $s = 1$, $\therefore l = o$ and $\dot{l} = \dot{a}\sqrt{s^2 - 1} = o$: whence from the equation A it follows that all the uneven fluxions of l are $= o$, becauſe any odd fluxion of the equation muſt contain in each term the inferior odd fluxions of l. For the conveniency

 of

of applying Prob. 1. let $x = \dot{l}$, and (A) $\ddot{l} = \dot{a}^2 + x^2$, taking the $n-2$ fluxion of this equation by Prob. 1. $\overset{n}{\dot{l}} = 2\,\dot{x}\,\overset{n-3}{\dot{x}}.\,\overline{n-2} + 2\,\overset{3}{\dot{x}}\,\overset{n-5}{\dot{x}}\,\frac{\overline{n-2}.\,\overline{n-3}.\,\overline{n-4}}{3.\quad 2.\quad 1} + 2\,\overset{5}{\dot{x}}\,\overset{n-1}{\dot{x}}\,\frac{\overline{n-2}\ -\ -\ \overline{n-6}}{5.\ 4\ -\ -\ 1} +$ &c.

substituting for the fluxions of x, $\ddot{l}$, $\overset{3}{\dot{l}}$, &c. and dividing by 1, 2 - - n we get the general equation

$$\frac{\overset{n}{\dot{l}}}{1\ -\ -\ n} = 2.\,2\,\frac{\ddot{l}}{1.\ 2} \times \frac{\overset{n-2}{\dot{l}}}{1.\ 2\ -\ \overline{n-2}} \times \frac{\overline{n-2}}{\overline{n-1}.\ n} + 2.\,4\,\frac{\overset{4}{\dot{l}}}{1\ -\ -\ 4} \times$$

$$\frac{\overset{n-4}{\dot{l}}}{1\ -\ -\ \overline{n-4}} \times \frac{\overline{n-4}}{\overline{n-1}.\ n}$$ + &c. when $\frac{n}{2}$ is odd to be continued to $\frac{n-2}{4}$ terms. When $\frac{n}{2}$ is even, the last term is $\frac{\overset{\frac{n}{2}}{\dot{l}}}{1.\ 2^2\ -\ -\ n^2} \times \frac{\frac{n}{2} \times \frac{n}{2}}{\overline{n-1}.\ n}$.

Whence taking $n = 4, 6, 8$, &c.

$$l = \frac{\ddot{l}}{2} + \frac{l^4}{1.\ 2.\ 3.\ 4} +, \text{\&c.} = \frac{a^2}{1.2} + \frac{a^4}{1.3.4} + \frac{a^6}{1.3.3.5} + \frac{17\,a}{1.3.3.5.7.8}$$

$$+ \frac{31\,a^{10}}{1.3.3.5.7.9.5} + \text{\&c.}$$

Example

EXAMPLE IV. Having given the logarithmic ſecant of 45° to the modulus radius (r) = s, and the logarithmic ſecant of any arch $a = s + l$ to find a in a ſeries aſcending by the powers of l.

SOLUTION. Let n = the ſecant of a. Then by the circle $\dot{a} = \frac{\dot{n}\, r^2}{n\sqrt{n^2 - r^2}}$ alſo becauſe $s + l =$ log. n to the modulus r, $\dot{l} = \frac{r\dot{n}}{n}$ ∴ $\dot{a} = \frac{r\dot{l}}{\sqrt{n^2 - r^2}}$ or (A) $\dot{a}^2 \times \overline{n^2 - r^2} = r^2\, \dot{l}^2$. To facilitate the computations of $\dot{a}$, $\ddot{a}$, $\dot{\ddot{a}}$, &c. from the equat. A, when $l = o$, a = the arc of 45, and when alſo $n^2 = 2r^2$, let $\dot{a}^2 = x^2 =$ B. Now becauſe $\dot{n} = \frac{n\dot{l}}{r}$.

1ſt. Fluxion of $n^2 = 2n\dot{n} = \frac{2\dot{l}n^2}{r} = 2^2 r\dot{l}$, ſubſtituting for l, $\dot{l}$.

2d. Fluxion of $n^2 = \frac{2\dot{l}}{r} \times \frac{2\dot{l}n^2}{r} = 2^3\, \dot{l}^2$.

— — — — — — —

m^{th} Fluxion of $n^2 = \frac{2^{m+1}\, \dot{l}^m}{r^{m-2}}$

WHENCE

WHENCE from the equation (A) $B \times \overline{n^2 - r^2} = r^2 \dot{l}^2$, we have by Prob. 2.

$(\dot{A})$ $\dot{B} r^2 + B\, 2^2 r l = 0$ or $\dot{B} = -\frac{4 l^3}{r}$.

$(\overset{2}{\dot{A}})$ $\ddot{B} r^2 + 2 \dot{B}\, 2_2 r l + B\, 2^3 l^2 = 0$ or $\ddot{B} = \frac{24 l^4}{r^2}$.

$(\overset{3}{\dot{A}})$ $\dddot{B} r^2 + 3 \ddot{B}\, 2^2 r l + 3 \dot{B}\, 2^2 r l_2 + B \frac{2^4 l^3}{r} = 0$ or $\overset{3}{\dot{B}} = -208 \frac{l^5}{r^5}$.

$(\overset{4}{\dot{A}})$ $\overset{4}{\dot{B}} r^2 + 4 \overset{3}{\dot{B}}\, 2^2 r l + 6 \overset{2}{\dot{B}}\, 2^3 l^2 + 4 \dot{B} \frac{2^4 l}{r} + B \frac{2^5 l^4}{r^2} = 0$ or $\overset{4}{\dot{B}} = 2400 \frac{l^6}{r^4}$.

$(\overset{5}{\dot{A}})$ $\overset{5}{\dot{B}} r^2 + 5 \overset{4}{\dot{B}}\, 2^2 r l^2 + 10 \overset{3}{\dot{B}}\, 2^2 l^2 + 10 \overset{2}{\dot{B}} \frac{2^4 l^3}{r} + 5 \dot{B} \frac{2^5 l^4}{r^2} + B \frac{2^6 l_5}{r^3} = 0$ or $\overset{5}{\dot{B}} = -\frac{34624\, l^7}{r^5}$.

&c.

Now when $a = 45^\circ$, $x = \dot{a} = \dot{l}$, whence taking the fluxions of the equation $x^2 = B$ by Prob. 1, and fubftituting for $\dot{l}$, l, we get

$2 l \dot{x} = \dot{B} = -\frac{4 l^3}{r}$, or $\ddot{a} = \dot{x} = -\frac{2 l^2}{r}$.

$2 l \ddot{x}$

$$2l\ddot{x} + 2\dot{x}^2 = \ddot{B} = \frac{24 l^4}{r^2} \text{ or } \overset{3}{\dot{a}} = \ddot{x} = \frac{8 l^3}{r^2}$$

$$2l\overset{3}{\dot{x}} + 2.\,3\,\dot{x}\ddot{x} = \overset{3}{\dot{B}} = -\frac{208 l^5}{r^3} \text{ or } \overset{4}{\dot{a}} = \overset{3}{\dot{x}} = -\frac{56 l^4}{r^3}$$

$$2l\overset{4}{\dot{x}} + \begin{array}{l} 2.\,3\,\overset{2}{\dot{x}}{}^2 \\ 2.\,4\,\overset{3}{\dot{x}}\dot{x} \end{array} = \overset{4}{\dot{B}} = \frac{2400 l^6}{r^4} \text{ or } \overset{5}{\dot{a}} = \overset{4}{\dot{x}} = \frac{560 l^5}{r_4}$$

$$2l\dot{x}^5 + \begin{array}{l} 2.\,5\,\dot{x}\overset{4}{\dot{x}} \\ 2.10\,\ddot{x}\overset{3}{\dot{x}} \end{array} = \overset{5}{\dot{B}} = -34624 \text{ or } \overset{6}{\dot{a}} = \overset{5}{\dot{x}} = -\frac{7232 l^6}{r_5}$$

&c. &c.

Hence while l by flowing from o becomes l, a from a ſemiquadrant becomes =

$$\text{a ſemiquadr.} + l - \frac{l^2}{r} + \frac{4 l^3}{3 r^2} - \frac{7 l^4}{3 r^3} + \frac{14 l^5}{3 r^4} - \frac{452 l^6}{45 r^5} + \&c.$$

The two laſt examples are ſeries of Gregory's from the Comm. Epiſt. For an account and different methods of inveſtigating them ſee Scrip. Log. Vol. III. preface and pages 443, &c. 480, &c.

Example V. To expand the multinomial, $\overline{a + bz + cz^2 + dz^3 + \&c.}|^n$ where n is of any denomination whole, negative or fractional. (De Moivre Miſcell. Analytica, p. 87).

Let

LET $A = B^n = \overline{a + bz + cz^2 + dz^3 + \&c.}|^n$ Then making $z = o$, and substituting for $\dot{z}$, z

$$\dot{B} = bz$$

$$\ddot{B} = 1.\ 2\ cz^2 \qquad \text{or} \qquad \frac{\ddot{B}}{1.\ 2} = cz^2$$

$$\overset{3}{\dot{B}} = 1.\ 2.\ 3\ dz^3 \qquad \frac{\overset{3}{\dot{B}}}{1.\ 2.\ 3} = dz^3$$

$$\overset{4}{\dot{B}} = 1.\ 2.\ 3.\ 4\ ez^4 \qquad \frac{\overset{4}{\dot{B}}}{1 \ldots 4} = ez^4$$

&c. &c. &c.

