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Page 7. *line* 15. *for* $\dot{\varphi} \times FG$ *read* $\dot{\varphi} \times FH$
ib. *ib. for* $\dot{z}$ *read* z
16. 23. *for* § 4. *read* § 14
ib. *ib. for* $a \cos \varphi^2$ *read* $a^2 \cos \varphi$
205. 8. *for* $\sin(n-\nu)=$, &c. *read* $n-\nu=$, &c.
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Page 6. *line* 4. *for* $\tan \frac{O-o}{2}$ *read* $\tan \frac{M}{2}$. Alſo *ibid. line* 8. *for* 1 *read* R.

ADVERTISEMENT.

THE ROYAL SOCIETY OF EDINBURGH having refolved on an annual publication of their Tranfactions, it has become neceffary to make fome changes in the arrangement of the volumes. The Papers, whether Phyfical or Literary, are now introduced promifcuoufly, without being diftinguifhed, as before, by thofe feparate titles ; and the divifion into PART I, PART II, &c. is to be underftood as referring only to the order of time, according to which the different collection of Papers, that are to enter into the fame volume, fhall happen to be publifhed. At the end of the laft Part of each volume will be given the Hiftory of the Society, including, as ufual, the Biographical Accounts, the Lift of Members, and fuch details concerning the proceedings of the Society as it may be thought proper to lay before the Public.

EDINBURGH,
Aug. 1. 1799.

⁂ The General Title will be given along with the laft Part of the Volume.

I. INVESTIGATION *of certain* THEOREMS *relating to the* FIGURE *of the* EARTH. *By JOHN PLAYFAIR*, F. R. S. EDIN. *and Professor of Mathematics in the University of Edinburgh.*

[*Read Feb.* 5. 1798.]

1. THE obſervations which have been made to determine the magnitude and figure of the earth, have not hitherto led to reſults completely ſatisfactory. They have indeed demonſtrated the compreſſion or oblateneſs of the terreſtrial ſpheroid, but they have left an uncertainty as to the quantity of that compreſſion, extending from about the one hundred and ſeventieth, to the three hundred and thirtieth part of the radius of the equator. Between theſe two quantities, the former of which is nearly double of the latter, moſt of the reſults are placed, but in ſuch a manner that thoſe beſt entitled to credit are much nearer to the leaſt extreme than to the greateſt. Sir ISAAC NEWTON, as is well known, ſuppoſing the earth to be of uniform denſity, aſſigned for the compreſſion at the poles $\frac{1}{230}$, near-

ly a mean between the two limits juſt mentioned; and it is probable, that, if the compreſſion is leſs than this, it is owing to the increaſe of the denſity toward the centre. Boscovich, taking a mean from all the meaſures of degrees, ſo as to make the poſitive and negative errors equal, found the difference of the axes of the meridian $= \frac{1}{248}$. By comparing the degrees meaſured by Father Leisganic in Germany, with eight others that have been meaſured in different latitudes, La Lande finds $\frac{1}{311}$, and, ſuppreſſing the degree in Lapland, which appears to err in exceſs, $\frac{1}{331}$ for the compreſſion. La Place makes it $\frac{1}{321}$; Sejour $\frac{1}{307}$, and, laſtly, Carouge and La Lande $\frac{1}{300}$.

These reſults, which reduce the excentricity of the meridians ſo much lower than was once ſuppoſed, agree well with the obſervations of the length of the pendulum made in different latitudes. Were the earth a homogeneous body, Sir Isaac Newton demonſtrated, that the diminution of gravity under the equator would be $= \frac{1}{230}$, expreſſed by the ſame fraction with the compreſſion at the poles. M. Clairault made afterwards a very important addition to this theorem: for he ſhewed, that, if the earth be not homogeneous, but have a denſity that varies with any function of the diſtance from the centre, the two fractions, expreſſing the compreſſion at the poles, and the diminution of gravity at the equator, when added together, muſt be of the ſame amount as in the homogeneous ſpheroid, that is, muſt be $= \frac{2}{230}$ or $\frac{1}{115}$. Now, the ſecond pendulum is concluded, from the beſt and moſt recent obſervations, to be longer at the pole than at the equator by $\frac{1}{185}$, and this, taken from $\frac{1}{115}$, leaves $\frac{1}{304}$ for the compreſſion at the poles.

2. But

2. But though $\frac{1}{300}$, or ſome fraction not very different from it, ſhould be admitted as the moſt probable value of the compreſſion, or ellipticity, as it is called, of the terreſtrial ſpheroid, it ſtill remains to be explained, why all the obſervations, conſidering the care with which they have been made, do not agree more nearly with this concluſion. Among the cauſes that may be aſſigned for this inconſiſtency, though unavoidable miſtakes, and the imperfection of inſtruments, muſt come in for a part, there can be little doubt that local irregularities in the direction of gravity have had the greateſt ſhare in producing it. Of theſe irregularities, that which ariſes from the attraction of mountains has had its exiſtence proved, and its quantity, in one caſe, aſcertained, by the very accurate obſervations of the preſent Aſtronomer-Royal at Schehallien in Perthſhire. We may trace the operation of this cauſe in many of the degrees that have been actually meaſured. Thus, in the degree at Turin, when divided into two parts, and each eſtimated ſeparately, that which was to the north of the city, and pointed toward Monte Roſa, the ſecond of the Alps in elevation, and the firſt perhaps in magnitude, was found greater in proportion than that toward the ſouth, the plummet having been attracted by the mountain above mentioned, and the zenith made of conſequence to recede toward the ſouth. There are no doubt ſituations in which the meaſurement of a ſmall arch might, from a ſimilar cauſe, give the radius of curvature of the meridian infinite, or even negative.

But there is another kind of local irregularity in the direction of gravity, that may alſo have had a great effect in diſturbing the accuracy of the meaſurement of degrees. The irregularity I mean is one ariſing from the unequal denſity of the materials under and not far from the ſurface of the earth; and this

this caufe of error is formidable, not only becaufe it may go to a great extent, but becaufe there is not any vifible mark by which its exiftence can always be diftinguifhed. The difference between the primary and fecondary ftrata is probably one of the chief circumftances on which this inequality depends. The primary ftrata, efpecially if we include among them the granite, may often have three times the fpecific gravity of water, whereas the fecondary, fuch as the marly and argillaceous, frequently have not more than twice the fpecific gravity of that fluid. Suppofe, then, that a degree is meafured in a country where the ftrata are all fecondary, and happens to terminate near the junction of thefe with the primitive or denfer ftrata, the line of which junction we fhall alfo fuppofe to lie nearly eaft and weft; the fuperior attraction of the denfer ftrata muft draw the plummet toward them, and make the zenith retire in the oppofite direction; thus diminifhing the amplitude of the celeftial arch, and increafing, of confequence, the geodetical meafure affigned to a degree. From fuppofitions, no way improbable, concerning the denfity and extent of fuch maffes of ftrata, I have found, that the errors, thus produced, may eafily amount to ten or twelve feconds.

3. WHILE we continue to draw our conclufions, about the figure of the earth, from the meafurement of fingle degrees, there appears to be no way of avoiding, or even of diminifhing, the effects of thefe errors. But if the arches meafured are large, and confift each of feveral degrees, though there fhould be the fame error in determining their celeftial amplitudes, the effect of that error, with refpect to the magnitude and figure of the earth, will become inconfiderable, being fpread out over a greater interval; and it is, therefore, by the comparifon of two fuch arches that the moft accurate refult is likely to be obtained. But, in purfuing this method, fince the arches meafured cannot

be

be treated as ſmall quantities, or mere fluxions of the earth's circumference, the calculation muſt be made by rules quite different from thoſe that have been hitherto employed. Theſe new rules are deduced from the following analyſis.

4. Let the ellipſis ADBE (fig. 1. Pl. I.) repreſent a meridian paſſing through the poles D and E, and cutting the equator in A and B. Let C be the centre of the earth, AC, the radius of the equator, $= a$, and DC, half the polar axis, $= b$. Let FG be any very ſmall arch of the meridian, having its centre of curvature in H; join HF, HG cutting AC in K and L. Let φ be the meaſure of the latitude of F, or the meaſure of the angle AKF, expreſſed, not in degrees and minutes, but in decimals of the radius 1; then the exceſs of the angle ALG above AKF, that is, the angle LHK or GHF will be $= \dot{\varphi}$, and therefore FG $= \dot{\varphi} \times$ FG. Alſo, if the elliptic arch AF $= \dot{z}$, FG $= \dot{z} = \dot{\varphi} \times$ FH.

But FH, or the radius of curvature at F, is $=$

$$\frac{a^2 b^2}{(a^2 - a^2 \operatorname{ſin}^2\varphi + b^2 \operatorname{ſin}^2\varphi)^{\frac{3}{2}}} = a^2 b^2 (a^2 - a^2 \operatorname{ſin}^2\varphi + b^2 \operatorname{ſin}^2\varphi)^{-\frac{3}{2}},$$ as is demonſtrated in the conic ſections. Therefore, if c be the compreſſion at the poles, or the exceſs of a above b, $b^2 = a^2 - 2ac + c^2$, or becauſe c is ſmall in compariſon of a, if we reject its powers higher than the firſt, $b^2 = a^2 - 2ac$, and FH $= a^3 (a - 2c)(a^2 - a^2 \operatorname{ſin}^2\varphi + a^2 \operatorname{ſin}^2\varphi - 2ac \operatorname{ſin}^2\varphi)^{-\frac{3}{2}} = a^3 (a - 2c)(a^2 - 2ac \operatorname{ſin}^2\varphi)^{-\frac{3}{2}}$.

But $(a^2 - 2ac \operatorname{ſin}^2\varphi)^{-\frac{3}{2}} = a^{-3}(1 - \frac{2c}{a} \operatorname{ſin}^2\varphi)^{-\frac{3}{2}} = a^{-3}(1 + \frac{3c}{a} \operatorname{ſin}^2\varphi)$ nearly, rejecting, as before, the terms that involve c^2, &c. Hence FH $= (a - 2c)(1 + \frac{3c}{a} \operatorname{ſin}^2\varphi) = a - 2c + 3c \operatorname{ſin}^2\varphi$.

Now,

Now $\dot{z} = \dot{\varphi} \times$ FH, therefore $\dot{z} = \dot{\varphi}\,(a - 2c + 3c \sin^2\varphi) = (a - 2c)\,\dot{\varphi} + 3c\,\dot{\varphi} \sin^2\dot{\varphi}$. But $\sin^2\varphi = \frac{1 - \cos 2\varphi}{2}$, therefore $\dot{z} = (a - 2c)\dot{\varphi} + \frac{3}{2}c\dot{\varphi} - \frac{3c\dot{\varphi}}{2}\cos 2\varphi$, and taking the fluent $z = \left(a - \frac{c}{2}\right)\varphi - \frac{3c}{4}\sin 2\varphi$. To this value of z no conſtant quantity is to be added, becauſe it vaniſhes when $z = o$.

THEREFORE an arch of the meridian, extending from the equator to any latitude φ, is $= a\varphi - \frac{c}{2}\left(\varphi + \frac{3}{2}\sin 2\varphi.\right)$.

5. THIS theorem is alſo eaſily applied to meaſure an arch of the meridian, intercepted between any two parallels of the equator.

THUS, if MN be any arch of the meridian, φ' the latitude of M, one of its extremities, and φ'' that of N, its other extremity, we have $\text{AM} = a\varphi' - \frac{c}{2}\left(\varphi' + \frac{3}{2}\sin 2\varphi'\right)$, and

$$\text{AN} = a\varphi'' - \frac{c}{2}\left(\varphi'' + \frac{3}{2}\sin 2\varphi''\right).\ \text{Therefore the arch}$$

$$\text{MN} = a\,(\varphi'' - \varphi') - \frac{c}{2}\left((\varphi'' - \varphi') + \frac{3}{2}\sin 2\varphi'' - \frac{3}{2}\sin 2\varphi'\right).$$

6. IF, therefore, MN be an arch of ſeveral degrees of the meridian, the length of which is known by actual meaſurement, and alſo the latitude of its two extremities M and N, this laſt formula gives us an equation, in which a and c are the only unknown quantities. In the ſame manner, by the meaſurement of another arch of the meridian, an equation will be found, in which a and c are likewiſe the only unknown quantities. By a compariſon, therefore, of theſe two equations, the values of a and c, that is of the radius of the equator, and its exceſs above half the polar axis, may be determined.

THUS, if l be the length of an arch meaſured, m the co-efficient of a, and n of c, computed by the laſt formula; and if l' be be the length of any other arch, m' the coefficient of a, and n' of c, computed in the ſame manner, we have $ma - nc = l$,

and $m'a - n'c = l'$.

Whence $a = \frac{n'l - nl'}{mn' - m'n}$; $c = \frac{m'l - ml'}{mn' - m'n}$ and $\frac{c}{a} = \frac{m'l - ml'}{n'l - nl'}$. It may be uſeful, in the numerical calculation, to obſerve alſo that $c = \frac{ma - l}{n}$.

7. The arch of the meridian, which was meaſured in Peru, compared with that meaſured in France, will afford an example of the application of theſe formulas.

The amplitude of the arch meaſured in Peru was 3°. 7′. 1″, and its length 176940 toiſes. To reduce this to the level of the ſea, above which it was elevated 1226 toiſes, 66 toiſes muſt be ſubtracted, and again 12 toiſes added to adapt it to the mean temperature of the atmoſphere. Thus corrected it is 176886 toiſes. The arch meaſured begun 36″ north of the equator, and extended to the parallel of 3°. 6′. 25″ ſouth; we ſhall ſuppoſe it to have begun under the equator, and to have extended to the parallel of 3°. 7′. 1″, a ſuppoſition which can produce no ſenſible error, and will ſomewhat ſimplify the calculation. Thus φ, in the preceding formula, is an arch of 3°. 7′. 1″ expreſſed in decimals of the radius 1, and ſo we have $m = .0544009$, $n = .1086408$, and $l = 176886$.

Again, the amplitude of the whole arch meaſured in France from Dunkirk to Perpignan is 8°. 20′. 2″½, and its length 475496 toiſes. The northern extremity of this arch is in latitude 51°. 2′. 1″, and the ſouthern in 42°. 41′. 58″½. Hence $\varphi'' = .8907045$, and $\varphi' = .7452459$, and therefore $m' = .1454586$, $n' = .0585735$, $l = 475496$.

Therefore, $a = \frac{n'l - nl'}{mn' - m'n} = 3273325$ toiſes;

$c = \frac{m'l - ml'}{mn' - m'n} = 10917$ toiſes,

and $\frac{c}{a} = \frac{1}{300}$ nearly.

 Wherefore

Wherefore alſo the longer axis of the meridian is to its conjugate, or a is to b as 300 to 299.

This proportion agrees well with that which was already pointed out as the moſt probable reſult, from the compariſon of ſingle degrees, and from obſervations of the pendulum. As theſe concluſions are obtained by different methods, they tend greatly to confirm one another.

8. From this too it ſeems highly probable, that the uncertainty which yet remains with reſpect to the true figure of the earth will be entirely removed by the meaſurement of ſome other conſiderable arches of the meridian. Such an arch will be furniſhed by the ſurvey of Great Britain begun by General Roy, and ſtill continued in a ſtyle of accuracy ſo much ſuperior to any other ſyſtem of geometrical operations that has ever yet been executed. In drawing the concluſions from obſervations made with ſuch exactneſs, it may be neceſſary to employ a more accurate approximation than has been done in the preceding *formulæ*, by retaining the ſecond power of c. The equations to be reſolved will thus become of the ſecond order, but as the unknown quantities can be nearly found by the ſolution of a ſimple equation, the farther approximation to their true values will be accompanied with no difficulty.

9. Concerning this farther approximation it may be uſeful however to remark, that if c^2 be retained, its coefficient in the formula of § 4. will be $\frac{1}{16a}\left(\varphi + \frac{15}{4}\text{ ſin } 4\varphi\right)$;

and therefore in the formula of § 5. it will be

$$\frac{1}{16a}\left(\varphi'' - \varphi' + \frac{15}{4}(\text{ſin } 4\varphi'' - \text{ſin } 4\varphi')\right).$$

If then the quantity

$$\frac{1}{16a}\left(\varphi'' - \varphi' + \frac{15}{4}(\text{ſin } 4\varphi'' - \text{ſin } 4\varphi')\right),$$

computed for any arch of the meridian, be put $= g$, and the ſame

ſame, computed for any other arch, be $= g'$, the equations of § 6. will become,

$$ma - nc + \frac{gc^2}{a} = l, \text{ and}$$

$$m'a - n'c + \frac{g'c^2}{a} = l'.$$

10. HERE if we put d for the value of a, as given by the formula $\frac{n'l - nl'}{mn' - m'n}$; and b for the value of c, as given by the formula $\frac{m'l - ml'}{mn' - m'n}$, alſo v for the correction to be made on d, and u for the correction to be made on b, ſo that $a = d + v$, and $c = b + u$, by ſubſtituting theſe values of a and c in the two laſt equations we have $mv - nu + \frac{g(b+u)^2}{d+v} = 0$, and

$$m'v - n'u + \frac{g'(b+u)^2}{d+v} = 0.$$

Hence, rejecting all the terms that involve v^2, u^2, or uv, we have $dmv - dnu + gb^2 + 2gbv = 0$,
and $dm'v - dn'u + g'b^2 + 2g'bv = 0$.
Therefore, $v = \frac{(ng' - n'g)\,b^2}{(n'm - nm')\,d + (gn' - g'n)\,2b}$, alſo

$$u = \frac{g'b^2\,(dm + 2gb) - gb^2\,(dm' + 2g'b)}{dn'\,(dm + 2gb) - dn\,(dm' + 2g'b)}.$$

And, again, by rejecting thoſe terms that are ſmall in compariſon of the reſt, $v = \frac{b^2\,(ng' - n'g)}{d\,(n'm - nm')}$, and

$$u = \frac{b^2\,(g'm - gm')}{d\,(n'm - nm')}.$$

THUS v and u are found, and of conſequence $d + v$ and $b + u$, that is a and c, without neglecting any terms that are not of an order leſs than $\frac{c^2}{a}$; and when it is conſidered that $\frac{c^2}{a}$ is leſs than $\frac{1}{22500}$, it will readily be allowed that it is quite unneceſſary to carry the approximation farther.

 11. THE

11. The ſame thing that renders the compariſon of large arches of the meridian uſeful for leſſening the effect of errors ariſing from irregularities in the direction of gravity, makes it ſerve to diminiſh the effect of all the errors of the aſtronomical obſervations at the extremities of the arches, from whatever cauſe they ariſe. They are all diffuſed over a greater interval, and have an effect proportionally leſs in diminiſhing the accuracy of the laſt concluſion.

12. The meaſurement therefore of large arches of the meridian, eſpecially if performed in diſtant countries, is likely to furniſh the beſt *data* for aſcertaining the true figure of the earth; and on this account extenſive and accurate ſurveys, ſuch as that above mentioned, are no leſs intereſting to ſcience, in general, than conducive to national utility. The ſurvey of this Iſland, when completed, will furniſh an arch of the meridian, beginning at the ſame parallel where that meaſured in France terminates, and nearly of the ſame extent, ſo that the length of an arch of more than 16°, or almoſt a twentieth of the earth's circumference, will become known. The different portions of this arch compared with one another, or with the arch meaſured in Peru, will afford a variety of *data* for determining the true figure of the earth.

But ſurveys of the kind now referred to, afford likewiſe other materials from which the ſolution of this great geographical problem may be deduced. Theſe are chiefly of two ſorts, viz. the magnitude of arches, either of the curves perpendicular to the meridian, or of the circles parallel to the equator. Examples of the firſt of theſe have been given by General Roy and Mr Dalby; the obſervations which follow are directed toward both.

13. With reſpect to the meaſurement of arches perpendicular to the meridian, it may be obſerved, that the directions of gravity

gravity at different points of ſuch arches do not interſect one another at all, unleſs the diſtances of thoſe points from the ſaid meridian be very ſmall. On this account the meaſurement of a large arch perpendicular to the meridian would involve in it conſiderable difficulty; to avoid which it is neceſſary that the arch meaſured be but ſmall, or one that does not greatly exceed a ſingle degree. Such meaſurements are of courſe obnoxious to all the errors that ariſe from the deflection of the plumb-line, and cannot therefore furniſh *data* for determining the figure of the earth, equally valuable with thoſe which may be derived from large arches of the meridian. The method of determining the figure of the earth, from degrees of the perpendicular to the meridian, is not however without its advantages, and in certain circumſtances is preferable to any other that proceeds by the meaſurement of arches equally ſmall. This method is twofold; as a degree of the meridian may be compared with a degree of the perpendicular to it in the ſame latitude; or two degrees perpendicular to the meridian, in different latitudes, may be compared with one another. The advantages peculiar to each will appear from the following inveſtigation.

14. Let it be required to find the axes of an elliptic ſpheroid, from comparing a degree of the meridian in any latitude with a degree of the curve perpendicular to the meridian in the ſame latitude.

Let the ellipſis ADBE (fig. 1. Pl. I.) repreſent a meridian, of which a degree is meaſured at F. Let the perpendicular to the meridian in F meet the leſs axis DE in R. Then R will be the centre of curvature of the circle cutting the meridian at right angles in F; for at any point in that circle indefinitely near to F, the direction of the plumb-line, or of gravity, as it always paſſes through the axis DE, will cut DE in R; it will therefore

alſo

alſo interſect FR in R, ſo that R is the centre, and RF the radius, of curvature of the perpendicular to the meridian. Let H be the centre of curvature of the meridian itſelf at F: draw FO perpendicular to DE, and let the latitude of F, or the angle OFR $= \varphi$. Alſo let AC $= a$, CD $= b$, and $a - b = c$, as before.

THEN from the nature of the ellipſis, FO $= \frac{a^2 \operatorname{cof} \varphi}{\sqrt{a^2 \operatorname{cof}^2 \varphi + b^2 \operatorname{fin} \varphi^2}}$, and becauſe fin FRO : 1 : : FO : FR, that is,

cof φ : 1 : : FO : FR, FR $= \frac{a^2}{\sqrt{a^2 \operatorname{cof}^2 \varphi + b^2 \operatorname{fin} \varphi^2}}$; and this, therefore, is the radius of curvature of the ſection of the ſpheroid perpendicular to the meridian at F. But the radius of curvature of the meridian at F, that is FH $= \frac{a^2 b^2}{\sqrt{a^2 \operatorname{cof}^2 \varphi + b^2 \operatorname{fin}^2 \varphi}}$,

therefore FR : FH : $\frac{a^2}{(a^2 \operatorname{cof} \varphi^2 + b^2 \operatorname{fin} \varphi^2)^{\frac{1}{2}}}$: $\frac{a^2 b^2}{(a^2 \operatorname{cof} \varphi^2 + b^2 \operatorname{fin} \varphi^2)^{\frac{1}{2}}}$, and

dividing both by $\frac{a^2}{(a^2 \operatorname{cof} \varphi^2 + b^2 \operatorname{fin} \varphi^2)^{\frac{1}{2}}}$, we have

FR : FH : : $a^2 \operatorname{cof} \varphi^2 + b^2 \operatorname{fin} \varphi^2 : b^2$.

15. IF then D be the length of a degree of the meridian at F, and D′ the length of a degree of the circle at right angles to it, D′ : D : : $a^2 \operatorname{cof} \varphi^2 + b^2 \operatorname{fin} \varphi^2 : b^2$, and $\frac{D'}{D} = \frac{a^2 \operatorname{cof} \varphi^2 + b^2 \operatorname{fin} \varphi^2}{b^2}$

$= \frac{a^2}{b^2} \operatorname{cof} \varphi^2 + \operatorname{fin}^2 \varphi$. Hence $\frac{D'}{D} - \operatorname{fin} \varphi^2 = \frac{a^2}{b^2} \operatorname{cof} \varphi^2$ and $\frac{a}{b} =$

$\frac{\sqrt{\frac{D'}{D} - \operatorname{fin} \varphi^2}}{\operatorname{cof} \varphi}$.

THIS laſt formula, therefore, gives the ratio of a to b when D, D′ and φ are known.

16. To find a and b themſelves, if $m = 57.2957$, &c. or the number of degrees in the radius, ſo that mD′ $=$ FR

$=$

$= \dfrac{a^2}{(a^2 \operatorname{cof} \varphi^2 + b^2 \operatorname{fin} \varphi^2)^{\frac{1}{2}}}$, and fince it has been already fhewn that $\dfrac{a^2}{b^2} = \dfrac{\frac{D'}{D} - \operatorname{fin} \varphi^2}{\operatorname{cof} \varphi^2}$, or $b^2 = \dfrac{a^2 \operatorname{cof} \varphi^2}{\frac{D'}{D} - \operatorname{fin} \varphi^2}$, therefore $mD' =$

$$\frac{a^2}{\left(a^2 \operatorname{cof} \varphi^2 + \dfrac{a^2 \operatorname{cof} \varphi^2}{\frac{D'}{D} - \operatorname{fin} \varphi^2} \times \operatorname{fin} \varphi^2\right)^{\frac{1}{2}}} = \frac{a}{\operatorname{cof} \varphi \left(1 + \dfrac{\operatorname{fin} \varphi^2}{\frac{D'}{D} - \operatorname{fin} \varphi^2}\right)^{\frac{1}{2}}} \text{ and } a =$$

$$mD' \operatorname{cof} \varphi \sqrt{1 + \frac{\operatorname{fin} \varphi^2}{\frac{D'}{D} - \operatorname{fin} \varphi^2}}.$$

Now, $1 + \dfrac{\operatorname{fin} \varphi^2}{\frac{D'}{D} - \operatorname{fin} \varphi^2} = \dfrac{\frac{D'}{D}}{\frac{D'}{D} - \operatorname{fin} \varphi^2} = \dfrac{1}{1 - \frac{D}{D'} \operatorname{fin} \varphi^2}$, therefore

$$a = \frac{mD' \operatorname{cof} \varphi}{\sqrt{1 - \frac{D}{D'} \operatorname{fin} \varphi^2}}.$$

17. This value of a is very convenient for logarithmical calculation; for if $\operatorname{fin} \varphi \sqrt{\frac{D}{D'}}$ be computed, it will always be lefs than 1, becaufe D' is greater than D, and therefore may be taken for the fine of an arch ψ, of which arch $\sqrt{1 - \frac{D}{D'} \operatorname{fin} \varphi^2}$ will of courfe be the cofine, fo that $a = \dfrac{mD' \operatorname{cof} \varphi}{\operatorname{cof} \psi}$.

The fame method may be ufed for finding $\frac{a}{b}$ from the formula in § 15.

In the fame manner that a has been found, we will obtain

$$b = \frac{mD' \operatorname{cof} \varphi^2}{\left(1 - \frac{D}{D'} \operatorname{fin} \varphi^2\right) \sqrt{\frac{D'}{D}}}.$$

If we examine thefe formulas in the extreme cafes, viz. when $\varphi = 90°$, and when $\varphi = 0$, we fhall have in the former cafe $a = \frac{0}{0}$, becaufe $\operatorname{cof} \varphi = 0$, and alfo $D' = D$, fo that

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$1 - \frac{D}{D'} \sin^2\varphi = 0$. Here therefore a is indefinite, and may be of any magnitude whatever; and it is evident that this is the refult which the formula ought to give: becaufe at the pole, or when $\varphi = 90°$, the perpendicular to the meridian is itfelf a meridian, and therefore the meafurement of the two degrees, D and D′, is but the fame with the meafurement of one degree.

When $\varphi = 0$, that is at the equator, the circle perpendicular to the meridian is the equator itfelf, and we have then $a = mD'$, a being determined in this cafe by the degree of the equator alone. Here alfo we have $\frac{a}{b} = \sqrt{\frac{D'}{D}}$, which is known to be true.

18. The preceding formulas may be rendered more fimple, if we aim only at an approximation, which indeed is all that is neceffary in this inquiry. Since c denotes the compreffion, or fince $a - c = b$, and therefore $a^2 - 2ac = b^2$ nearly, confequently the radius of curvature of the meridian at F, that is

$$mD = \frac{a^2(a^2 - 2ac)}{(a^2 - 2ac\sin\varphi^2)^{\frac{3}{2}}} = \frac{a^3(a - 2c)}{a^3\left(1 - \frac{2c}{a}\sin\varphi^2\right)^{\frac{3}{2}}} =$$

$(a - 2c)\left(1 - \frac{3c}{a}\sin\varphi^2\right)$, or $mD = a - 2c + 3c\sin\varphi^2$. In the fame manner $mD' = a + c\sin\varphi^2$. From thefe equations we obtain, rejecting always the higher powers of c,

$$c = \frac{m(D' - D)}{2\cos\varphi^2},\quad a = mD' - \frac{m(D' - D)\sin\varphi^2}{2\cos\varphi^2};\ \text{and}\ \frac{c}{a} = \frac{D' - D}{2D'\cos\varphi^2}.$$

These formulas may be transformed into others a little more convenient for computation, by putting $\sec\varphi^2$ inftead of $\frac{1}{\cos\varphi^2}$, and $\tan\varphi^2$ inftead of $\frac{\sin\varphi^2}{\cos\varphi^2}$; we have then,

$$c = \frac{m}{2}(D' - D)\sec\varphi^2,$$

$$a = mD' - \frac{m}{2}(D' - D)\tan\varphi^2,\ \text{and}$$

$$\frac{c}{a} = \frac{(D' - D)}{2D'}\sec\varphi^2.$$

19. We

19. We may apply theſe formulas to the computation of $\frac{c}{a}$, &c. from the degrees of the meridian and perpendicular, meaſured in the ſouth of England. We find, in one example, (*Phil. Tranſ.* 1795, p. 537.), that D = 60851 fathoms, D′ = 61182, the latitude, or φ being = 50°. 41′. From this $\frac{c}{a} = \frac{D'-D}{2D' \text{coſ}^2\varphi} = \frac{331}{2 \times 61182 \times (\text{coſ } 50°. 41')^2} = \frac{1}{148.4}$, which is nearly the ſame reſult with that deduced in the paſſage juſt referred to. Indeed the ſolution of this problem, contained in the *Trigonometrical Survey*, is quite unexceptionable; and the theorems here offered are not given as containing a more accurate ſolution, but one that is in ſome reſpects more ſimple.

The above compreſſion, if the remarks already made be well founded, is much too great, being more than double of what was obtained from comparing the whole arch of the meridian meaſured in France with the whole of that meaſured in Peru. At the ſame time it is right to obſerve, that all the other compariſons of the degrees of the meridian, with thoſe of the curve perpendicular to it, made from the obſervations in the ſouth of England, agree nearly in giving the ſame oblateneſs to the terreſtrial ſpheroid. For this circumſtance, it is certainly not eaſy to account; the unparalleled accuracy with which the whole of the meaſurement has been conducted, makes it in the higheſt degree improbable that it ariſes from any error; and even if errors were to be admitted, it is not likely that they ſhould all fall on the ſame ſide. The authors of the *Trigonometrical Survey* ſeem willing, therefore, to give up the elliptic figure of the earth, (*Ibid.* p. 527.); but before we abandon that very natural and ſimple hypotheſis, it may perhaps be worth while to attend to the following conſiderations.

20. In the part of England, where the meaſures we are now treating of have been taken, the ſtrata are of chalk, and though of great extent, are bordered, on all the ſides that we have acceſs to examine by ſtrata much denſer and more compact. Toward the weſt the chalk is ſucceeded by limeſtone, and that limeſtone by the primitive ſchiſtus and granite of the weſt of Devonſhire and of Cornwall. On the eaſt we may ſuppoſe that ſomething of the ſame kind takes place, though the ſea prevents us from obſerving it, as the chalky and argillaceous beds extend in this direction to the coaſt, and probably to ſome diſtance beyond it. Now the meridian of Greenwich may be conſidered as dividing the tract of country, occupied by theſe lighter ſtrata, into two parts, in ſuch a manner, that the plummet being carried to a diſtance from it, either eaſt or weſt, approaches to the denſer ſtrata, and is of courſe attracted by them, ſo that the zenith is forced back, as it were, to the meridian of Greenwich, and does not recede from it, in the heavens, at ſo great a rate as the plummet itſelf does, on the earth. Hence the longitudes from this meridian, eſtimated by the arches in the heavens, intercepted between the zenith and the ſaid meridian, will appear leſs than they ought to do; and too much ſpace on the ſurface of the earth will of conſequence be aſſigned as the meaſure of a degree. In this way D' is made too great; and we may ſuppoſe the circumſtances ſuch that D, on going north or ſouth, is not enlarged in the ſame proportion; hence $\frac{D'-D}{D'}$ will be augmented, and of courſe $\frac{c}{a}$ will be repreſented as too great. This explanation may perhaps appear very hypothetical, and it is certainly propoſed merely as a hypotheſis. It is a hypotheſis, too, that lays claim only to a temporary indulgence, as it is propoſed at the very moment when it may be brought to the trial, and when, by a further continuation of the ſurvey toward the north,

north, it will probably be determined how far the diſtribution of the ſtrata of this country affects the direction of gravity. It will indeed be curious to remark what irregularities take place on advancing into the denſer ſtrata of the north. The limeſtone and ſandſtone ſtrata of the middle part of the Iſland will ſucceed to the chalk of the ſouth, the primitive and denſer ſtrata ſtill occupying the weſt, at leaſt at intervals, as in Wales, Cumberland, and Galloway. Further to the north, that is, beyond the Tay, the ſtrata become entirely primitive, moſt of them of the denſeſt kind, and in the interior of the Iſland, with a very few exceptions, continue the ſame to its moſt northern extremity. In the ſurvey of Britain, therefore, ſeveral ſituations muſt occur where the plummet, paſſing from lighter to denſer ſtrata, ought to give indications of ſome irregularities in the direction of the gravitating force. It will be ſeen hereafter how far theſe conjectures are verified by experience.