WHENCE from the equation $A = B^n$ we have by Cor. Prob. 1.

$$\dot{A} = na^{n-1}bz$$

$$\ddot{A} = 2.\ na^{n-2}cz^2 + n.\ \overline{n-1}\ a^{n-2}b^2z^2$$

$$\overset{3}{\dot{A}} = 3.\ 2\ na^{n-1}dz_3 + 3.\ 2.\ n.\ \overline{n-1}\ a^{n-2}bcz + n.\ \overline{n-1}.\ \overline{n-2}\ b^3z^3$$

&c. &c.

$$\therefore A = a^n + \dot{A} + \frac{\ddot{A}}{1.\ 2} + \&c. = a + na^{n-1}bz + \frac{n.\ \overline{n-1}}{1.\ 2}a^{n-2}b^2z^2 + \frac{n.\ \overline{n-1}.\ \overline{n-2}}{1.\ 2\ \ 3}a^{n-3}b^3z^3 + \&c.$$

$$n.\ a^{n-1}cz^2 + n.\ \overline{n-1}\ a^{n-2}bcz^3 + \&c.$$

$$+ na^{n-1}dz^3 + \&c.$$

&c. &c.

THE

THE law of continuation as far as regards the products of z, and its coefficients a, b, c, &c. is evident from Prob. 1. and agrees with that given by De Moivre. The law of the coefficients of these products is also immediately derived. For let $a^{n-p}\,b^{q}\,c^{r}\,d^{s}$ be any product, then because it occurs in the terms $\overset{q+2r+3s}{\dot{A}}$, it follows from Taylor's Theorem and Prob. 1. that its coefficient is

$$\frac{1}{1.2.3 \cdots \overline{q+2r+3s}} \times n.\overline{n-1}.\overline{n-2} \cdots \overline{n-p-1} \times \frac{\overline{q+2r+3s} \times \overline{q+2r+3s-1} \times \cdots 1}{q \times \overline{q-1} \times \cdots 1 \times r\,\overline{r-1}\ 1 \times s \times \overline{s-1} \times \cdots 1}$$

$$= \frac{n.\overline{n-1}.\overline{n-2} \cdots \overline{n-p-1} \quad (p \text{ or } \overline{q+r+s} \text{ terms})}{q \times \overline{q-1} \times \cdots 1 \times r \times \overline{r-1} \times \cdots 1 \times s \times \overline{s-1} \times \cdots 1}.$$ The same as has been demonstrated by De Moivre for integral values of n.

EXAMPLE VI. From the equation $(m)\ az + b\overset{2}{z} + c\overset{3}{z} + d\overset{4}{z} +$ &c. $= gy + hy^2 + iy^3 + ky^4 +$ &c. to find the value of z when z and y begin together.

SOLUTION. Taking by Cor. Prob. 1. the successive fluxions when z and $y = o$, and y is substituted for $\dot{y}$.

$$(\dot{m})\ a\dot{z} = gy \text{ or } \dot{z} = \frac{gy}{a} = Ay \text{ putting } A = \frac{g}{a}$$

$$(\ddot{m})\ a\overset{2}{\dot{z}} + 1.2\,A^2 by^2 = 1.2\,hy^2 \text{ or } \frac{\overset{2}{\dot{z}}}{1.2} = \frac{h - bA^2}{a}y^2 = By^2$$

$(\overset{3}{\dot{m}})\ a\overset{3}{\dot{z}} + 1.2.3.2.1\ ABby^{2} + 1.2.3\ A^{3}cy^{3} = 1.2.3\ iy^{3}$

or $\dfrac{\ddot{z}}{1.2.3} = \dfrac{i - 1.2\ ABb - A^{3}c}{a}\, y^{3} = Cy^{3}$

$(\overset{4}{\dot{m}})\ a\overset{4}{\dot{z}} + 1.2.4.3.2.1\ ACby^{4} + 1.2.4.3\ B^{2}by^{4} + 1.2.3.4.3$

$A^{2}Bcy^{4} = 4.3.2.1\ hy^{4}$ or $\dfrac{\overset{4}{\dot{z}}}{1.2.3.4} = \dfrac{k - 1.2ACb - B^{2}b - 3A^{2}Bc}{a}\, y^{4} = Dy^{4}$

&c. &c.

or $z = \dfrac{g}{a}y + \dfrac{h - bA^{2}}{a}y^{2} + \dfrac{i - 1.2\ ABb - A\overset{3}{c}}{a}\overset{3}{y} +$

$\dfrac{k - 1.2\ ACb - B^{2}b - 3\ A^{2}BC}{a}\, y^{4} +$ &c. A, B, C, &c. being the coefficients of the preceding terms.

The laws of continuation are readily derived by help of Prob. 1. for calling the exponent of a, 1 of b, 2 &c. and of A, 1 of B, 2 &c. the coefficient of y^{m} is a fraction the denominator of which is a, and the numerator the difference between the coefficient of y^{m} in the given equation, and the ſum of products of the capital and ſmall letters with numeral coefficients derived by the following laws:

1. To the ſmall letter the exponent of which is n are to be affixed n capital letters, ſo that the ſum of their exponents ſhall be m: this is to be done as often as poſſible with each ſmall letter.

2. The

2. The numeral coefficient of any product $A^p\, B^q\, C^r =$

$$\frac{1.2 \cdots\cdots \overline{p+q+r} \times \overline{p+2q+3r} \times \overline{p+2q+3r-1} \cdots 1}{1.2 \cdots\cdots \overline{p+2q+3r} \times p.\overline{p-1} \times \cdots 1 \times q \times \overline{q-1} \times \overline{q-2} \cdots 1\, r \times \overline{r-1} \cdot 1}$$

$$= \frac{1.2 \cdots\cdots \overline{p+q+r}}{p.\overline{p-1} \cdots 1 + q \times \overline{q-1} \cdots 1 \times r \times \overline{r-1} \cdots 1} = \text{the number}$$

of permutations of AAA (p things) BB (q) CC (r). These laws of continuation are the same as stated by De Moivre*, and deduced by him from the application of the multinomial theorem.

Example VII. From the mean anomaly of a planet to deduce the eccentric anomaly in a series ascending by the powers of the excentricity. Fig.

Solution. Let APB be the semi-elliptic orbit described about the focus S and centre C, and P the planet: then drawing RPD perp. to AB meeting the circle described on the diameter AB, the $\angle$ ACR will be the eccentric anomaly. Let the mean anomaly $= m$ (rad. $= 1$) the eccentric anomaly $= c$, AC $= 1$, and CS the eccentricity $= e$. Then m : circumference : : area ASP : area of the ellipse : : area ASR : area of the circle $\because$ because CR $= 1$, $m = 2$ area ASR $= 2$ ACR $+ 2$ CSR $=$ BR $\times$ CR $+$ CS $\times$ DR $= c + e\,s,c$ or $m = c + e\,s,c$.

 Let

* Philosophical Transactions, Vol. XX. p. 190.

LET the fucceffive fluxions of this equation be taken by Prob. 2 and 3, when $e = o$, and $c = m$

$$\dot{c} + \dot{e}\, s, m = o$$

$$\ddot{c} + 2\,\dot{e}\,\dot{c}\, cs, m = o$$

$$\overset{3}{\dot{c}} + 3\,\dot{e}\,\ddot{c}\, cs, m - 3\,\dot{e}\,\dot{c}^2\, s, m = o$$

$$\overset{4}{\dot{c}} + 4\,\dot{e}\,\dot{c}^3\, cs, m - 4\dot{e}.\,3\,\overset{2}{\dot{e}}\,\dot{c}\, s, m - 4\,\dot{e}\,\overset{3}{\dot{c}}\, cs, m = o$$

whence fubftituting for $\dot{e}$, e

$$\dot{e} = -e\, s, m.\ \ddot{c} = -2\,\dot{c}\,c\, cs, m = 2^2\, s, m \times cs, m = e^2\, s, 2m$$

$$\overset{3}{\dot{c}} = -3e^2\,\overset{2}{\dot{c}}\, cs, m + 3e\dot{c}^2\, s, m = -3e^3 \times s, \overline{2m \times cs, m - s^3, m} =$$

$$-\tfrac{3}{4}\, e^3 \times \overline{3s, 3m - s, m}.$$

$$\overset{4}{\dot{c}} = -4\,\dot{e} \times \overset{3}{\dot{c}}\, cs, m - 3\,\overset{2}{\dot{c}}\,\dot{c}s, m - \overset{3}{\dot{c}}\, cs, m = 4e^4 \times \overline{2s, 4m - s, 2m}$$

&c. &c.

$$\therefore c = m + \dot{c} + \frac{\ddot{c}}{1.2} + \&c. = m - e\, s, m + \frac{e^2}{1.2}\, s, 2m - \frac{e^3}{1.2.4} \times$$

$$\overline{3s, 3m - s, m} + \frac{c^4}{1.2.3} \times \overline{2s, 4m - s, 2m} + \&c.$$

THIS feries is in effect the fame as the feries given by Keil, but is much better adapted for computation, and befides has the advantage of being applicable to phyfical aftronomy; which the feries of Keil is not.*

EXAMPLE

M. De la Grange has given a moft elegant theorem for expreffing in a feries afcending by the powers of t any function of x, when $x =$ any function of $u + tX$, X being a function of x. By help of his beautiful theorem, the value of c is immediately deduced from the equation $m = c + e\, s, c$. But as the theorem is only adapted to equations of that particular form, it appears equally eligible to deduce the value of c by the above method, becaufe including the demonftration the method of De la Grange is not fhorter. See Coufin's Aftro. Phyf. Art. 20, page 15.

Example VIII. From the mean anomaly of a planet to deduce the true anomaly in a series ascending by the powers of the eccentricity.

Solution. Let the semi-axis major $AC = 1$. The eccentricity $CS = e$, the anomaly $AST = a$, $m =$ the corresponding mean anomaly measured in the circle the rad. of which $= 1$, and the periphery P. Then as the areas are proportional to the times, and therefore to the mean anomalies: Fig.

Flux. area AST : area of the ellipse :: $\dot{m}$: P ∴ $\frac{1}{2} ST^2 \times$ flux. ∠ AST ($\dot{a}$) = flux. area AST $= \frac{\dot{m} \times \text{area of the ellipse}}{P} = \dot{m} \times \frac{1}{2}\sqrt{1-e^2}$ or $\dot{a} \times ST^2 = \dot{m}\sqrt{1-e^2}$. But by the prop. of the ellipse $ST = \frac{1-e^2}{1-e\,cs, a}$. Hence $\dot{a} \times \frac{\overline{1-e^2}|^{\frac{1}{2}}}{\overline{1-e\,cs, a}|^2} = \dot{m}$ or $\overline{1-e^2}|^{\frac{3}{2}} \times \dot{a} \times \overline{1 + 2e\,cs, a + 3e^2\,cs^2, a + 4e^3\,cs^3, a + \&c.} = \dot{m}$. Let $A = \text{fl}\;\dot{a}\,cs\,a$, $B = \text{fl}\;\dot{a}\,cs^2, a$, $C = \text{fl}\;\dot{a}\,cs^3, a$, &c. and $L = \overline{1-e^2}|^{-\frac{3}{2}}$. Then $a + 2eA + 3e^2B + 4e^3C + \&c. = Lm$. From this equation the series is to be deduced by successively taking its fluxions by Prob. 2. making e flow uniformly &c. $e = 0$

1. $\dot{a} + 2\dot{e}A = L\dot{m} = 0$

2. $\ddot{a} + 2.\,2\dot{e}\dot{A} + 3.\,2\dot{e}^2B = 3\dot{e}^2m$

3. $\overset{3}{\dot{a}}$

$$3.\ \overset{3}{\dot{a}} + 3.\ 2\ \dot{e}\ \ddot{A} + 3.\ 3.\ 2\ \dot{e}^2\ \dot{B} + 4.\ 3.\ 2\ \dot{e}^3\ C = \overset{3}{\dot{L}}\,m = 0$$

$$4.\ \overset{4}{\dot{a}} + 4.\ 2\dot{e}\,\overset{3}{\dot{A}} + 6.\ 3.\ 2\dot{e}^2\,\overset{2}{\dot{B}} + 4.\ 4\ 3.\ 2\dot{e}^3\,\dot{C} + 5.\ 4.\ 3.\ 2\ \dot{e}^4\ D = \ddot{L}m = 45\dot{e}^4$$

&c. &c.

Now fince $A = s, a$

By Prob. 3. $\dot{A} = \dot{a}cs, a$ $\quad$ $B = \frac{1}{2}a + \frac{1}{4}s, 2a$

$\ddot{A} = \ddot{a}\,cs, a - \dot{a}^2 s, a$ $\quad$ $\dot{B} = \frac{1}{2}\dot{a} + \frac{1}{2}\dot{a}\,cs, 2a$

$\overset{3}{\dot{A}} = \overset{3}{\dot{a}}cs, a - 3\ddot{a}\dot{a}s, a - \dot{a}^3 cs, a$ $\quad$ $\ddot{B} = \frac{1}{2}\ddot{a} + \frac{1}{2}\ddot{a}\,cs, 2a - \dot{a}^2 s, 2a$

$C = \frac{3}{4}s, a + \frac{1}{12}s, 3a$ $\quad$ and $D = \frac{5}{8}a + \frac{1}{4}s, 2a + \frac{1}{32}$

$\dot{C} = \frac{3}{4}\dot{a}cs\, a + \frac{1}{4}\dot{a}\,cs, 3a$

&c. &c.

Let thefe values be fubftituted in the above equations, and we deduce making $\dot{e} = e$ from the

1ft. Equat. $\dot{a} = -2es, m$

2d. $\ddot{a} = \frac{5}{2}e^2 s, 2m$

3d. $\overset{3}{\dot{a}} = -e^3 \times \overline{\frac{13}{2}s, 3m + \frac{5}{2}s, m}$

4th. $\overset{4}{\dot{a}} = e^4 \times \overline{\frac{103}{4}s, 4m + 11s, 2m}$

&c. &c.

Whence $a = m + \left.\begin{matrix} 2e \\ \frac{1}{4}e^3 \end{matrix}\right\} s, m \left.\begin{matrix} +\frac{5}{4}e^2 \\ -\frac{11}{24}e^4 \end{matrix}\right\} s, 2m - \frac{13}{12}e^3 s, 3m + \frac{103}{96}$ $e^4 s, 4m +$ &c.

The

The ſecond power of the eccentricity or two terms of the ſeries will be ſufficient for the orbits of the Earth and Venus. The third power of the eccentricity or three terms for Jupiter, Saturn, and the Georgium Sidus, and four for Mars. But ſix or ſeven are neceſſary for Mercury. It is more tedious than difficult to continue this ſeries to a greater number of terms. The above ſolution of this uſeful and celebrated problem, beſides being direct is greatly ſhorter than any before given: Even than the method of Cagnioli, given by De La Lande, in the third volume of his Aſtronomy, edition 1792, where the ſeries is continued to the ninth power of the eccentricity. By a very ingenious artifice there given the ſolution by indeterminate coefficients is very conſiderably ſhortened. The legality of that artifice might however be juſtly doubted, and the truth of the concluſion deduced ſuſpected, unleſs verified by other methods.

Example IX. From the equation

$c x^n \dot{x} + y \dot{x} = a \dot{y}$ to find y by a ſeries aſcending by the powers of x, n being a whole poſitive number (Simpſon's Fluxions, Vol. II. 293).

Solution. When $x = o$, let $y = Y$. Then taking the fluxions of the given equation when $x = o$, and x flows uniformly.

1ſt. $\dot{y} \dot{x} = a \ddot{y}$

2d. $\ddot{y} \dot{x} = a \overset{3.}{y}$

- - - -

- - - -

n^{th}.

$n^{th}.\ n.\overline{n-1}\ ---\ 1\ c\overset{n+1}{\dot{x}} + \overset{n}{\dot{y}}\dot{x} = a\overset{n+1}{\dot{y}}$

$-----------\ \overset{m}{\dot{y}}\dot{x} = a\overset{m+1}{\dot{y}}$

$m^{th}.\ -------$

&c. &c.

THEN becaufe when $x = 0$, and $y = Y$, $\dot{y} = Y\dot{x}$ we immediately deduce

$$\ddot{y} = \frac{Y\dot{x}^2}{a^2},\ \overset{3}{\dot{y}} = \frac{Y\dot{x}^3}{a^3}\ ----\ \overset{n+1}{\dot{y}} = n.\overline{n-1}\ --\ 1\,\frac{c\overset{n+1}{\dot{x}}}{a} + \frac{Y\dot{x}^n}{a^n},\ \overset{m+1}{\dot{y}} = \frac{\overset{m}{\dot{y}}\dot{x}}{a}, \text{ \&c.}$$

$\because$ fubftituting for $\dot{x}$, x; $y = Y + Y \times \overline{\frac{x}{a} + \frac{x^2}{1.2\,a^2} + ----\ \frac{x^n}{1.2.-na^n} + \frac{x^{n+1}}{1.2.\overline{n+1}\,a^n}} + \text{\&c.} + \frac{cx^{n+1}}{\overline{n+1}\,a} + \frac{cx^{n+2}}{\overline{n+1}.\overline{n+2}\,a^2} + \text{\&c.}$

THIS example was given to remark that fometimes by this method we may derive a general folution from the particular one. For although the above folution is only a particular one viz. when x is fuch that the feries will converge, yet becaufe we know that $1 + \frac{x}{a} + \frac{x^2}{1.2.a^2} + \text{\&c.} =$ no. the hyp. log. of which is $\frac{x}{a}$, and alfo becaufe $\frac{cx^{n+1}}{\overline{n+1}.\,a} + \frac{cx^{n+2}}{\overline{n+1}.\overline{n+2}\,a^2} + \text{\&c.} =$

$$ca^n \times 1.2\,--\,n \times \overline{1 + \frac{x}{a} + ---\ \frac{x^{n+1}}{1.2\,--\,\overline{n+1}\,a^{n+1}} + \text{\&c.} - 1 + \frac{x}{a} + --\ \frac{x^n}{1.2.\,--\,n\,a^n}}$$

$\frac{x}{1.2\cdots n a^n} = c a^n \times 1.2\cdots n \times$ no. hy. log. $\overline{\frac{x}{a} - 1 + \frac{x}{a} + \cdots \frac{x}{1.2\cdots n a^n}}$; if M = no. hyp. log. of which is 1, $y = \mathrm{Y}\,\mathrm{M}^{\frac{x}{a}} + \overline{1.2\cdots n c a^n} \times \mathrm{M}^{\frac{x}{a}} - 1.2\cdots n c a^n + 1.2\cdots n c a^{n-1} x + 3.4\cdots n c a^{n-2} x^2 + \cdots + c x^n$ is the general equation of the fluents.