21. A REMARK, that is in no danger of being reckoned hypothetical, is, that the concluſion derived from the compariſon of degrees of the meridian, with degrees of the circle perpendicular to it, becomes of neceſſity more liable to error as we advance into higher latitudes. The reaſon is, that whatever error is committed in determining the magnitude of $D' - D$, muſt be multiplied into the ſquare of the ſecant of the latitude, in order to give its full effect in changing the value of the fraction $\frac{c}{a}$. For it has been ſhewn, that $\frac{c}{a} = \frac{1}{2}\left(\frac{D' - D}{D'}\right) \sec^2\varphi$; now, if we ſuppoſe the error committed in aſcertaining $D' - D$ to be in all caſes the ſame, the error of the fraction $\frac{D' - D}{D'}$ will alſo be in all caſes nearly the ſame, the denominator D' being but little affected either by the ſuppoſed error, or by the change of latitude. But this error, which may thus be conſidered as a conſtant

 quantity,

quantity, when multiplied into $\frac{1}{2}$ fec $^2\varphi$, gives the variation or error in $\frac{c}{a}$, which error therefore increafes, *cæteris paribus*, as the fquare of the fecant of the latitude, fo that, on approaching the pole, it increafes without limit, and is ultimately infinite. Comparifons of this kind may therefore be expected to give refults the more accurate the nearer they are to the equator, under which circle they will be the moft accurate of all. Here, again, however, another circumftance muft be taken into confideration, viz. that the method of afcertaining the differences of longitude by the convergency of the meridians, fo convenient in furveys of this kind, is applicable only in high latitudes. In a trigonometrical furvey, therefore, of a country lying much farther fouth than Britain, a different method of afcertaining the longitudes of places muft neceffarily be adopted.

22. THE theorems, which were next propofed to be confidered, are thofe that determine the figure of the earth from the meafures of degrees of the curve perpendicular to the meridian, in different latitudes. For this purpofe let D′ be a degree of one of thefe curves, in the latitude φ', and D″ a degree of one them, in another latitude φ''. Then c being the compreffion, as before, we have by § 18. $m\text{D}' = a + c \text{ fin}^2\varphi'$,

and alfo $m\text{D}'' = a + c \text{ fin}^2\varphi''$.

Hence $m(\text{D}' - \text{D}'') = c\,(\text{fin}^2\varphi' - \text{fin}^2\varphi'')$, and

therefore $c = \frac{m\,(\text{D}' - \text{D}'')}{\text{fin}^2\varphi' - \text{fin}^2\varphi''}$.

THIS formula may be rendered more convenient for calculation, by confidering that $\text{fin}^2\varphi' = \frac{1 - \text{cof} 2\varphi'}{2}$, fo that

$\text{fin}^2\varphi' - \text{fin}^2\varphi'' = \frac{1 - \text{cof}\,2\varphi' - 1 + \text{cof}\,2\varphi''}{2} = \frac{\text{cof}\,2\varphi'' - \text{cof}\,2\varphi'}{2}$. But

$\text{cof}\,2\varphi'' - \text{cof}\,2\varphi' = 2\,\text{fin}\,(\varphi' + \varphi'') \times \text{fin}\,(\varphi' - \varphi'')$, wherefore

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$\text{ſin}^2\varphi' - \text{ſin}^2\varphi'' = \text{ſin}(\varphi' + \varphi'') \times \text{ſin}(\varphi' - \varphi'')$, and

$$c = \frac{m(D' - D'')}{\text{ſin}(\varphi' + \varphi'') \times \text{ſin}(\varphi' - \varphi'')}.$$

23. IN the ſame manner, becauſe $mD' = a + c\,\text{ſin}^2\varphi'$, by ſubſtituting for c, we have

$$mD' = a + \frac{m(D' - D'')\,\text{ſin}^2\varphi}{\text{ſin}(\varphi' + \varphi'') \times \text{ſin}(\varphi' - \varphi'')}, \text{ and}$$

$$a = mD' - \frac{m(D' - D'')\,\text{ſin}^2\varphi}{\text{ſin}(\varphi' + \varphi'') \times \text{ſin}(\varphi' - \varphi'')}.$$

24. LASTLY, ſince $mD' = a + c\,\text{ſin}^2\varphi'$,
and $mD'' = a + c\,\text{ſin}^2\varphi''$,
dividing the firſt of theſe equations by the ſecond, and rejecting the higher powers of c, we have

$\frac{D'}{D''} = 1 + \frac{c}{a}(\text{ſin}^2\varphi' - \text{ſin}^2\varphi'')$, and therefore,

$$\frac{c}{a} = \frac{\frac{D'}{D''} - 1}{\text{ſin}^2\varphi' - \text{ſin}^2\varphi''}.$$ Hence alſo

$$\frac{c}{a} = \frac{\frac{D'}{D''} - 1}{\text{ſin}(\varphi' + \varphi') \times \text{ſin}(\varphi' - \varphi'')};$$ or more conveniently for calculation by logarithms, $\frac{c}{a} = \frac{D' - D''}{D''\,\text{ſin}(\varphi' + \varphi'') \times \text{ſin}(\varphi' - \varphi'')}$.

25. WE may compare this value of $\frac{c}{a}$ with that obtained in § 18. from other data, in order to determine which of the two methods of finding $\frac{c}{a}$ is to be preferred, under given circumſtances. Suppoſe, for inſtance, a degree of the curve perpendicular to the meridian, in the latitude φ' to be D', and a degree of the meridian itſelf in the ſame latitude to be Δ'; it is required to find in what other latitude φ'', a degree D'', perpendicular to the meridian, muſt be meaſured, in order that the compariſon of D' and D'', and of D' and Δ, may give values of $\frac{c}{a}$, in which the probable error is the ſame.

HERE

HERE, agreeably to an obfervation already made, we may, in order to eftimate the error produced in $\frac{c}{a}$, in confequence of an error in the determination of D′, and D″, and Δ, fuppofe the error to affect D′ — D″, or D′ — D″ only, without paying any regard to the variation of D′ in the denominator. Therefore, fince by § 18. we have $\frac{c}{a} = \frac{D' - \Delta}{2D' \cos{}^2\varphi'}$, and again, by § 24. $\frac{c}{a} = \frac{D' - D''}{D'(\sin{}^2\varphi' - \sin{}^2\varphi'')}$, if we fuppofe equal errors in determining D′ — Δ, and D′ — D″, and alfo that thefe are the only errors, their effect will be the fame, in both cafes, if $2 \cos\varphi'^2 = \sin({}^2\varphi' - \sin{}^2\varphi'')$. Now, if we fuppofe φ'' the quantity fought, and add $\cos\varphi'^2$ to both fides of the preceding equation, then $3\cos{}^2\varphi' = \sin{}^2\varphi' + \cos{}^2\varphi' - \sin{}^2\varphi'' = 1 - \sin{}^2\varphi' = \cos{}^2\varphi''$. The latitude φ'' therefore muft be fuch, that $\cos\varphi'' = \sqrt{3} \times \cos\varphi'$. If, therefore, φ' be fuch that $\cos\varphi' = \frac{1}{\sqrt{3}}$, the cofine of φ'' will be $= 1$, and φ'' therefore $= 0$. Now, 54°. 44′ is the arch of which the cofine $= \frac{1}{\sqrt{3}}$ nearly, therefore, if a degree of the meridian, and of the perpendicular to it, be meafured in latitude 54°. 44′, the comparifon of thefe with one another will give a refult as accurate as if the degree of the perpendicular, in that latitude, were compared with the degree at the equator, and more accurate of confequence than if any other degree of the perpendicular to the meridian, were to be compared with D′.

26. HENCE, alfo, the comparifon of the degree of the meridian, and of the perpendicular to it, in the fouth of England, is better than if a degree of the perpendicular meafured in that latitude were compared with a degree at the equator. For if, in the equation $\cos\varphi'' = (\cos\varphi') \times \sqrt{3}$, we make $\varphi' = 50°.\ 41'$, (or any thing lefs than 54°. 44′,) φ'' will come out impoffible.

27. IT

27. It may be ſhewn, too, nearly in the ſame manner, that if a degree of the perpendicular to the meridian were meaſured in Siberia, as far north as the latitude of 70°, ſuppoſing that to be poſſible, and compared with a degree in latitude 45°, or even conſiderably farther ſouth, it would not give a reſult ſo exact as the degree of the meridian and perpendicular meaſured in the ſouth of England. This ſhews, that the method of aſcertaining the figure of the earth, propoſed by the authors of the *Trigonometrical Survey*, (*Phil. Tranſ.* ibid. p. 529.), as a ſubject of future inquiry, is leſs exact than that which is founded on their own obſervations.

28. We may alſo aſcertain, by the ſame means, the relative accuracy of the method of finding the figure of the earth, from the compariſon of a degree of the meridian with a degree of the perpendicular in the ſame latitude, and of the method of reſolving the ſame problem by the compariſon of two degrees of the meridian in different latitudes.

If, then, D be a degree of the meridian, and D′ of the perpendicular, in latitude φ, and if Δ be a degree of the meridian in a different latitude φ', it is required to find whether the moſt accurate value of $\frac{c}{a}$ will be found, by comparing D and D′, or D and Δ.

Since we have, by what has been already ſtated, § 4.

$$mD = a - 2c + 3c \sin^2\varphi, \text{ and}$$

$$m\Delta = a - 2c + 3c \sin^2\varphi', \text{ we have alſo}$$

$$\frac{D}{\Delta} = 1 + \frac{3c}{a}(\sin^2\varphi - \sin^2\varphi') \text{ and therefore,}$$

$$\frac{c}{a} = \frac{D - \Delta}{3\Delta(\sin^2\varphi - \sin^2\varphi')}.$$

Now, it has been already ſhewn, that, by comparing D and D′ we have $\frac{c}{a} = \frac{D' - D}{2D' \cos^2\varphi}$. Suppoſing, therefore, equal errors

to

to be committed in the determination of $D - \Delta$, and of $D' - D$, and alfo paying no regard to the inequality of Δ and D' in the denominators of thefe fractions, as it is not fo great as materially to affect the quantity that is fought for here, we fhall have the errors in $\frac{c}{a}$ nearly the fame in both formulas, when φ and φ' are fuch that $2 \operatorname{cof}^2\varphi = 3 \operatorname{fin}^2\varphi - 3 \operatorname{fin}^2\varphi'$, or when $\frac{2}{3} \operatorname{cof}\varphi^2 = \operatorname{fin}^2\varphi - \operatorname{fin}^2\varphi'$, that is, adding $\operatorname{cof}^2\varphi$ to both fides, $\frac{5}{3} \operatorname{cof}^2\varphi = \operatorname{fin}^2\varphi + \operatorname{cof}^2\varphi - \operatorname{fin}^2\varphi'$, and, therefore, $\frac{5}{3} \operatorname{cof}^2\varphi = 1 - \operatorname{fin}^2\varphi' = \operatorname{cof}^2\varphi'$, or $\operatorname{cof}\varphi' = (\operatorname{cof}\varphi)\sqrt{\frac{5}{3}}$.

29. If, therefore, $\operatorname{cof}\varphi = \sqrt{\frac{3}{5}}$, $\operatorname{cof}\varphi' = 1$, that is $\varphi' = 0$, fo that Δ, the fecond of the degrees of the meridian, muft in this cafe be under the equator. But $\sqrt{\frac{3}{5}}$ is the cofine of $39°.\ 14'$, in which latitude therefore if D and D′ be meafured, the refult, by comparing them with one another, is as exact as if D were compared with the degree under the equator. Hence, if D and D′ are meafured in a lower latitude than the above, the refult will be more exact, than if D were compared with the degree at the equator.

If we fuppofe D and D′, meafured in the fouth of England, fo that $\varphi = 50°.\ 41'$; then we will have $\varphi' = 35°.\ 7'$, fo that D muft be compared with a degree of the meridian as far fouth as $35°.\ 7'$, in order that the refult may be as good as when D and D′ are compared with one another.

From this it is evident, that the method of comparing degrees of the meridian, and perpendicular in the fame latitude, has even an advantage over the comparifon of degrees of the meridian in different latitudes, unlefs thefe laft are taken at a confiderable diftance from one another.

In

In this way may many uſeful concluſions be derived concerning the degree of credit due to meaſurements already made, as well as with reſpect to the ſelection of the places where they are to be made hereafter. On theſe I ſhall enter no further at preſent, and ſhall only add, that, beſides the advantages or diſadvantages which the method of comparing together degrees of the meridian and perpendicular in the ſame latitude has, and which are ſubjects of calculation, it has another advantage, which in the caſe of the Britiſh ſurvey is undoubtedly very great, viz. that all the *data* are furniſhed from one ſyſtem of trigonometrical operations; executed according to the ſame plan, with the ſame inſtruments, and by the ſame obſervers.

30. One other application of geometrical meaſurements to diſcover the figure of the earth yet remains to be conſidered. This is the compariſon of an arch of the meridian with an arch of a parallel of latitude which croſſes it. The meaſure of a parallel of latitude can be executed readily, and is not confined to a ſmall arch as in the caſe of a perpendicular to the meridian. The plumb-line, while it is carried along the circumference of a parallel to the equator, tends continually to the ſame point in the earth's axis, ſo that there is no difficulty in aſcertaining the amplitude of the arch meaſured, providing there be no unuſual diſturbance of the direction of gravity. As an arch of a parallel to the equator, however, is not the ſhorteſt line between two points on the ſurface of the ſpheroid, the meaſurement along that ſurface will not give the length of the arch truly. To obviate this difficulty, it is only neceſſary to follow the method ſo properly introduced into the *Trigonometrical Survey*, of reducing the meaſures, both of lines and angles, to the chords and to the planes of the rectilineal triangles contained by them. In this way, the chord of an arch of a parallel of latitude may be determined, however great the arch; and it is worthy of being remarked, that, whatever be the deflections of the plumb-line at the intermediate ſta-

tions, when the reductions are all properly made, the length of the chord meaſured will not be affected by them; the amplitude of the arch indeed may be affected by ſuch deflections, if they happen at its extremities; but the effect of this error will be rendered the leſs, the greater the arch that is meaſured. We may ſuppoſe, therefore, that the chord of a large arch of a parallel of latitude is meaſured, and the amplitude of the arch itſelf at the ſame time accurately aſcertained. This laſt may be done, either by meaſuring the convergency of the meridians, if it be in a high latitude, or by any other method of aſcertaining differences of longitude which admits of great accuracy. The chord being thus given in fathoms, and the arch ſubtended by it being given in degrees and minutes, the radius of the parallel itſelf becomes known.

31. Now, if we would compare the radius of a parallel thus found, with a large arch of the meridian, we ſhall have by that means a determination of the figure of the earth, not leſs to be relied on than that given in the beginning of this paper. The inveſtigation is eaſy by help of the theorems in § 5. and 6. Let FO be the radius of a parallel to the equator, which paſſes through F, the latitude of which is φ, and is ſuppoſed known; and let FO found by the method juſt deſcribed be $= r$, then, as in § 4. $r = \frac{a^2 \operatorname{cof}\varphi}{\sqrt{a^2 \operatorname{cof}\varphi^2 + b^2 \operatorname{fin}\varphi^2}} = \frac{a \operatorname{cof}\varphi^2}{a\sqrt{1 - \frac{2c}{a}\operatorname{fin}\varphi^2}}$, according to the method of reduction followed in the preceding articles of this paper. Then, becauſe $\sqrt{1 - \frac{2c}{a}\operatorname{fin}\varphi^2} = 1 + \frac{c}{a} \operatorname{fin}\varphi^2$ nearly, we have $r = a \operatorname{cof}\varphi\ (1 + \frac{c}{a} \operatorname{fin}\varphi^2) = a \operatorname{cof}\varphi + c \operatorname{fin}\varphi^2 \operatorname{cof}\varphi$, or if we divide by $\operatorname{cof}\varphi$, $\frac{r}{\operatorname{cof}\varphi} = a + c \operatorname{fin}\varphi^2$. Let $\frac{r}{\operatorname{cof}\varphi} = l$, then $l = a + c \operatorname{fin}\varphi^2$.

32. AGAIN,

32. Again, if ϕ' and ϕ'' are the latitudes of the extremities of an arch of the meridian, the length of which has been meaſured, and found $= l'$, then, according to § 5. we have.

$$l' = a\,(\phi'' - \phi') - \tfrac{c}{2}\left((\phi'' - \phi') + \tfrac{3}{2}\,(\text{ſin } 2\phi'' - \text{ſin } 2\phi')\right).$$

If, therefore, m be the coefficient of a, in the former equation, and n the coefficient of c; and if m' be the coefficient of a, in the latter equation, and n' of c, we have, as in § 6.

$$a = \frac{n'l - nl'}{mn' - m'n}, \text{ and } c = \frac{m'l - ml'}{mn' - m'n}, \text{ or ſince } m = 1,$$

$$a = \frac{n'l - nl'}{n' - m'n}, \text{ and } c = \frac{m'l - l'}{n - m'n};\ \text{alſo } \frac{c}{a} = \frac{m'l - l'}{n'l - nl'}.$$

33. In this way of determining a and c, the parallel of latitude may either interſect the arch of the meridian meaſured or not. If it interſect that arch, this method may have the ſame advantage that was taken notice of in another ſolution, viz. that the whole of the *data* may be furniſhed from the ſame ſyſtem of trigonometrical operations. Thus, in the ſurvey of Great Britain, an arch of 5 or 6 degrees of a parallel to the equator might be meaſured, and compared with the whole length of the meridian, comprehended between the northern and ſouthern extremities of the Iſland, amounting nearly to 9 degrees.

It is plain, from what has already been ſaid, that the reſult deduced from this compariſon would poſſeſs every advantage, and would be entitled to more credit, than any determination of the figure of the earth that is yet known.

34. On the ſuppoſition that, in a ſurvey of a country, the meaſurement is made along a ſeries of triangular planes, all given in poſition and magnitude, there is yet another method of determining the figure of the earth, more general than any of the former. On the ſuppoſition juſt mentioned, it is evident, that the length of a ſtraight line, or chord, drawn from a given

 angle

angle of any one of thefe triangles, to a given angle of any other of them, may be found by trigonometrical calculation. Let the latitudes be obferved at the extremities of this chord, and alfo the difference of longitude; then, from the nature of an ellipfoid, the length of this fame chord may be expreffed, in terms of the axes a and b, together with the latitudes of the extremities of the chord, and the difference of longitude between them; and this expreffion being put equal to the length of the chord meafured will give an equation, in which all the quantities are known, except a and b. Further, if $a = b + c$, and if the faid expreffion be reduced into a feries, with the powers of c afcending, that feries will converge very rapidly, becaufe c is fmall in refpect of a; then, for a firft approximation, we may reject all the terms that involve the powers of c higher than the firft, by which means we fhall have a fimple equation of the form $ma + nc = l$, where m and n are functions of the latitudes and difference of longitude, and l is the length of the chord.

Now, if a fimilar equation be derived from the meafurement of any other chord, thefe two equations will give a and c in the fame manner as in § 6.; and thus, from the meafurement of any two chords, the figure of the earth will be determined.

35. The length of the chords, thus meafured, fhould be great, fo that they may, if poffible, fubtend angles of feveral degrees, and their pofition will be moft favourable when one of them is in the plane of the meridian, and the other nearly at right angles to it. The numerical computation will be found lefs laborious than might be imagined; but the complete folution of the problem, and the full detail of the inveftigation, I am under the neceffity of delaying to fome future communication.

There feems to be but one difficulty of any confequence that ftands in the way of this method of determining the figure of the earth. It arifes from this, that the afcertaining the pofition

tion of the fuppofed feries of triangular planes relatively to one another, involves in it the allowance to be made for the terreftrial refraction, which it muft be confeffed is not accurately known, and is the more difficult to determine, that it is unavoidably combined with the irregularities in the direction of gravity. It is poffible, indeed, to feparate thefe two fources of error, but not without a fyftem of experiments inftituted directly for that purpofe.

36. THE determination of the difference of longitude, which enters neceffarily into this problem, except in the cafe when both chords are in the direction of the meridian, muft alfo be performed with great accuracy. Among the different ways of doing this, that which proceeds by obferving the convergency of the meridians, though the beft accommodated to the nature of a trigonometrical furvey, is not the leaft liable to objection. For, not to mention that it is only practicable in high latitudes, we muft obferve, that it always implies a correction on account of the ellipticity of the meridian, which is therefore neceffarily hypothetical, and depends on the very thing that is to be found. This inconvenience, however, may be obviated by repeated approximations, and by an accurate folution of *fpheroidal* triangles. On this latter fubject it was my intention to offer to the Society fome theorems, that contain more direct and fuller rules for this kind of trigonometry than any that I have yet met with. I am under the neceffity, however, of referving thefe, as well as the folution of the problem above mentioned, for the fubjects of fome future communication. In the mean time, I think it is material to obferve, that the principle laid down by Mr DALBY, viz. that in a fpheroidal triangle, of which the angle at the pole and the two fides are given, the fum of the angles at the bafe is the fame as in a fpherical triangle, having the fame fides, and the fame vertical angle, is not ftrictly true, unlefs the excentricity of the

the ſpheroid be infinitely ſmall, or the triangle be very nearly iſoſceles. The application of the principle may therefore lead into error, unleſs it be made with due attention to theſe reſtrictions. The gentleman, juſt named, will forgive a remark, which I certainly ſhould not have made, if I had been leſs intereſted for the ſucceſs of the work, in which he has aſſiſted with ſo much ability.

II.

II. ACCOUNT *of certain* PHENOMENA *obſerved in the* AIR VAULT *of the* FURNACES *of the* DEVON IRON WORKS *; *together with ſome practical* REMARKS *on the* MANAGEMENT *of* BLAST FURNACES. *By Mr* ROEBUCK, *in a* LETTER *to Sir* JAMES HALL, *Bart. Communicated by Sir* JAMES HALL.

[*Read, July* 2. 1798.]

SIR,

I HAVE examined my memorandums, concerning the obſervations I made on the condenſed air in the *air vault* of the Devon Iron Works, near Alloa; and, according to your requeſt, I now tranſmit you an account of them; and alſo of an experiment I made, when a partner and manager of theſe works, in order to increaſe the produce of blaſt furnaces.

THE two blaſt furnaces at Devon are of large dimenſions, each being 44 feet high, and about 13 feet wide in the boſhes, or wideſt part, and are formed on a ſteep bank, by two pits ſunk in a very ſolid ſtratum of coarſe grained freeſtone.

THESE pits were afterwards ſhaped and lined in the uſual manner of blaſt furnaces, with common bricks and fire bricks, and the hearth was laid with large blocks of the ſtone that had been dug out, and which ſerve the purpoſe of fire ſtones. At the back of the two furnaces, next the bank, the air vault is ex-

cavated

* THESE iron works are on the banks of the river Devon, which runs into the Frith of Forth near Alloa. They are 3 miles from Alloa, and 8 from Stirling.

cavated, and formed by a mine drove in the ſolid rock, diſtant from the furnaces about 16 feet. The bottom of the air vault is only about 4 feet higher than the level of the bottom of the furnaces. This vault has an aperture at one end to receive the air from the blowing machine, and has two at the oppoſite end, one of which receives the eduction pipe, and the other is a door to give admittance occaſionally into the vault. As the rock is extremely cloſe and ſolid, the vault is dry, except that a little water ouzes very gently from the ſide next the bank in ſmall drops, and does not appear to exceed an Engliſh pint in 24 hours.

THESE furnaces are provided with air, or blaſt, as it is termed, by the means of a fire-engine of the old, or NEWCOMEN'S conſtruction. The diameter of the ſteam cylinder is $48\frac{3}{4}$ inches; and the ſquare area of its piſton being about $1866\frac{1}{2}$ ſquare inches, the power of this ſort of engine cannot be rated at more than 7 lb. to the ſquare inch, amounting in all to about 13062 lb. This power was employed to work an air pump, or blowing cylinder, of 78 inches diameter, and about 7 feet long. The number of ſquare inches on the piſton of the air pump is 4778, and therefore this area, being multiplied by $2\frac{3}{4}$, will produce 13139, being a reſiſtance that nearly balances the above-rated power, and ſhows that the air, which was expelled from the air pump, could not be condenſed more in the ordinary way of working, than with a compreſſing power of about $2\frac{3}{4}$ lb. on each ſquare inch. As the engine was not regulated, at firſt, to make a longer ſtroke than about 4 feet 8 inches, only one furnace being uſed, the quantity of air expelled at each ſtroke of the machine was about 155 cubic feet, which it diſcharged through a valve into the air vault, about 16 times in a minute. When two furnaces afterwards were blown, the engine was regulated to work much quicker, and with a longer ſtroke. The air vault is 72 feet long, 14 feet wide, and 13 feet high; and contains upwards of 13,000 cubic feet, or above 80 times the

contents

contents of the air pump. The top, ſides, and bottom of this vault, where the leaſt fiſſure could be diſcovered in the beds of the rock, were carefully caulked with oakum, and afterwards plaſtered, and then covered with pitch and paper. The intention of blowing into the vault is to equalize the blaſt, or render it uniform, which it effects more completely than any machinery ever yet contrived for the ſame purpoſe. The air is conducted from the vault by the eduction pipe, of 16 inches diameter, into an iron box or wind cheſt, and from this it goes off to each furnace, in two ſmaller pipes that terminate in nozles, or blow-pipes, of only 2½ to 3¼ inch diameter, at the tweer of the furnace.

When the furnace was put in blaſt, after having been filled with coakes, and gently heated for more than ſix weeks, the keepers allowed it to have but little blaſt at firſt, giving it a ſmall blow-pipe of about 2¼ inch diameter, and likewiſe letting off a very conſiderable quantity of air, at the eſcape, or ſafety valve on the top of the iron wind cheſt, as it is a received though erroneous opinion among them, that the blaſt muſt be let on very gradually for ſeveral months. From the conſtruction of this valve, it was impoſſible to aſcertain the exact proportion of the blaſt they thus parted with, but I believe it was very conſiderable. The conſequence was, that the furnace, after it had been in blaſt for ſeveral days, never ſeemed to arrive at its proper degree of heat, but was always black and cold about the tweer in the hearth, and appeared in danger of choking, or gobbing, as it is termed.

After various experiments tried in vain, by the keepers and the Company's engineer, and others, (indeed they tried every thing, except giving the furnace a greater quantity of air, which, as I afterwards aſcertained, was all that it wanted), they concluded, that the air vault was the cauſe of the whole miſchief; and, to confirm their opinion, they ſaid they had now discovered that water was, in considerable quantities, driven out of the air vault through the blow-pipe, which cooled the furnace; and they insisted, that the power of the engine was such as to force water out of the solid rock; so that this method of equalizing the blast never would succeed. The other managing partner was so much alarmed by these representations, that he began to consult with the engineer, and others, about finding a substitute for the air vault at any expence.

As the plan of the blowing apparatus had been adopted at my recommendation, and was now so loudly condemned on account of the water, I had other motives, than mere interest, for trying to become better acquainted with the phænomena attending it. I accordingly determined to go into the air vault, and to remain inclosed in the condensed air while the engine was blowing the furnace. It is an experiment that perhaps never was made before, as there never existed such an opportunity. I could not persuade the engineer, or any other of the operative people about the work, to be my companions, as they imagined that there was much danger in the experiment. Mr NEIL RYRIE, however, one of the clerks of the Devon Company, had sufficient confidence in my representations to venture himself along with me.

THE machine had been stopped about two hours previous to our entering the vault, and we found a dampness and mistiness in it, which disappeared soon after the door was shut fast upon us, and the engine began to work in its usual manner. After four or five strokes of the engine, we both experienced a singular sensation in our ears, as if they were stopped by the fingers, which continued as long as we remained in the condensed air. Our breathing was not in the least affected. I had no thermometer with me, but the temperature of the air felt to us the same as that without the vault. Sound was much magnified, as we perceived, when we talked to each other, or struck any

thing;

thing; particularly, the noiſe of the air eſcaping at the blow-pipe, or waſte valve, was very loud, and ſeemed to return back to us. There was no appearance of wind to diſturb the flame of our candles; on the contrary, I was ſurpriſed to find, that when we put one of them into the eduction pipe, which conveys the wind from the vault to the furnaces, it was not blown out. There was not the ſmalleſt appearance of any drops of water iſſuing out of this pipe. The ouzing and dropping of water from the ſide of the rock, next the bank, ſeemed the ſame as before the condenſation was made in the vault. In ſhort, every thing appeared, in other reſpects, the ſame as when we were in the common atmoſphere. Having remained about an hour in the condenſed air, and ſatisfied ourſelves that no water, during that time, that we could in the leaſt diſcover, was agitated and forced out of the rock and vault by the power of the blaſt, as was imagined and inſiſted on, we gave the ſignal to ſtop the engine. As ſoon as it ceaſed to work, and the condenſation abated, and before the door of the vault was unſcrewed, *the whole vault, in a few ſeconds, became filled with a thick vapour, ſo that we could hardly ſee the candles at four or five yards diſtance.* The door being now opened, the work people, anxious to know our ſituation, and what had occurred, came into the vault, and prevented any further obſervations.

I NOW endeavoured to account for this curious appearance of the water, which only ſhewed itſelf occaſionally, in very ſmall quantities, at the tweer, and at a hole I ordered to be made in the bottom of the wind cheſt to collect it more accurately, for it never was obſerved, but either when the engine, after working ſlowly, was made to work quicker, or, after having been ſtopped for a few minutes, was ſet to work again.

I CONSIDERED the vapour which we had diſcovered in the vault to ariſe from the moiſture of the ſide of the rock next the furnace, which being expelled by the great heat of the furnace,

 and

and converted into vapour, was able to force its way through the pores of the rock into the vault, but that being in a manner confined within the rock, by the preſſure of the condenſed air, it found itſelf at liberty to come into the vault, only when the condenſation abated conſiderably, or was totally removed by the going ſlow, or ſtopping of the engine. It alſo occurred to me, that the air, in a ſtate of condenſation, might poſſibly be capable of holding a greater quantity of water in ſolution, which might precipitate ſuddenly into vapour or miſt when the condenſation abated. I imagined, therefore, that the very ſmall quantities of water we at times diſcovered, proceeded from nothing elſe but this vapour, in its paſſage to the furnace along with the blaſt, being condenſed into water, by the coolneſs of the eduction pipe and iron wind cheſt. The quantity of water did not appear to amount to a gallon in twenty-four hours.

A FEW days after I had made this experiment, the water ceaſed entirely to make its appearance, either at the tweer, or at the hole in the wind cheſt; but the furnace did not come into heat for a long while after, and indeed not till the keepers let much more air into it by a larger blow-pipe, and allowed leſs air to eſcape at the ſafety valve. It is probable that the rock was now become perfectly dry by the continued heat of the furnace.

MY experiment had the good effect to remove all the prejudices againſt the plan I had adopted of blowing the furnaces, and likewiſe prevented the other partner from laying out a large ſum of money, by ſtopping the works, and altering the blowing machinery. Indeed, it has ſince been admitted, by all who have ſeen it at work, to be the moſt ſimple and effective method of equalizing the blaſt of any yet put in practice.

THIS experiment led me, ſome time afterwards, to apply a wind guage that I contrived, to aſcertain preciſely the ſtate of the

the condenſation of the air thrown into the furnaces. I found that a column of quick ſilver was raiſed five inches, and ſometimes, though ſeldom, ſix inches, and, in the interval of the return of the engine to receive air into the air pump, it fell only half of an inch. At this time only one furnace was worked. But when two furnaces were in blaſt, the engine only raiſed the mercurial guage about 4 inches, becauſe the Devon Company, for certain reaſons, did not, while I continued a partner, think proper to allow the blowing machinery to be completed, by the putting to work their ſecond boiler of 20 feet diameter for the fire engine, according to my original deſign, which, by adjuſting the machinery, would have enabled us to blow two furnaces, with two boilers, with as much effect, in proportion, as one furnace with one boiler. This inſtrument had the advantage of enabling the work people to diſcover the real power of their blaſt, and know the exact condition of the air valves, and the gearing of the blowing piſton; for if theſe were not tight, and in order, (although the engine might, to appearance, be doing well, by making the ſame number of diſcharges of the air pump as uſual *per* minute), yet the wind guage would not riſe ſo high, and would ſhew that there was an imperfection ſome where, by reaſon of a quantity of air eſcaping at the valves, or piſton, that could not ſo eaſily otherwiſe be known. This contrivance was conſidered as of much uſe, and was afterwards always quoted in the Company's journal books, to ſhow the actual ſtate of the blowing machine, in comparing the daily produce of the furnaces.