As the above examples have confiderably extended the length of this tract, the fubject fhall be concluded by a few obfervations.

The Theorem of Taylor may be more generally expreffed, for if z be a quantity compofed of two or more independent quantities x, y, v, &c. then while x, y, v, &c. by flowing uniformly become $x + \dot{x}$, $y + \dot{y}$, $v + \dot{v}$, &c. z will become $z + \dot{z} + \frac{\ddot{z}}{1.2} +$ &c. There can be no difficulty in applying what has been before done to cafes of this kind. It may be worthy of remark, however, that by this method when fluxions are fuch that the fluents are expreffed in integral powers, they may be found *a priori*: for if z be a function of x, y, &c. where x, y, &c. are independent quantities, and Z the value of z when x, y, &c. $= o$, then becaufe $z = \mathrm{Z} + \dot{z} + \frac{\ddot{z}}{1.2} +$ &c. and becaufe $\frac{\ddot{z}}{1.2}$, &c. are derived from $\dot{z}$ by taking the

the fluxions, making $\dot{x}, \dot{y}$, &c. conftant, it follows that z may be deduced from $\dot{z}$ by taking the fucceffive fluxions of $\dot{z}$ by the former problems.

Examples. The fluent of $\overset{n}{x}\dot{x} = \text{Cor.} + \frac{n.\overline{n-1} - - - 1^{n+1}}{1.2 - - - \overline{n+1}} x = \frac{x^n}{n+1} + \text{Cor.}$

The fluent of $3\,x^2 y \dot{x} + x^3 \dot{y} + 2\,x y^2 \dot{x} + 2\,x^2 y \dot{y} =$ (taking x and $y = o$, and fubftituting for $\dot{x}$ and $\dot{y}$, x and y)

$$\frac{3.\,3.\,2\,x^3 y + 3.\,2\,x^3 y + 2.\,3.\,2\,x^2 y^2 + 2.\,3.\,2\,x^2 y^2}{1.\;2.\;3.\;4} = x^3 y + x^2 y^2.$$

The fourth example when n is odd is an inftance of finding fluents *a priori* by this method. If $\dot{x} = Y\dot{y}$, where Y is an algebraic function of y, then by common algebra reducing this equation to integral values, and taking the fluxions *particularly* by the former rules, it will be known whether x the fluent can be had in finite terms; in fome cafes, very readily, in many, however, the difficulty will greatly exceed the inverfe method, but this difficulty may be probably obviated by given the fubject that attention it feems to deferve.

But it ought to be remarked when there are two or more independent variable quantities, that the given fluxion muft be poffible, that is, muft have originated from a fluent. Thus for inftance $y\dot{x}$ is not a poffible fluxion, for it cannot have originated from any flowing quantity wherein x and y are independent.

The

THE above method may alſo be applied with conſiderable advantage to the finite variations of ſpherical triangles, and in many inſtances ſeries may be deduced more convenient in aſtronomical computations than the theorems for finite differences.

ACCOUNT OF THE WEATHER

At Londonderry in the Year 1799,

By WILLIAM PATERSON, *M.D. and M.R.I.A.*

Months.	Prevalent Winds.	Fair Days.	Showery.	Wet.	Total.	Hail.	Snow.	Frost.	Thunder and Lightning.
January	S W	10	18	3	31	0	6	14	0
February	W	11	14	3	28	4	9	9	1
March	W	12	18	1	31	4	4	4	0
April	N	15	15	0	30	0	2	5	0
May	N W	8	18	5	31	2	0	0	0
June	N	16	12	2	30	2	0	0	1
July	W	12	15	4	31	0	0	0	1
August	W	1	25	5	31	0	0	0	0
September	S E	6	21	3	30	1	0	0	1

in the latter part, it ſoftened in its rigour, it was often bluſtry —*March*, equally as February, was remarkable for blowing weather, with ſeveral ſqually gales at night and heavy ſhowers.—Although the warm winds exceeded the cold in number, in the points taken ſingly, yet taken together, the cold were to the warm as 22 to 19; and upon the whole it was a rigorous unpleaſant month.—*April* was remarkably keen and bluſtry; the oldeſt perſon living did not remember ſo much ſnow in this month; the greateſt part of it fell on the 5th, which was little ſhort of the 8th of the preceding February with reſpect to the degree of wind from the SE. the piercing cold, and drifting ſnow.—*May* was alſo cold, with ſome ſevere blowing weather, particularly the 23d and 24th, which were moſt ſtormy at night; and the temperature did not ſoften till the 28th.—*June* was cold in the beginning, but afterwards contained a good deal of bright, fair, warm weather.—During the field-work might have been better performed by a little attention.—In *October* the rain was moſtly in frequent heavy ſhowers; there were ſeveral fair intervals; and there were ſeveral very uſeful freſh breezes.—Hail of an unuſual ſize fell the 14th, about 3 miles S. E. of Derry.—*November* was a cold, blowing month, with much denſe fog, and frequent ſevere ſhowers of both rain and hail; yet the cold and windy weather, together with ſeveral fair days and dry intervals, anſwered an excellent purpoſe to the farmer.—In one of the ſtormy nights, the 6th, the large metal vane was blown from the cupola of the Exchange, but no perſon was hurt.—Though the prevalent winds in *December* were from the cold points, E. and S. E. with ſome ſmart gales, yet the degree of congelation was not proportionably keen.—Little rain fell; but there was a good deal of foggy and hazy weather.

NOTE.—*The greateſt degree of* heat *was on the* 8*th of June, when the thermometer roſe to* 74°; *barometer* 30. 31; *hygrometer* $35\frac{1}{2}$; *wind S. calm, fair, and bright. The greateſt degree of* cold *took place on the* 30*th of January, when the thermometer dropped to* 21°; *barometer* 29. 60; *hygrometer* 38 3-4; *W. fair, froſt, and fog. The annual quantity of rain was about* 36 *inches, which exceeds that of* 1798, 3 *inches, and that of* 1797, 5 *inches.*

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Months.	Prevalent Winds	Fair Days.	Showery.	Wet.	Total.	Hail.	Snow.	Frost.	Thunder and Lightning.
January	S W	10	18	3	31	0	6	14	0
February	W	11	14	3	28	4	9	9	1
March	W	12	18	1	31	4	4	4	0
April	N	15	15	0	30	0	2	5	0
May	N W	8	18	5	31	2	0	0	0
June	N	16	12	2	30	2	0	0	1
July	W	12	15	4	31	0	0	0	1
Auguſt	W	1	25	5	31	0	0	0	0
September	S E	6	21	3	30	1	0	0	1
October	S W	5	20	6	31	4	0	2	4
November	W	11	13	6	30	5	0	6	1
December	E	21	9	1	31	0	4	13	0
Total -	W	128	198	39	365	22	25	53	9
1798	W	126	207	32	365	22	14	29	2

☞ The greater part of the numbers in the 10th column denote Lightning alone, particularly in the months of September, October, and November, when it took place moſtly in the night, and in the uſual months for Thunder very little occurred; the cauſe of which may be aſcribed to the atmoſpherical electricity having been conveyed to the earth by the conductor, *rain*, before it had time to accumulate in the atmoſphere and form thunder clouds.

GENERAL REMARKS.

January a good deal of hazy and foggy weather, with both moderate and keen froſt, whilſt the winds were chiefly from the mildeſt points; the barometer was many days, at the beginning of the month, above 30, and varied little, though ſometimes there was heavy rain; and the ſtrongeſt freezing took place with the wind at weſt.—The winds were in general not only ſoft in temperature, but moderate in force, there not being more than 4 or 5 blowing days.—Theſe circumſtances point out the character of this month as unuſual for the ſeaſon of the year; it ſeems to be remarkable for a mixture of gentle winds, ſharp froſt, and damp, foggy air.—*February* contained a great proportion of blowing weather, particularly on the 7th and 8th at night, when there were extraordinary high and penetrating ſqually gales with conſiderable quantities of round ſnow; between this and the preceding month there were 12 days of uninterrupted freezing; and whilſt, in the latter part, it ſoftened in its rigour, it was often bluſtry—*March*, equally as February, was remarkable for blowing weather, with ſeveral ſqually gales at night and heavy ſhowers.—Although the warm winds exceeded the cold in number, in the points taken ſingly, yet taken together, the cold were to the warm as 22 to 19; and upon the whole it was a rigorous unpleaſant month.—*April* was remarkably keen and bluſtry; the oldeſt perſon living did not remember ſo much ſnow in this month; the greateſt part of it fell on the 5th, which was little ſhort of the 8th of the preceding February with reſpect to the degree of wind from the S E. the piercing cold, and drifting ſnow.—*May* was alſo cold, with ſome ſevere blowing weather, particularly the 23d and 24th, which were moſt ſtormy at night; and the temperature did not ſoften till the 28th.—*June* was cold in the beginning, but afterwards contained a good deal of bright, fair, warm weather.—During the greater part of the fair weather there was a freſh breeze from the N. and ſometimes there was a covered ſky, threatning rain, though none fell; whilſt upon the whole the air poſſeſſed a conſiderable drying quality.—*July* produced a quantity of rain, principally in heavy ſhowers, yet hay was well ſaved, owing to great abſorption and evaporation going on at the ſame time, in conjunction with frequent freſh breezes—The leading character of *Auguſt* was wetneſs; but as there were ſeveral freſh breezes and many fair intervals, more might have been done in works of huſbandry than was really effected.—The beginning of *September* was remarkably warm; and there were ſome fair days, though a duſty like hazineſs of the air indicated much diſengaged moiſture, which was confirmed by the hygrometer. Yet the nature of the weather was ſuch in general, with reſpect to exemption from the moiſture collecting in clouds, and good circulation by the winds, that field work might have been better performed by a little attention.—In *October* the rain was moſtly in frequent heavy ſhowers; there were ſeveral fair intervals; and there were ſeveral very uſeful freſh breezes.—Hail of an unuſual ſize fell the 14th, about 3 miles S. E. of Derry.—*November* was a cold, blowing month, with much denſe fog, and frequent ſevere ſhowers of both rain and hail, yet the cold and windy weather, together with ſeveral fair days and dry intervals, anſwered an excellent purpoſe to the farmer.—In one of the ſtormy nights, the 6th, the large metal vane was blown from the cupola of the Exchange, but no perſon was hurt—Though the prevalent winds in *December* were from the cold points, E. and S. E. with ſome ſmart gales, yet the degree of congelation was not proportionably keen.—Little rain fell, but there was a good deal of foggy and hazy weather.

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July	30,27	6t		
August	30,28	29t	15	2,995141
September	30,50	2(	10	2,285765
October	30,50	26t	19	1,753733
November	30,60	21(	13	1,182292
December	30,75	20t	6, and on 5 fell Snow	1,024653
	30,51		160, and on 20 fell fome Snow	22,584859 Total of the Year.

N. B. The Statements of the Mo land during thofe Months.

Synoptical View of the State of the Weather at Dublin in the Year 1799.

By RICHARD KIRWAN, *Esq. L.L.D. Pref. R.I.A. and F.R.S.*

1799.	BAROMETER.					THERMOMETER.			RAIN.	
	Higheft.	Day it happened.	Loweft.	Day it happened.	Mean of the Month.	Higheft in the Day.	Loweft at Night.	Mean.	Days.	Inches.
January - -	30,60	4th, E.	29,38	23d, NW. to W.	30,09	50,	23,	36,75	11, and on 2 fell Snow	1,471245
February - -	30,38	25th, W.	29,10	21ft, Var. S. to W. & NE.	29,82	54,	14,50	36,4	14, and on 8 fell Snow	3,54687
March - -	30,46	6th, W. & E.	29,37	19th, S. to W.	29,67	50,	30,	39,0	17, and on 5 fell Snow	1,418751
April - -	30,50	14th, E.	29,05	18th, E. to W.	29,85	55,	28,	40,75	23, and on 2 fell Snow	3,947975
May - -	30,67	16th, Var. S. W. & N.	29,70	21ft, W.	30,09	65,	35,	48,1	15	0,867014
June - -	30,61	11th, E.	29,32	4th, W.	30,31	74,	44,5	54,8	7	0,985243
July - -	30,27	6th, W.	29,35	18th, E.	[illegible]	70,	44,	56,9	10	1,123177
Auguft - -	30,28	29th, W. N.	29,46	5th, S.	[illegible]	65,	43,5	54,8	15	2,995141
September - -	30,50	2d, W.	29,25	29th, S.	[illegible]	[illegible]	41,	51,3	10	2,285765
October - -	30,50	26th, W.	29,00	31ft, SE. & W.	[illegible]	[illegible]	32,50	44,45	19	1,753733
November - -	30,60	21ft, W.	28,86	1ft, W.	29,98	52,50	29,50	41,3	13	1,152292
December - -	30,75	20th, N. to E.	29,47	2d, E. to S.	30,20	45,	23,	36,15	6, and on 5 fell Snow	1,024653
	30,51		29,276		29,977			45,06	160, and on 20 fell fome Snow	22,58459 Total of the Year.

N. B. The Statements of the Months of June, July, Auguft and September, may not be altogether to be depended upon, as the Author was abfent in England during thofe Months.

POLITE LITERATURE.

Some OBSERVATIONS *upon the* GREEK ACCENTS. *By* ARTHUR BROWNE, *Esq. Senior Fellow of Trinity College, Dublin, and M. R. I. A.*

HAVING lately had an opportunity of converfing with fome modern Greeks, it appeared to me, that it might not be unacceptable to the Academy to communicate fome obfervations which I made as to their mode of ufing and applying the accents, about the proper meaning and application of which fo much controverfy has arifen. December 14th 1799.

To make thefe obfervations intelligible, I muft briefly recal to the recollection of the Academy fome of the moft celebrated opinions which have been urged concerning thefe accents, both as to their ancient exiftence and as to their ufe.

Grævius, Stevens, and Ifaac Voffius in an exprefs treatife on the fubject endeavoured to prove them of modern invention, infifting that none are to be found in either infcriptions or manufcripts antecedently to the period of about 170 years before Chrift. Hennin imagines that they were the invention of the Arabians

fo

ſo late as the eighth century, and were uſed only in poetry, and intended to aſcertain the pronunciation of the Greeks, and to oppoſe the barbariſm of nations who raiſed and depreſſed the tone of the voice according to the cuſtom of their own language without any regard to the true quantity of ſyllables.* Wetſtein, the learned profeſſor of Baſle, in his Diſſertatio de Accentruum Græcorum antiquitate & uſu, argues for the uſe of accents from the earlieſt days, and thinks that when the mode of writing was in capital letters equi-diſtant from each other, without diſtinction either of words or phraſes, that accents noted by viſible marks were abſolutely neceſſary to diſtinguiſh ambiguous words, and to point out their proper meaning.

The writers of the laſt century were no leſs divided as to the uſe of the accents than as to their antiquity; ſome inſiſting that they marked tones or intonation—the raiſing or lowering of the voice in pronouncing certain ſyllables of words; while others confound them with quantity, or at leaſt aſſerted that quantity was influenced or affected by them.

These diſputes have been revived with no ſmall ardour in our own times. About 1754, a learned anonymous treatiſe appeared upon

* This ſeems abſurd, becauſe the accents do not accord with quantity, and therefore would ſo have ſet them wrong inſtead of right. No, the uſe of the accents muſt have been to prevent their pronouncing always according to the quantity of the ſyllable, and to ſhew them when the Greeks did not do ſo.

upon accents, denying their antiquity and ſupported by numerous arguments and quotations. About ſix years afterwards Mr. Foſter's celebrated work appeared, ſtriving to prove that they were only marks of intonation, and in 1764 was publiſhed the Accentus Redivivi of Mr. Primatt, aſſerting their antiquity, and admitting that they do affect metrical quantity, in ſo much, according to his opinion, as to be deſtructive of it.

From this laſt opinion it neceſſarily followed, in his opinion, and that of many others, that however it may be right to uſe them in proſe, they are not calculated to regulate the recitation of verſe; and hence the common dictum which is ſo often heard from the ſons of Oxford and Cambridge, that we are to read by accent in proſe and by quantity in verſe.

About ten years ſince a ſmall work appeared, but of great erudition, ſuppoſed, and now I believe not denied, to be written by a learned prelate of the Engliſh church, entitled De Rhythmo Græcorum; and at a much later period, a Treatiſe on the Proſodies of the Greek and Latin Languages, aſcribed to another celebrated prelate on the Engliſh bench, and fraught with abundant learning, and intimate knowledge of Greek literature. In the firſt work I would only at preſent refer the reader to the fifth chapter, where the author oppugns the opinion *aliam eſſe in ſoluta oratione ſcanſionem rhythmicam, aliam in metris*, in oppoſition to Faber,

Faber, Dacier, Pearce, Clarke and others; but from the latter it is neceſſary to quote an obſervation or two to prepare us for an application of the facts hereafter to be mentioned. The very learned author, after contending for the antiquity of the accents, totally condemns the rule which has been mentioned, that we are to read by accent in proſe and quantity in verſe, *obſerving truly, that it is not very probable that any people ſhould have had two pronunciations eſſentially different, one for proſe, and another for verſe.* He equally condemns the poſition that proſe as well as verſe in Greek muſt be read by quantity, that is, as he ſays, by the Latin accent, and thinking that the Greek accentual marks expreſs the true ſpeaking tones of the language, propoſes rules of recitation on the bold ſuppoſition that tone was not always laid on *connected* words, where the accentual marks appear; whoſe poſition however was not changed, to prevent the confuſion which would follow from making the poſition of the written mark different in connected, from what it is in iſolated words: and he juſtly cenſures the printing of books unaccented, one of which, an edition of Theocritus, had eſcaped from the Clarendon preſs. He holds that though in placing accent, regard is had to quantity *, euphoniæ gratia, and though it therefore may be a ſymptom of quantity, it is never a cauſe of

* For, ſays he, the general ſound of the word will be more or leſs agreeable, according as ſyllables at certain diſtances from the ſeat of the acute accent are long or ſhort. Hence, if accent were placed without any regard to quantity, it would often ſeduce the ſpeaker into a violation of quantity, for the ſake of the general euphony of the word.

of it, and never creates it; and he calls the opinion of Mr. Primatt and others, that the acute accent lengthens the tone of the ſyllable on which it falls, a common prejudice. But he doth not deny that accent will often be at war with quantity, unleſs tranſpoſed in the manner by him recommended. Thus in the line

Μῆνιν ἄειδε θεὰ Πηληϊάδεω Ἀχιλῆος,

the word, Αχιλῆος, muſt be pronounced Ἀχιλήος.