I HOPE you will not think me tedious, when I explain to you another experiment, which appears to me to be of conſiderable importance to all manufacturers of caſt iron.

I HAD reaſon to conjecture, from my own obſervations on the effects of blowing machinery on blaſt furnaces, as well as from

from the knowledge I had acquired from my father Dr ROEBUCK, and from my communications with other experienced iron masters, that a great part of the power of ſuch machinery was miſapplied in general practice, by throwing air into furnaces with much greater velocity than neceſſary, and that, if this velocity was, to a certain degree diminiſhed, the ſame power, by properly adjuſting the blowing machinery, of whatever nature, would be capable of throwing into the furnace a proportionally greater quantity of air. For, "*Since the quantities of any fluid, iſſuing through the ſame aperture, are as the ſquare roots of the preſſure;*" it follows, that it would require *four* times the preſſure, or power, to expel *double* the quantity of air, through the ſame aperture, in the ſame time: But if the area of the aperture was doubled, then the quantity of air expelled by the ſame power, and in the ſame time, would be increaſed in the ratio of the ſquare root of 2 to 1, though its velocity would be diminiſhed exactly in the ſame proportion. Again: I conſidered that the quantity and intenſity of heat, produced in blaſt furnaces, and conſequently its effects in increaſing the produce, might be only in proportion to the quantity of air decompoſed in the proceſs of combuſtion, without regard to its greater velocity; that is to ſay, whether or not the ſame quantity of air was forced, in the ſame time, into the furnace through a ſmall pipe, or through one of larger dimenſions; for, in attending to the proceſs of a common air furnace for remelting of iron, where there is a very large quantity of air admitted through the large areas between the bars, it is well known, that a much greater intenſity of heat is produced than takes place in a blaſt furnace, and yet the air does not enter into the fire through the bars with increaſed denſity or great velocity. I therefore thought it probable, that increaſing the *quantity* of air, thrown into the blaſt furnace in a conſiderable degree, although the *velocity or denſity* might be much

much lefs, would have the effect of increafing its heat, and operations, and produce. And as, from the principles above ftated, with regard to the machinery, I faw I could greatly increafe the quantity of air thrown into the furnace, by enlarging the diameter of the blow-pipe, and regulating the engine accordingly, without being obliged to employ more power, I was anxious to make this experiment.

A fyftem of management, of which I did by no means approve, was adopted by the other partners of the Devon Company, foon after the works were begun to be erected; and, in the profecution of it, they ordered their fecond furnace to be put in blaft, without permitting thofe meafures to be taken that were neceffary to provide and maintain a fufficient ftock of materials; and alfo without allowing their blowing machine to be completed, according to the original defign, by the addition of its fecond boiler. As might have been expected, a trial of feveral months to carry on two furnaces, with only half the power of fteam that was neceffary, and an inadequate ftock of materials, proving unfuccefsful, the Company, as a remedy, inftead of making up the above deficiencies, ordered one of the furnaces to be blown out, and ftopped altogether. This improper meafure, however, afforded me the opportunity of immediately putting in practice the plan I have mentioned.

When one of the furnaces was ftopped, the other continued to be blown by a blow-pipe of $2\frac{3}{4}$ inches diameter, and the produce of the furnace, for feveral weeks thereafter, was not 20 tons of iron *per* week at an average. The engine at this time was making about 16 ftrokes a minute, with a ftroke of the air pump, about 4 feet 8 inches long; but when I altered the diameter of the blow-pipe, firft to 3, and immediately after to $3\frac{1}{4}$ inches diameter, and regulated the working gears of the engine, fo as to make a ftroke of 5 feet 2 inches long, and about 19 ftrokes in a minute, on an average, the produce was immediately ly increaſed. It continued to be, on an average of nine months immediately after this improvement, at the rate of 33 tons of iron *per* week, of as good quality as formerly; for during this period, from the 21ſt November 1795 to July 30. 1796, this one furnace yielded 1188 tons of iron. No more coals were conſumed in working the blaſt engine, or other expences about the blowing machine incurred, and therefore no more power was employed to produce this great effect. It is alſo of much importance to remark, that the conſumption of materials, from which this large produce was obtained, was by no means ſo great as formerly. The furnace required very conſiderably *leſs fuel, leſs ironſtone, and leſs limeſtone*, than were employed to produce the ſame quantity of iron by the former method of blowing; and according to the ſtatements made out by the Company's orders, as great a change was effected in the œconomical part of the buſineſs.

From the ſucceſs of this experiment, ſo well authenticated, and continued for ſeveral months, I am led to be of opinion, that all blaſt furnaces, by a proper adjuſtment of ſuch machinery as they are provided with, might greatly and advantageouſly increaſe their produce, by aſſuming this as a principle, viz. "*That with the given power it is rather by a great quantity of air thrown into the furnace, with a moderate velocity, than by a leſs quantity thrown in with a greater velocity, that the greateſt benefit is derived, in the ſmelting of ironſtones, in order to produce pig-iron.*" However, it is by experiment alone, perhaps, that we can be enabled to find out the exact relations of power, velocity, and quantity of air requiſite to produce a *maximum* of effect *.

But, an unfortunate diſagreement among the partners of the Devon Company, put it out of my power to make further progreſs

* If Q be the quantity of a fluid, iſſuing in a given time through an aperture of the diameter D, V its velocity, and P the power by which it is forced through the aperture: then the area of that aperture being as D^2, the quantity of the fluid iſſuing in the given time will be as VD^2, or $VD^2 = Q$.

Again,

grefs in this matter, by laying me under the neceffity, two years ago, of withdrawing myfelf entirely from the concern.

I have the honour to be, refpectfully,

SIR,

Your moft obedient fervant,

JOHN ROEBUCK.

EDINBURGH, June 30. 1798.

To Sir James Hall, Bart.

IN order to illuftrate what is faid above, a ground plan of the air vault and furnaces of the Devon Iron Works is given in Plate I.; of which the explanation follows:

EXPLANATION OF FIG. 2. PL. I.

A The air vault, formed by a mine drove in the folid rock of coarfe grained freeftone.

B The blowing cylinder.

C The pipe that conveys the air from the blowing cylinder to the air vault.

D The eduction pipe that carries the air from the air vault to the iron wind cheft.

AGAIN, this quantity multiplied into its velocity, will be as the *momentum* of the fluid expelled, or as the power by which it is expelled, that is, $V^2 D^2 = P$, or $VD = \sqrt{P}$.

HERE, therefore, if D is given, V is as $\sqrt{P}$, as Mr ROEBUCK affirms. Alfo, becaufe $V = \frac{Q}{D^2}$, and alfo $V = \frac{\sqrt{P}}{D}$, $Q = D\sqrt{P}$, fo that, while P remains the fame, Q will increafe as D increafes, and V will diminifh in the fame ratio.

THE problem, therefore, of throwing the greateft quantity of air into the furnace, with a given power, ftrictly fpeaking, has no *maximum*, but the largeft aperture of which the engine can admit muft be the beft. It is probable, however, that there is a certain velocity with which the air ought to enter into the furnace; this will produce a limitation of the problem, which, as Mr ROEBUCK fuggefts, is not likely to be difcovered but by experiment. J. P.

E The iron wind cheſt, (about $2\frac{1}{2}$ feet cube), in which is inſerted a wind-gauge, repreſented in fig. 3.

F F The two blow-pipes for each furnace, which terminate in apertures of $3\frac{1}{4}$ inches diameter at the tweers of the furnaces.

G G The two blaſt furnaces, placed in two pits ſunk in the ſolid rock.

H H The timps of the furnaces from whence the caſt-iron is run off into the caſting room, L L.

O The door to give occaſional admittance into the air vault.

M The excavation, in which is placed the blowing machine.

Explanation of Fig. 3.

A The end of the wind-gauge, (about 12 inches long), which is open to the atmoſphere, being half filled with quickſilver.

B The end that is inſerted in the iron wind cheſt, and expoſed to the preſſure of the condenſed air of the air vault.

III.

III. EXPERIMENTS *on* WHINSTONE *and* LAVA. *By Sir JAMES HALL, Bart.* F. R. S. & F. A. S. EDIN.

[*Read, March* 5. *and June* 18. 1798.]

THE experiments defcribed in this paper were fuggefted to me many years ago, when employed in ftudying the *Geological Syftem* of the late Dr HUTTON, by the following plaufible objection, to which it feems liable.

GRANITE, porphyry, and bafaltes, are fuppofed by Dr HUTTON to have flowed in a ftate of perfect fufion into their prefent pofition; but their internal ftructure, being univerfally rough and ftony, appears to contradict this hypothefis; for the refult of the fufion of earthy fubftances, hitherto obferved in our experiments, either is glafs, or poffeffes, in fome degree, the vitreous character.

THIS objection, however, lofes much of its force, when we attend to the peculiar circumftances under which, according to this theory, the action of heat was exerted. Thefe fubftances, when in fufion, and long after their congelation, are fuppofed to have occupied a fubterraneous pofition far below what was then the furface of the earth; and Dr HUTTON has afcribed to the modification of heat, occafioned by the preffure of

 the

the ſuperincumbent maſs, many important phenomena of the mineral kingdom, which he has thus reconciled to his ſyſtem.

One neceſſary conſequence of the poſition of theſe bodies, ſeems, however, to have been overlooked by Dr Hutton himſelf: I mean, that, after their fuſion, they muſt have cooled very ſlowly; and it appeared to me probable, on that account, that, during their congelation, a cryſtallization had taken place, with more or leſs regularity, producing the ſtony and cryſtallized ſtructure, common to all unſtratified ſubſtances, from the large grained granite, to the fine grained and almoſt homogeneous baſalt. This conjecture derived additional probability from an accident ſimilar to thoſe formerly obſerved by Mr Keir, which had juſt happened at Leith: a large glaſs-houſe pot, filled with green bottle glaſs in fuſion, having cooled ſlowly, its contents had loſt every character of glaſs, and had completely aſſumed the ſtony ſtructure.

These views made part of a paper which I had the honour of laying before this Society in 1790 *; and about the ſame time I determined to ſubmit my opinions to the teſt of experiment. I communicated this intention to all my friends, and in particular to Dr Hutton; from him, however, I received but little encouragement. He was impreſſed with the idea, that the heat to which the mineral kingdom has been expoſed was of ſuch intenſity, as to lie far beyond the reach of our imitation, and that the operations of nature were performed on ſo great a ſcale, compared to that of our experiments, that no inference could properly be drawn from

* Particular reaſons induced me not to publiſh this paper at full length; but, wiſhing to preſerve a record of ſome opinions peculiar to myſelf which it contained, I introduced a ſhort abſtract of it into the Hiſtory of the Tranſactions.

from the one to the other. He has ſince expreſſed the ſame ſentiments in one of his late publications, *(Theory of the Earth, vol. I. p. 251.)*, where he cenſures thoſe who " judge of the " great operations of the mineral kingdom, from having kin- " dled a fire, and looked into the bottom of a little crucible."

But, notwithſtanding my veneration for Dr Hutton, I could not help differing from him on this occaſion: For, granting that theſe ſubſtances, when in fuſion, were acted upon by a heat of ever ſo great intenſity, it is certain, nevertheleſs, that many of them muſt have congealed in moderate temperatures, ſince many are eaſily fuſible in our furnaces; for it is impoſſible that a ſubſtance ſhould congeal at a higher point than that at which it may afterwards be melted. If, then, theſe phenomena depend upon the circumſtances of congelation, the imitation of the natural proceſs is an object which may be purſued with rational expectation of ſucceſs; and, could we ſucceed in a few examples on a ſmall ſcale, and with eaſily fuſible ſubſtances, we ſhould be entitled to extend the theory, by analogy, to ſuch as, by their bulk, or by the refractory nature of their compoſition, could not be ſubjected to our experiments. It is thus that the aſtronomer, by obſerving the effects of gravitation on a little pendulum, is enabled to eſtimate the influence of that principle on the heavenly bodies, and thus to extend the range of accurate ſcience to the extreme limits of the ſolar ſyſtem.

Encouraged by this reaſoning, I began my projected ſeries of experiments in the courſe of the ſame year (1790), with very promiſing appearances of ſucceſs. I found that I could command the reſult which had occurred accidentally at the glaſs-houſe; for, by means of ſlow cooling, I converted bottle glaſs, after fuſion, into a ſtony ſubſtance, which again, by the application of ſtrong heat, and ſubſequent rapid cooling, I reſtored to the ſtate of perfect glaſs. This operation I performed repeatedly with the ſame ſpecimen, ſo as to aſcertain that the cha-

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racter of the refult was ftony or vitreous, according to the mode of its cooling.

SOME peculiar circumftances interrupted the profecution of thefe experiments till laft winter, when I determined to refume them. Deliberating on the fubftance moft proper to fubmit to experiment on this occafion, I was decided by the advice of Dr HOPE *, well known by his difcovery of the Earth of Strontites, to give the preference to whinftone.

THE term whinftone, as ufed in moft parts of Scotland, denotes a numerous clafs of ftones, diftinguifhed in other countries by the names of bafaltes, trap, wacken, grünftein and porphyry. As they are, in my opinion, mere varieties of the fame clafs, I conceive that they ought to be connected by fome common name, and have made ufe of this, already familiar to us, and which feems liable to no objection, fince it is not confined to any particular fpecies †.

THE following experiments were performed with various kinds of whinftone, and have likewife been extended to lava. To inveftigate the relation between thefe two claffes of fubftances, feems, in the prefent ftate of geology, an object of confiderable importance; for they refemble each other in fo many refpects, that we are naturally led to afcribe the formation of both to the fame

* IN the courfe of laft winter, when I firft thought of refuming my experiments, I propofed to this gentleman, that, in imitation of a practice, common in the Academy of Sciences of Paris, we fhould perform them in company. To this propofal he cheerfully agreed; but, before any experiments had been begun, he found himfelf fo much occupied by profeffional duties, that he could not beftow upon the fubject the time which it neceffarily required; and we gave up the idea of working in company.

† IN characterifing the particular fpecimens, I have adopted, with fcarcely any variation, defcriptions drawn up by Dr KENNEDY, whofe name I fhall have occafion frequently to mention in the courfe of this paper. In the employment of terms, we have profited by the advice of Mr DÉRIABIN, a gentleman well verfed in the language of the Wernerian School.

ſame cauſe, and to believe that whinſtone, as well as lava, has been expoſed to the action of heat. In the courſe of the paper, I ſhall mention ſeveral accidental reſults, which, if conſidered ſeparately, might ſeem unworthy of notice, but which, by affording the means of compariſon between the two claſſes, are of great ſervice in the general inveſtigation.

The whinſtone firſt employed was taken from a quarry* near the Dean, on the Water of Leith, in the neighbourhood of Edinburgh. This ſtone is an aggregate of black and greeniſh-black hornblend, intimately mixed with a pale reddiſh-brown matter, which has ſome reſemblance to felſpar, but is far more fuſible. Both ſubſtances are imperfectly and confuſedly cryſtallized in minute grains. The hornblend is in the greateſt proportion; and its fracture appears to be ſtriated, though in ſome parts foliated; that of the reddiſh-brown matter is foliated. The fracture of the ſtone *en maſſe* is uneven, and it abounds in ſmall facettes, which have ſome degree of luſtre. It may be ſcratched, though with difficulty, by a knife, and gives an earthy ſmell when breathed on. It frequently contains ſmall ſpecks of pyrites.

On the 17th of January 1798, I introduced a black lead crucible, filled with fragments of this ſtone, into the great reverberating furnace at Mr Barker's iron foundery. In about a quarter of an hour, I found that the ſubſtance had entered into fuſion, and was agitated by a ſtrong ebullition. I removed the crucible, and allowed it to cool rapidly. The reſult was a black glaſs, with a tolerably clean fracture, interrupted however by ſome ſpecks.

In ſubſequent experiments, I endeavoured, by ſlow cooling after fuſion, to prevent the whinſtone from becoming vitreous, and to compel it to reſume its original character by cryſtallization. In this I ſo far ſucceeded as to obtain a ſubſtance, which was not glaſs, though it did not poſſeſs the properties of

* Called Bell's Mills Quarry.

of whinſtone. The production of this intermediate ſubſtance, which much reſembled the liver of an animal, is accompanied with ſome curious particulars, which I ſhall enumerate and explain in another part of this paper. On ſome occaſions, too, I obtained a vitreous maſs in which were a multitude of little ſpheres, having a dull or earthy fracture.

At laſt, on the 27th of January, I ſucceeded completely in the object I had in view. A crucible, containing a quantity of whinſtone, melted in the manner above deſcribed, being removed from the reverberatory, and conveyed rapidly to a large open fire, was immediately ſurrounded with burning coals, and the fire, after being maintained ſeveral hours, was allowed to go out. The crucible, when cold, was broken, and was found to contain a ſubſtance, differing in all reſpects from glaſs, and in texture completely reſembling whinſtone. Its fracture was rough, ſtony and cryſtalline; and a number of ſhining facettes were interſperſed through the whole maſs. The cryſtallization was ſtill more apparent in cavities produced by air bubbles, the internal ſurface of which was lined with diſtinct cryſtals*.

Having ſhewn this reſult to ſeveral of my friends, Dr Hope regretted that the ſubſtance, previouſly to its artificial cryſtallization, had not been reduced to the ſtate of ſolid glaſs; ſince the adverſaries of the ſyſtem might allege, that, during the action of heat, the original cryſtallized texture of the ſtone had never been completely deſtroyed. Being convinced of the propriety of this obſervation, I determined, in future, to reduce the ſtone firſt to glaſs, and to perform the cryſtallization after a ſecond fuſion.

For this purpoſe, with the aſſiſtance of Dr Kennedy, to whoſe co-operation I am greatly indebted for the ſucceſs of all the following experiments, I reduced a quantity of the ſame whinſtone to

* I showed this reſult at a meeting of the Society on 5th of February.

to moſt perfect black glaſs. A crucible, filled with fragments of this glaſs, being then expoſed to a heat, which, from previous trials, was judged to be more than ſufficient to reduce its contents to fuſion, the fire was very gradually lowered till all was cold. I thus expected to obtain a reſult ſimilar to that laſt mentioned, but found, to my great ſurpriſe, that the fragments had never been in complete fuſion, ſince they ſtill, in a great meaſure, retained their original ſhape. This extraordinary fact, which afterwards led to the diſcovery of ſome curious properties of whinſtone, will be fully accounted for in a ſubſequent part of the paper.

ANOTHER portion of the ſame glaſs being perfectly melted by a very ſtrong heat, the temperature was reduced to about 28 of WEDGWOOD, and was maintained at that pitch during ſix hours. The reſult was a perfectly ſolid maſs, cryſtallized to a certain depth from the outſide, though ſtill vitreous in the heart. In another experiment, performed like the laſt in all reſpects, except that the heat was maintained at 28 during twelve hours, I obtained a maſs entirely cryſtalline and ſtony throughout, with facettes appearing in the ſolid parts, and ſmall cryſtals ſhooting into ſome of the cavities.

SOON after I had communicated theſe reſults to Dr HOPE, he performed, with complete ſucceſs, an experiment ſimilar to the firſt, in which I had obtained a cryſtallized ſubſtance, by the gradual cooling of the melted ſtone. The ſame was likewiſe, ſoon afterwards, performed by Mr BOSWELL of Auchinleck.

MY experiments, already deſcribed, were confined to one ſpecies of whinſtone; but have ſince been extended to ſix other varieties. They were all firſt reduced to glaſs by the application of a ſtrong heat, and ſubſequent rapid cooling. After a ſecond fuſion they were cryſtallized, by being kept long in a ſtationary temperature, between 28 and 30. This laſt operation was beſt performed in a long and narrow muffle, wholly ſurrounded with burning coals, according to a practice long followed by Dr KENNEDY, by which the heat could be maintained with so great steadiness as to render the result almost certain.

THE fusibilities were determined in an open muffle, in which a fragment of the substance under trial was placed contiguous to a pyrometer piece. As soon as the fragment, in consequence of the gradual rise of heat, had so far softened as to yield to the touch of a bent iron rod, the pyrometer was removed and measured. The fusibilities, thus obtained, in degrees of WEDGWOOD's scale *, have been stated in a table, to which I would be understood always to refer. I have distinguished the crystallized substances, obtained from the glasses, by the name of *crystallite*, a term suggested by Dr HOPE. It may be observed in this table, that the original whins soften in a range from 38 to 55; the glasses from 15 to 24, and the artificial crystallites from 32 to 45.

No. 1. *Whin of Bell's Mills Quarry.*

THIS stone was the subject of all the foregoing experiments, which were frequently repeated with success on a large scale.

IN trying the fusibility of the glass obtained from it, a curious circumstance occurred, which accounts for the unexpected results

* THE measurement of the temperatures may be relied upon as accurate; they were determined by two sets of pieces, one purchased by me during the lifetime of the late Mr WEDGWOOD, and the other likewise made by him, belonging to Dr KENNEDY. The two sets correspond exactly; and Dr KENNEDY's had, at his request, been carefully examined by the present Mr WEDGWOOD, who found them true by his father's original standard.

refults already mentioned. I had placed in the muffle a long and flender fragment of this glafs, with its extremities refting on two fupports of clay, and its middle unfupported. Having then increafed the temperature by flow degrees, I expected to difcover the loweft point of emollefcence, by obferving when the fragment funk by its own weight. The muffle having attained a moderate heat, I obferved the glafs to lofe its fhape a little. Wifhing to fee it completely melted, the fame heat was continued, but no further change took place. The heat was then raifed feveral degrees, but without effect. At laft, being urged ftill further, the glafs funk down completely between its fupports. The pyrometer being then withdrawn, denoted a temperature above 30.

It occurred to me, that, on this occafion, the glafs, by the firft application of heat, had foftened, and then had cryftallized, fo as to become hard again; that, in cryftallizing, it had acquired fuch infufibility as to yield to no heat under 30. I immediately confirmed this conjecture by the following experiment.

A piece of the fame glafs, placed in a cup of clay, was introduced into the muffle, heated to 21. In one minute it became quite foft, fo as to yield readily to the preffure of an iron rod. After a fecond minute had elapfed, the fragment, being touched by the rod, was found to be quite hard, though the temperature had remained ftationary. The fubftance, thus hardened, had undergone a change throughout; it had loft the vitreous character; when broken, it exhibited a fracture like that of porcelain, with little luftre; and its colour was changed from black to dark brown. Being expofed to heat, it was found to be fufible only at 31; that is, it was lefs fufible than the glafs by 13 or 14 degrees.

Numerous and varied experiments have fince proved, in the cleareft manner, that, in any temperature, from 21 to 28 inclufive, the glafs of this whin paffes from a foft, or liquid ftate, to a folid one, in confequence of cryftallization ; which is differently performed at different points of this range. In the lower points, as at 23, it is rapid and imperfect ; in higher points, flower and more complete, every intermediate temperature affording an intermediate refult. I likewife found, that cryftallization takes place, not only when the heat is ftationary, but likewife when rifing or finking, provided its progrefs through the range juft mentioned is not too rapid. Thus, if the heat of the fubftance, after fufion, exceeds one minute in paffing from 21 to 23, or from 23 to 21, the mafs will infallibly cryftallize, and lofe its vitreous character.

These facts enabled me to account for the production of the fubftance refembling the liver of an animal, which I obtained in my firft attempts to cryftallize the melted ftone. Not being then aware of the temperature proper for complete cryftallization, I had allowed it to be paffed over rapidly by the defcending heat, and I had begun the flow cooling in thofe lower points, at which the formation of this intermediate fubftance takes place.

By the fame means I was enabled to explain the other unexpected refult, which I obtained in endeavouring to convert the glafs of this ftone into cryftallite. The fire applied to the crucible, containing fragments of the glafs, had been raifed very flowly, which I know to have been the cafe by fome circumftances of the experiment. The glafs had foftened by the firft application of heat, but had cryftallized again as the heat gradually rofe ; fo that the fubftance confolidated, while ftill fo vifcid as to retain the original fhape of the fragments; at the fame time it acquired fuch infufibility as to refift the application of higher degrees of heat during the reft of the procefs.

No. 2.

No. 2. *Whin of the Rock of Edinburgh Caſtle.*

This is a baſalt of a blackiſh blue colour. Its grain is fine, and its fracture uneven, partaking of the ſplintery. It is in general homogeneous, although, in ſome pieces, a very few minute cryſtals of hornblend are perceptible. It has ſome luſtre, from a number of ſmall ſhining facettes; has an earthy ſmell when breathed on; and gives fire ſlightly with ſteel.

The pure glaſs which this whin yielded, by rapid cooling after a moderate heat, was cryſtallized in three experiments, and produced maſſes greatly reſembling the original. In one of theſe, formed on a large ſcale in the glaſs-houſe, the reſemblance is ſo ſtrong, both as to colour and texture, that it would be difficult, or perhaps impoſſible, to diſtinguiſh them, but for a few minute air bubbles viſible in the artificial cryſtallite. The glaſs is leſs fuſible than that of No. 1. and ſeems not to poſſeſs the property of producing the liver cryſtallite.

No. 3. *Whin of the Baſaltic Columns on Arthur's Seat, near Edinburgh.*

Its baſis is a baſalt of a dark grey colour, and uneven fracture. It contains numerous laminar cryſtals of felſpar, which ſeem to be almoſt colourleſs, and have conſiderable luſtre and tranſparency. It alſo contains ſome black hornblend. It has an earthy ſmell when breathed on, and gives ſparks ſlightly with ſteel.

In the temperature of 100, or upwards, the whole was changed to pure black glaſs; but in a more moderate heat, (about 60), the felſpar remained unchanged, while the hornblend diſappeared, and formed a glaſs along with the baſis of the ſtone.

Both kinds of glaſs yielded highly characteriſed cryſtallites; that laſt mentioned, having its felſpars entire, produced a ſubſtance like porphyry, in which the white felſpars were embedded in a black cryſtalline baſis. The cryſtals formed in this baſis are ſo complete in one example, that they are ſeen projecting into the cavities, and ſtanding erect on the external ſurface, ſo as to make it ſparkle all over. Theſe black cryſtals ſeem to be hornblend of new formation. We have found, by ſome late experiments, that they are conſiderably more refractory than the cryſtallite in which they lie, and are equally infuſible with ſome ſpecies of natural hornblend.

No. 4. *Whin from the neighbourhood of Duddingſtone Loch.*

It has for its baſis a black baſalt of an uneven fracture. In it are embedded augit in numerous cryſtals, felſpar in a ſmaller proportion, and diſperſed grains of olivin. The felſpar ſeems to be greeniſh-white, with conſiderable luſtre and tranſparency. The ſtone gives fire with ſteel, and has a ſlight earthy ſmell when breathed on. Its glaſs yields a fine grained cryſtallite, like that of No. 1.

No. 5. *Whin of Saliſbury Craig near Edinburgh.*

This ſpecies is an aggregate of black hornblend, and of a greeniſh-white matter, both in minute grains. The greeniſh-white matter reſembles felſpar, but is much more fuſible. The general characters are nearly the ſame with thoſe of the ſpecimen already deſcribed, No. 1. It has conſiderable luſtre, chiefly from the hornblend; an earthy ſmell when breathed on; and gives ſome ſparks with ſteel. Its glaſs yielded a highly facetted cryſtallite,

cryſtallite, approaching to the ſtructure of the original whinſtone, No. 4.

No. 6. *Whin from the Water of Leith.*

It is found in great blocks in the bed of the river, and has been brought there no doubt from a maſs of the ſame kind in the mountains above. It conſiſts of black hornblend, and of a whitiſh matter reſembling felſpar, as in No. 1. and No. 5. Theſe two ſubſtances are nearly in equal proportion, and are confuſedly and imperfectly cryſtallized in minute maſſes. If the whitiſh ſubſtance were felſpar, this ſtone, as well as that laſt mentioned, would be the grünſtein of Werner; but this white ſubſtance is far more fuſible than felſpar, and melts at a lower heat than the hornblend, with which it is mixed. It has an earthy ſmell when breathed on, and may be ſcratched with difficulty by a knife.

In fuſion and cryſtallization it reſembled the other whins. A fragment ſimilar to this in all reſpects, which I found in the neighbourhood of Edinburgh, manifeſted ſo ſtrong a diſpoſition to cryſtallize, that, though cooled in the open air after fuſion, it was found ſtony in the heart, with a vitreous outſide. When cryſtallized, however, with every precaution, it yielded no remarkable reſult.

No. 7. *Whin of the Baſaltic Columns of Staffa.*

I received this ſpecimen from a gentleman who broke it from the original rock. It is baſalt of a bluiſh-black colour. It is fine grained and homogeneous; and its fracture is uneven. It has a ſmall degree of luſtre, from a number of minute ſhining points perceptible in a ſtrong light. It gives an earthy ſmell when breathed on, and may be ſcratched with difficulty by a knife. It yielded a perfect and very hard glaſs, which, in a regulated heat, produced

produced a uniform ſtony cryſtallite, greatly reſembling the original.

It has thus been ſhown, that all the whins employed aſſume, after fuſion, a ſtony character, in conſequence of ſlow cooling; and the ſucceſs of theſe experiments, with ſo many varieties, entitles us to aſcribe the ſame property to the whole claſs. The arguments, therefore, againſt the ſubterraneous fuſion of whinſtone, derived from its ſtony character, ſeem now to be fully refuted.

Experiments on Lava.

In the inveſtigation of Dr Hutton's ſyſtem, great advantage may be expected from an examination of lavas. They have undoubtedly flowed on the ſurface by means of heat; and whinſtone, according to his hypotheſis, having flowed in the bowels of the earth by the influence of the ſame agent, the two claſſes ought to poſſeſs many properties in common, by which the hiſtory of both may be illuſtrated.

I have been enabled to inſtitute a compariſon between them, by means of a cabinet of volcanic productions which I collected in 1785, in company with Dr J. Home of this Society, on Veſuvius, Ætna, and the Lipari Iſles. On this occaſion we were greatly aſſiſted by the celebrated M. Dolomieu *, who accompanied us in part of our expedition. This author complains, in his writings, that travellers, in collecting volcanic productions, have brought away only the ſuperficial ſcoria of lavas, which nearly reſemble each other in all caſes, and convey no

* Though I differ widely from this gentleman in many of his theoretical opinions, I cannot too ſtrongly expreſs my admiration of his merit as a natural hiſtorian. His deſcriptions of countries, as well as of minerals, preſent the moſt lively repreſentations to the mind of the reader, which, in the numerous inſtances I have witneſſed, are perfectly correct.

no idea of the real character of the lava, which can only be ſeen in the interior parts of the currents. In forming our collection we ſcrupulouſly avoided this error, and choſe ſuch ſpecimens only as were the moſt compact and free from the ſcorified appearance of the ſurface.

When theſe ſolid lavas are compared with our whinſtones, the reſemblance between the two claſſes is not only ſtriking at firſt ſight, but bears the cloſeſt examination. They both conſiſt of a ſtony baſis, which frequently contains detached cryſtals of various ſubſtances, ſuch as white felſpar and black hornblend. The analogy between the two claſſes ſeems to hold through all their varieties; and I am confident that there is not a lava of Mount Ætna to which a counterpart may not be produced from the whinſtones of Scotland.

This reſemblance in external character is accompanied with an agreement no leſs complete in chemical properties. But before I mention the experiments which tend to prove this agreement, it will be neceſſary firſt to examine the opinion of two very celebrated authors concerning lavas. M. Dolomieu and Mr Kirwan, though they differ widely in many reſpects, agree in believing, that lavas have never been acted upon by a heat of ſufficient intenſity to produce complete fuſion; and endeavour, each by an hypotheſis peculiar to himſelf, to account for their fluidity. The opinion of theſe gentlemen is of ſuch importance in the preſent queſtion, and the arguments they have uſed are ſo extraordinary, that I muſt beg leave to quote their words at full length.

M. Dolomieu ſtates his opinion in the following paſſage, *(Iſles Ponces, p. 7.)*: " Il eſt eſſentiel de conſtater, par beau-
" coup d'exemples et d'obſervations, quelques vérités que j'ai
" annoncées il y a pluſieurs années, ſavoir, que le feu des vol-
" cans ne dénature pas ordinairement les pierres qu'il a miſes
" en état de fuſion; qu'il ne les altere pas au point de ne pou-
" voir les reconnoître, de ne pas diſtinguer quelle a pu être la

" base des laves; que ce feu agit différemment que le feu de " nos fourneaux, tel que nous l'employons dans la chimie et " dans les arts; qu'il produit dans les laves une fluidité qui " n'a aucun rapport avec la fluidité vitreuse, que nous opérons, " lorsque nous traitons à grand feu les mêmes matières qui leur " servent de base, et lorsque nous voulons rendre aux laves elles " mêmes leur fluidité. Celui des volcans n'a point d'intensité; " il ne peut pas même vitrifier les substances les plus fusibles, " tels que les schorls, qui se trouvent comme parties consti- " tuantes dans l'intérieur des laves; il produit la fluidité par " une espece de dissolution, par une simple dilation, qui permet " aux parties de glisser les unes sur les autres, et peutêtre encore " par le concours d'une autre matière qui sert de véhicule à la " fluidité."