Although I never could aſſent to a poſition ſo ſtrongly contradictory to the teſtimony of my ear as that of the acute accent not lengthening the ſyllable upon which it falls; and although my mind was much impreſſed with a ſaying of Mr. Primatt, that it is one of the extraordinary powers of the acute accent, even to change the real quantity, and with his aſſertion, that the opinion of Meſſieur de Port Royal, that the accent only raiſes the voice but gives no duration in pronouncing, is falſe; I found myſelf diſpoſed to acquieſce in the ſentiment that the accents denoted only tone, or elevation and depreſſion of the voice: and this theory ſeemed to complete the perfection of the Greek language, apparently aiming at more accuracy, and greater freedom from ambiguity than any other language ever did; as to the time of an action by the variety of its tenſes, as to the number of agents by its addition of the dual, as to the object of the act by its three voices, as to the varying pronunciation of its tribes by its analyſis of the dialects, and as to the diſtinction of words written and ſpelt in the ſame manner, by its accents. We know that ſome nations, particularly the Chineſe, have ſo uſed the accents. They have, ſay the miſſionaries, but about three hundred and fifty words in their language.* Confuſion is avoided by the accents, though theſe are not eaſily diſtinguiſhed by an European ear; we knew that this muſt ſometimes have been the caſe in Greece. as in the inſtances of Ανὰ & Διὰ. The illuſtration from our national and provincial accents is obvious †.

It occurred to me, however, that it was very ſurpriſing that no author on the ſubject ſeemed to have taken the pains to enquire what was the pronunciation of the modern Greeks, or their mode of uſing the accents: is it that no inference can be drawn from their uſage, as to that of the ancients? this is eaſily ſaid, but it has not been ſaid by any of theſe writers. The argument from the Italian pronunciation of Latin giving us no inſight into that of the Romans, doth not apply; for the incurſions of barbarous ſwarms, like ſucceſſive overflowings of the ocean, have waſhed away every trace of connection between the ancient and modern inhabitants of Italy, and perhaps there are more deſcendants of the Romans to be found in

* Others ſay twelve hundred, and that the nouns are only three hundred and twenty-ſix—all monoſyllables. From the combination of theſe all their compounds ariſe. The Greek language has but about three hundred radicals. The Greeks, it has been ſaid, had but two accents; the acute never riſing above a fifth higher than the grave, though it might leſs: the Chineſe many, with intervals much ſmaller, and more exactly marked and limited.

† *E. G.* a vulgar Scotchman would ſay whence cáme you, how dó you: a common Iriſhman, whence came yóu, how do yóu—and an Engliſh farmer perhaps would ſay whence cãame you. The firſt puts the acute accent on the middle word, the ſecond on the laſt, and the drawl of the Engliſh farmer is marked by the circumflex.

in other countries, for inſtance, in Spain, than in Italy itſelf. Certainly the Spaniſh language has more obvious affinity to the Latin than the Italian has. But the hiſtory of Greece has been far different. Twelve hundred years have elapſed ſince the Weſtern or Latin empire was overturned, but we muſt remember that the Eaſtern or Greek empire exiſted till about 300 years ſince, and down as late as the reign of Henry VII; and the Grecian people has not been exterminated, but remained ever ſince, uſing its own religion and language, though in ſubjection to the Turkiſh yoke. It is the ſame people, as much as the Welch are ſince they were conquered by Edward the Firſt, and I do not ſee why their mode of pronunciation ſhould be more altered. Twelve hundred years have elapſed ſince Latin was a living language, but Greek is a living language to this day. I ſpeak from my own knowledge when I ſay, that the prayer books uſed by the Greek ſailors, the only deſcription of men of that nation whom we can expect to ſee here, are in ancient Greek,—they are able to read the ancient Greek authors, though from want of education not able to tranſlate them fluently, and their letters written in modern Greek are eaſily to be underſtood by us, and differ from ancient Greek, allowing for the ignorance and uncouth ſtile of a mariner, little more than one ancient dialect did from another. I ſhall produce one to the Academy, now in my poſſeſſion.

Impressed with theſe ſentiments I felt myſelf intereſted, when I heard that a Grecian ſhip, whoſe ſeizure has ſince been the occaſion of a remarkable ſuit in the Court of Admiralty, and of the conſequent detention of the ſeamen for a conſi-

derable

derable time, had been driven by ſtreſs of weather into the port of Dingle in this kingdom. This ſhip, called La Madona del Caſo San Speridione, Captain Demetrio Antonio Polo, belonged to Patraſs, a town ſituated not far from the ancient Corinth. The buſineſs of their ſuit brought the captain and ſeveral of the crew to Dublin, and was the occaſion of their remaining in this metropolis for a conſiderable time. I took the opportunity of frequently converſing with them, and though their want of erudition and information might ſeem an argument againſt drawing any inference from their practice, to me it appeared the contrary, becauſe it gave me the unprejudiced and unpremeditated modes of pronunciation of perſons who could not underſtand or know the reaſons of my enquiries, or purport of my obſervations. The reſult was, to my great ſurpriſe, that the practice of the modern Greeks is different from any of the theories contained in the books I have mentioned: it is true they have not two pronunciations for proſe and for verſe, and in both they read by accent, and ſo far confirm the theory of the learned biſhop, the lateſt writer I have mentioned; But they make accent the cauſe of quantity; they make it govern and control quantity; they make the ſyllable long on which the acute accent falls, and they allow the acute accent to change the real quantity: in theſe latter reſpects therefore they agree with Mr. Primatt, but they deſert him when he therefore concludes that poetry is not to be read by accent—they always reading poetry as well as proſe by accent. Whether any inference can hence be drawn as to the pronunciation of the ancients, I muſt leave, after what I have pre-

miſed

miſed above, to men of more learning, but I think it at leaſt ſo probable as to make it worth while to communicate to the Academy the inſtances which occurred in proof of this aſſertion more particularly. Of the two firſt perſons whom I met, one, the ſteward of the ſhip, an inhabitant of the iſland of Cephalonia, had had a ſchool education: he read Euripides and tranſlated ſome eaſier paſſages without much difficulty. By a ſtay in this country of near two years he was able to ſpeak Engliſh very tolerably, as could the captain and ſeveral of the crew, and almoſt all of them ſpoke Italian fluently. The companion however of the ſteward could ſpeak only modern Greek, in which I could diſcover that he was giving a deſcription of the diſtreſs in which the ſhip had been, and though not able to underſtand the context could plainly diſtinguiſh many words, ſuch as δενδρα—ξυλον, and amongſt the reſt the ſound of Ανθρωπος pronounced ſhort; this awoke my curioſity, which was ſtill more heightened when I obſerved that he ſaid Ανθρωπων long, with the ſame attention to the alteration of the accent with the variety of caſe, which a boy would be taught to pay at a ſchool in England*. Watching therefore more cloſely, and

* It will not be ſuppoſed that this man knew the rule, ſi ultima ſit longa, acuitur penultima, ſi brevis, antepenultima. I cannot avoid here lamenting the total inattention to the rules of accent in our ſchools in Ireland. Suppoſe it to be an uſeleſs part of learning, if cuſtom in England has made it thought ornamental and neceſſary, the Iriſh ſcholar who is ignorant of it will be cenſured, however undeſervedly. I have known men of high literary name in this country who did not know the meaning of the marks which diſtinguiſh encliticks, and gave to oxytones the very converſe of their real meaning. An Engliſh ſcholar who publiſhes a Greek claſſic, could accent it without looking on an accented copy.

and aſking the other to read ſome ancient Greek, I found that they both uniformly pronounced according to accent, without any attention to long or ſhort ſyllables where accent came in the way; and on their departure, one of them having bade me good day, by ſaying Καλη̄μερα, to which I anſwered Καλημε̆ρα, he with ſtrong marks of reprobation ſet me right, and repeated Καλημε̄ρα; and with like cenſure did the captain upon another occaſion obſerve upon my ſaying Socrătes inſtead of Socrātes.

I NOW felt a vehement wiſh to know whether they made the diſtinction in this reſpect uſually made between verſe and proſe, but from the little ſcholarſhip of the two men with whom I had converſed, from the ignorance of a third whom I afterwards met, (who however read Lucian with eaſe, though he did not ſeem ever to have heard of the book,) and on account of my imperfect mode of converſing with them all, I had little hopes of ſatisfaction on the point, nor was I clear that they perfectly knew the difference between verſe and proſe.