Mr Kirwan censures this supposition as strange and inconceivable; but in my opinion, that which he has brought forward is not less so. In the *Elements of Mineralogy, second edition, vol. I. p.* 396. he says: " Now, there are but three sorts of fu- " sion with which we are acquainted: that which produces por- " celain; that which produces enamels and semi-vitrifications; " and that which produces glass. By inspecting lava we shall " find that very little of it has been in any of these states; since " therefore it has flowed, it is plain it has derived its liquifac- " tion not from the fusion of its own materials, but from that " of some foreign substance mixed with it. This fact is so plain, " that it has even struck M. Dolomieu, in the midst of his " prepossessions, in favour of some strange inconceivable power, " which he attributes to volcanic fire, of melting earthy sub- " stances, without effecting an alteration in their sensible quali- " ties. " I hope," says he, " to prove, that lavas contain, in " their interior, a combustible matter, which burns and con- " sumes in the same manner as other inflammables," *Isles Pon-* " *ces*, 10. Yet he neglects telling us what this matter is; though " it plainly appears to be no other than sulphur and bitumen,

" of

" of which an immenſe quantity is found in all volcanos, " which liquifies in a low degree of heat, and cauſes all the " ſtony ſubſtances to flow that are immerſed in it."

The ſuppoſitions which theſe gentlemen have thus advanced, and have ſeriouſly maintained in various parts of their works, have ariſen in both from the belief, that, in our fires, nothing but glaſs can be produced from a lava after complete fuſion. This being taken for granted, it would certainly be very difficult to explain the phenomena of actual eruptions, by means of the known agents of nature. Recourſe has therefore been had, by one of theſe gentlemen, to a hypothetical modification of theſe agents; and by the other to the influence of ſubſtances, which have left behind them no trace of their exiſtence *, and which, had they been preſent, could not have produced the effects aſcribed to them.

According to both ſuppoſitions, the heat of volcanos is conceived to be of very little intenſity; but the few obſervations I had occaſion to make, which are confirmed by innumerable facts related by travellers, convince me that it muſt far exceed what is requiſite for the moſt perfect fuſion of the lavas, and of all the ſubſtances contained in them †; and the experiments already deſcribed ſuperſede the neceſſity of ſuppoſing any thing different from the common courſe of nature; for they afford, analogically, an eaſy ſolution of the difficulty, by ſhowing that glaſs is not the only reſult of fuſion, and that whin, a ſubſtance like lava, when cooled ſlowly after fuſion, reſumes

* None of the lavas I have ſeen contained the ſmalleſt veſtige of petroleum; nor did I meet with any ſulphur but what was evidently produced by the condenſation of vapours, riſing through crevices, long after the eruptions had ceaſed.

† I conceive, therefore, that the formation of the inſulated ſubſtances contained in lavas, as well as the other peculiarities of internal ſtructure, poſſeſſed by lavas in common with granite and baſaltes, muſt be aſcribed in all of them to cryſtallization during ſlow cooling after fuſion, as I ſtated formerly in Spring 1790, (*Tranſ. Edin. vol. III.*). The year following, Dr Beddoes preſented to the Royal Society of London a paper, in which he alſo explains the character of granite and baſaltes by cryſtallization, in conſequence of ſlow cooling.

resumes its stony character. But, not content with analogy alone, I resolved to ascertain the truth of these conclusions in a direct manner, and performed the following experiments with specimens of six different lavas, four of which, to my certain knowledge, had made part of external volcanic currents. In the present state of geology, too much pains cannot be bestowed in ascertaining that the specimens collected are really lavas, since this circumstance has been frequently overlooked, as I shall endeavour to shew, when I speak of the differences between them and whinstone.

No. 1. *Lava of Catania.*

This is the celebrated lava, which, in 1669, laid waste great part of the town of Catania. The interior part of the current, (accurately described by M. Dolomieu, *Isles Ponces, p.* 256.*), from which the subject of our experiment was taken, consists of a light grey basis, interspersed with crystals of felspar and of schorl, (augit). It bears a general resemblance to the rock of the basaltic columns on Arthur's Seat, and exhibited the same phenomena in our experiments. After strong heat, the whole was reduced, by rapid cooling, to pure black glass; but when the heat applied was moderate, the felspars remained unchanged. Being maintained, after a second fusion, in a temperature of 28, both these glasses yielded stony and crystallized substances, somewhat less fusible than the original; and when

* "Elle est formée d'une pâte de roche de corne grise, à grains fins, mêlée "d'écailles, et de cristaux de feld-spath de même couleur; elle contient un très "grand nombre de cristaux de schorl noir, et de grains de crysolites jaunes, les uns "et les autres quelquefois chatoyans, de différentes couleurs dans leurs fractures.— "——————Cette lave a une cassure seche, et un grain rude, surtout dans "le centre des courans; c'est là où elle a toujours conservé une couleur plus claire, "qui doit être celle de sa base; sur les bords es les surfaces elle s'est fort noircie; "elle y a acquis une assez forte action sur l'aiguille aimantée que celle du centre n'a "presque point."

when expofed to a temperature of 22, they cryftallized rapidly, like moft of the whins, into the liver cryftallite. This laft property is common to all the lavas.

No. 2. *Lava of Sta Venere.*

THIS current has flowed in the neighbourhood of a little chapel, called Sta Venere, above the village of Piedimonte, on the north fide of Mount Ætna. Owing to the ftrong refemblance which it bears to ftones fuppofed not volcanic, we took care that our fpecimens fhould be broken from the actual current; and to one of them, though moftly compact, is attached a fcorified mafs, which had made part of the external furface. The folid part is of a black, or rather dark blue, colour, very fine grained and homogeneous, having a multitude of minute and fhining facettes vifible in the fun; in this, and in other circumftances, it greatly refembles the rock of Edinburgh Caftle. This lava is the fecond in M. DOLOMIEU's *Catalogue*, and is well defcribed, p. 186 *.

THE pure black glafs formed from this lava yielded, in the regulated heat, the moft highly cryftallized mafs we have obtained from any lava or whin.

No. 3. *Lava of La Motta di Catania.*

THIS is likewife compact and homogeneous, but for a number of fmall yellow grains of chryfolite fcattered through it, (defcribed by M. DOLOMIEU, p. 191 †). It has been thrown up by a partial eruption burfting through the fandftone hills which

* "LAVE homogene noire : fon grain eft fin et ferré, il eft un peu brillant, comme "micacé lorfqu'on le préfente au foleil; fa caffure nette et feche eft conchéide "comme celle du filex."

† IT belongs to the fifth variety of his compact lavas.

which ſurround Mount Ætna. The ſituation of this maſs is ſingular: It reſts upon a little hill, formed of looſe ſcoria, the ſummit and ſides of which are covered by the ſtony maſs, ſo that no crater is viſible. It ſtruck me on ſeeing it, and I found M. Dolomieu had formed the ſame opinion, that the lava had riſen up in a perpendicular direction, and had flowed over on all ſides. Its great thickneſs, and ſmall extent, ſeem to favour a conjecture which this naturaliſt has formed with regard to ſeveral lavas, that they were erupted at the bottom of an ocean which once covered Sicily, and, being quickly cooled by the contact of water, had been prevented from flowing far. The conjecture ſeems plauſible enough *; and, having no proof that this ſubſtance made part of an external current, as I have with reſpect to the firſt two mentioned, I do not exhibit it as a lava with the ſame confidence. Whatever be its hiſtory, however, it poſſeſſes the chemical properties common to whin and lavas.

Its glaſs yielded a dark grey cryſtallite of uniform texture. Beſide it in the drawer, now on the table, I have placed a cryſtallite, formed from the whin No. 1. which reſembles it in every reſpect.

No. 4. *Lava of Iceland.*

I received the ſpecimen from a perſon who found it on the ſpot; but not being acquainted with the circumſtances of its original poſition, I cannot be certain that it is a lava. It has however every appearance of being ſuch.

It is a blue homogeneous ſubſtance, having ſome chryſolites ſcattered irregularly through it. Nearly half its bulk is occupied

* M. Dolomieu aſcribes the formation of part of Mount Ætna itſelf to a ſimilar cauſe. I ſhall have occaſion, in another part of this paper, to conſider that opinion.

pied by large air holes, which do not appear to have contained any extraneous matter.

It produced a very fufible glafs, from which was formed a cryftallite much more refractory than the original.

No. 5. *Lava of Torre del Greco.*

This lava, which flowed from Vefuvius to the fea in the middle ages, has been an object of much attention, on account of its confpicuous bafaltic form. It confifts of a grey bafis, the fracture of which is coarfe and rough, and in which are embedded large and well characterized cryftals of fchorl, (augit), with a few chryfolites, (olivins).

It was found to be lefs fufible than any of the others, yet its glafs cryftallized in a lower temperature.

No. 6. *Lava of Vefuvius, eruption* 1785.

From the circumftances in which the above five lavas have been feen to cryftallize after fufion, it can fcarcely be doubted that the fame procefs takes place in a volcanic ftream, which, in confequence of its bulk, muft cool with confiderable flownefs, and that a vitreous character would be affumed by the whole mafs, were it cooled with fufficient rapidity.

The truth of this laft opinion is demonftrated by fome facts which I accidentally obferved, long before my prefent views had occurred, when, in fpring 1785, I had an opportunity of examining a ftream of lava, which flowed from Vefuvius. The eruption was comparatively fo gentle, that I was able, though not without inconvenience, to approach and examine the fiery

ftream.

ſtream on three different days. It was in general concealed by a thin white ſmoke, which the wind blew aſide occaſionally, ſo that I could diſtinctly ſee the lava as it burſt from the ſide of the hill. It was then of a bright white heat, and flowed with the agility and rapidity of water, in all reſpects reſembling melted iron running from the furnace. The liquid, at its firſt emergence, manifeſted a ſtrong effervefcence, which ſubſiding as the heat abated, ſhewed itſelf at laſt only in the burſting of ſome very large bubbles, accompanied with a white ſmoke. Where I approached the ſtream, it was ſtill of a ſtrong red heat, and had the conſiſtence of honey. I thruſt a ſtick into it with eaſe, to the end of which ſome of the lava adhering, by its viſcidity, allowed itſelf to be drawn out into threads, and was found, when cold, to have a ſhining ſurface, and a vitreous fracture.

Being thus convinced that I had met with a lava of glaſs, I prepared ſome moulds of ſtucco, in which I meant to take caſts with that rare ſubſtance; and with this view returned to the mountain. I found the ſtream was not ſo liquid as at firſt, but I was able, by means of a ladle fixed on the end of a pole, to lift the ſpecimen now before us in a ſtate like dough. I then preſſed it with a ſeal, by which means, though too coarſe to receive an accurate impreſſion, it took the ſhape it now bears, which is that of the ladle. It is very porous, one-third of it nearly being occupied by air holes. It contains a great number of ſmall white cryſtals of Veſuvian garnet, embedded in a black ſubſtance, which completely reſembles the glaſs obtained in our experiments from lava by rapid cooling after fuſion. Beſides all their other properties, it poſſeſſes the fuſibility of the glaſſes, ſince it ſoftens completely at 18, that is, 14 or 15 degrees below the ſoftening point of any of the ſtony lavas. Being expoſed to the proceſs of regulated cooling, it gave the ſame reſult as all the other lava glaſſes. In the lower points

points it yielded a liver cryſtallite infuſible under 30, and in the higher a ſtony ſubſtance like a common lava or whin, and fuſible only at 35.

WHAT has been ſaid is applicable to the interior parts of lavas; but I was at a loſs to underſtand the ſtate of their external ſurface, which, cooling much more rapidly, might be expected to poſſeſs a vitreous character; yet glaſs is not found on the ſurface of lavas, except in a very few caſes, and has occurred only in a ſingle ſpot on Ætna. This difficulty was removed, however, by the following conſideration: Though the ſurface of a lava cools with far more rapidity than the reſt of the maſs, yet, owing to the contact of the fiery ſtream, that rapidity can never be very great; and we muſt ſuppoſe that the temperature of the ſurface employs more than a minute or two in deſcending from 23 to 21. Where this happens, we have ſhown that the ſubſtance conſolidates into the liver cryſtallite, which completely reſembles the ſcoria of a lava. A ſmall fragment of the maſs, which I took from the running ſtream, being placed in the temperature of 22, loſt its vitreous character in two minutes, as already ſtated; and had the maſs itſelf been allowed to remain but a very little longer in the ſtream, it would certainly have acquired, as well as the reſt of the ſurface, the dull character of ſcoria.

THE ſame property accounts for the cruſt which is formed on the ſurface of flowing lavas, and which conſtitutes ſo remarkable a feature in their hiſtory. Were lava to congeal after the manner of pitch or wax, by an uniform and gradual increaſe of viſcidity throughout, no cruſt would be formed, or if, by the action of cold air, the upper ſurface were to harden a little, it might be ſoftened again by an influx of freſh matter a very little hotter than itſelf. In lavas, however, as we have proved, when the ſurface cools down to 21, it rapidly congeals to a hard ſubſtance, capable of reſiſting any heat under 30. The

 cruſt

cruſt thus formed ſerves as a pipe, within which the flowing lava is confined. In ſeveral places on Ætna we meet with vaſt galleries, along which, and out of which, the lava has flowed, leaving the cruſt entire *.

The irregular manner in which a lava flows, when not extremely heated, may likewiſe be referred to the ſame cauſe. On the lower part of the running ſtream a cruſt is formed, ſo ſtrong as to retard its progreſs during a certain time, but the liquid behind, accumulating by degrees, at laſt acquires ſufficient ſtrength to force open the cruſt; the lava then flows out with rapidity, and continues its courſe till it is again retarded by the formation of a new cruſt.

These experiments ſeem to eſtabliſh, in a direct manner, what I had deduced, analogically, from the properties of whinſtone, namely, that the ſtony character of a lava is fully accounted for by ſlow cooling after the moſt perfect fuſion; and, conſequently, that no argument againſt the intenſity of volcanic fire can be founded upon that character. We are therefore juſtified in believing, as numberleſs facts indicate, that volcanic heat has often been of exceſſive intenſity.

In the compariſon inſtituted between whin and lava, the two claſſes are found to agree ſo exactly in all their properties which we have examined, as to lead to a belief of their abſolute identity. This identity has been fully eſtabliſhed by Dr Kennedy, who has performed an exact analyſis † of ſeveral of the very ſpecimens of whinſtone and lava mentioned in this paper; by which he diſcovered, that the elements of the two claſſes are the ſame: above all, that they both contain 4 or 5 *per cent.* of ſoda. Their agreement in this eſſential circumſtance ſeems to account for their common properties, whilſt the varieties of proportion, among their component elements, correſpond to the ſlight differences

* As at Malpertui above Piedimonte.

† An account of Dr Kennedy's analyſis is publiſhed in this volume.

ferences of refult we have obferved between the individuals of the fame clafs *.

So clofe a refemblance affords a very ftrong prefumption in favour of Dr HUTTON's fyftem, according to which both claffes are fuppofed to have flowed by the action of heat; but the circumftances under which they were expofed to this action being materially different, we have reafon to look for indications of that difference. Such are not wanting.

CALCAREOUS fpar frequently occurs in whinftone, either in veins or in detached nodules, but is never found in lava, and could not exift in a volcanic ftream; for heat, in fuch circumftances, would infallibly drive off the carbonic acid, and compel the lime to unite with the other component elements of the mafs. In whinftone, which Dr HUTTON fuppofes to have flowed, at fome remote period, in crevices of the earth, at a great depth below what was then its furface, the weight and ftrength of the fuperincumbent mafs of ftrata † has been fufficient to

refift

* THOUGH chemifts have hitherto overlooked, in their experiments, the mode in which bodies were cooled after being reduced to a ftate of fufion; yet many refults, which we are now entitled to afcribe to flow cooling, have been occafionally obferved. The flag of a furnace bears a ftrong refemblance to what we have called the liver cryftallite, and is probably formed in the fame manner. I have feen a mafs poffeffing, in a great meafure, the ftony character of whins and lavas, which was produced in a lime-kiln by the fufion of an impure limeftone; and Dr BEDDOES has obferved a cryftallized texture in the flags of fome iron furnaces. I am informed, that the celebrated Mr KLAPROTH has defcribed fome ftriking examples of cryftallization after fufion, which he obtained in expofing various fubftances to the heat of the porcelain furnace at Berlin.

† IT may be afked, what has become of this fuperincumbent mafs; and by what means it has been removed. Dr HUTTON anfwers, that it has been gradually worn away during an immenfe courfe of ages, by the action of thofe caufes which continue, under our eyes, to corrode the furface of the globe: That the folid parts, being conveyed to the bottom of the ocean, are there depofited in beds of fand and gravel, which, in fome future revolution, being expofed to heat, may be again converted into ftony ftrata.

THE

refift the expanfion of the carbonic acid, and to conftrain it, upon the principle of PAPINS digefter, to continue in combination with the lime. This compound feems to have entered readily into fufion, along with the whinftone, but to have kept feparate from it, as oil feparates from water through which it has been diffufed, thus giving rife to the fpherical form, which the nodules of calcareous fpar generally exhibit with more or lefs regularity *.

THIS circumftance accounts for an appearance which has miffled fome of the early obfervers of our minerals. Many whinftone

THE whole of this fyftem appears to me well founded, except in what regards the removal of the fuperincumbent mafs, which has been performed, I conceive, in a very different manner. I am inclined to agree on this point with M. PALLAS, M. DE SAUSSURE, and M. DOLOMIEU, and to believe that, at fome period very remote with refpect to our hiftories, though fubfequent to the induration of the mineral kingdom, the furface of the globe has been fwept by vaft torrents, flowing with great rapidity, and fo deep as to overtop the mountains; that thefe torrents, by removing and undermining the ftrata in fome places, and by forming in others immenfe depofits, have produced the broken and motley ftructure, which the loofe and external part of our globe every where exhibits.

IN the Alps and in Sicily I have witneffed feveral of thofe curious facts, upon which M. DE SAUSSURE and M. DOLOMIEU found their opinion, and which feem to juftify their conclufions. I have likewife obferved, in this country, many phenomena which denote the influence of fimilar agents. Lord DAAR, who joins me in agreeing with Dr HUTTON in almoft every article but this, has added great weight to the argument by fome general obfervations on lakes, and by fome very interefting facts which he has obferved in the Highlands of Scotland. We propofe to purfue this fubject, and to lay the refult of our inquiries before the Society. Dr HUTTON, in the fecond volume of his *Theory of the Earth*, has taken great pains to refute all that has been faid about thefe torrents; but, in my opinion, their exiftence is not only quite confiftent with his general views, but feems deducible from his fuppofitions, almoft as a neceffary confequence. When the ftrata, according to his fyftem, were elevated from the bottom of the fea, the removal of fo much water, if not performed with unaccountable flownefs, muft have produced torrents, in all directions, of exceffive magnitude, and fully adequate to the effects I have thus afcribed to them.

* THE modifications of the action of heat, occafioned by preffure, which have been taken into account by no geologift but Dr HUTTON, diftinguifh his theory from all other igneous theories.

ſtone rocks externally reſemble very porous lavas, but when broken are always found to be quite compact internally, and to contain numerous round nodules of calcareous ſpar. Near the ſurface, the nodules, being waſhed out by rain, have left the cavities which have given riſe to this deception. The ſpherical form of the air holes in lavas, and of the nodules of calcareous ſpar in whins, ſeems to have been produced by a cauſe common to both, the mutual repulſion of two fluids intermixed, but not diſpoſed to unite.

It muſt be owned, that this theory of calcareous ſpar is as yet hypothetical; but it is ſupported by ſtrong analogy, and promiſes to be of ſervice, by leading to deciſive experiments and obſervations. I cannot help believing, that, by a careful examination of the volcanic countries, facts may yet be diſcovered which will throw light on this ſubject. In order to promote and direct ſuch reſearches, I ſhall beg leave to ſtate ſome obſervations which I made in thoſe countries in 1785, before I was attached to any ſyſtem of geology.

It is generally ſuppoſed, that ſome lavas of Ætna contain calcareous ſpar and zeolite; but this I conceive to be a miſtake. It is true, as I have ſeen, that many rocks of Ætna contain theſe ſubſtances in abundance; but in my opinion theſe rocks are no lavas, but have flowed ſubterraneouſly like our whins, and are the ſame with them in every reſpect. A particular diſtrict of Ætna, comprehending the Cyclopian Iſles, and the country round La Trezza, and the Caſtle of Jaci, is decidedly of this deſcription; and veſtiges of the ſame kind occur in other parts of the mountain. In one place foſſile coal has been found, and in another we ſaw marine ſhells. In the neighbourhood of Bronte we obſerved a high ridge formed of ſtrata of ſandſtone and limeſtone, partly overflowed and concealed by recent lavas, but ſo placed as to render it evident that its continuation formed no inconſiderable part of the mountain. Thus, Ætna being compoſed, partly of the ſubterranean, and partly of the external productions

tions of fire, may be expected to afford numberless opportunities of pursuing the comparison between these two classes *.

A most interesting scene for such a comparison occurs likewise on Vesuvius. The history of this volcano is simpler than that of Ætna, for it has been evidently formed, with all its appendages, by the continued action of external eruptions, which have raised it, at some remote period, from the bottom of a sea, occupying all the Campi Phlegræi, and washing the surrounding Appenines. The whole volcano seems once to have consisted of a single large cone, the greatest part of which has sunk during some violent eruption, probably that which took place in the time of Pliny, leaving a fragment of its basis, now called the mountain of Somma. This fragment retains its original shape; and on the side fronting the towns of Somma and Otajano, the external conical surface, along which the ancient lavas had flowed, is still entire. Fronting the centre of the cone, Somma breaks off abruptly, and presents a vertical cragg, some hundred feet in height, which is concave inwards. From the gulf, produced by the ruin of the ancient mountain, though not exactly from its centre, have arisen the explosions which, by repeated accumulation, have formed the present cone of Vesuvius. Next the sea, this cone has extended itself so as completely to cover all remains of the ancient one, forming a continued slope from the crater to the foot of the mountain. On the opposite side it meets the base of the craggs of Somma, and forms an angle, into which many successive streams of lava have flowed, producing a narrow horizontal valley, in the form of a crescent, called the *Atrio del Cavallo*. From this valley the craggs of Somma present a complete view of the internal structure of the ancient mountain, corresponding, in most things, to what might have been supposed.

The various substances, deposited successively on the external surface of the ancient cone, being cut vertically in this cragg, their

* M. Dolomieu has observed this distinction; but supposes that the masses which we conceive to have flowed subterraneously were erupted at the bottom of the sea.

their fucceffion is diftinctly feen, the fection of each ftratum prefenting to the view part of a horizontal circle; the whole confifts of alternate layers of thin ftreams of lava, and very thick beds of loofe frothy *rapilli*, which laft being thrown into the air in a foft ftate, had fallen in fhowers on the fides of the mountain.

In various places the regularity of this arrangement is interrupted by certain vertical lavas, from two feet to ten or twelve in thicknefs, which crofs the ftrata juft defcribed in an irregular manner, and pafs upward, without diftinction, through the folid beds, and through the loofe ones. It immediately occurred to us *, that thefe lavas muft have flowed in fiffures of the ancient mountain; and we accounted for them by fuppofing, that a melted ftream, flowing along the external furface, had met in its courfe with one of thofe crevices which are formed in all great eruptions, and had flowed into it fo as to return again into the heart of the mountain. This conjecture very nearly agrees with thofe advanced by M. Dolomieu, and by M. Breislack, who both mention thefe vertical lavas of Somma †.

I have fince been induced to confider this phenomenon, which formerly feemed to prefent only an amufing variety in the hiftory of volcanic eruptions, as of the utmoft confequence in geology, by fupplying an intermediate link between the external and the fubterraneous productions of heat. I now think, that, though we judged rightly in believing thofe lavas

to

* I saw this place in company with Dr J. Home in 1785.

† M. Dolomieu conceives thefe lavas to have flowed over the lips of the crater, (*Ifles Ponces, p.* 100.); M. Breislack, that they had firft filled the open cavity of the crater, and from thence had flowed into crevices formed in its fides, "che "una lava avendo riempita la cavita del cratere fi foffe infinuata per quefte fendi-"ture," (*Topografia Fifica della Campania, p.* 115.). This laft mentioned work, publifhed in 1798, contains many interefting and accurate defcriptions. Should the circumftances of the times permit, the author will have it in his power to follow out, with every advantage, the hints I have fuggefted.

to have flowed in crevices, we were miftaken as to their direction; for inftead of flowing downwards, I am convinced they have flowed upwards, and that the crevices have performed the office of pipes, through which lateral explofions have found a vent. This will appear in the higheft degree probable, when we attend to the known hiftory of volcanic eruptions. It generally happens, that the lava begins to flow from the fummit, in confequence of the crater being filled with liquid matter up to the brim. At that moment the bafis of the mountain muft be preffed outwards by a very great hydroftatical force, equal to the weight of a column of liquid lava as high as the mountain itfelf. It is natural then to expect, that this preffure, affifted by ftrong percuffions of explofion, fhould lacerate the body of the hill, and form great rents. The lava, urged upwards by the fame preffure, would flow through thefe rents, and emerge at the furface with violence. The difcharge would continue through this channel till the propelling force had ceafed, when the rents would be left full of lava; which, cooling in that pofition, would produce vertical lavas, fuch as thofe of Somma. This fuppofition is confirmed by various phenomena: The lava ceafes to flow from the crater as foon as a lateral eruption has begun; when it rufhes with fuch violence from the fide of the mountain as to fly to a great height into the air, like a *jet d'eau;* and it often makes its appearance, in the fame inftant, at various mouths, which are not fcattered at random, but placed in one continued line, indicating the difcharge from a rent. Some circumftances likewife, which I obferved on a clofe examination of the vertical lavas, indicate that the crevices had performed the office of pipes. Frequently the fubftance at the middle differs from that on each fide, whilft the fides refemble each other exactly. I explain this, by fuppofing that the lava, which had firft flowed through the pipe, and had coated its fides with folid matter, had been followed

lowed by a ſtream ſomewhat different, which had remained there on the cooling of the whole. In one caſe, I found lava on each ſide, and in the middle tuffa, which is generally ſuppoſed to have been erupted in the ſtate of watery mud. In another, the ſubſtance in immediate contact with the mountain is vitreous, the reſt being common lava. This is fully explained by our experiments, if we ſuppoſe the ſtream to have flowed into a cold crevice.

To apply theſe obſervations to the general hiſtory of the globe: It is evident that the vertical lavas bear the cloſeſt reſemblance, in point of poſition, to veins of every deſcription, which, in all parts of the world, are found penetrating the ſtrata, and which, according to Dr HUTTON, have flowed by means of ſubterraneous heat. The veins, or dikes, (as they are called), of whinſtone, which ſo commonly occur in this country, differ from them in no circumſtance which I had the means of obſerving. It is therefore natural to expect that, if examined with particular care, their agreement will be found complete. Of this, however, we muſt not form too ſanguine expectations; for though the vertical lavas of Somma have undoubtedly ſuſtained the preſſure of a great ſuperincumbent maſs, we have no proof that this force was ſufficiently ſtrong, as Dr HUTTON ſuppoſes was the caſe in whinſtone, to repreſs the volatility of carbonic acid. On the other hand, as we are yet entirely ignorant of the degree of force requiſite for this purpoſe, we have no proof that it has been too weak. All the veins of Somma I examined were abſolutely compact, except one which was full of pores. I am unable to determine whether this was the real poroſity of a lava, or whether, as in our whins, it aroſe from the removal, near the ſurface, of nodules of calcareous ſpar. Granting that theſe pores were real air holes, the circumſtance was peculiar to that ſingle ſtream, and may have been owing to an inferior degree of preſſure; for, in this reſpect, we have reaſon to look for the greateſt diverſity. Some vertical ſtreams muſt have flowed while the mountain was yet low; others may have found vent at a low level; in both which caſes, the preſſure would be feeble: whereas other ſtreams, communicating with elevated lateral eruptions, would ſuſtain, in their lower parts, the full reaction of deep columns of liquid lava, and may be expected to exhibit the effects of great preſſure. Should any future traveller be fortunate enough to meet with a nodule of calcareous ſpar in a lava, occupying the crevice of a mountain formed by undoubted external eruptions, all that has been ſaid of the effects of preſſure would ceaſe to be hypothetical, and this fundamental article of Dr HUTTON's theory would be eſtabliſhed beyond diſpute.

I HAVE now examined the relation between whinſtone and lava in various points of view; and the reſult of the inveſtigation, by ſhowing the intimate connection between the two claſſes, tends ſtrongly to confirm the ideas of Dr HUTTON. I flatter myſelf, likewiſe, that the experiments, independently of the general views of geology, are of ſome value, by accounting for the ſtony character of lavas, and thus enabling us to diſpenſe with the various myſtical ſuppoſitions which have of late perplexed the hiſtory of volcanic phenomena.

TABLE

TABLE OF FUSIBILITIES.

	Subſtances.	*Original ſoftened.*	*Glaſs ſoftened.*	*Cryſtallite ſoftened.*
No. 1.	Whin of Bell's Mills Quarry.	40.	15.	32.
2.	Whin of Caſtle Rock.	45.	22.	35.
3.	Whin of Baſaltic Column, Arthur's Seat.	55.	18.	35.
4.	Whin near Duddingſtone Loch.	43.	24.	38.
5.	Whin of Saliſbury Craig.	55.	24.	38.
6.	Whin from the Water of Leith.	55.	16.	37.
7.	Whin of Staffa.	38.	$14\frac{1}{2}$.	35.
No. 1.	Lava of Catania.	33.	18.	38.
2.	Lava of Sta Venere, Piedimonte.	32.	18.	36.
3.	Lava of La Motta.	36.	18.	36.
4.	Lava of Iceland.	35.	15.	43.
5.	Lava of Torre del Greco.	40.	18.	28.
6.	Lava of Veſuvius, 1785.	18.	18.	35.

IV. *A* Chemical Analysis *of* Three Species *of* Whinstone, *and* Two *of* Lava. *By Robert Kennedy,* M. D. F. R. S. & F. A. S. Edin.

[*Read, December* 3. 1798.]

ON the 5th of Auguſt laſt, I announced to the Society that I had diſcovered ſoda in ſeveral varieties of the whinſtone * of Scotland, and alſo in lava from Mount Ætna; but did not deſcribe the various experiments to which theſe ſubſtances had been ſubjected in my examination of them. In the following paper, therefore, I have the honour of laying an account of theſe experiments before the Society.

ANALYSIS I.

Baſalt of Staffa.

The ſpecimen of this baſalt, ſubmitted to the following analyſis, was given me by a gentleman, who brought it himſelf from the celebrated baſaltic columns in Staffa. A deſcription of its external mineralogical characters may be found in Sir James Hall's paper, (p. 55. of this volume), to which I beg leave to refer.

This

* The name whinſtone is uſed throughout this paper in a generic ſenſe, comprehending baſalt, trap, certain kinds of porphyry, wacken, and ſome other ſtones of the argillaceous claſs.

This basalt, though reduced to fine powder, does not effervesce with acids. The colour of the powder is greyish, and when wet greenish. By being exposed to a low red heat, the colour of the stone is changed to brown. It is not attracted by the magnet, either in its natural state, or after being heated red hot.

Its specific gravity, taken in distilled water at the temperature of 60°. of Fahrenheit, I found to be 2.872.

Some small pieces being exposed to a low red heat for half an hour, lost 5 *per cent.* in weight; and when the stone was reduced to powder, and heated red hot, the loss was the same. I also examined the effects of high heat on it, in the following manner. Having made some small crucibles of the porcelain clay of Cornwall, which I used on account of its great purity and infusibility, I baked them in pretty strong fires, generally above 100 of Wedgwood. As soon as they were cold, they were each exactly weighed. A portion of the basalt in fragments, also weighed, being put into one of these small crucibles, and a pyrometer into another of the same size, both were placed in a Hessian crucible. A small flat cover, also made of the porcelain clay, was laid upon each; and then a lid was carefully luted on the Hessian crucible with clay and sand. The apparatus thus prepared was next set into a furnace; and the fire being raised gradually till it appeared to have attained the pitch desired, it was kept as equal as possible for about an hour. The small crucible, and the melted basalt it contained, being weighed as soon as cold, it was easy to determine how much weight was lost.