AT length having met with the commander of the ſhip, and his clerk Athanaſius Κο̆νομος, and finding that the latter had been a ſchoolmaſter in the Morea, and had here learnt to ſpeak Engliſh fluently, I put the queſtion to them in the preſence of a very learned College friend, and at another time, to avoid any error, with

with the aid of a gentleman who is perfectly maſter of the Italian language. Both the Greeks repeatedly aſſured us that verſe as well as proſe was read by accent, and not by quantity; and exemplified it by reading ſeveral lines of Homer, with whoſe name they ſeemed perfectly well acquainted.

I SHALL give an inſtance or two of their mode of reading:

Βῆ δ' ἀκέων παρὰ θῖνα πολυφλοίσβοιο θαλάσσης,

Τὸν δ' ἀπαμειβόμενος προσέφη πόδας ὠκὺς Ἀχιλλεὺς,

Ἐς δ' ἐρέτας ἐπιτηδὲς ἀγείρομεν, ἐς δ' ἑκατόμβην

They made the ε in ακέων — προσέφη and ερέτας long.

But when they read

Κλῦθί μευ, Ἀργυρότοξ', ὃς Χρύσην ἀμφιβέβηκας,

They made the ſecond ſyllable of the firſt word Κλυθι ſhort, notwithſtanding the acute accent: on my aſking why, they deſired me to look back on the circumflex on the firſt ſyllable, and ſaid it thence neceſſarily followed, for it is impoſſible to pronounce the firſt ſyllable with the great length which the circumflex denotes, and not to ſhorten the ſecond. The teſtimony of the ſchoolmaſter might be vitiated, but what could be ſtronger than that of theſe ignorant mariners as to the vulgar common practice of modern Greece, and it is remarkable that this confirms the opinion of Biſhop Horſley, that the tones of words in connection are not always the ſame with the tones of ſolitary words, though in thoſe of more than one ſyllable the accentual marks do not change their poſition. I muſt here add that theſe men confirmed an obſervation of

our

our late revered and lamented Prefident, that we are much miftaken in our idea of the fuppofed lofty found of πολυφλοίσβοιο θαλασσης; that the Borderers on the coaft of the Archipelago take their ideas from the gentle laving of the fhore by a fummer wave, and not from the roaring of a winter ocean, and they accordingly pronounced it Polyphlifveo Thalaffes.

I OWN that the obfervations made by me on the pronunciation of thefe modern Greeks brought a perfectly new train of ideas into my mind. I propofe them, with humility, for the confideration of the learned, but they have made a ftrong impreffion upon me, and approached, when compared with other admitted facts, nearly to conviction. In fhort, I am ftrongly inclined to believe, that what the famous treatife fo often mentioned on the profodies of the Greek and Latin languages mentions as the peculiarity of the Englifh, that we always prolong the found of the fyllable on which the acute accent falls, is true, and has been true of every nation upon earth. We know it is true of the modern Italians—they read Latin in that refpect juft as we do, and fay, Arma virūmqūe cānŏ, and, In nōvă fert animus, as much as we. And when we find the modern Greeks following the fame practice, furely we have fome caufe to fuppofe that the ancients did the fame. In the Englifh language, indeed, quantity is not affected, becaufe accent and quantity always agree.* Bifhop Horfley endeavoured

* The great refemblance between the Perfian and Englifh languages, in many refpects, has been obferved by Sir W. Jones.—Here is another: I had the pleafure of hearing

endeavoured to prove that they did ſo in Greek, but this is on the bold ſuppoſition that the accent doth not fall where the mark is placed. The objection to this hypotheſis, which ſeems to have been admitted by all writers, and conſidered as deciſive by ſome as to proſe, by all as to verſe, is that ſuch a mode of pronunciation or reading muſt deſtroy metre, or *Rhuthmos.* From this poſition, however univerſal, or however it may have been taken for granted, I totally diſſent. That it will oppoſe the metre or quantity I readily agree, but that it will deſtroy the Rhythmos, by which, whatever learned deſcriptions there may have been of its meaning, I underſtand nothing more than the melody or ſmooth flowing of the verſes or their harmony if you pleaſe, if harmony be properly applied to ſucceſſive and not ſynchronal ſounds. On the contrary, nothing can be more diſagreeable or unmelodious than the reading verſe by quantity, or ſcanning of it, as it is vulgarly called. Let us try the line ſo often quoted—

Armă vĭrūmqŭe cănō, Trōjǣ qui prīmŭs ăb ōrīs,

inſtead of Ārmă vĭrūmqūe cānŏ, Trōjǣ qūi prīmŭs ăb ōrīs,

or, In nŏvă, &c.

No man ever defined Rhuthmos better than Plato, *ordinem quendam qui in motibus cernitur*; the motion or meaſure of the

hearing a native of Lucknow, but born of Perſian parents, who was lately in Dublin, Abu Talib Khan, read an ode of Hafiz; accent and quantity always went together: Bedéh Sakée meï Bakée, &c. &c.: with reſpect to the poſition of the accent, Sir W. Jones remarks, that the Perſians, like the French, uſually accent the laſt ſyllable of the word, and the *ſtrength* of accent which he has noted was remarkable in the gentleman I have mentioned, and almoſt amounted to recitative.

verſe may be exact, and yet the order, arrangement and diſpoſition of the letters and ſyllables, ſuch as to be grating and unmelodious to the ear. In like manner the feet of the verſe may be exact, but the ſtreſs laid upon particular ſyllables of it which follows the quantity may totally deſtroy the melody: in ſhort, the radical error ſeems to be the confuſion of quantity with melody, and the ſuppoſition that whatever is at war with quantity and metre muſt be at war with melody.* I ardently agree with the praiſes of the author of the Accentus Redivivi on the Scholiaſtes ad Hephæſtionem, that Rhythmus trahit tempora ut vult, & ſæpe breve tempus facit, ut ſit longum; on which the treatiſe de Rhythmo Græcorum obſerves, if this be true, plane actum eſt de metris. I admit it if they come in oppoſition to Rhythmos or melody. With reſpect to proſe I think this is acknowledged, why not with reſpect to verſe? That it is acknowledged with reſpect to proſe, Dacier and Pearce argue from the famous paſſage of Longinus, where he ſays, that the paſſage of Demoſthenes ſo famous for its pleaſing ſound, *τȣ̃το τὸ ψήφισμα*, conſiſts entirely of dactyl rhythms. *Ψήφισμα* then as pronounced by him was a dactyl, not a dactyl meaſure, but a dactyl rhythm, and it is remarkable

* I ſpeak with much heſitation, however, when I recollect, that a moſt revered and moſt beloved, and truly great man*, who honoured me with his friendſhip, and whoſe loſs the world deplores, was of a totally different opinion, and once repeated to me, to oppoſe mine, with much emphaſis, theſe lines of the third book of the Odyſſey:

Ἠέλιος δ' ἀνόρȣσε, λιπὼν περικαλλέα λίμνην,
Οὐρανὸν ἐς πολύχαλκον, ἵν' ἀθανάτοισι φαείνῃ,
Καὶ θνητοῖσι βροτοῖσιν ἐπὶ ζείδωρον ἄρȣραν.

* The late Primate.

markable that the modern Greeks pronounced it in the ſame way; how can it be otherwiſe if the acute accent be laid on the firſt ſyllable, ψήφισμα. There is a dactyl then in written metre, and a dactyl in pronunciation, and the ſame word ſhall when written, and when pronounced, be of different meaſure. Apply the ſame to verſe. Ψήφισμα is an Antibacchius for the purpoſe of the poet in meaſuring his verſe, but it doth not follow that he may not pronounce it as a dactyl. I dare to ſay if Longinus had been ſpeaking, not of the mode in which Demoſthenes and all Grecians pronounced the word, but of the *pes* of the word, he would not have ſaid it was a dactyl. The poet in conſtructing his verſe muſt take the ſyllables as he finds them, and has no power to alter beyond a very little poetic licenſe, for nude conſtruction doth not admit of emphaſis; but the ſpeaker, or the writer are not ſo confined, and it was probably to mark their variaens to the barbarous nations which overwhelmed Greece that accents were introduced, if they really were introduced at ſo late a period.

To illuſtrate what has been ſaid, let any man try how eaſy it is to make a verſe in perfect meaſure that ſhall be grating or unmuſical to the ear, and another without meaſure, agreeable and muſical. For inſtance, who can diſcover muſic in this line,

O Fortunati Mercatores, gravis annis,

or who would know it was poetry without being told ſo.

Colitur

Colitur Hybernia Divis virifque dilecta.

is a nonfenfe verfe which has juft occurred to my fancy, in quantity perfectly falfe, but in found, perhaps, not unmufical; and this is the reafon why the Englifh have wifely and properly chofen to read Latin verfe by accent and not by quantity, as I verily believe the old Romans did, becaufe they could not bear the found of the verfe when otherwife pronounced; would the profaic line before mentioned be improved by reading

O Fōr tūnā tī Mēr cātō rēs gravis annis, ?

The French, though they apply the word accent differently from other nations, may, in my fenfe of the word, illuftrate my meaning; the reafon why the heroic verfe of the French appears fo intolerable to us, is, that we attempt to read it by quantity; it then comes out exactly like our twelve fyllable verfe, ufually with us confined to ballads, and the famous verfe of Corneille

Rome, l'objet unique de mon raffentiment.

dances on the ear exactly like

Ye belles and ye flirts, and ye pert little things.