In this manner some of the basalt was exposed to a heat of 72 of Wedgwood, at which it was vitrified, and lost exactly the same weight as in a low red heat. At 160 the effects were in every respect the same; the loss not being greater in that intense fire. The small crucibles, in which the pyrometers had been

been placed, did not in these experiments lose the smallest weight.

The volatile matter thus driven off by heat is partly water, as the following experiment shows: I put half a pound Troy of the basalt in fragments, into a small Wedgwood retort, and luted to it a receiver, into an aperture of which was fitted one end of a glass tube, the other end being adapted to a pneumatic apparatus. The retort was then heated slowly to redness, and kept moderately red hot for two hours. In the receiver some water was condensed. Some gas also passed over; but I could not ascertain with precision either its quantity or its nature, as it was mixed with the air of the receiver. I have not made farther experiments on the volatile matter contained in whins; but it deserves to be examined with attention.

This basalt being exposed to heat in a muffle, was found to soften at 38 of Wedgwood *.

Some of it being reduced to fine powder, was boiled in thirty times its weight of water for half an hour. After filtration the water was examined with different chemical tests, but gave no precipitate with any, except a slight cloud with nitrate of silver; and a portion being evaporated to dryness, left only some thin streaks on the bottom of the glass.

Having premised these particulars, I proceed to describe the analysis.

1. One hundred grains of the basalt, reduced to fine powder in a Wedgwood mortar †, were mixed, in a small retort, with about

* The fusibility of this, and the other substances to be afterwards mentioned, I examined with Sir James Hall. For this purpose, a small piece of each was placed, with a pyrometer as near to it as possible, in an open muffle previously heated to redness. It could thus be seen perfectly during the operation, and the fire being raised, as soon as it was found to be soft, when pressed slightly by an iron rod, the degree of heat was ascertained by measuring the pyrometer.

† The mortar I used was not scratched by any of the whins or lavas mentioned in this paper.

about 1200 grains of muriatic acid; and a receiver being adapted, the mixture was gradually heated till it boiled. It was at firſt of a browniſh-yellow colour, but afterwards became browniſh. Part of the powder was diſſolved. To diſtill off the uncombined acid, the heat was continued till the mixture began to grow thick. It was then diluted with boiling diſtilled water, and poured on a filter; and the undiſſolved part, after proper edulcoration, being dried and heated red hot a few minutes, weighed 67½ grains, and was greyiſh-white.

2. THE filtered ſolution was of a faint yellowiſh-brown colour. Being ſaturated with cauſtic ammonia, a bulky precipitate was thrown down, which was carefully ſeparated by filtration. It had at firſt a dirty greeniſh-colour, which was afterwards changed to brown by the action of the air.

3. THE ſolution, after being freed from this precipitate, was perfectly colourleſs and tranſparent. I dropt into it a ſmall quantity of ſulphuric acid, which produced no cloud; conſequently the ſolution contained neither barytes nor ſtrontian. It was then evaporated to a ſmall quantity, and treated with carbonate of ammonia. Some white earth was thrown down, apparently carbonate of lime, which after being waſhed, dried, and heated red hot a few minutes, weighed 6½ grains.

4. THE inſoluble reſiduum, No. 1. which weighed 67½ grains, I mixed in a ſilver crucible with a ſolution of cauſtic potaſh, containing as much alkali as was equal to twice the weight of the reſiduum. This mixture, being evaporated to dryneſs, was expoſed for one hour to a red heat, in which it melted. When cold, the maſs was green. After being ſoftened, and waſhed out of the crucible with boiling diſtilled water, it was ſuperſaturated with muriatic acid, by which the greater part was diſſolved. This mixture, being then evaporated to a ſmall quantity, became gelatinous. It was next diluted with water, digeſted, and filtered. Some ſilex remained on the filter, which, after

proper

proper washing, being dried, and heated red hot a quarter of an hour, weighed 43 grains, and was perfectly white. To learn whether this silex was free from every other earth, I mixed a part of it with four parts of carbonate of soda, and melted the mixture in a silver crucible. Water, being poured on the melted mass, dissolved it entirely into a liquor silicum, which was diluted largely, and saturated exactly with an acid. No precipitate appeared, even after six or eight days; therefore these 43 grains were pure silex.

5. The solution, No. 4. (from which the silex had been separated), was of a light greenish colour. Caustic ammonia, when poured into it, threw down a brownish precipitate. Having carefully separated this precipitate, and washed it on a filter, I dropt into the remaining solution, which was now colourless and transparent, a small quantity of sulphuric acid, in order to detect barytes or strontian. No precipitate was formed. The solution was then evaporated to a small quantity, and treated with carbonate of ammonia, by which a second portion of carbonate of lime was obtained. Its weight, after being heated red hot, was $9\frac{1}{2}$ grains.

6. The brownish precipitates, thrown down from the solutions No. 2. and 5. by caustic ammonia, had the appearance of argil mixed with iron. To separate the argil, these precipitates were mixed together, and boiled, while still moist, in a solution of caustic potash, in a silver crucible. A part was dissolved; but a spongy matter remained, of a darker brown colour than at first, which was collected on a filter.

7. Into the caustic-alkaline solution I poured sulphuric acid, till slightly in excess, and neutralised it again by carbonate of soda. The argil was precipitated; which being sufficiently washed, was redissolved in diluted sulphuric acid. This solution was then mixed with some acetite of potash, and gave, by successive evaporations, small regular crystals of alum. At last

it

it became gelatinous; and being evaporated to drynefs, and diluted again with water, 1 grain of filex was left. The remaining folution produced, to the laft drop, cryftals of alum*. I diffolved thefe cryftals in water, and precipitated the argil by carbonate of ammonia. After being carefully wafhed, dried, and heated red hot a quarter of an hour, it weighed 12 grains.

8. THE brownifh matter, No. 6. infoluble in cauftic potafh, feemed to be oxyd of iron; and after having been heated red hot, weighed $24\frac{1}{4}$ grains. I powdered this mafs, and poured on it fome acetous acid, in order to detect magnefia; but nothing was diffolved. It was next treated with nitric acid, which diffolved the iron, but left 4 grains of filex. The iron being precipitated, dried, and heated red hot, weighed 20 grains, and was magnetic. Sufpecting that fome argil might ftill be mixed with it, from having efcaped the action of the cauftic potafh, I diffolved 5 grains in muriatic acid, and precipitated the iron by Pruffian alkali. Having feparated the blue precipitate, I boiled the folution with carbonate of foda, and obtained 1 grain of argil. Thefe 20 grains confifted, therefore, of 16 grains of iron, and 4 grains of argil.

THE remaining part of the iron was melted for an hour, with ten times its weight of nitre, in order to detect manganefe. The mixture, however, when cold, was not greenifh; and water made a colourlefs folution of the faline matter, which did not become turbid, when expofed fome days to the action of the air †.

* IT therefore contained none of the earth which VAUQUELIN lately difcovered, and to which he has given the name of glucine.

† I THINK, however, it is probable, that this bafalt contained a fmall quantity of manganefe, both from the brownifh colour of the folution, No. 1. and from the green colour which the undiffolved refiduum gave, by fufion, with cauftic potafh.

9. The two portions of white earth above mentioned, which feemed to be carbonate of lime, weighed together 16 grains. To feparate magnefia, if any were mixed with this earth, I put it into a little water, and added fulphuric acid till flightly in excefs. Sulphate of lime was produced. Having poured fome alkohol into this mixture, I filtered it, and wafhed the fulphate of lime with more alkohol diluted with water. The filtered liquor was then boiled with carbonate of foda, but no magnefia was precipitated.

These 16 grains were therefore carbonate of lime, of which, according to Mr Klaproth's calculation, about 9 grains were pure lime.

One hundred parts of the bafalt of Staffa contain, according to the above analyfis:

Silex, -	(No. 4. 7. and 8.). -	48
Argil, -	(No. 7. and 8.). -	16
Oxyd of iron, -	(No. 8.). - -	16
Lime, -	(No. 9.). - -	9
Moifture, and other vol. matter, - -		5
		94

The fum is 94 parts; confequently there is a lofs of 6 *per cent.*

About a year ago I analyzed fpecimens of fome of the whins in the neighbourhood of Edinburgh, and found, that the fum of the earths and iron, feparated by the analyfes, never amounted to more than 93 or 94 *per cent.*; fo that the lofs was always equal to that juft mentioned. It was this circumftance which firft led me to fufpect that fome faline fubftance exifted in in theſe ſtones; and their conſiderable fuſibility favoured the ſuſpicion.

SOON after theſe analyſes were made, I obſerved another circumſtance, which amounted to an abſolute proof of the whins containing ſomething of a ſaline nature, in combination with their earthy baſes. Moſt of the artificial cryſtallites, made by Sir JAMES HALL, which I had always an opportunity of examining, threw out on their ſurfaces, two or three weeks after their formation, a white efflor eſcence, which had a very ſalt taſte. It was in too ſmall quantity to be collected and examined; but when waſhed off, it was often formed a ſecond time.

I WAS thus convinced of the exiſtence of ſome ſaline ſubſtance in theſe bodies, and made different experiments with ſeveral of them, in order to ſeparate it, and aſcertain its nature; and ſoon found that it was ſoda.

I SHALL next deſcribe ſome of the methods by which this alkali was moſt eaſily ſeparated from the earthy parts of the whins.

Experiments to obtain the Soda, and determine its Quantity.

HAVING broken ſome of the baſalt of Staffa to ſmall fragments, I weighed 400 grains, and ground the whole with water to an extremely fine powder, in a WEDGWOOD mortar. The powder, and the water with which it had been ground, were then put into a ſmall retort, and mixed with about 1200 grains of ſulphuric acid, which I had carefully diſtilled for this operation. I placed the retort in the ſand bath of a ſmall furnace which I uſe for analyſes, adapted a receiver, raiſed the fire till the acid began to diſtill ſlowly, and carried on the diſtillation to dryneſs. Water was then poured into the retort, and boiled, the mixture thrown on a filter, and the undiſſolved reſiduum ſufficiently waſhed. This reſiduum was next treated a ſecond time

time with a fresh portion of sulphuric acid; and afterwards boiled with water, filtered and washed, exactly in the same manner as before. The undissolved part of the stone was now almost white.

The filtered solutions being mixed together, were evaporated to dryness; and the saline mass which remained was heated red hot for one hour in a clean and new Hessian crucible. When cold the mass was of a brick red colour. Having powdered it well, I boiled it in water, poured the whole on a filter, and washed the reddish matter carefully. This filtered liquor, in which all the soda, separated from the basalt by the sulphuric acid, was dissolved in the state of sulphate of soda, could contain only a small quantity of earthy matter; for the greater part of the sulphate of argil, and of iron, formed by the first part of the process, must have been decomposed by the red heat, to which the mass was afterwards exposed. Accordingly, the solution being treated with carbonate of ammonia, only a small quantity of a precipitate was thrown down, which was carefully separated. The solution was then evaporated to dryness. A saline mass remained, consisting, in part, of sulphate of ammonia; to separate which the whole was exposed to heat in a small crucible, and when it ceased to emit fumes, the heat was increased to redness. A fixed white salt was left, which weighed 25 grains.

This salt I redissolved in water, added some carbonate of ammonia, and heated the mixture till it boiled. A small quantity of an earthy precipitate was again thrown down, which being separated by filtration, the solution was evaporated to dryness, and the salt which remained heated red hot a second time. By these successive operations, all the earthy matter, at first dissolved, was separated. The salt now weighed 23 grains, andhad all the properties of sulphate of soda. These properties were the following:

1. It

1. It was not volatile in a moderate red heat.

2. After being thus dried, it diſſolved readily in about ſix times its weight of water, at the temperature of 60 of Fahrenheit.

3. This ſolution gave, by evaporation, cryſtals exactly the ſame in form as artificial ſulphate of ſoda; and theſe cryſtals effloreſced in dry air.

4. A part of the ſolution of this ſalt being boiled with carbonate of ſoda, gave no precipitate; a proof that it contained no earthy matter.

5. Some of the ſolution being mixed with a very ſtrong ſolution of acid of tartar, remained unaffected; the ſalt therefore contained no potaſh.

6. Some of the ſalt being diſſolved in water, was decompoſed by nitrate of barytes; and the ſulphate of barytes produced was ſeparated by filtration. The nitric acid, thus united to the alkaline baſis, formed a ſaline compound, which, in the next place, was mixed and deflagrated with charcoal. By waſhing the coaly reſiduum, and evaporating the water, I obtained pure carbonate of ſoda, which effloreſced readily in the air.

There can be no ſuſpicion of the retort which was uſed furniſhing any part of the alkali; for I weighed it previouſly in a balance of great accuracy; and after the operation was finiſhed, found its weight exactly the ſame as at firſt, and the luſtre of the glaſs altogether unimpaired.

The whin which I next ſubmitted to examination, for the purpoſe of ſeparating the ſoda, was taken from a quarry near the Water of Leith *. I uſed a conſiderable quantity, 800 grains; which were diſtilled twice with ſulphuric acid, and then treated in every reſpect exactly as the preceding. The ſulphate of ſoda obtained, amounted to 43 grains.

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* This ſpecies is the firſt mentioned in Sir James Hall's paper. When powdered, it effervefced ſlightly with acids. I did not analyze it; but, in the courſe of the proceſs for detecting ſoda, one of the earthy precipates proved, upon examination, to be magneſia. It is the only whin in which I have found this earth.

I AFTERWARDS ſubjected ſome other whinſtones to the ſame kind of proceſſes, and in each ſpecies found ſoda. The nitric and muriatic acids alſo diſſolve a certain quantity of the alkali contained in theſe ſubſtances; but their action is weaker than that of the ſulphuric acid.

BY the experiments now deſcribed, there were ſeparated from 100 parts of each of the whins, between five and ſix parts of ſulphate of ſoda, which may be equal to two or three parts of pure ſoda. But as theſe two or three parts, when added to the ſum of the earths and iron, did not account for the loſs of 6 or 7 *per cent.* always obſerved in my analyſes, I was ſatisfied that the whole of the alkali was not obtained by the proceſſes which were followed; even although, in that with the whin from the Water of Leith, it had been expoſed, in very fine powder, to the action of the ſulphuric acid, at a boiling heat, for more than eighteen hours. It appeared neceſſary, therefore, to try other methods; and after ſome conſideration it occurred to me, that if the powdered whins could be expoſed, while red hot, to the vapours of the ſulphuric acid, alſo in a red hot ſtate, its power in ſeparating the whole of the alkali from the earthy baſes of theſe ſubſtances, would probably be greatly increaſed in ſo high a temperature. I ſucceeded in applying a red heat both to the powdered ſtone, and to the acid at the ſame time, by the following means.

SOME of the baſalt of Staffa being mixed, in very fine powder, with three parts of ſulphuric acid, the mixture was evaporated ſlowly to dryneſs in a ſand bath. The dry maſs was then heated gradually to redneſs, and kept in the fire for one hour. It was next powdered, and boiled in water; and the water being filtered, was treated with carbonate of ammonia, which threw down a ſmall quantity of a browniſh precipitate. After ſeparating this precipitate by filtration, the liquor was evaporated to dryneſs, and the ſulphate of ſoda, which was left, was purified

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in the manner already defcribed, and heated red hot. It amounted to 9 parts for every 100 parts of the bafalt employed.

In this experiment, the fulphuric acid was firft united to a part of the lime, of the argil, and of the iron, contained in the ftone; and afterwards, when the mafs was expofed to a red heat, the acid was driven off party or wholly from thefe, and applied in red hot vapours to every part of the powder; by which its action appears to have been rendered much more powerful, as 9 *per cent.* of fulphate of foda was produced: and by the fame procefs, fo fimple and eafy to execute, I got from the reft of the fubftances, to be mentioned in this paper, from 8 to 11 *per cent.* of fulphate of foda, although, when the were merely boiled in the acid, the quantity of this falt never exceeded 5 or 6 *per cent.*

As the proportion of acid and alkali in neutral falts has not been hitherto determined with certainty, the quantity of foda in thefe whins cannot be exactly known. But it is probable that 9 parts of fulphate of foda, dried by a red heat, do not contain lefs than $3\frac{1}{2}$ or 4 parts of pure alkali *; which muft therefore be confidered as the weight in 100 parts of the bafalt of Staffa: and as $3\frac{1}{2}$ or 4 parts of foda, when added to the fum of the earths and iron, amount nearly to the 100 parts of the ftone employed in the analyfis, this calculation may be reckoned very near the truth. For the fame reafons I think it likely, that the greater part, or the whole of the foda, was obtained from the bafalt by the procefs which has been laft defcribed.

It is well known among the friends of the late Dr Hutton, that he made fome experiments on zeolite; by which he concluded, that foda entered into the compofition of that fubftance.

* This is nearly the proportion given by Mr Kirwan.

ſtance *. He has not mentioned the circumſtance in any of his works; but Dr BLACK has been accuſtomed, as he informed me himſelf, to take notice of it in his lectures on chemiſtry, for many years. It is my intention to analyze ſome ſpecies of zeolite; and if the reſults ſeem of any importance, they ſhall be laid before the Society.

AMONG the experiments on the baſalt of Staffa, already deſcribed, it has been obſerved, that, when the powder was boiled in water, a ſlight precipitate was produced in the water by nitrate of ſilver, thus indicating ſome traces of muriatic acid. As it appeared of importance to determine how much of this acid the baſalt contained, I ſubjected ſome of it to examination for that purpoſe.

Experiments to aſcertain the Quantity of Muriatic Acid in the Baſalt of Staffa.

ONE hundred grains of the ſtone, in fine powder, were mixed in a ſmall retort with ſome nitric acid; and a receiver being adapted, the mixture was boiled gently, till the greater part of the acid had diſtilled over. The liquor in the receiver being examined with nitrate of barytes, remained unaffected; but gave a ſlight cloud with nitrate of ſilver, which ſhewed that it contained ſome muriatic acid.

THE maſs in the retort being diluted with water, the whole was filtered; and this filtered liquor produced no cloud with nitrate of barytes, but gave, like the former, a ſlight precipitate with nitrate of ſilver.

IN

* IN the 26th volume of the *Annales de Chimie*, *p.* 119. M. SCHERER, in a letter to VAN MONS, ſays, that he was informed by Dr BLACK, that Dr HUTTON had, long ago, found *potaſh* in zeolite. In this ſtatement M. SCHERER is incorrect; becauſe it was ſoda, as above mentioned, which Dr HUTTON obtained from that ſubſtance.

In the next place, the undiffolved refiduum was mixed with twice its weight of very pure cauftic potafh, and expofed to a low red heat, for an hour, in a filver crucible. The mafs was then diluted with water, fuperfaturated flightly with nitric acid, and filtered. With this folution nitrate of barytes produced no effect; confequently, thefe experiments fhow, that the ftone in queftion does not contain any traces of fulphuric acid. With nitrate of filver, however, the folution gave a white precipitate, more abundant than the two preceding. The different portions of muriate of filver, being collected, and dried on a fand bath, weighed only 4 grains.

As a fourth part of muriate of filver confifts of acid, according to the moft correct experiments hitherto publifhed, thefe 4 grains confequently indicate, in 100 parts of this bafalt, only about one of muriatic acid. All the whins and lavas, to be mentioned in the remaining part of this paper, were found, by a fimilar procefs, to contain about the fame quantity.

According to the refults of thefe different proceffes, there are, in 100 parts of the bafalt of Staffa,

Silex, - - -	48
Argil, - - -	16
Oxyd of iron, - - -	16
Lime, - - - -	9
Moifture, and other vol. matter, -	5
Soda, about - - - -	4
Muriatic acid, about - - -	1
	99

I have thus detailed exactly the various experiments performed in analyzing this fpecies. As all the other whins, and the lavas, which follow, were analyzed in the fame manner, and exhibited nearly the ſame chemical properties, the reſults only, in each example, ſhall be mentioned.

ANALYSIS II.

Whin of Saliſbury Rock.

THE ſpecimen employed was choſen from the ſouth ſide of the cragg, and was perfectly hard and free from decompoſition, as the particular ſpot from which I broke it had been quarried a ſhort time before.

A DESCRIPTION of its external characters may be found in p. 54. of this volume. Its powder is light greeniſh-grey; but after being wet, acquires a dirty green colour. When heated to redneſs, it becomes light brown. Though not attracted by the magnet in its natural ſtate, it becomes magnetic after being heated red hot. It does not efferveſce with acids. When expoſed to a low red heat for half an hour, whether in fragments or in powder, it loſes 4 *per cent.* in weight. It ſoftens at 55 of WEDGWOOD. Its ſpecific gravity is 2.802. After being boiled, in the ſtate of fine powder, in ten or twelve parts of muriatic acid, the inſoluble reſiduum amounted to 65 *per cent.*

I ANALYZED 200 grains of this whin, and found that it contained, in 100 parts,

Silex, - - - -	46
Argil, - - -	19
Oxyd of iron, - -	17
Lime, - - - -	8
Moiſture, and other vol. matter, -	4
Soda, about - - -	3.5
Muriatic acid, about - -	1
	98.5

ANALYSIS

ANALYSIS III.

Whin of the Calton Hill, near Edinburgh.

The rock of this hill varies much in different parts; but its general character is that of porphyry. The piece I chose for analysis was taken from the south side, about ten or twelve feet below its highest point; and was free from calcareous spar.

The external characters of this particular piece are as follow: It consists of a greyish basis, containing rhomboidal crystals of felspar of a light reddish-brown colour; and small spherical masses of green earth. The basis, in its fracture, is uneven, and earthy, and has no lustre. It can be scratched easily with a knife, and gives an earthy smell when breathed on. The green earth is soft, and is affected in some degree by water; and being decomposed by the weather, as well as the veins and nodules of calcareous spar, which are very common in this hill, the rock in many places is extremely porous.

The specimen I have described may be called argillaceous porphyry. It effervesces slightly with acids; so that the lime which it contains must be united to carbonic acid. Its powder is light grey, with a certain shade of purple. When heated to redness, it becomes of a brown colour. It is not attracted by the magnet, either in its natural state, or after ignition. By being exposed to a low red heat for half an hour, it loses 5 *per cent.* of its weight. It softens at 44 of Wedgwood. Its specific gravity is 2.663, as nearly as I could ascertain, from the effect which the water had in making it crumble down.

One hundred parts contain,

Silex, - - - -	50
Argil, - - - -	18.50
Oxyd of iron, - - -	16.75
Carbonate of lime, - - -	3
Moisture, and other vol. matter, -	5
Soda, about - - -	4
Muriatic acid, about - -	1
	98.25

ANALYSIS IV.

Lava of Catania. Ætna.

This lava, and the species to be next mentioned, were brought from Mount Ætna by Sir James Hall and Dr James Home. At their request I analyzed specimens of each. Mineralogists are well acquainted with these lavas, from the descriptions which have been given of them by M. Dolomieu; therefore it is unnecessary for me to mention their external characters.

The lava of Catania gives a powder of a light grey colour, which is very little changed in appearance by being heated red hot. After it is wet, it becomes dark grey. When this lava is reduced to small fragments, some parts of it are attracted by the magnet, and others are not. In fine powder it is but feebly attracted; and after ignition its qualities in this respect do not seem to be altered. It softens at 33 of Wedgwood. The specific gravity of the pieces most free from air bubbles, is 2.795.

I have exposed this lava, in the manner already described, to various degrees of heat, from redness to 158 of Wedgwood, and constantly observed that it never lost the smallest weight.

When

When boiled like the whins in muriatic acid, the part remaining undiſſolved weighed 68 *per cent.*

There are in 100 parts,

Silex, - - - - -	51
Argil, - - - -	19
Oxyd of iron, - - -	14.50
Lime, - - - -	9.50
Soda, about - - -	4
Muriatic acid, about - -	1
	99.

ANALYSIS V.

Lava Sta Venere, Piedimonte, Ætna *.

This ſpecies gives, like the preceding, a greyiſh powder, which becomes dark grey when wet, and its colour is ſcarcely affected by being expoſed to a low red heat. It is but feebly attracted by the magnet, whether in ſmall fragments or in powder; and its qualities in this reſpect are not changed by low ignition.

It ſoftens at 32 of Wedgwood, and does not loſe any weight in fires between 150 and 160. After being boiled in muriatic acid, it left 68 parts *per cent.* undiſſolved. Its ſpecific gravity is 2.823.

In

* Dolomieu, in deſcribing this lava, ſays, that its fracture is conchoidal, like that of ſilex. The ſpecimen which I analyzed had an uneven fracture; and its colour was blackiſh-blue: in other reſpects, however, it anſwered to Dolomieu's deſcription.

In 100 parts I found,

Silex, - - - -	50.75
Argil, - - - -	17.50
Oxyd of iron, - - -	14.25
Lime, - - - -	10
Soda, about - - -	4
Muriatic acid, about - -	1
	97.50

In analyzing theſe two lavas, I examined the different ſolutions, with particular attention, for magneſia, and ſulphuric acid; but could not detect any traces of either of theſe ſubſtances.

The reſults of theſe analyſes ſhow, that whins, and a certain claſs of lavas, taken from remote quarters of the globe, conſiſt of the ſame component elements, united in each, nearly in the ſame proportion. The only circumſtance in which they materially differ, is the loſs of ſome volatile matter in the fire, which is peculiar to the whins alone.

We need not be now ſurpriſed at the facts mentioned by Dolomieu, and others, of ſoda being found about volcanos, or upon the ſurface of lavas; as it has thus been ſhown to exiſt in theſe ſubſtances in combination with their earthy baſes.

The facts and experiments I am next to mention will prove, that whins and lavas are not the only ſtones which contain ſoda;

da; and will even render it probable, that this alkali is widely diffufed through mineral bodies.

Soon after I firft difcovered it in whins, and had communicated the circumftance to Sir James Hall, he fent me, from a high fandftone rock in his eftate, a quantity of the ftone decompofed; and informed me that there was a faline efflorefcence mixed with it, which was collected along with the loofe matter, and which feemed, by the tafte, to be fea-falt. The place has been long called by the common people the *Salt Heugh*. By fimply boiling fome of the decompofed fandy part in water, and afterwards filtering and evaporating the water, I obtained regular cryftals of fea-falt, mixed with a fmall quantity of fulphate of foda.

Hence it appeared likely, that common falt would be found to be one of the component parts of ordinary fandftone ftrata. To verify this important obfervation, I next examined two hard and folid fpecimens, taken from fome depth below the furface, and perfectly free from decompofition. The firft was broken from a quarry about two miles to the weftward of Edinburgh. A portion of it being reduced to minute grains, of fuch a fize as the particles of the ftone feemed to have confifted of originally, was mixed with fome diluted nitric acid, and boiled with it gently for three hours. The acid, after being filtered, was examined with nitrate of barytes, with which it produced only a flight cloud. But nitrate of filver, when poured into it, threw down a copious white precipitate of muriate of filver, indicating the prefence of muriatic acid.

After this precipitate was feparated by filtration, the liquor which paffed through was evaporated to drynefs. A faline matter remained, which being mixed with fome charcoal, and heated, deflagrated like nitrous falts. Having wafhed the coaly refiduum, and evaporated the water, I got fome perfectly pure carbonate of foda. There had been, therefore, in the fand-

ftone,

ſtone, ſome common ſalt, which, by this proceſs, was decompoſed, and its acid and alkali collected ſeparately ; but whether the whole of the ſalt was obtained, and what proportion it bore to the earthy parts, I cannot determine, as the ſtone itſelf was not analyzed.

The next ſpecimen was taken from a ſtratum of ſandſtone, which lies below the hill of Saliſbury Craig ; and I choſe this ſpecies, becauſe the whin to which it is contiguous has already been ſhown to contain ſoda. Some of it being reduced to the ſtate of fine ſand, and treated in every reſpect as the preceding, gave a portion of muriate of ſilver, and of carbonate of ſoda. The exiſtence, therefore, of ſea-ſalt in theſe varieties of ſandſtone, is thus fully eſtabliſhed*.

The celebrated Mr Klaproth of Berlin has already ſhown, that potaſh enters into the compoſition of ſeveral ſtony ſubſtances ; and by the experiments deſcribed in this paper, the other fixed alkali, ſoda, has alſo been proved to exiſt in mineral bodies, as it has been ſeparated from nine different varieties ; all of which alſo contain a certain quantity of muriatic acid.

As cauſtic fixed alkali was much uſed in theſe analyſes, I ſhall conclude this paper by deſcribing, in a few words, the method by which I prepare it ; both becauſe its purity is of the greateſt importance, and becauſe the proceſs I employ differs, in ſome circumſtances, from that of moſt chemiſts. Having obtained an alkali free from all earthy matter, either by burning white tartar, or by repeated ſolutions and cryſtallizations of carbonate of ſoda, I diſſolve it in a conſiderable quantity of water. The requiſite proportion of lime being ſlacked, and allowed to cool, it is diluted with water, and then mixed with the ſolution of

* Since theſe experiments were performed, I have ſeen ſeveral decompoſed ſandſtones, on the ſurfaces of which there was an effloreſcence of common ſalt.

of the alkali. This mixture is frequently ftirred during two or three days; for when no heat is applied, the lime requires a certain time to attract the whole of the carbonic acid. In the next place, the mixture is filtered through a piece of linen placed in a funnel, and water poured on it till the whole of the alkali is wafhed out. As it paffes through, it is poured, at intervals, from the bottle which receives it firft, into a fecond that is clofely ftopped.

In making the fixed alkalis cauftic, it is ufual to boil them with the lime; but as moft kinds of limeftone contain a fmall quantity of filex or argil, and as thefe earths, when in a ftate of divifion, are foluble in boiling cauftic alkali, there is a probability, when heat is applied, of its being thus rendered impure. This is my reafon for carefully avoiding heat in the firft part of the procefs; and I have not found that lime made from chalk, or from the purer kinds of limeftone *, give any impurity whatever to the alkali, when mixed with it cold.

The folution, in its weak ftate, is firft evaporated in a bafon of hammered iron, polifhed; but when it is fomewhat concentrated, I carry on the evaporation in a difh made of the pureft filver, reduced from luna cornea. After being boiled to a fmall quantity, it is allowed to cool, and then put into a well-ftopped bottle for fome days; during which, if the evaporation has been continued long enough, the neutral falts cryftallize, as Mr Lowitz has pointed out. Afterwards, the folution is carefully decanted from thefe falts, and again evaporated in the filver difh, till it acquires the confiftence of thin oil †. In this ftate, fo little water is prefent, that any part of the alkali, which

 may

* Mr Klaproth ufes lime made of Carrara marble, which he boils with the alkali. (*Beiträge*, vol. i. preface).

† Mr Lowitz, in defcribing his procefs for cryftallizing cauftic potafh, directs the laft evaporation to be performed in a glafs retort. This method is very erroneous; as the alkali, when heated and concentrated, will diffolve large quantities of the glafs.

may be united to carbonic acid, cryſtallizes, and alſo any neutral ſalt that may remain; and the ſolution being decanted a ſecond time, is obtained perfectly pure, colourleſs, and tranſparent.

In the laſt boiling it is ſomewhat difficult to obſerve the exact degree of concentration at which all the alkaline carbonate will cryſtallize; and if the evaporation is carried too far, the cauſtic alkali, if potaſh, cryſtallizes itſelf: ſo that ſeveral evaporations are ſometimes requiſite.

When no more water remains in the ſolution than is juſt ſufficient to hold the cauſtic alkali diſſolved, it contains nearly half its weight of alkali; but the exact quantity is eaſily known, by evaporating a portion to dryneſs in a ſilver crucible. Before uſing ſuch a ſolution for analyſes, I aſcertain its purity in the following manner: Some of it being ſuperſaturated with perfectly pure nitric acid, is examined with nitrate of barytes and of ſilver; with neither of which, if properly made, it will give the ſmalleſt cloud; conſequently it can contain no ſulphuric or muriatic acid.

Another portion being ſaturated exactly with a pure acid, the whole is evaporated to dryneſs, and the ſalt left is rediſſolved in a little water. If any earth were contained in the cauſtic alkali, it would remain thus undiſſolved; but when made as above deſcribed, I have never, in this examination, obſerved the ſmalleſt ſediment.

After the alkali is purified from neutral ſalts, and from the part united to carbonic acid, it may itſelf be cryſtallized by farther evaporation, as Mr Lowitz has ſhown. But this proceſs ſeems of no uſe in chemical analyſis, as the alkali is previouſly obtained altogether pure.

V.

V. *A* New Method *of reſolving* Cubic Equations. *By James Ivory,* Esq. *Communicated by John Playfair,* F.R.S. Edin. *and Profeſſor of Mathematics in the Univerſity of Edinburgh.*

[*Read, 6th May* 1799.]

1. I Divide cubic equations into two varieties or ſpecies: the one, comprehending all cubic equations with three real roots; the other, all thoſe with only one real root.

2. Let φ denote any angle whatever, and let $\tau = \tan\varphi$, the radius being unity: let alſo $z = \tan\frac{\varphi}{3}$: then from the doctrine of angular ſections we have

$$\tau = \frac{3z - z^3}{1 - 3z^2},$$

which being reduced to the form of an equation, is

$$z^3 - 3\tau z^2 - 3z + \tau = 0.$$

Now, from what is commonly taught in angular ſections, z, in this equation, may denote, not only $\tan\frac{\varphi}{3}$, but alſo $\tan\left(\frac{\varphi}{3} + 120^\circ\right)$, or $\tan\left(\frac{\varphi}{3} + 240^\circ\right)$. It is to be remarked, too, that any value whatſoever may be aſſigned to τ, poſitive or negative, and without limit or reſtriction as to magnitude. The equation, then, has three different values of z for every given value of τ; and it belongs to the ſpecies of cubic equations, having three real roots.

3. Again

3. Again I aſſume this expreſſion,

$$\frac{1-\tau}{1+\tau}=\frac{(1-z)^3}{(1+z)^3}.$$

Let there be conceived an equilateral hyperbola, of which the ſemiaxes are each equal to unity, and let a ſtraight line be drawn to touch the hyperbola at its vertex: Conceive alſo a ſtraight line to be drawn from the centre, to cut off a ſector from the hyperbola itſelf, or from its oppoſite, or conjugate hyperbolas, and to intercept a part τ (eſtimated from the vertex) on the tangent line: And, in like manner, let another ſtraight line be drawn from the centre to cut off another ſector, that ſhall be one third part of the former ſector, and to intercept a part z on the tangent line: Then the relation of τ and z will be as in the expreſſion here aſſumed, viz.

$$\frac{1-\tau}{1+\tau}=\frac{(1-z)^3}{(1+z)^3}.$$

I shall not ſtop to demonſtrate this propoſition reſpecting the hyperbola: it eaſily follows from the known properties of that curve. I mention it merely with the view of marking the ſtrict analogy that ſubſiſts between the two varieties of cubic equations. It is ſufficient for our purpoſe to remark, what is indeed very evident from the nature of the aſſumed expreſſion, that, whatever value be aſſigned to τ, z has always one real correſpondent value, and only one.

From our aſſumed expreſſion we get

$$\tau=\frac{(1+z)^3-(1-z)^3}{(1+z)^3+(1-z)^3}=\frac{3z+z^3}{1+3z^2},$$

which being reduced to the form of an equation, is

$$z^3-3\tau z^2+3z-\tau=0.$$

This equation has only one value of z for every given value of τ; and it belongs to the ſpecies of cubic equations having only one real root.

4. In

4. In order to give to the two equations, inveſtigated above, the utmoſt generality of which they are capable, I write $\frac{\tau}{R}$ for τ, and they finally become,

$$\text{I. } Rz^3 - 3\tau z^2 - 3Rz + \tau = 0,$$
$$\text{II. } Rz^3 - 3\tau z^2 + 3Rz - \tau = 0,$$

in which two equations, R and τ repreſent any numbers, poſitive or negative, and altogether unlimited and arbitrary as to magnitude.

I consider the two preceding equations as the ſimple caſes, or ſimple forms, of the two ſpecies of cubic equations: And the method of reſolution I have to propoſe is, to reduce every cubic equation whatſoever to one or other of theſe two forms.

The firſt of the above forms is an equation belonging to the circle. It expreſſes the relation between the tangent of an arch, and the tangent of the third part of that arch: and it has, in all caſes, three real roots. If we take the angle φ, of which the tangent is $\frac{\tau}{R}$, the radius being unity; the three roots of the equation, or the three values of z, are, tan $\frac{\varphi}{3}$, tan $\left(\frac{\varphi}{3}+120°\right)$, and tan $\left(\frac{\varphi}{3}+240°\right)$.

The ſecond of theſe forms is an equation belonging to the hyperbola. It expreſſes the relation between the tangents of two hyperbolic ſectors, of which the one is triple of the other; and it has, in all caſes, only one real root. From the expreſſion aſſumed (Art. 3.) above, whence this equation was deduced, we get, $\frac{1-z}{1+z} = \sqrt[3]{\frac{1-\tau}{1+\tau}}$: therefore,

$$z = \frac{1-\sqrt[3]{\frac{1-\tau}{1+\tau}}}{1+\sqrt[3]{\frac{1-\tau}{1+\tau}}}: \text{ or, writing } \frac{\tau}{R} \text{ for } \tau,\ z = \frac{1-\sqrt[3]{\frac{R-\tau}{R+\tau}}}{1+\sqrt[3]{\frac{R-\tau}{R+\tau}}}: \text{ And}$$

ſo z is found by extracting the cubic root of a given number.

The

The two forms differ from one another only in the ſigns of their terms. The firſt and third terms, as well as the ſecond and fourth, have always unlike ſigns in the firſt form ; but always like ſigns in the ſecond form. This property reſpecting the ſigns of the alternate terms, by which the one equation is eſſentially diſtinguiſhed from the other, I ſhall denominate the " *Characteriſtic of the form.*"

5. I PROCEED, now, to ſhew, in what way any propoſed cubic equation may be reduced to one or other of the two forms.

LET the propoſed equation be,

$$x^3 + Ax^2 + Bx + C = 0,$$

where A, B, C denote any given coefficients, poſitive or negative. I aſſume $x = \frac{a+z}{b+z}$: a and b being indeterminate quantities, and z a new unknown quantity. And it is to be obſerved, that the ſuppoſition of $x = \frac{a+z}{b+z}$ is always poſſible, provided a be not equal to b : for if a be not equal to b, a value may be aſſigned to z, ſuch, that the fraction $\frac{a+z}{b+z}$ ſhall be equal to any number whatever, poſitive or negative. But if $a = b$, the value of $\frac{a+z}{b+z}$ is not altered, whatever number z may denote.

HAVING ſubſtituted, and taken away the denominators, we get,

$$(a+z)^3 + A(a+z)^2(b+z) + B(a+z)(b+z)^2 + C(b+z)^3 = 0.$$

THE

The terms of this expreſſion are now to be evolved and arranged, according to the powers of z: which being done, we ſhall find,

$$\left.\begin{array}{llll} a^3 & +\ 3a^2\times z & +\ 3a\times z^2 & +\ 1\times z^3 \\ +\ Aa^2b & +\ 2Aab\times z & +\ Ab\times z^2 & \\ & +\ Aa^2\times z & +\ 2Aa\times z^2 & +\ A\times z^3 \\ +\ Bab^2 & +\ Bb^2\times z & & \\ & +\ 2Bab\times z & +\ 2Bb\times z^2 & \\ & & +\ Ba\times z^2 & +\ B\times z^3 \\ +\ Cb^3 & +\ 3Cb^2\times z & +\ 3Cb\times z^2 & +\ C\times z^3 \end{array}\right\} = 0.$$

In order to reduce this equation to our forms, we muſt equate three times the coefficient of z^3 to the coefficient of z, either with the ſame or different ſigns: and alſo the coefficient of z^2 to three times the abſolute term, likewiſe with the ſame or different ſigns. For in the forms the coefficient of z^3 and z are R and $\mp$ 3R: and the coefficient of z^2 and the abſolute term are, — $3r$ and $\pm r$. Now, in the transformed equation above, three times the coefficient of z^3 is $3 + 3A + 3B + 3C$, which I write thus, $(3 + 2A + B) + (A + 2B + 3C)$: And in like manner, for three times the abſolute term, I write $(3a^3 + 2Aa^2b + Bab^2) + (Aa^2b + 2Bab^2 + 3Cb^3)$. This being obſerved, we ſhall have theſe two equations for determining a and b:

$$\left.\begin{array}{l} 3 + 2A + B \\ + A + 2B + 3C \end{array}\right\} = \mp \left\{\begin{array}{l} 3a^2 + 2Aab + Bb^2 \\ Aa^2 + 2Bab + 3Cb^2, \end{array}\right.$$

$$\left.\begin{array}{l} 3a + 2Aa + Ba \\ + Ab + 2Bb + 3Cb \end{array}\right\} = \mp \left\{\begin{array}{l} 3a^3 + 2Aa^2b + Bab^2 \\ + Aa^3 + 2Bab^2 + 3Cb^3. \end{array}\right.$$

6. It is manifeſt, from the manner in which I have written the two equations for determining a and b, that they depend upon

upon the two following more simple equations of the quadratic form:

$$3a^2 + 2Aab + Bb^2 = \mp (3 + 2A + B),$$
$$Aa^2 + 2Bab + 3Cb^2 = \mp (A + 2B + 3C).$$

For these two equations are no other than the two parts of the first of the preceding equations: and if we multiply the first of them by a, and the second by b, we shall have the two parts of the second of the preceding equations.

To determine a and b, I now write $M = 3 + 2A + B$; $N = A + 2B + 3C$; and $ay = b$: thus we have,

$$a^2 \times (3 + 2Ay + By^2) = \mp M,$$
$$a^2 \times (A + 2By + 3Cy^2) = \mp N.$$

Multiply the first equation by N; the second by M; subtract the one from the other; and divide by a^2; and there will result this equation for y:

$$(3N - AM) + 2(AN - BM)y + (BN - 3CM)y^2 = 0.$$

And in this equation there is no ambiguity of signs.

If we suppose $y = 1$, the equation last found is equivalent to the identical equation $MN - MN = 0$. One value of y is therefore unity. But this is precisely the case of $a = b$, which we have noticed above to be inapplicable to the present purpose. Nor is it to be wondered at that this value of y is of no use in the present inquiry: for it is manifest that it does not at all depend on the given quantities A, B, C, M and N, but merely on the peculiar manner in which the equation is constituted. We learn from hence, however, that there is always another value of y, whatever numbers A, B, C, M and N may denote; because every quadratic must have two roots or none at all.

Quadratic equations, with one root $= 1$, may be supposed to be thus generated: $(y - 1) \times (my - n) = my^2 - (m + n)y + n = 0$, the

the two roots being 1 and $\frac{n}{m}$: Comparing this formula with our equation, we have $n = 3N - AM$ and $m = BN - 3CM$: And so we get,

$$y = \frac{3N - AM}{BN - 3CM}.$$

Now, $a^2 = \frac{\mp M}{3 + 2Ay + By^2}$ and $b = a \times y$: therefore,

$$a = (BN - 3CM) \times \mp\sqrt{\frac{\mp M}{3(BN-3CM)^2 + 2A(3N-AM)(BN-3CM) + B(3N-AM)^2}}$$

$$b = (3N - AM) \times \pm\sqrt{\frac{\mp M}{3(BN-3CM)^2 + 2A(3N-AM)(BN-3CM) + B(3N-AM)^2}}$$

The quantities a and b are therefore found by a single extraction of the square root: and they have each two values, one positive, and the other negative. It is indifferent which of these two values of a and b be taken, provided they are correspondent values, so that $b = a \times y$. It is to be remarked too, that a and b have always real values, on account of the double sign prefixed to M; for that sign is to be taken that will render the radical quantity positive. And it is to be carefully noted which of the two signs is necessary, that a and b may have real values: because on this depends the characteristic of the reduced equation, and whether it is to be referred to the first or second form, and, consequently, whether it has three roots, or only one. If the sign $-$ is requisite that a and b may have real values, then the reduced equation will have the characteristic of the first form, and will have three roots. But if the sign $+$ is requisite for that end, the reduced equation will have the characteristic of the second form, and will have only one root. All this is manifest from the statement in Art. 5.

7. The rule, or law, according to which the preceding formulæ for a and b are constituted, is sufficiently simple and perspicuous; and the formulæ are therefore, in that respect, convenient for practice. But in examining the expression in the

 denominator

denominator of the radical, I find that it is always divifible by M, the quantity in the numerator: and that thus the formulæ may be exhibited in another fhape, having this advantage, that it will introduce fmaller numbers in the arithmetical operations requifite for computing a and b.

I WRITE $Q \times M = 3(BN - 3CM)^2 + 2A(BN - 3CM)(3N - AM) + B(3N - AM)^2$, and evolving by actual multiplication

$$\begin{aligned} Q \times M = {} & 3B^2 . N^2 - 18BC . MN + 27C^2 . M^2 \\ & + 6AB . N^2 - 2A^2B . MN \\ & \qquad - 18AC . MN + 6A^2C . M^2 \\ & + 9B . N^2 - 6AB . MN + BA^2 . M^2 \end{aligned}$$

Now, the coefficient of N^2 is, $3B^2 + 6AB + 9B = 3B(B + 2A + 3) = 3B \times M$: dividing therefore by M, we get,

$$\begin{aligned} Q = {} & 3B \times N^2 - (18BC + 2A^2B + 18AC + 6AB) \times N \\ & + (27C^2 + 6A^2C + BA^2) \times M. \end{aligned}$$

And if for N and M we fubftitute their values $(A + 2B + 3C)$ and $(3 + 2A + B)$, we fhall find,

$$Q = 12B^3 + 81C^2 - 54ABC + 12A^3C - 3A^2B^2$$

all the other terms deftroying one another, except thefe five.

ALL the terms in this value of Q being divifible by 3, I change Q and put

$$Q = 4B^3 + 27C^2 - 18ABC + 4A^3C - A^2B^2$$

$$\text{or, } Q = (4B^3 + 27C^2) - 2AC(9B - A^2) + A^2(2AC - B^2)$$

And fince 3QM is now equal to the denominator of the radical in the preceding formulæ for a and b, we have the following new formulæ,

$$a = \pm \frac{BN - 3CM}{\sqrt{\mp 3Q}}.$$

$$b = \pm \frac{3N - AM}{\sqrt{\mp 3Q}}.$$

WHAT

WHAT was remarked above, with regard to the double ſign prefixed to M, is now to be applied to the double ſign prefixed to Q.

8. THE preceding inveſtigation ſupplies us with the following rule, or criterion, by which to determine, whether any propoſed cubic equation has three real roots or not:

THE propoſed equation will have three real roots, when the amount of the expreſſion

$$(4B^3 + 27C^2) - 2AC(9B - A^2) + A^2(2AC - B^2)$$

is negative: But if this expreſſion is poſitive, the equation will have only one real root: And, (as will afterwards be ſhewn), when the amount of the expreſſion is $= 0$, the equation will have two equal roots.

9. HAVING now found a and b, if we ſubſtitute $\frac{a+z}{b+z}$ for x in the propoſed equation, we ſhall have an equation for z that will come under one of our two forms, and from which z (and conſequently the root or roots of the propoſed equation) may therefore be found. But ſuch ſubſtitution is not neceſſary. For if we go back to Art. 5. and compare the transformed equation with the forms, we ſhall find $R = 1 + A + B + C$, and $3R = M + N$: alſo $-3r = (3a + 2Aa + Ba)$ $+ (Ab + 2Bb + 3Cb) = Ma + Nb$: Therefore

$$\frac{r}{R} = -\frac{Ma + Nb}{M + N}.$$

Whence the value or values of z are found by what is obſerved in Art. 4.

10. I SHALL now give the reſult of the whole analyſis in the form of a general rule for the reſolution of cubic equations, and add a few examples by way of illuſtration.

LET the propoſed equation be

$$x^3 + Ax^2 + Bx + C = 0.$$

1. COM-

1. Compute, $M = 3 + 2A + B$

$$N = A + 2B + 3C$$
$$m = BN - 3CM$$
$$n = 3N - AM,$$

And $Q = (4B^3 + 27C^2) - 2AC(9B - A^2) + A^2(2AC - B^2)$

And let it be carefully noted under which of the two following cafes the equation comes:

Case I. When Q is negative.

Case II. When Q is pofitive.

To thefe two cafes a third may be added, viz. when $Q = 0$: but of this cafe I fhall treat in one of the following examples.

2. Compute alfo, $a = \frac{\pm m}{\sqrt{\mp 3Q}}$

$$b = \frac{\pm n}{\sqrt{\mp 3Q}} = \frac{an}{m}$$

and $\tau = -\frac{Ma + Nb}{M + N}$.

3. Then, if the equation comes under cafe I: Find the angle φ, of which the tangent is τ, the radius being unity: take $z = \tan\frac{\varphi}{3}$, $z = \tan\left(\frac{\varphi}{3} + 120°\right)$, and $z = \tan\left(\frac{\varphi}{3} + 240°\right)$: And the three roots of the equation will be found, by fubftituting thefe values of z in the formula $x = \frac{a + z}{b + z}$.

4. But if the equation comes under Cafe II. we muft compute

$$z = \frac{1 - \sqrt[3]{\frac{1-\tau}{1+\tau}}}{1 + \sqrt[3]{\frac{1-\tau}{1+\tau}}},$$

And the only root of the equation will be found, by fubftituting this value of z in the formula $x = \frac{a + z}{b + z}$.

The

THE following examples are chiefly taken from Dr HUTTON's "*Tracts Mathematical and Philosophical.*" *Tract* 5.

11. EXAMPLE 1. Let the proposed equation be

$$x^3 - 6x^2 + 11x - 6 = 0.$$

Here, $A = -6$, $B = +11$, $C = -6$: therefore

$$M = +2$$
$$N = -2$$
$$m = +14$$
$$n = +6$$
$$Q = -4$$

So that we have here Case I.

$$a = \frac{14}{2\sqrt{3}} = \frac{7}{\sqrt{3}}$$

$$b = \frac{6}{14} \times a = \sqrt{3}$$

$$\text{and } \tau = -\frac{2.\frac{7}{\sqrt{3}} - 2.\sqrt{3}}{2-2} = \infty$$

Now, $\tan 90° = \infty$, and also $\tan 270° = \infty$: And we may take either of these angles for φ: Take $\varphi = 90°$, then,

$$z = \tan\frac{\varphi}{3} = \tan 30° = +\frac{1}{\sqrt{3}}$$

$$z = \tan\left(\frac{\varphi}{3} + 120°\right) = \tan 150° = -\tan 30° = -\frac{1}{\sqrt{3}}$$

$$z = \tan\left(\frac{\varphi}{3} + 240°\right) = \tan 270° = \infty$$

THEREFORE, $x = \dfrac{\frac{7}{\sqrt{3}} + \frac{1}{\sqrt{3}}}{\sqrt{3} + \frac{1}{\sqrt{3}}} = \frac{8}{4} = 2$

$$x = \frac{\frac{7}{\sqrt{3}} - \frac{1}{\sqrt{3}}}{\sqrt{3} - \frac{1}{\sqrt{3}}} = \frac{6}{2} = 3$$

$$x = \frac{\frac{7}{\sqrt{3}} + \infty}{\sqrt{3} + \infty} = 1.$$

And so the three roots are 1, 2, and 3.

SINCE

Since $M + N = 3(1 + A + B + C)$: it is obvious, that when $M + N = 0$, we ſhall alſo have $1 + A + B + C = 0$; and ſo one root of the equation is unity. It is manifeſt too, that, in this caſe, the transformed equation for z becomes ſimply, $3z^2 - 1 = 0$: whence $z = \pm\frac{1}{\sqrt{3}}$; and conſequently the other two roots of the equation are, $x = \frac{a + \frac{1}{\sqrt{3}}}{b + \frac{1}{\sqrt{3}}}$, and $x = \frac{a - \frac{1}{\sqrt{3}}}{b - \frac{1}{\sqrt{3}}}$.

Or, writing for a and b, their values, $\frac{m}{\sqrt{-3Q}}$ and $\frac{n}{\sqrt{-3Q}}$, the two roots are, $x = \frac{m + \sqrt{-Q}}{n + \sqrt{-Q}}$ and $x = \frac{m - \sqrt{-Q}}{n - \sqrt{-Q}}$.

12. Example 2. Let there be propoſed

$$x^3 - 6x^2 + 9x - 2 = 0.$$

Here $A = -6$; $B = +9$; $C = -2$: And hence,

$$M = 0$$
$$N = +6$$
$$m = +54$$
$$n = +18$$
$$Q = -108.$$

And we have here again Caſe I.

$$a = +3$$
$$b = +1$$
$$\tau = -1$$

Therefore $\phi =$ Arc. tan $- 1 = 145°$, therefore,

$$z = \tan\frac{\phi}{3} = \tan 45° = 1,$$

$$z = \text{t.}\left(\frac{\phi}{3} + 120°\right) = \text{t.}\ 165° = -\text{t.}\ 15° = -(2 - \sqrt{3}),$$

$$z = \text{t.}\left(\frac{\phi}{3} + 240°\right) = \text{t.}\ 285° = -\text{t.}\ 75° = -(2 + \sqrt{3});$$

Therefore, $x = \frac{3+1}{1+1} = \frac{4}{2} = 2$;

x

$$x=\frac{3-2+\sqrt{3}}{1-2+\sqrt{3}}=\frac{\sqrt{3}+1}{\sqrt{3}-1}=\frac{(\sqrt{3}+1)^2}{2}=2+\sqrt{3};$$

$$x=\frac{3-2-\sqrt{3}}{1-2-\sqrt{3}}=\frac{\sqrt{3}-1}{\sqrt{3}+1}=\frac{(\sqrt{3}-1)^2}{2}=2-\sqrt{3};$$

And the three roots are 2, $2+\sqrt{3}$, and $2-\sqrt{3}$.

13. I TAKE the following example to illuſtrate the caſe of two equal roots.

EXAMPLE 3. Let the equation be

$$x^3-7x^2-5x+75=0.$$

Here $A=-7$: $B=-5$: $C=+75$. Therefore,

$$M=-16$$
$$N=+208$$
$$m=+2560$$
$$n=+512$$
$$Q=0.$$

SINCE, then, $Q=0$, it is manifeſt that a, b, and τ will be all infinitely great. But though a and b be infinite, it is to be remarked that $\frac{a}{b}=\frac{m}{n}$.

SINCE $\tau=\infty$, we have $\phi=90°$, or $\phi=270°$: Take $\phi=90°$: Then,

$$z=\tan\frac{\phi}{3}=\tan 30=\frac{1}{\sqrt{3}}$$

$$z=\tan\left(\frac{\phi}{3}+120°\right)=\tan 150°=-\tan 30°=-\frac{1}{\sqrt{3}}$$

$$z=\tan\left(\frac{\phi}{3}+240°\right)=\tan 270°=\infty$$

WE have then, in the firſt place, for two roots,

$$x=\frac{a+\frac{1}{\sqrt{3}}}{b+\frac{1}{\sqrt{3}}}$$

$$x=\frac{a-\frac{1}{\sqrt{3}}}{b-\frac{1}{\sqrt{3}}}.$$

And

And ſince a and b are infinitely great, it is manifeſt that theſe two roots are equal to one another, and each $= \frac{a}{b} = \frac{m}{n}$.

We can infer nothing with regard to the value of the third root, derived from the infinite tangent, unleſs we can aſcertain the proportion which that infinite tangent bears to the infinitely great quantities a and b. The general relation of $\tau = \tan \phi$, and $z = \tan \frac{\phi}{3}$ is thus expreſſed $\tau = \frac{3z - z^3}{1 - 3z^2}$: and τ becomes infinitely great; 1ſt, When $1 - 3z^2 = 0$; 2dly, When z is infinitely great.

Now the values of z, derived from the equation $1 - 3z^2 = 0$, are $z = + \frac{1}{\sqrt{3}}$, and $z = - \frac{1}{\sqrt{3}}$: And theſe are preciſely the values of z uſed above in determining the two equal roots.

Again, we have $\frac{\tau}{z} = \frac{3 - z^2}{1 - 3z^2}$: And it is manifeſt that the greater z is, the nearer $\frac{\tau}{z}$ approaches to $\frac{1}{3}$: ſo that, ultimately, when τ and z are greater than any finite magnitudes, we have $\frac{\tau}{z} = \frac{1}{3}$ and $z = 3\tau$. But $\tau = \frac{-Ma - Nb}{M + N}$; therefore, $z = \frac{-3Ma - 3Nb}{M + N}$ and $x = \frac{a + z}{b + z} = \frac{a - \frac{3Ma + 3Nb}{M + N}}{b - \frac{3Ma + 3Nb}{M + N}}$. If now we write $\frac{an}{m}$ for b, in this expreſſion, the infinite quantity a may be thrown out by diviſion, and we ſhall have the value of the root in finite quantities only. The expreſſion being properly reduced, we ſhall have, $x = \frac{2Mm + N(3n - m)}{M(3m - n) + 2Nn}$.

When, therefore, $Q = 0$, two roots are equal to one another, and each $= \frac{m}{n}$: And we have this formula by which to compute the third root, $x = \frac{2Mm + N(3n - m)}{2Nn + M(3m - n)}$.

Applying

APPLYING this rule to our example, we have:

for the two equal roots, $x = \frac{m}{n} = \frac{2560}{512} = 5$

for the third root, $x = \frac{-81920 - 212992}{-114688 + 212992} = \frac{-294912}{+98304} = -3$

14. EXAMPLE 4. Let the equation be

$$x^3 - 7x^2 + 18x - 18 = 0.$$

Here A $= -7$; B $= +18$; C $= -18$. Therefore,

$$\begin{aligned} M &= +7 \\ N &= -25 \\ m &= -72 \\ n &= -26 \\ Q &= +72. \end{aligned}$$

So that we have here Case II.

$$a = \frac{-72}{\sqrt{216}} = \frac{-36}{3\sqrt{6}}$$

$$b = \frac{-26}{\sqrt{216}} = \frac{-13}{3\sqrt{6}}$$

$$r = +\frac{73}{54\sqrt{6}}$$

Therefore, $\sqrt[3]{\frac{1-r}{1+r}} = \sqrt[3]{\frac{54\sqrt{6}-73}{54\sqrt{6}+73}} = \frac{2\sqrt{6}-1}{2\sqrt{6}+1}$

HENCE $z = \dfrac{1-\sqrt[3]{\frac{1-r}{1+r}}}{1+\sqrt[3]{\frac{1-r}{1+r}}} = \dfrac{1}{2\sqrt{6}}$

and $x = \dfrac{-\frac{36}{3\sqrt{6}} + \frac{1}{2\sqrt{6}}}{-\frac{13}{2\sqrt{6}} + \frac{1}{2\sqrt{6}}} = \dfrac{-72+3}{-26+3} = \dfrac{-69}{-23} = +3.$

And 3 is the only root of the equation.

15. WHEN r is a surd as $\frac{p}{q\sqrt{r}}$, the value of z (in Case II.) always involves radicals of this form, $\sqrt[3]{\frac{q\sqrt{r}-p}{q\sqrt{}+p}}$; out of which the root may sometimes be extracted; and so the value of z will be expressed by a surd of the same kind as r.

THE method I have followed to find when this can be done, being very ſimple and eaſy in practice, I ſhall here briefly deſcribe it.

$$\text{We have } \tau = \frac{3z + z^3}{1 + 3z};$$

$$\text{write } \frac{p}{q\sqrt{r}}, \text{ for } \tau, \ \frac{\mu}{\nu\sqrt{r}} \text{ for } z: \text{ then}$$

$$\frac{p}{q\sqrt{r}} = \frac{3r\mu\nu + \mu^3}{(r\nu^3 + 3\mu^2\nu)\sqrt{r}},$$

whence theſe two equations are formed.

$$p = 3r\mu\nu + \mu^3$$
$$q = r\nu^3 + 3\mu^2\nu,$$

from which it is manifeſt, that μ is a diviſor of p, and ν a diviſor of q. I ſeek then amongſt the diviſors of p for a number μ, and amongſt the diviſors of q for a number ν, that will ſatisfy the two equations above: or rather, that will ſatisfy theſe two following,

$$\nu^2 = \frac{p - \mu^3}{3r\mu},$$

$$\mu^2 = \frac{q - r^3}{3\nu}.$$

If two ſuch numbers are to be found amongſt the diviſors of p and q, then will $z = \frac{\mu}{\nu\sqrt{r}}$: but if not, we are to conclude that the value of z cannot be expreſſed this way.

THUS, in the laſt example $\tau = \frac{73}{54\sqrt{6}}$, it is manifeſt that 73 admits no diviſor but 1: therefore $\mu = 1$, and on trial I find $\nu = 2$, which two numbers ſatisfy the two equations, and therefore $z = \frac{1}{2\sqrt{6}}$.

16. THE ſame method applies alſo to Caſe I. For, in this caſe,

$$\tau = \frac{3z - z^3}{1 - 3z^2}$$

and

and ſubſtituting $\frac{p}{q\sqrt{r}}$ for τ, and $\frac{\mu}{\nu\sqrt{r}}$ for z, we derive theſe two equations,

$$p = 3r\mu\nu^2 - \mu^3$$
$$q = r\nu^3 - 3\mu^2\nu,$$

whence it is manifeſt that μ is a diviſor of p, and ν a diviſor of q, as before. It will be eaſier for trial to write the equations thus:

$$\nu^2 = \frac{p+\mu^3}{3r\mu}$$
$$\mu^2 = \frac{r\nu^3 - q}{3\nu}$$

And let it be obſerved, that we may here give to μ and ν any ſigns conſiſtent with the condition, that $\frac{p+\mu^3}{3r\mu}$ and $\frac{r\nu^3-q}{3\nu}$ (the values of ν^2 and μ^2) are poſitive numbers.

It is to be remarked too, that, in this caſe, z has three values. If, however, we can find one value this way, the two others are readily obtained. For if v be one value of z, the two other values are $\frac{v+\sqrt{3}}{1-v\sqrt{3}}$ and $\frac{v-\sqrt{3}}{1+v\sqrt{3}}$: becauſe theſe values are the tangents of two arches that differ from the arch of which v is the tangent by 120° and 240°.

Though this is a matter more curious than uſeful, I ſhall add one more example for the ſake of illuſtration.

Let the equation be

$$x^3 - 39x^2 + 479x - 1881 = 0.$$

Here A = − 39; B = + 479; C = − 1881: And

$$M = + 404$$
$$N = - 4724$$
$$m = + 16976$$
$$n = + 1584$$
$$Q = - 25600$$

And the equation belongs to Caſe I.

$$a = \frac{16976}{160\sqrt{3}} = \frac{1061}{10\sqrt{3}}$$

$$b = \frac{1584}{160\sqrt{3}} = \frac{99}{10\sqrt{3}}$$

$$\tau = \frac{-39032}{-43200\sqrt{3}} = -\frac{4879}{5400\sqrt{3}}$$

To find whether z can be expreffed by a furd as $\frac{\mu}{\nu\sqrt{r}}$: I write $p = -4879$, $q = 5400$, and $r = 3$; and I have thefe two equations,

$$\nu^2 = \frac{-4879 + \mu^3}{9\mu}$$

$$\mu^2 = \frac{3\nu^3 - 5400}{3\nu}$$

I find, that 7, 17, and 41, are divifors of 4879. It is evident that $+7$ would give ν^2 negative: I therefore try -7, and it does not fucceed: Neither does $+17$; but trying -17, I find $\nu^2 = 64$, and confequently $\nu = \pm 8$: And -8 is found to fucceed in the other equation: therefore $\mu = -17$ $\nu = -8$, and $z = \frac{17}{8\sqrt{3}}$.

We have then, $z = \frac{17}{8\sqrt{3}} = v$

$$z = \frac{v - \sqrt{3}}{1 + v\sqrt{3}} = \frac{-7}{25\sqrt{3}}$$

$$z = \frac{v + \sqrt{3}}{1 - v\sqrt{3}} = \frac{41}{-9\sqrt{3}}$$

Therefore, $x = \dfrac{\frac{1061}{10\sqrt{3}} + \frac{17}{8\sqrt{3}}}{\frac{99}{10\sqrt{3}} + \frac{17}{3\sqrt{3}}} = \frac{8488 + 170}{792 + 170} = \frac{8658}{962} = 9$

$$x = \frac{\frac{1061}{10\sqrt{3}} - \frac{7}{25\sqrt{3}}}{\frac{99}{10\sqrt{3}} - \frac{7}{25\sqrt{3}}} = \frac{5305 - 14}{495 - 14} = \frac{5291}{481} = 11$$

$$x = \frac{\frac{1061}{10\sqrt{3}} - \frac{41}{9\sqrt{3}}}{\frac{99}{10\sqrt{3}} - \frac{41}{9\sqrt{3}}} = \frac{9549 - 410}{891 - 410} = \frac{9139}{481} = 19.$$

And fo the three roots are, 9, 11, and 19.

END OF PART FIRST.

VI. REMARKS *on a* MIXED SPECIES *of* EVIDENCE *in* MATTERS *of* HISTORY: *With an* EXAMINATION *of a new* HISTORICAL HYPOTHESIS, *in the* Mémoires pour la Vie de PETRARQUE, *by the Abbé de SADE.* *By ALEXANDER FRASER TYTLER*, ESQ. F. R. S. EDIN. *Judge-Advocate of North Britain.*

[*Read by Mr PLAYFAIR, August* 21. 1797.]

IN matters of hiſtorical reſearch, there is a kind of circumſtantial evidence which ariſes from the combination of known or authenticated facts, with critical argument on the import of doubtful paſſages of authors, which the reaſoner endeavours to interpret, by bringing together, comparing, and making the one illuſtrate the other; ſo as to draw from the whole a degree of poſitive and certain information, which thoſe authenticated facts are not of themſelves ſufficient to convey, and which thoſe paſſages, taken ſeparately, are incapable of furniſhing. This complex ſpecies of evidence, it muſt be owned, is, with reſpect to its power of conviction, much inferior to that which ariſes from the ordinary proofs on which authentic hiſtory depends; for example, the teſtimony of actual witneſſes to the facts related; or the poſitive information of authors, derived from clear and well authenticated records: But, at the ſame time, as in matters of hiſtory we have not always that beſt kind of evidence on which to found our belief, we are from neceſſity often compelled to reſort to this inferior, circumſtantial and analogical ſpecies of probation, in order to form to ourſelves a rational creed on many matters of doubt, on which the mind is unwilling to reſt in abſolute uncertainty.