But whoever vifits the French theatre will perceive no fuch ridiculous faltation of meafure, but a folemn and ferious cadence governed by accent, adapted to the fubject and to the fcene, which almoft prevents the auditors from perceiving that it is verfe.

It will be here immediately faid, that I confound accent with emphafis: I do not; I include in the idea inflection of voice, but

in

in a ſecondary manner. No perſon can, in my humble opinion, lay a ſtreſs or emphaſis on any ſyllable without making it long, nor is it ever made long (I will not ſay it is abſolutely impoſſible, I ſpeak of the fact) without either elevating or depreſſing the voice. Let any man try to expreſs ſtrongly the negative, *I cannot*, he will ſpeak with an acute accent, elevate his voice, lay an emphaſis, and prolong the ſyllable. I remember a celebrated member of a houſe of parliament, not long ago, remarkable for his circumflex on this very word. Mr. Primatt highly commends an author on the accents, who ſays, no elevation of the voice can be made ſenſible in pronouncing, whatever may be done in ſinging,* without ſome ſtreſs or pauſe, which is always able to make a ſhort ſyllable long. I ſay, converſely, that no ſtreſs or pauſe is ever made without ſome elevation of the voice, either purely, i. e. in an acute tone, or mixed, that is, in an acute tone ending in a grave, and commonly called a circumflex.

It will be aſked then what is the uſe of metre or meaſure in verſe, if we are not to read by it; and here is the grand difficulty, and I own with candor I cannot anſwer it with perfect ſatisfaction to my own mind: to thoſe indeed who ſay we are to read by accent in proſe, it may be equally aſked what is the uſe

* The treatiſe on the proſodies argues, that in muſic length of ſound and acuteneſs of tone are not always united, and endeavours to confute Mr. Primatt, who attempts to account for this, without admitting that it can be ſo in ſpeaking.

ufe of long or fhort fyllables in profe, if we are not to attend to them when accent comes in the way: but to gentlemen on the other fide, I can only anfwer, that in the firft place accent doth not always interfere, and then quantity is our guide, and accent often accords with quantity. Secondly, metre determines the number of feet or meafures in each verfe, and thereby produces a general analogy and harmony through the whole, and it is to be obferved, that, as I apprehend, accent doth not change the number of feet, though it doth the nature or fpecies of them. Thus when we read

Arma virumque cǎno, Trōjæ qui primus ab oris,

we do not make more feet than when we fcan the line, nor employ more time than in pronouncing the next line in which the accent happens to accord with the quantity, viz. Italiam fato profugus, Lavinaque venit. Thirdly, The poet in meafuring his verfe certainly muft be confined to fome certain number and order of long and fhort fyllables, in order to produce a concordance through the whole, and even to regulate the pofition of accent, which though not fubdued by quantity will certainly have fome relation to it, euphoniæ gratia; but furely the length or fhortnefs of a fyllable cannot determine where emphafis fhall be placed—that muft depend on the meaning and the thought; and it would be moft abfurd for the poet to fay to the reader, you fhall not reft upon this emphatic and fignificative word becaufe its fyllables are fhort, and wherever there is a reft, there muft be length and intonation.

On

On the whole, then, I am inclined to conclude, not only that the ancient Greeks as well as the modern read both verſe and proſe by accent, which, indeed, the learned biſhop before alluded to always inſiſts, but alſo, which he denies, that they ſuffered the accents to control and alter the quantity; he does not indeed deny this, if the tones are given where the accentual marks are placed, but he denies that they were ſo given. Dacier, Pearce and Clarke admit that they read proſe by accent, not by quantity. The learned prelates contend that they could not have had a different mode of reading proſe and verſe. I accept both propoſitions, though without admitting their inferences,* and the combination of thoſe propoſitions proves my opinion, which however I do not advance dogmatically or decidedly, but with that feeling which I think becomes every member of this Academy, of wiſhing to advance uſeful or ornamental knowledge by free diſcuſſion and the ſuggeſtion of ſuch ideas as ſeem to him worthy at leaſt of the conſideration of the literary world. In the idea that accent muſt affect quantity I have numerous ſupporters as well as opponents. I only differ from the former in thinking that verſe muſt ſtill be read by accent. I ſhall not trouble the ſociety further but by the addition of a copy of a letter written by a Greek ſailor belonging to the ſhip I have mentioned to the agent ſent over here by the

 Turkiſh

* Of the former that verſe is not to be read by accent: of the latter, that though it is, its quantity is not thereby affected.

Turkiſh ambaſſador to watch the intereſt of the cargo, written in the preſent year, which the latter was ſo good as to give to me to ſhew the analogy between the modern and ancient language of Greece. It will be obſerved that this humble mariner uſes the accents with as much attention as any ſcholar.

This letter ſo much reſembles ancient Greek, that we might almoſt ſuppoſe it was ſo, and that the writer had at ſchool acquired this faculty; but Mr. Barthold, to whom it was addreſſed, who perpetually converſed with the ſailors in modern Greek, aſſured me that it was entirely modern, and that he could not have correſponded or converſed in ancient Greek. Mr. Barthold had reſided a long time in Conſtantinople and in the Morea, and was perfectly well acquainted with the language of the modern Greeks. I never ſaw any book in modern Greek, but I know the New Teſtament in that language was publiſhed at Oxford in the preſent century, at the time when ſome modern Greeks were brought there for education, who, however, by their exceſſive idleneſs, diſappointed expectation. But what ſuppoſition can be more ſtrange than that a parcel of Greek ſailors, or any one of them, ſhould chooſe to correſpond in ancient Greek. And I have the poſitive teſtimony of Barthold, that this letter is written in the common language of the country, and indeed he deſired me to obſerve the words introduced from the Italian, ſuch as *ton intereſſon*; and if he had written it from his education at ſchool, the terminations

nations and cafes would not be fo entirely foreign from the ancient. I cannot, therefore, doubt, efpecially when I compare it with the language I heard fpoken by all the crew, and when I mention that I faw the log-book of the fhip written in Greek which I could underftand, that this is a fpecimen of modern Greek: the dates and days of the month in the log-book differed from the ancient Greek in the fmalleft circumftance only, thus the 18th of January was Ιωνυαριυ ογδοεκατη, inftead of οκτο και δεκατη, I have another of thefe letters in my poffeffion much longer, with which I therefore have not troubled the Academy. I fhall conclude with obferving, that thefe modern Greeks always for accents ufed the word Οξεα, thereby confirming the opinion that there is properly no accent but the acute, the grave being the negative of accent; and we muft remember that the word προσωδιαι, in the ancient Greek language, is the term ufed for accents: which word, when tranflated into Latin, is accentus or ad cantus, implying elevation of voice, or a kind of fong, *fuperadded* or raifed on the common tone of the voice, and cannot apply to the *grave*, which is negation of any departure from the ufual level.

Tranflation

Tranſlation of the Greek Letter on the oppoſite Page: *

Cork, 1799, Auguſt 3d.

To the noble, rich merchant, Signior Barthold,
humbly, worſhipingly, and lovingly.

On the 17th of the laſt month I wrote to you a letter from Dingle, writing and exhorting you, that you would take care and better the intereſt of me deſtitute. That you might know how the other men grieved or held me, often ſignifying to me, where againſt me they ſpoke every day at their meſs, that they would not have me; and I again appeaſed them, calling and crying out, and to me they gave ear. I exhort you, if you love God, and for the ſake of your children, to write me a letter, as how you know of your generoſity, that I may have and know how I ſhall conduct myſelf, and that I may convey the men to London, or may carry them to Dublin, and beg that I may have an anſwer how I ſhall conduct myſelf, and I ſhall as you may direct.

Theſe, and I remain an outcaſt among the mountaineers,

Your ſervant,

CONSTANTINE ANDRIA.

* The oppoſite is a *Fac Simile* of the original.

Κόρφους — 1799 : απρίλ : 3 —

„ προς τον ευγενή άρχον εκλαμπρότατε, κύρ [illegible] δημιουρ, [illegible], Καί αποκρισις —

„ „ „ είς τας : 17 — [illegible] [illegible] ένα γράμμα από [illegible], γεγραμμένος, και περιέχοντας, να [illegible] και έμειναν, [illegible], το [illegible] [illegible] [illegible], πολλά, τα [illegible] [illegible] [illegible], με έχουν, πολλά [illegible], ώστε όπου, με [illegible] κάθε ημέραν, [illegible] είς την [illegible] [illegible], [illegible] με [illegible] και εγώ [illegible] [illegible] [illegible], και [illegible], και με έχουν [illegible] [illegible] [illegible], αν [illegible] τον θεόν, και να [illegible] να μη γράψης έναν γράμμαν, ως κάθως, [illegible] [illegible], τόσοι έχω να [illegible], ή να [illegible] [illegible] με τας [illegible] είς την [illegible], ή να έρθω, αυτό είς το [illegible] Και [illegible] να έχω [illegible] [illegible] να [illegible] και εγώ, ως κάθως με [illegible] — —

Ταύτα Κύρ μένω, [illegible] είς τας [illegible] —

Δηλονας, [illegible]

ανδρέα [illegible], [illegible]

αυτήν την χάριν [illegible], όπου να μη γράψης, ή να [illegible] [illegible], ή να με [illegible], και δεν [illegible] να υποφέρω, τα [illegible], όπου [illegible] κάθε ημέραν —