THIS latter ſpecies of evidence, too, has its recommendation, from that pleaſing exerciſe which it gives to the reaſoning powers: for there is no mental occupation which demands more ingenuity, if we conſtruct ourſelves this artificial fabric of argument, or requires more judgment, if we attempt to analyze and examine its foundations when reared by another. The ſtructure of an hypotheſis of our own is a delightful occupation to the mind; and a pleaſure, perhaps very little inferior to this, is the critical examination of an hypotheſis framed by another. We proceed to either undertaking with a zeal, reſembling that which is felt by the adventurer on a voyage of diſcovery. We perſevere in it with an ardour which increaſes with the difficulties we encounter. Like the adventurer, too, we find our chief reward in the reſearch itſelf, whatever may be the actual value or real importance of its ultimate object.

OF the truth of theſe obſervations I have myſelf had full experience, while, at a ſeaſon when I thought myſelf warranted in ſeeking an amuſing exerciſe of the mind, I began to read over, with ſome attention, an elaborate work that I had formerly but haſtily peruſed, the *Mémoires pour la Vie de* PETRARQUE, by the Abbé de SADE; a compoſition which affords one of the moſt remarkable examples of an hiſtorical hypotheſis, ſupported by that compound ſpecies of evidence I have mentioned, which conſiſts in the combination of certain authenticated facts, with critical reaſoning on the import of various doubtful and contending authorities. As the examination of this hypotheſis, and the

the evidence on which it is built, has afforded me confiderable pleafure, while, at the fame time, I have flattered myfelf that the refult of this examination has been a complete detection of its infufficiency, I am prompted to fubmit my remarks to the confideration of this Society, which embraces, in its comprehenfive inftitution, equally the fubjects of philofophical refearch and of hiftorical and critical difquifition.

In the examination of this hypothefis, I laid down to myfelf certain rules, which, as they are of a general nature, may, as I imagine, be applied with propriety to all inveftigations of a fimilar kind, where the evidence is of that compound fpecies I have mentioned. Thefe rules are the following:

I. Where a doubtful fact is to be afcertained, by bringing together, comparing and weighing the fenfe of various paffages of an author's writings, the conftruction put on *ambiguous* expreffions ought to be fuch as is confonant with the fenfe of thofe paffages or expreffions, which, on the fame fubject, are *plain* and *unambiguous*.

II. Where a perfon's *character* and *manner of thinking*, *feeling*, or *acting*, are clear, from the general tenor of his life and writings, no interpretation ought to be given to doubtful paffages of thofe writings, which contradicts, or is inconfiftent with, fuch *character*, *fentiments*, and *conduct*.

III. Where many paffages concur to eftablifh the belief of the difputed fact, a fingle paffage, though apparently contradictory to that fuppofition, muft not be allowed weight, if it is poffible to give it an explanation confiftent with that opinion which is better fupported.

IV. In fuch a cafe, where many paffages concur to eftablifh the belief of a certain fact, and there appear one or two paffages in apparent contradiction to that belief, there is room to fufpect either

either an *error of tranſcription or typography*; or, if ſuch ſuppoſition is excluded, *interpolation or fabrication*.

V. In the ſuppoſition of interpolation or fabrication, there muſt of neceſſity be included a cogent and adequate *motive*; and therefore, where ſuch motive is utterly wanting, the ſuppoſition is not to be indulged.

VI. Where this motive is apparent, the preſumption of *falſehood* is in proportion to the ſtrength of the motive, the facility of executing the deception, and the weight of the oppoſing evidence.

VII. Where a paſſage is ſuſpected of interpolation or fabrication, it is moſt material to attend to the ſenſe of the *context*, or what immediately precedes and follows the paſſage in diſpute; as its *conſonancy* or *diſſonancy* is ſtrong matter of corroboration.

If theſe rules of evidence are well founded, they will afford a juſt criterion for the deciſion of all queſtions of hiſtorical controverſy, where the evidence is of a compound, circumſtantial and preſumptive nature; and where our belief is the conſequence not of authority but of argument. Of ſuch a nature is that hypotheſis of the Abbé de Sade, which I ſhall now proceed to examine.

The literary world owes very high obligations to Francis Petrarch; and it has been laudably zealous in repaying them by a grateful tribute of admiration and encomium. There is no character among the moderns whoſe talents have been more the ſubject of panegyric, both among his cotemporaries and with poſterity; nor is there any whoſe life has ſo frequently employed the pen of the biographer. The Baron de la Bastie, one of the lateſt of thoſe biographers, enumerates at leaſt fourteen, and moſt of theſe authors of ſome reputation, who have profeſſedly written biographical accounts of this eminent man; not

not to mention a ſtill more numerous claſs, who have incidentally treated of his life and literary character in critical diſſertations, journals, and memoirs of the ſtate of literature of his age and country.

AMIDST this variety of reſearch, it is natural to ſuppoſe that much has been brought to light, and, it is even a reaſonable ſuſpicion, that where there has been ſuch ardour of inveſtigation, there can remain but little ſubject of new diſcovery. So thought M. de la BASTIE, when, in the years 1740 and 1741, he publiſhed his excellent Account of the Life of PETRARCH, in the *Mémoires de l'Académie des Inſcriptions et Belles Lettres* *. He was not aware, that within a few years from the date of his own publication, the world was to receive a work, containing at leaſt ten times the quantity of all that had ever been written on the ſubject of PETRARCH, and profeſſing to be only *materials* for the compoſition of a Life of this remarkable man. The work I here allude to, is *Mémoires pour la Vie de* PETRARQUE, by the *Abbé de* SADE; a book which has met with conſiderable approbation from the public, and which merits the praiſe it has obtained; as, along with a very minute detail of the life of PETRARCH himſelf, and a very full account of his writings, it contains much curious and inſtructive information relative both to the literary and political hiſtory of the times which it commemorates. In a work which is extended to three large volumes in quarto, and which has been the fruit of the moſt elaborate reſearch, not only into all the printed works illuſtrative of the manners and hiſtory of the 14th century, but into public records and the archives of private families, it cannot be denied, that much new light has been thrown on the principal object of that reſearch, the *hiſtory* and character of PETRARCH. But it will be a matter of regret to many readers, that the conſequence

* Vols. xv. and xvii.

ſequence of theſe elaborate inquiries, ſhould be a diminution of that eſteem which the world has hitherto entertained for this eminent man, in the moſt important point of his character, his *morals* *.

The works of PETRARCH give evidence of his abilities as a Politician, Theologian, and Philoſopher; and it is in theſe characters that he appears chiefly to have been diſtinguiſhed by his cotemporaries; but it is not on theſe foundations that the laſting ſtructure of his fame has been reared. It is to thoſe incomparable verſes, in which he has celebrated the accompliſhments and bewailed the fate of the beautiful LAURA, that PETRARCH has been indebted for his permanent reputation. The hiſtory of the poet's paſſion for his lovely miſtreſs, muſt ever be regarded as forming the moſt intereſting portion of his annals. His character, in fact, took its tone from that predominant affection, which influenced his ſtudies, his habits of life, and all his purſuits and occupations. A love ſo pure, ſo ardent, and ſo laſting, is difficult to be paralleled in the hiſtory of human nature. PETRARCH was the paſſionate admirer of LAURA for twenty-one years, while ſhe was in life; and with unabated ardour of affection, he is ſaid to have bewailed her loſs for twenty-ſix years after her death.

The works of the poet himſelf bear the ſtrongeſt teſtimony that this paſſion, ſo remarkable both in its fervency and duration, was an honourable and virtuous flame. PETRARCH aſpired to the happineſs of being united to LAURA in marriage. We have reaſon to believe, from the evidence of his own writings, that the heart of LAURA was not inſenſible to his paſſion; and, although the term of his probation was tedious and ſevere, he had grounds for a hope, approaching to confidence, that he was

* Of this work of the Abbé de SADE, Mrs DOBSON has given a very amuſing epitome, in her *Life of* PETRARCH, 2 vols. 8vo.

was at laſt to attain the end of his wiſhes. Such are the ideas that we muſt entertain, from the writings of the poet himſelf, of the nature and object of his paſſion; and ſuch has been the uniform and continued belief of the world with regard to it, from his own days to the preſent. At length comes into the field a hardy but moſt uncourteous Knight, who, with a ſpirit very oppoſite to that of the heroes of chivalry, blaſts at once the fair fame of the virtuous LAURA, and the hitherto unſullied honour of her lover; and, proudly throwing down his gauntlet of defiance, maintains, that LAURA was a married woman, the mother of a numerous family; that PETRARCH, with all his profeſſions of a pure and honourable flame, had no other end in his unexampled aſſiduity of purſuit, than what every libertine propoſes to himſelf in the poſſeſſion of a miſtreſs; and that the lovely LAURA, though never actually unfaithful to her huſband's bed, was ſenſible to the paſſion of her *Ciciſbeo*, highly gratified by his purſuit, and, while ſhe ſuffered on his account much reſtraint and ſeverity from a jealous huſband, continued to give him every mark of regard which, without a direct breach of her matrimonial vow, ſhe could beſtow upon him. Such is the hypotheſis of the author of the *Mémoires pour la Vie de* PETRARQUE, on the ſubject of the loves of PETRARCH and of LAURA; and the eſtabliſhment of this hypotheſis, ſo injurious to the honour of both, is, in fact, the main ſcope of that moſt elaborate work.

THE principle of ſympathy is a noble part of the conſtitution of the human mind; and is, perhaps, the baſis of all the ſocial affections. In forming our opinions of the characters and conduct of other men, we involuntarily place ourſelves in their ſituation, and we judge of them as we ſhould wiſh to be judged ourſelves in ſimilar circumſtances. Hence, in every doubtful caſe, where the conduct of another is found to admit of oppoſite conſtructions, a candid mind will ever give its deciſion on the ſide of virtue and of honour. Hence, likewife, the warmeft indignation arifes in every ingenuous breaft, on obferving in others a violation of this rule of candour, in a propenfity to form unfavourable opinions of conduct or of character; we are prompted eagerly to fcrutinize the foundations of fuch illiberal opinions; and we conceive it a duty we owe to virtue and to honour, a tafk enjoined us by the refpect due to our common nature, to refute the calumny, expofe the artifices of the aggreffor, and reftore the injured to his juft eftimation.

PREVIOUSLY to the appearance of the work of the Abbé de SADE, thofe authors who had written the Life of PETRARCH, had, in treating of LAURA, univerfally acknowledged, that much uncertainty prevailed with regard to the real name, family and rank of this celebrated perfonage. In general, however, we find but two different opinions on this fubject. The one is, that her parents were of an honourable family in Provence; and that her father* refided at a fmall country feat or village in the territory of Avignon, near to the fources of the Sorga: the other, that fhe was fprung from the houfe of SADE, a daughter of that family, which is of an ancient date, and confiderable rank in the city of Avignon. The former of thefe opinions, which has been adopted by almoft all the Italian authors, is founded on a variety of paffages in the writings of the poet himfelf, of which I fhall afterwards take particular notice. The latter opinion, which

* To this gentleman VELUTELLO has given the name and title of HENRI CHIABAU, Lord of Cabrieres. This, however, is founded on too flender an authority to entitle it to credit. It is, in fact, only a conjecture of VELUTELLO himfelf, who was at pains to fearch the baptifmal regifter of the parifh in which Vauclufe is fituate; and, finding that a child of the name of LAURA was regiftered as being born to HENRI CHIABAU, Lord of Cabrieres, on the 4th of June 1314, he thence concluded, there being no other regiftration of that name which could poffibly apply to the object of his refearch, that this, for certain, was the miftrefs of PETRARCH.

which is that which chiefly prevails at Avignon, where the poet paſſed a conſiderable part of his life, is founded on the following circumſtances.

In the Ambroſian Library at Milan, there is preſerved a manuſcript copy of Virgil, which is ſaid to have been the property of Petrarch; and on the margins of which are many notes, alleged to be in the handwriting of the poet. One of theſe, which is written on the firſt page of the MS. is in the following words:

* "Laura, propriis virtutibus illuſtris, et meis longùm celebrata carminibus, primùm oculis meis apparuit, ſub primum adoleſcentiæ meæ tempus, anno Domini 1327, die 6. menſis Aprilis, in Eccleſia Sanctæ Claræ Avinioni, horâ matutinâ; et in eâdem civitate, eodem menſe Aprilis, eodem die ſexto, eâdem horâ primâ, anno autem 1348, ab hac luce lux illa ſubtracta eſt; cum ego fortè tunc Veronæ eſſem, heu fati mei neſcius! rumor autem infelix per literas Ludovici mei, me Parmæ reperit, anno eodem, menſe Maio, die 19. manè. Corpus illud caſtiſſimum atque pulcherrimum, in loco *Fratrum Minorum* repoſitum eſt, ipſo die mortis, ad veſperam. Ani-

 "mam

* Translated.

"Laura, illuſtrious by the virtues ſhe poſſeſſed, and celebrated during many years by my verſes, appeared to my eyes, for the firſt time, on the 6th day of April, in the year 1327, at Avignon, in the church of St Claire, at 6 o'clock in the morning. I was then in my early youth. In the ſame town, on the ſame day, and at the ſame hour, in the year 1348, this light, this ſun withdrew from the world. I was then at Verona, ignorant of the calamity that had befallen me. A letter I received from my Ludovico, on the 19th of the following month, brought me the cruel information. Her body, ſo beautiful, ſo pure, was depoſited, on the day of her death, after veſpers, in the church of the Cordeliers. Her ſoul, as Seneca has ſaid of Africanus, I am confident, returned to heaven, from whence it came. For the purpoſe of often dwelling on the ſad remembrance of ſo ſevere a loſs, I have written theſe particulars in a book that comes frequently under my inſpection. I have

" mam quidem ejus, ut de AFRICANO ait SENECA, in cœlum " unde erat rediisse mihi persuadeo. Hoc autem ad acerbam rei " memoriam, amara quâdam dulcedine scribere visum est, hoc " potissimùm loco qui sæpe sub oculis meis redit; ut cogitem " nihil esse debere quod ampliùs mihi placeat in hac vita, et " effracto majori laqueo, tempus esse de Babylone fugiendi, " crebra horum inspectione, ac fugacissimæ ætatis æstimatione " commoneat. Quod prævià DEI gratià facile erit, præteriti " temporis curas supervacuas, spes inanes, et inexpectatos exitus " acriter et viriliter cogitanti."

THE evidence of this note, supposing it authentic, positively fixes the burial-place of PETRARCH's LAURA to have been in the church of the *Fratres Minores*, or Cordeliers, at Avignon. At the distance of near two hundred years from the death of LAURA, (*anno* 1533,) a Florentine gentleman, of the name of MANELLI, being at that time at Avignon, amused himself in making researches into every particular relative to the history of PETRARCH and of LAURA; and, with the aid of two others, MAURICE de SEVES * and a M. BONTEMPS, who were instiga- [illegible] ted

have thus prepared for myself a pleasure mingled with pain. My loss, ever present to my memory, will teach me, that there is no longer any thing in this life which can afford me delight: That it is now time that I should renounce Babylon, since the chain which bound me to it with so tender an attachment, is broken. Nor will this, with the assistance of Almighty GOD, be difficult. My mind, turning to the past, will set before me all the superfluous cares that have engaged me; all the deceitful hopes that I have entertained; and the unexpected and afflicting consequences of all my projects."

* PROBABLY MAURICE SCEVE, a French poet, cotemporary with CLEMENT MAROT, of whose composition there are some pieces in the 4to edition of the works of MAROT, published at the Hague, 1731, by the Abbé LANGLET du FRESNOY. (under the fictitious name of the Chevalier GORDON de PERCEL).

ted by a like curiofity, examined every quarter of this church of the Cordeliers, in the hope of tracing fome memorial of LAURA, whom, on the evidence of the manufcript note on *Virgil*, they believed to have been there interred. In one of the chapels of that church, called *Capella della Croce*, in which was the burial-place of the family of SADE, they found a large flat ftone, which bore no infcription whatever. This, if it afforded no indication of being the grave of LAURA, at leaft gave no proof to the contrary; they therefore opened the grave in queft of further evidence. At firft they perceived nothing but earth, intermixed with fmall bones, among which was an entire jawbone. Examining, however, more minutely, they difcovered a little cafket of lead, faftened with a brafs wire, on opening which they found a piece of parchment, folded, and fealed with green wax, together with a medal of bronze, on the one fide of which was the figure of a very little woman, *(figura d'una donna picciolifflma)*, in the attitude of uncovering her bofom with both her hands; and around, in the way of legend, were only thefe four letters, M. L. M. I.

IN endeavouring to explain this infcription, it occurred to MAURICE de SEVES, that the four letters might probably be thus interpreted, *Madonna* LAURA *morta jace.* This was plainly nothing more than conjecture; and, had the tomb contained no other evidence of a more decifive nature, all hitherto difcovered had been of little confequence to afcertain the object of inquiry. The fealed parchment was, therefore, next examined; and, although it is owned that the writing was at firft quite illegible, the characters being fo defaced, (as well they might, after lying two hundred years in the diffolved materials of a human body), yet MAURICE de SEVES, by examining it expofed to the ftrong rays of the fun, is faid to have at laft made out diftinctly

ftinctly the following fonnet, of which he took a fair tranfcript *:

Qui ripofan quei cafte e felici offa
Di quella alma gentile e fola in terra.
Afpro't dur faffo hor ben teco hai foterra
El vero honor la fama e belta fcoffa.
Morte ha del verde Lauro fvelta e moffa
Frefca radice, e il premio de mia guerra
Di quattro luftri ; e piu fe anchor non erra
Mio penfer trifto ; e il chiude in pocha foffa.
Felice pianta ; in borgo de Avignone
Nacque e mori ; e qui con ella jace
La penna, el ftil, l'inchioftro e la ragione.
O delicate membra, O viva face
Che anchor me cuoci e ftruggi, inginocchione
Ciafcun preghi IL SIGNOR *te accepti in pace.*

THIS

* THE fonnet is here given exactly, as to orthography and punctuation, from the copy which the Abbé de SADE fays he took from the original.

THUS almoft literally tranflated :

Here now repofe thofe chafte, thofe bleft remains
Of that moft gentle fpirit, fole in earth!
Harfh monumental ftone, that here confin'ft
True honour, fame and beauty, all o'erthrown!
Death has deftroy'd that Laurel green, and torn
Its tender roots; and all the noble meed
Of my long warfare, paffing (if aright
My melancholy reckoning holds) four luftres.
O happy plant! Avignon's favour'd foil
Has feen thee fpring and die;—and here with thee
Thy poet's pen, and mufe, and genius lies.
O lovely, beauteous limbs! O fire divine,
That even in death haft power to melt the foul!
Heaven be thy portion, peace with GOD on high!

THIS ſonnet is apparently written in the perſon of PETRARCH; but being, in the judgment of BEMBO, BECCATELLI, MURATORI, and the beſt of the Italian critics, very inferior, in point of merit, to the other compoſitions of the poet, they have ſuppoſed it the work of another, who had here aſſumed his character, in compoſing an epitaph in honour of LAURA. It is remarked, from the evidence of the note on VIRGIL, that PETRARCH was at Parma at the time when LAURA was interred at Avignon; and therefore, that this inſcription, though written under his character, could not poſſibly be of his compoſition. If, however, ſuch an inſcription was actually found in 1533, in the tomb of the church of the Cordeliers, whoever was its author, it would ſeem very clearly to indicate, that this was in reality the grave of PETRARCH'S LAURA; and the place of her interment, being a chapel erected by the houſe of SADE as a burial-place for all of their family, a ſtrong confirmation thence ariſes, of the tradition current at Avignon, that LAURA was of that family; and a reaſonable foundation ſeems to be laid for that hypotheſis of the author of the *Mémoires*, that ſhe was the ſame perſon with LAURA de NOVES, who was married to HUGH de SADE, and from whom the whole of the branches of that family now exiſting are lineally deſcended.

THERE are, however, many circumſtances that tend to bring into doubt, or rather that ſeem entirely to confute, this fundamental fact, that LAURA either died or was buried at Avignon.

IF we admit the evidence of the note on VIRGIL, LAURA was *born* at Avignon, and *died* in the ſame place. The works of the poet, indeed, contain the moſt poſitive information that LAURA *died* in the ſame place where ſhe was *born*, and where ſhe had paſſed the greateſt part of her life; but they likewiſe furniſh evidence that this place was not Avignon, but ſome ſmall village or country-ſeat in the territory of Avignon, near to the ſource of the *Sorga*, or the fountain of *Vaucluſe*.

IN

In the *Trionfo della Morte*, Part 2. the poet feigns that Laura, on the night after her death, appeared to him in a viſion: and, in the courſe of a long converſation, in which ſhe acknowledges, that ſhe had ever felt for him a mutual paſſion, and endeavours to ſatisfy him, that every ſingularity of her conduct, however harſhly he might at the time have judged it, was prompted by the ſincerity of her affection for him; ſhe ſays in one paſſage,

In tutte l'altre coſe aſſai beata,
In una ſola a me ſteſſa diſpiacqui;
Che 'n troppo umil terren *mi trovai nata:*
Duolmi ancor veramente ch'io non nacqui
Almen più preſſo al tuo fiorito nido;
Ma aſſai fu bel paeſe ov' io ti piacqui.

"Fortunate enough I was in other reſpects: this only I "regretted, that the place of my birth was too humble: at leaſt "I had cauſe to repine at this circumſtance, that it was not "nearer to the beautiful country of thy nativity. Yet that "region was indeed ſufficiently beautiful where I had the hap-"pineſs to pleaſe thee." It was impoſſible that Laura could have termed the city of Avignon *umil terreno*, or that ſhe could have been aſhamed of it as the place of her birth. At that time Avignon was the Papal reſidence, and one of the moſt ſplendid cities in the ſouth of Europe; a city, indeed, where luxury and corruption of manners had attained to ſuch a height, that Petrarch himſelf characterizes it by the epithet of the *Gallic Babylon* *.

In the fourth ſonnet of the 1ſt Part of his *Sonetti e Canzoni*, the poet has the following remarkable alluſions, which may per-

haps

* Epist. lib. ſine tit. Ep. 16.

haps be thought to border a little on impiety. Divine Providence, he obferves, has thought fit to difplay its wonders, by choofing its moft illuftrious inftruments, either from a fervile condition, or from a low and obfcure place of origin. Of this he gives for examples, the mean occupations of the Apoftles, the obfcurity of the birth-place of our Saviour, who, difdaining imperial Rome, chofe an inconfiderable town of Judæa for the place of his nativity; and laftly, the humble origin of the matchlefs LAURA, that refplendent fun of beauty, who rofe upon the world from a fmall obfcure village:

Ed or di picciol borgo un fol n' ha dato,
Tal, che natura e 'l luogo fi ringrazia,
Onde sì bella donna al mondo nacque. Son. 4. Part. 1.

THE fituation of this *picciol borgo*, or fmall village, is likewife diftinctly pointed out. It was in the neighbourhood of the hills that rife above the fountain of *Vauclufe*, the fpring of the *Sorga*. The poet fends a prefent to a friend, of two birds which he had caught, and he accompanies the gift with a fonnet, in which the birds are fuppofed thus to addrefs the perfon to whom they are fent:

A piè de' colli, ove la bella vefta
Prefe delle terrene membra pria
La donna, che colui ch' a te ne 'nvia,
Speffo dal fonno lagrimando defta;
Libere in pace paffavam, &c. Son. 8. Part. 1.

" At the foot of thofe hills where that fair nymph was born,
" who oft caufes him who fends this prefent to pafs the fleeplefs
" night in tears, we once enjoyed the fweets of liberty," &c.

In the 37th ſonnet of the ſecond Part, PETRARCH, addreſſing himſelf to the ſpirit of LAURA, thus expreſſes himſelf:

Mira 'l gran ſaſſo donde Sorga naſce,
E vedravi un che ſol tra l'erbe e l'acque
Di tua memoria e di dolor ſi paſce:
Ove giace 'l tuo albergo e dove nacque
Il noſtro amor.——

" Behold," ſays he, " yonder great rock from whence the Sorga " ſprings, and there thou wilt ſee a ſolitary being, who, amidſt " the green fields and ſtreams, feeds on thy remembrance and " on his own ſorrows. 'Twas there thy habitation lay; and " there our loves began."——

In the 155th ſonnet of the 1ſt Part, *(Almo Sol,)*, the poet, addreſſing himſelf to the ſun, complains, that when his light is withdrawn, and the night comes on, he is deprived of what he moſt delights in, the ſhadow that falls from that low hill, " where " ſparkles that ſweet fire; where from a ſlender twig the beau- " teous laurel grew;" and he laments, that the darkneſs hides from his eyes " that bleſſed ſpot, where, with its miſtreſs, his heart " for ever dwells."

O ſole,——
—Fuggendo mi toi quel ch' i' più bramo:
L' ombra che cade da quell' umil colle,
Ove ſfavilla il mio ſoave fuoco,
Ove 'l gran lauro fu picciola verga;
Creſcendo, mentr' io parlo; a gli occhi tolle
La dolce viſta del beato loco,
Ove 'l mio cor con la ſua donna alberga.

So, in the 40th ſonnet of the 2d Part, the poet ſays,

Quella per cui con Sorga ho cangiat' l Arno.—

a

a paſſage clearly marking, that his preference for the Sorga aroſe from LAURA's reſiding on its banks.

THE 17th Canzone, Part. 1., *(Di penſier in penſier)*, furniſhes a ſimilar inference:

——————Oltra quell' alpe,
Là, dove 'l ciel è più ſereno e lieto,
Mi rivedrai ſovr' un ruſcel corrente,
Ove l'aura ſi ſente
D' un freſco ed odorifero laureto:
Ivi è 'l mio cor, e quella che 'l m' involа.

" Beyond thoſe hills where the air is mild and ſerene; beſide " the ruſhing brook, where the gale wafts from yon freſh laurel's " leaves its rich perfume; 'tis there ſhe dwells who reft me of " my heart." On this paſſage GESUALDO remarks, in a note, " Era preſſo al fiume, ov' albergava Madonna LAURA, che ſi " dolce ſpirava." " It was near the river where lay the reſidence " of LAURA, that laurel whoſe fragrance was to him ſo delight" ful."

IN the 52d ſonnet of the 2d Part, the poet thus deſcribes his feelings on returning to Vaucluſe, after the death of LAURA:

Sento l'aura mia antica; e i dolci colli
Veggio apparir onde 'l bel lume nacque
Che tenne gli occhi miei mentr 'al ciel piacque
Bramoſi e lieti; or li tien triſti e molli.—
Vedove l'erbe, e torbide ſon l'acque;
E voto e freddo 'l nido in ch' ella giacque.—

" Once more I breathe that dear accuſtomed air: Once more I " view thoſe beautiful hills, whence that reſplendent light aroſe " which once gave joy to theſe eyes; while heaven ſo pleaſed to " bleſs me with her ſight; but now, alas! has ſteeped them for

" ever in tears.——Widowed are thoſe green fields,—and turbid
" is that ſtream,—and void and cold the neſt in which ſhe
" lay."

MANY paſſages of the poet's writings likewiſe very clearly indicate that LAURA died in the ſame place where ſhe was born, and where ſhe had paſſed the greateſt part of her life.

THUS, in the 53d ſonnet of the 2d Part:

È queſto 'l nido in che la mia Fenice
Miſe l' aurate e le purpuree penne;
Che ſotto le ſue ali il mio cor tenne.—
E me laſciato hai quì miſero e ſolo,
Tal, che pien di duol ſempre al loco torno
Che per te conſecrato onoro e colo.
Veggendo a' colli oſcura notte intorno
Onde prendeſti al ciel l' ultimo volo.—

" Here is the neſt in which my Phœnix lay, and couched her
" golden and empurpled wings;—and here ſhe left me miſerable
" and alone. Thus, for ever a prey to grief, I turn me to that
" dear ſpot of earth which ſhe has conſecrated, and which, on
" that account, I venerate and honour: I view thoſe hills, now
" dark and deſolate, from whence ſhe winged her flight to
" heaven."

So, likewiſe, in that beautiful ſonnet on his return to Vaucluſe, *Valle, che de' lamenti miei ſe' piena*, &c. he gives the moſt explicit intimation, that the grave of LAURA was in that very place, and amidſt the ſame ſcenes where he ſo often had enjoyed the happineſs of her ſociety;

Valle,

Valle, che de' lamenti miei se' piena ;
Fiume che spesso del mio pianger cresci ;
Fere silvestre, vaghi augelli, e pesci,
Che l' una e l' altra verde riva affrena ;
Aria de' miei sospir calda e serena ;
Dolce sentier, che sì amaro riesci ;
Colle, che mi piacesti, or mi rincresci,
Ov' ancor per usanza amor mi mena ;
Ben riconosco in voi l' usate forme,
Non, lasso ! in me ; che da sì lieta vita
Son fatto albergo d'infinita doglia.
Quinci vedea 'l mio bene ; e per quest 'orme
Torno a veder, ond' al ciel nuda è gita
Lasciando in terra la sua bella spoglia. Son. 32. Part. 2.

Thou lonely vale, where in the fleeting years
Of tender youth I breath'd my am'rous pain ;
Thou brook, whose silver stream receiv'd my tears,
Thy murmurs joining to my sorrowing strain ;
I come, to visit all my former haunts again !

O green-clad hills, familiar to my sight !
O well-known paths, where oft I wont to rove,
Musing the tender accents of my love !
Long use, and sad remembrance now invite,
Again to view the scenes which once could give delight !

Yes, ye are still the same !—though here I meet
No more that angel form, which beauty shed
On universal nature ! Her dear feet
Oft trod your paths :—here rests in hallow'd earth her head !

In one of his Latin eclogues, in which the poet celebrates LAURA under the fictitious name of GALATEA, three nymphs, NIOBE, FUSCA and FULGIDA, are introduced in conversation, and one of them asks the others to point out to her the place where

where GALATEA was buried; which FULGIDA does in these words:

Carpe iter, qua nodosis impexa capistris
Colla boum, crebrasque canum sub limine parvo
Videris excubias, gilvosque ad claustra molossos:
Hic locus tua damna tegit; jamque aspice contrà,
Hic GALATEA *sita est.*———

"Take your way yonder, where you will see the oxen yoked
"by the neck, and the watch-dogs guarding the entry of a small
"house. That hides from your sight what you are in search
"of; for on the other side of it is the burial-place of GALA-
"TEA," &c.

IT is evident that the passages above quoted from the writings of the poet himself, ascertaining both the place of LAURA's birth and of her burial to have been in the country, in some small village or villa in the neighbourhood of the hills, and of the source of the Sorga, stand in direct contradiction to the manuscript note on *Virgil**, and to the sonnet said to have been found in the grave

* THE note on *Virgil*, when contrasted with many passages of the poet's writings, exhibits likewise other intrinsic evidences of forgery. This note bears positive testimony, that PETRARCH saw LAURA for the first time at matin-prayers in the church of St Claire at Avignon. But, from many passages of the poet's writings, it appears, that his first interview with LAURA was in a solitary walk in the fields. Thus, in the 8th *Ballata*, Part. 1. (*Nova angeletta*):

Nova angeletta sovra l' ale accorta
Scese dal cielo in su la fresca riva,
Là 'nd io passava sol per mio destino:
Poi che senza compagna e senza scorta
Mi vide; un laccio che di seta ordiva
Tese fra l' erba, ond' è verde 'l cammino:
Allor fui preso. ——

So,

grave of the church of the Cordeliers. The queſtion, then, is, To which of the oppoſing proofs are we to give our faith? To this queſtion we ſhall certainly not long heſitate to give an anſwer, when we conſider, that the one claſs of proofs admits of no ſuſpicion of fabrication or impoſture; while the other is extremely ſuſpicious, and may be altogether a forgery. It is impoſſible to ſuppoſe, that numberleſs paſſages, interſperſed through the works of PETRARCH, indicating the place of LAURA's birth and death, all naturally connected with the ſubject treated of, though ſome furniſhing their evidence not in poſitive terms, but only by inference, ſhould every one of them be fabricated; and that the forger ſhould have been able to

So, likewiſe, ſonnet 157. Part. 1.:

Una candida cerva ſopra l'erba
Verde, m' apparve con duo corna d'oro,
Fra due riviere à l'ombra d'un alloro.

And yet more clearly, in the 3d of his Latin eclogues:

DAPHNE, *ego te ſolam deſerto in littore primùm*
Aſpexi; dubius hominemne Deamne viderem.

The evidence ariſing from theſe paſſages, that PETRARCH's firſt interview with his miſtreſs was not in a church, but in the fields, is thus controverted by the Abbé de SADE. "Je ſuis perſuadé que ces allégories qui préſentent des images riantes de la campagne doivent être entendues des dehors d'Avignon, ou des charmes du printemps, qui eſt la ſaiſon dans laquelle PETRARQUE vit LAURE pour la première fois. Convenoit-il qu'il parlât de l'égliſe de S^te^ CLAIRE en rapellant la première époque de ſon amour? Un poëte qui parle de ces choſes-là, eſt bien-aiſe d'égayer la ſcene; il n'ira pas la placer dans une égliſe: et ſur une choſe ſi peu importante en elle-même, il ne doit pas s'aſſujettir à la vérité hiſtorique; il ſe permet toutes les fictions qui peuvent rendre ſes vers agréables." (Notes, vol. i. p. 57.). If this mode of reaſoning is to be admitted, it is equally effectual againſt thoſe paſſages which the Abbé has brought in ſupport of his own hypotheſis, as thoſe which militate againſt it; and tends, indeed, to invalidate the whole evidence brought from the poems of PETRARCH in proof of any part of his hiſtory.

to inſert all theſe fabricated paſſages in every one of the manuſcripts of the author's works which are to be found in Europe. Nor even, ſhould we allow this ſtrange undertaking to have been practicable, is it poſſible to figure a motive capable of inducing to the attempt? For, what intereſt had any man to ſhew, that the LAURA of PETRARCH was born and died in an obſcure reſidence in the country, while nothing more was proved to aſcertain her origin or connections? But this reaſoning will not apply to thoſe proofs which are brought to ſhew that LAURA was born and died at Avignon. The manuſcript note on *Virgil*, and the ſonnet ſaid to have been found in the grave, ſtand evidently in a very different predicament. Here the forgery was eaſy; the motive to it ſtrong and alluring. At the diſtance of two hundred years from the death of PETRARCH, it was no difficult matter for the poſſeſſor of this manuſcript of *Virgil*, which is ſaid to exhibit a great number of notes on its margin, in the genuine handwriting of the poet, to have fabricated one additional note in imitation of that handwriting, of which he had before him ſo many ſpecimens; nor did it require a great meaſure of ingenuity to compoſe a ſingle ſonnet, written in the perſon of PETRARCH, and that too perhaps of equal merit with many of his genuine compoſitions. Of both forgeries the motive was probably the ſame, which has inſtigated the Abbé de SADE to the compilation of his elaborate work, the deſire of vindicating to this houſe the relation to ſo celebrated a perſonage as the LAURA of PETRARCH: though, as we ſhall preſently ſee, the Abbé's hypotheſis goes beyond the pretenſions of his anceſtors, and much further than the evidence of this note and ſonnet can conduct him; even ſuppoſing both were of the moſt certain authenticity.

WE have already remarked, that the purpoſe of the ſearch into the graves in the church of the Cordeliers, was to aſcertain the

the truth of that report, which gave the LAURA of PETRARCH to the houfe of SADE. Two or three perfons, zealous in this refearch, unite their endeavours for that purpofe, and their inveftigation is naturally directed to the family burial-place, which is a chapel of that church termed *Capella della Croce*. Among other monuments in this chapel a graveftone is difcovered, which bears no infcription; and, on opening the grave, fome bones are found, and a cafket, containing a medal and a parchment. The medal has on it the figure of a very little woman. Here, fay our antiquaries, is, in all probability, the figure of LAURA *. Around the figure are four letters, M. L. M. I. This, fays one of them, (MAURICE de SEVES), is undoubtedly to be interpreted, *Madonna* LAURA *morta jace;* though it is confeffed that monumental infcriptions and the legends of medals are rarely found in the vernacular tongue; and though M. L. M. I. might have been read twenty different ways, with as much plaufibility as in the way that MAURICE choofes to read it. The point, then, being already fettled that this muft be the grave of LAURA, the parchment, to be fure, muft contribute its relative evidence to the fame effect. This parchment, however, is found at firft to be utterly illegible: a fact not at all furprifing. The wonder is, that the parchment itfelf fhould have been difcoverable: for, parchment being an animal fubftance, muft, one fhould naturally fuppofe, have gone into total diffolution, in much lefs time than two hundred years, when inclofed in the fame coffin with a putrefying carcafe, from the juices of which a leaden box, fhut only with a brafs wire, would be a very infufficient protection. But the parchment is faid to have been actually found. It could not, however, be read without the aid

 of

* WE do not recollect any paffage of the writings of PETRARCH which marks that LAURA was of fmall ftature: nor is it eafy to conceive how the medal, reprefenting only a fingle figure, could accurately determine the fize or ftature of the perfon reprefented.

of the ſtrongeſt light. It therefore was not read in the little dark chapel of the church *. MAURICE de SEVES, in all probability, puts it in his pocket, and takes it home for his deliberate examination. He had already given his opinion on the legend of the medal, which aſcertained LAURA's relation to the grave. His credit, therefore, as an antiquary, is at ſtake to ſupport this opinion, by the further evidence of the rotten parchment; and next day he produces a fairly written ſonnet, which he declares to have copied from the original, of which, with much pains, he had at length been able to decypher the characters. The original is never afterwards heard of. A copy, written likewiſe on parchment, is preſerved at this day among the archives of the family of SADE, which they pretend to be the original, found in the grave, and regard accordingly as a moſt precious document. But, though mutilated a little to give it the air of antiquity, as its writing is extremely diſtinct, and may be eaſily read, that circumſtance of itſelf demonſtrates, that it cannot be the parchment which, even two hundred years ago, was not without the utmoſt difficulty decyphered by MAURICE de SEVES, with the aid of a very ſtrong light.

THE ſonnet or epitaph itſelf is generally eſteemed a poor compoſition; and the Italian authors are indignant that it ſhould ever have been ſuſpected to have come from the hand of PETRARCH: but PETRARCH's compoſitions are not all of tranſcendent merit; and its intrinſic character is not, perhaps, of itſelf, ſufficient to bring its authenticity into doubt. The medal and the lead box are ſaid to have been ſeen about the middle of the preſent century, in the poſſeſſion of the monks of the Convent of St CLAIRE; but there they are no longer; and the Abbé

* "SACELLUM, in quo illud (ſepulchrum) videtur, obſcurum eſt. Siniſtra ingreſſus habet altare muro adſtructum, ante quod, ſub grandi ſaxo, ſine omni ornatu et inſcriptione, LAURA cubat." PHIL. THOMASINI, PETR. *Rediviv.* p. 111.

Abbé de SADE mentions a ſhrewd conjecture of the Superior of the Convent, "that they had been ſold to ſome *Seigneur Anglois.*" Monks are very dexterous in the fabrication of all ſorts of relics, and it was no bad policy of the *Fratres Minores*, to attract the notice of ſtrangers to their convent, by the exhibition of a medal and a lead box, the evidences of a curious and diſputed point of hiſtory: nor is it at all improbable, that afterwards a knaviſh friar, more ſtudious of his own intereſt than that of his convent, might have purloined thoſe precious relics, and got his own price for them.

SUCH is the hiſtory of thoſe celebrated documents, which are ſaid to prove the place of LAURA's birth and death to have been Avignon, and to aſcertain that her grave was in the chapel of the church of the Cordeliers in that city, and in the burial-place of the family of SADE. I have ſtated fairly the whole of this evidence, which reſts entirely on the manuſcript note on *Virgil* and this miſerable ſonnet; and I leave that evidence to be balanced by thoſe numberleſs paſſages of the poet's undoubted and authentic writings, which moſt unequivocally aſſert, that LAURA was neither born, nor died, nor was buried, at Avignon; but that ſhe was born in a ſmall village or country reſidence in the neighbourhood of Vaucluſe, where ſhe paſſed her life in tranquil and humble retirement *; that ſhe died there, and was buried in the ſame place. If the proofs on both ſides

T 2 are

* *In nobil ſangue vita humile e queta,* Son. 180. Part. 1.

Non la conobbe il mondo mentre l' hebbe:
Conobbil' io ch' a pianger qui rimaſi. Son. 67. Part. 2.

With what propriety or conſiſtence with truth could the poet have thus expreſſed himſelf of LAURA de NOVES, the wife of a perſon of high rank, and who had paſſed the whole of her life in all the gaiety and ſplendour of the court of Avignon? Still leſs would the pious PETRARCH have borrowed a Scripture expreſſion, addreſſed to the Saviour of the world, (St JOHN's *Goſpel*, chap. xvii. v. 25.) and applied it, falſely too, to the object of an adulterous paſſion.

are impartially weighed, I am much deceived, indeed, if any perſon competent to judge of this ſpecies of evidence, will heſitate to decide that the latter is infinitely preponderating.

The arguments by which the Abbé de Sade has endeavoured to invalidate the poſitive evidence contained in the writings of Petrarch, that Laura was born, lived and died in an obſcure retreat in the country, are not undeſerving of attention; as they are ſtrong examples of that laboured ſophiſtry, which is uſually employed when one endeavours to ſupport a weak or falſe hypotheſis.

Avignon, ſays the Abbé, though a city of ſome celebrity, was far inferior in ſplendour to many of the Italian cities; and the mind of Petrarch, being filled with the idea of their magnificence, might not unnaturally have termed the former *picciol borgo*. Unfortunately, Petrarch at this time knew nothing of the ſplendid cities of Italy, but by deſcription, He was born at *Arezzo*, an inconſiderable town in the Florentine territory; and he was no more than ſeven years of age, when he, together with his whole family, removed to Avignon. This city, therefore, was at any rate the moſt ſplendid he had ever ſeen; unleſs perhaps he had had a tranſient glimpſe of Florence on his journey. It could, indeed, be only a diſtant proſpect; for his father was then in a ſtate of baniſhment from that capital, and durſt not enter it. We may judge, then, with what propriety Petrarch could have given the epithet of *picciol borgo* to Avignon, then the ſeat of the Papal reſidence, the perpetual reſort of the moſt ſplendid embaſſies from all the ſovereigns of Europe; and rendered illuſtrious, as the Abbé de Sade himſelf informs us, by frequent viſits of the moſt celebrated princes, many of whom had actually built palaces there *; a city, too, where, as the

* "Les Princes, non contents d'envoyer des Ambaſſadeurs au Pape dans les moindres occaſions, ne dédaignoient pas d'aller ſouvent en perſonne à Avignon, traiter avec

the same author acknowledges, the vaſt concourſe of ſtrangers had neceſſarily introduced every ſpecies of voluptuouſneſs and debauchery *.

But ſuppoſing for a moment that Avignon, thus populous, ſplendid and luxurious, really ſeemed a *picciol borgo* in the eyes of Petrarch, whoſe mind, as the Abbé ſays, was filled with the great idea of the cities of Italy, (what we have indeed ſhewn to be only an *idea*, as he had never ſeen them), we cannot allow the ſame falſe eſtimate in the mind of Laura, with reſpect to the moſt conſiderable city which, for certain, *ſhe* had ever ſeen. If ſhe had been born at Avignon, it certainly muſt have merited in her eyes a very different eſtimation from that of an *umil terreno*, which ſhe was aſhamed to avow as the place of her nativity. Yet ſuch, we have ſeen, the poet deſcribes to have been her eſtimation of the place of her birth; the only circumſtance, ſhe ſays, reſpecting her origin, of which ſhe had reaſon to be aſhamed †.

But the *picciol borgo* and the *umil terreno*, ſays the Abbé, was not properly the city of Avignon itſelf; it was only a ſuburb, a *fauxbourg* of that city, and thus might well have merited thoſe

epithets.

avec lui des grands interêts de leurs états. Le Roi de Mayorque et le Dauphin de Viennois y avoient même des demeures fixes.—Le palais du Roi de Naples étoit où l'on voit à-préſent le monaſtère de S[te] Ursule, qu'on appelle *les Royales*, parcequ'elles habitent le palais d'un Roi. Un grand concours d'étrangers dans une ville, y entraine néceſſairement la licence et la débauche," &c. *Mem. de* Pet. tom. i. p. 68.

* Of the very great populouſneſs of this city at that time, we may form a general idea from a remarkable and melancholy proof. The peſtilence which ravaged Italy and the ſouth of France in the year 1348, and of which Boccacio has given, in the introduction to his *Decamerone*, a moſt eloquent and impreſſive deſcription, cut off, in the ſpace of three months, a hundred and twenty thouſand of the inhabitants of Avignon. *Hiſt. Piſtol. Mem. de* Petr. tom. ii. p. 456.

† See *ſuprà*, p. 132.

epithets. To this we ſhall anſwer by a ſingle queſtion, which has been well put by M. de la Bastie to thoſe who contend for Laura's being born at Avignon: " Que dirions-nous d'un " poëte, qui addreſſant la parole à quelqu'un qui ſeroit né dans " le fauxbourg Ste Germain, croiroit devoir le plaindre de ce " qu'il n'eſt pas venu au monde dans une ville conſidérable? " Ceux qui naiſſent dans ce fauxbourg, ſont-ils moins nés à " Paris?"

But, continues the Abbé, we find, in a fragment which is printed at the end of all the editions of the works of Petrarch, the following expreſſion:

Dove Sorga e Durenza in maggior vaſo
Congiungon le lor chiare e torbide acque;—
Ivi, ond' a gli occhi miei el bel lume nacque.—

" That beautiful luminary was born where the *Sorga* and the " *Durance* unite their clear and turbid ſtreams in a larger chan- " nel." Can any thing, ſays he, mark more preciſely the ſitua- tion of Avignon?

The anſwer is, *firſt*, The authenticity of this fragment is not admitted. It is not found in the beſt manuſcripts of the works of Petrarch, nor have Velutello, Gesualdo, or Bembo, given it a place in their editions. In the ſplendid edition printed at Venice in 1756, it is found in an appendix, which the editor entitles, " Giunta d'alcune compoſitione del Petrarca che ſi " dicono da lui rifiutate." It is therefore a document of no authority whatever. But, *ſecondly*, Even ſuppoſing it genuine, the deſcription there given does by no means mark preciſely the ſituation of Avignon. The *Sorga* and the *Durance* do not join their ſtreams at Avignon. The Sorga falls into the Rhône five miles above Avignon, and the Durance ſix miles below that city. Theſe rivers, therefore, though they unite their ſtreams by both falling

falling into the Rhône, do not mark out Avignon as the point of junction, but rather mark the boundary of one side of a district termed the *Venaissin*, which comprehends the country for several miles adjacent to Avignon; and therefore, the description is equally indicative of *Vauclufe* and of *Cabrieres*, as it is of Avignon.

BUT this passage, supposing it genuine, will find its best explanation by a similar one, which occurs in the 10th of the poet's Latin eclogues, entitled LAUREA *occidens*; in which he bewails the death of his mistress under his favourite allusion of a *Laurel*:

——— Fuit alta remotis,
Silva locis qua se diversis montibus acti
Sorga nitens Rhodano, pallensque Ruentia miscent.
Hic mihi, quo fueram Tusco translatus ab Arno,
Sic hominum res fata rotant, fuit aridulum rus,
Dum colui indignè, atque operi successit egestas.——
Verum inter scopulos nodosaque robora quercûs,
Creverat ad ripam fluvii pulcherrima Laurus:
Huc rapior.———
Has ego delicias et opes, hæc regna putavi.—

" In a *remote quarter* of that country where the Sorga and Du-
" rance unite their streams, was a thick forest, where, after I
" was removed from the Tuscan vale of *Arno*, I possessed a little
" barren country seat.—Here, amidst the rocks and thickets of
" oak, near the borders of the stream, grew a most beautiful
" Laurel. This favourite object engrossed all my care. In this
" spot was my kingdom, and here I found my supreme delight."
Nothing certainly can more accurately picture the scenery of Vauclufe and its vicinity, where the poet feigns his beautiful Laurel to have sprung, amidst the rocks and thickets.

In the 13th Canzone *(Se 'l penſier)*, the poet addreſſes himſelf to a beautiful ſtream, on the borders of which his miſtreſs was wont frequently to walk. The commentators, in endeavouring to identify the ſcenery to which this poem refers, have, with great appearance of probability, ſuppoſed this rivulet to be the *Coulon*, which runs near to Cabrieres, where LAURA is believed to have dwelt, and conclude it to have been the ſame rivulet in which he had once ſurpriſed his miſtreſs bathing, quite naked, an incident to which he alludes in the firſt *Canzone*. But this ſuppoſition, contradicting his theory, appears to the Abbé de SADE quite unnatural and abſurd. He finds a baſon or pond in a garden cloſe by the walls of Avignon, which correſponds to a miracle with every thing here alluded to. As to the rivulet of Coulon, ſays he, it is no leſs than a *mile and a half* diſtant from Cabrieres, a circumſtance which puts its pretenſions out of the queſtion; as this would have been rather too long a walk for a lady, *promenade un peu forte pour une dame;* and PETRARCH himſelf muſt have croſſed a ſteep hill, and walked at leaſt *four miles and a half*, before he could have ſeen her there.

In a ſimilar ſtrain of weak and inconcluſive reaſoning, this author attempts to invalidate the evidence of the ſonnet with which PETRARCH accompanies his preſent of the birds, caught at the foot of thoſe hills where lay the birthplace of LAURA. Part of the city of Avignon, ſays the Abbé, is ſituated on a rocky eminence; and although the foot of that rock is now all built over, and comprehended within the precincts of the city, yet, in thoſe days it *might* have been open ground, and PETRARCH *might* there have amuſed himſelf in fowling, and have caught the birds in queſtion.

So likewiſe in the 184th ſonnet *(Il cantar nuovo)*, where the poet deſcribes the pleaſures of the morning in the country, the valleys reſounding with the ſweet ſong of the birds, and the murmuring

murmuring of the clear rivulets *, and tells, that rising early to hail the morning rays, he had the fortune to see two suns rise at the same moment; the sun that marks the vicissitude of day and night, and LAURA, who at that instant eclipsed his radiance by her own: the Abbé, with his usual facility of perversion, cites this sonnet as a proof that PETRARCH used frequently to walk the streets of Avignon before daybreak, to have the pleasure of seeing his mistress open her windows.

BUT the most amusing instance of this sophistical perversion of an author's clearest expressions, is to be found in the construction which is attempted to be put on the passage above quoted from the Latin eclogue of GALATEA †.

Carpe iter, qua nodosis impexa capistris, &c.

"Take your way yonder, where you will see the oxen yoked "by the neck, and the watch-dogs guarding the entry of a small "house.—On the other side of it is the burial-place of GALA"TEA." This has been ever reckoned as one of the most decisive testimonies, that the burial-place of LAURA was not in the city of Avignon, but in the country. It was necessary, therefore, to take off the force of this strong piece of evidence. It is admitted,

* *Il cantar nuovo, e 'l pianger de gli augelli*
In su'l dì fanno risentir le valli,
E 'l mormorar de' liquidi cristalli
Giù per lucidi freschi rivi e snelli.—
Così mi sveglio a salutar l' aurora,
E 'l sol, ch' è seco; e più l' altro, ond' io fui
Ne prim' anni abbagliato, e sono ancora:
I' gli ho veduti alcun giorno ambedui
Levarsi insieme, e 'n un punto e 'n un hora
Quel far le stelle, e questo sparir lui. Son. 183. Part. 1.

† SEE *suprà*, p. 138.

admitted, on all hands, that LAURA is figured under the name of GALATEA: but the yoked oxen, and the watch-dogs guarding the entry of a ſmall houſe, how can they poſſibly be applied to a church in the city of Avignon, or indeed to any thing elſe than a farm-houſe in the country? Nothing is more clear, ſay the advocates for this hypotheſis; the *oxen* yoked by the neck are the *friars* of the convent, the *fratres minores, qui ferunt jugum obedientiæ, laboris et religionis;* the *dogs* guarding the door are the ſame *friars*, (both dogs and oxen), who may well be termed dogs, becauſe they *bark* ſo much in their ſermons; *frequentes vigilias canum, prædicantium ſcilicet et latrantium.*

I PRESUME I have given a ſufficient ſpecimen of the Abbé de SADE's ingenuity in accommodating and bending to his own purpoſe thoſe paſſages of the poet's writings which are in the moſt direct contradiction to his hypotheſis; nor would an unprejudiced critic deſire a more ſatisfying proof of its futility, than the miſerable ſhifts to which its defenders are put in order to ſupport it. This hypotheſis, however, muſt yet be more thoroughly canvaſſed; and, for that purpoſe, I begin with a brief detail of its whole particulars.

THE Abbé de SADE endeavours to prove, that the miſtreſs of PETRARCH was LAURA de NOVES, the daughter of AUDIBERT de NOVES, a gentleman of noble birth in Provence; that ſhe was born at Avignon, in 1307 or 1308; that ſhe was married, in 1325, to HUGH de SADE, the repreſentative of a very ancient and honourable family in the territory of Avignon, to whom ſhe bore eleven children; that ſhe died at Avignon, in the year 1348, and was interred in the burial-place of the houſe of SADE, in the Church of the Cordeliers in that city. The Abbé, who is himſelf a younger ſon of that family, has proved the whole of thoſe leading circumſtances, in the hiſtory of his *progenitrix* LAURA de NOVES, by authentic documents in the archives of the houſe of SADE; and he has particularly eſtabliſhed the cer-

tainty

tainty of thefe facts, " that this LAURA, *the wife of* HUGH *de* " SADE, was born, died, and was buried at Avignon." If, therefore, it is equally demonftrable that the LAURA of PETRARCH was neither born, died, nor was buried at Avignon, it is plain fhe was a different perfon from LAURA de NOVES. The evidence of thefe latter facts we have already feen. It is clear, pofitive, and unambiguous. It might, therefore, be fufficient to reft the matter here without farther argument; for an hypothefis muft fall of itfelf, when the main props on which it is built are demolifhed. But I am tempted to go a great deal farther: and, allowing that the queftion as to the place of LAURA's birth and death were ftill a matter of doubt; nay, even admitting, for the fake of argument, that the LAURA of PETRARCH had, according to the tradition current at Avignon, been connected, in fome way or another, with the houfe of SADE, and on that account had been interred in the burial-place of that family; fuppofing, in fhort, the authenticity both of the note on *Virgil*, and the whole ftory of the grave, the medal and the parchment; I fhall now proceed to fhew, and, as I truft, to prove to abfolute conviction, that this LAURA, connected as we fhall fuppofe her with the houfe of SADE, and buried in their family-vault, *was never married.*

AND here, I muft previoufly remark, that the report which, it is allowed, was current at Avignon, that LAURA belonged to the family of SADE, was not that fhe was *connected by marriage* with that family, but that fhe was herfelf a defcendant of that houfe. The opinion that fhe was connected with it by marriage, was never entertained by any one of the family, till it found its origin in the whimfical vanity of this author of the *Mémoires*, the Abbé de SADE, who has laboured, in this voluminous work, to convince the world, that he himfelf is actually fprung from the body of the illuftrious LAURA. That fhe was *a daughter* of the houfe of SADE, had been a tradition, though, as we have

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ſeen, a very ill founded one, for ſome centuries. "The opinion "regarding LAURA," ſays VELUTELLO, "which was current "at Avignon, may have, in a great meaſure, ariſen from one "GABRIEL de SADE, an old nobleman of that city, with whom "I happened to converſe a great deal at two different times, "when I was at Avignon. He ſet forth, that he was deſcend-"ed from one HUGH de SADE, a brother of JOHN, who was "*the father of Madonna* LAURA, whom he underſtood to have "been the ſame who was celebrated by PETRARCH; and he "told me, that this JOHN de SADE had ſome poſſeſſions at "*Graveſons*, about two leagues from Avignon, where he lived "always in ſummer, but paſſed the winter at Avignon; and "that this LAURA, the daughter of JOHN de SADE, was, after "her death, interred in the family burial-place, in the Church "of the Cordeliers. But," continues VELUTELLO, "what con-"vinced me that this old man's ſtory was altogether a fiction, "was, that being aſked at what time this LAURA lived, he ſaid, "that by a certain teſtament which he had ſeen, ſhe muſt have "been a grown woman between the years 1360 and 1370: "Now we are certain," adds VELUTELLO "that PETRARCH's "LAURA died in the year 1348."

I QUOTE this paſſage for two reaſons; *firſt*, To ſhew that VELUTELLO, who lived within 150 years of the time of PETRARCH, who had been at the utmoſt pains to inveſtigate every trace of the hiſtory of LAURA, (a ſubject which he has treated in a ſeparate diſſertation), and who had himſelf converſed with this old gentleman of the family of SADE, from whom he ſuppoſes the report of LAURA's relation to that family to have actually originated, did not himſelf give credit to the ſtory, but conjectured it to be a mere fiction, and the offspring of the old man's vanity; and I mention it, To ſhew, *ſecondly*, that this report, thus traced nearly to its origin, did not make LAURA a *married woman*, or connected with the houſe of SADE by marriage, but

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a *daughter* of one of that family, namely of JOHN de SADE. At this time, therefore, when it may be preſumed the relation in which this celebrated perſon ſtood to their houſe, if there was any truth in it at all, muſt have been better known than it is now, we find there is not the ſmalleſt idea of that hypotheſis, which the preſent repreſentatives of this family are ſo anxious to eſtabliſh, namely, that ſhe was connected with them by marriage. Inſtead of being the wife of a M. de SADE, and the mother of a numerous progeny, we find, on going back two centuries, that the family themſelves believe her to have been a *daughter* of one of that houſe; nor is it diſcovered till the preſent age, which is above 400 years after this lady's death, that ſhe was the *wife* of their anceſtor, the mother of all the ſurviving branches of that houſe; and conſequently, that from her illuſtrious blood is ſprung the whole race, now exiſting, of that family, and among the reſt the ingenious Abbé, the author of this important diſcovery, which it has been the labour of his life to prove and authenticate.

ONE ſhould have naturally imagined, that this gentleman, ſo proud of his anceſtry, might have remained content with that portion of renown which appears to have ſatisfied the vanity of his forefathers, the attributing to their houſe the honour of having produced this illuſtrious lady. And indeed it is not eaſy to conceive how, in any juſt balance of moral eſtimation, the one ſpecies of adſcititious merit ſhould outweigh the other. On the contrary, a rigid moraliſt would infallibly decide, that there was more real honour that accrued to a family from having produced the pure, the chaſte, the coy, the maiden LAURA, the model of female dignity and propriety, the object of an ardent, but virtuous affection to the moſt illuſtrious character of the age; than from having acquired by marriage, a connection with a lady, who, whatever were her perſonal charms, had no title to the praiſe of exalted virtue, or of true female dignity;

who,

who, while joined in wedlock to a reſpectable huſband, and the mother of eleven children, continued, for above twenty years, to put in practice every artifice of a finiſhed coquette, to enſnare the affections, and keep alive the paſſions, of a gallant, whoſe attachment, from the celebrity of his name, was flattering to her vanity.

I MUST indeed acknowledge, that theſe notions are drawn from a ſyſtem of morals, with which the Abbé de SADE and moſt of his countrymen are but very little acquainted. I know that, in the opinion of moſt Frenchmen, a handſome married woman derogates not in the ſlighteſt degree from the rules either of virtue or of ſtrict propriety, while ſhe amuſes herſelf with the gallant attentions of all the young men of her acquaintance; and the moſt intimate reciprocation of tender ſentiments, while it is only an affair of the heart, is termed *une belle paſſion* *. This is preciſely what the Abbé de SADE ſuppoſes to have been the connection of PETRARCH and of LAURA. PETRARCH beſieged her with ardent and importunate ſolicitations, which had for their object the ordinary rewards of a lover. She never actually diſhonoured her huſband's bed; but ſhe made no ſcruple to avow to her lover that her heart was ſenſible to his flame; though at times ſhe found it neceſſary to feign a rigour and coldneſs of demeanour in order the better to keep alive the ardour of his paſſion. "Par ce petit manege," ſays the Abbé, "cette alternative de faveurs et de rigueurs bien ménagée, une "femme tendre et ſage amuſe, pendant vingt et un ans, le plus "grand poëte de ſon ſiecle, ſans faire la moindre breche à ſon "honneur."

* OUR author has even termed this amour, *une paſſion honnête*, (an honourable paſſion). Thus, in ſpeaking of Avignon, he ſays: "Une ville qui fait gloire de l'avoir élevé dans ſon ſein, et d'avoir été le théatre *d'une paſſion honnête*, qui lui a inſpiré de ſi beaux vers." *Mém. de* PET. tom. i. p. 29. And the ſame expreſſion occurs, tom. i. p. 111. where the Abbé propoſes this paſſion of the poet for the wife of another man, "as a model for all tender and virtuous hearts."

"honneur." *Mém. pour la Vie de* PET. *Préface aux François*, &c. And in the following paſſages he deſcribes this *petit manege* more particularly:

"LAURE, qui ne vouloit ni ſe donner à lui, ni le perdre, n'eut "pas plutôt apperçu les nouveaux efforts qu'il faiſoit pour briſer "ſes fers, qu'elle mit en uſage pour l'y retenir les petites ruſes "qui lui avoient juſqu'alors ſi bien réuſſi: air moins ſévère, "regards plus doux, petits mots en paſſant," &c. *Mém.* vol. ii. p. 311.——"LAURE ne pouvoit ſe reſoudre à perdre un amant "de cette trempe, qui l'aimoit depuis onze ans, et qui faiſoit de "ſi beaux vers pour elle: le rencontrant un jour dans les rues "d'Avignon, elle jetta ſur lui un de ces regards qui ſavoient ſi "bien le ramener." *Ibid*, p. 383.——"Ces petites ruſes d'une "*coquetterie innocente*, que LAURE ſavoit ſi bien mettre en "œuvre, eurent bientôt produit leur effet ordinaire." *Ibid.* p. 386.

FROM theſe paſſages we may judge, what idea this Reverend Abbé has formed to himſelf of the character of a reſpectable married woman, and the mother of a family; *une femme tendre et ſage*; who has the art to keep alive, by all the tricks of a coquette, the paſſions of her gallant for no leſs than twenty-one years; and all this *ſans faire la moindre breche à ſon honneur.* Obſerve too, that, for the better part of this period of faſcination, the lady herſelf could owe little of her power to her perſonal charms; for, if our author's hypotheſis be true, and LAURA de NOVES, the wife of HUGH de SADE, was the miſtreſs of PETRARCH, this lady muſt have been a grown woman before her marriage in the year 1325; ſhe had been married three years before PETRARCH firſt ſaw her; ſhe bore eleven children in the period of their acquaintance; and ſhe died when ſhe was about forty years of age, *with all the ſymptoms of an exhauſted conſtitution.* Yet all the while, this hackneyed and antiquated coquette, regardleſs of the character of a wife and a mother, is practiſing her *petit manege* of alternate favours and rigours to turn the head of an infatuated *enamorato*, whofe paffion was in itfelf an affront to virtue and morality, and amufe him for a lifetime with the expectation of favours which fhe is determined never to grant. Such, in the fyftem of the Abbé de SADE, is the all-accomplifhed LAURA, and fuch the refpectable and virtuous PETRARCH. How abfurd, how difgufting, how contemptible the one: how weak, how culpable, how difhonourable the other!

BUT let us now examine the particulars of that evidence on which this author has built an hypothefis, fo degrading to thofe characters whom the world has hitherto united to venerate and admire.

THE Abbé de SADE has, in a note at the end of the firft volume of his work, given a fhort abftract of the arguments which he has drawn from the works of the poet himfelf, to fhew that the LAURA of PETRARCH was a married woman. I fhall take them in the order in which they are given *.

" ALMOST all the world," fays he, " has believed that LAURA
" was unmarried: *Prefque tout le monde a cru que* LAURE *étoit fille.*
" VELUTELLO lays it down as a propofition abfolutely certain.
" *Per cofa certa habbiamo da tenere che non foffe mai maritata.* Ne-
" verthelefs," fays he " it is an undoubted truth that fhe was
" a married woman. PETRARCH himfelf expreffes it in fuch a
" manner as to put it beyond all queftion.

" 1*mò*, In fpeaking of LAURA, he terms her, in his Latin
" works, always *mulier* and *fœmina*, and never *virgo* or *puella*;
" and in his Italian works, always *madonna* or *donna*, and never
" *vergine*

* IN a fmall pamphlet, entitled " *An Effay on the Life and Character of* PETRARCH," written by the author of thefe Remarks, and printed in 1784, a brief fummary is given of the Abbé de SADE's arguments proving LAURA to be a married woman, to which the anfwers are in fubftance much the fame with what the reader will find here, though they are now given in a more ample form, and ftrengthened by additional matter of proof from the writings of PETRARCH.

" *vergine, donzella*, appellations always applied to unmarried
" women, and which found so peculiarly graceful in both lan-
" guages.

" *2dò*, IN sonnets 10. 162. &c. the poet speaks of the dress
" of LAURA, of the garlands she wore on her head, of the jewels
" and pearls with which she braided her hair; and, in sonnets
" 151. and 158., he mentions the magnificence of her garments.
" Now, in the age of PETRARCH, the young unmarried women
" wore neither garlands, nor pearls, nor jewels; they were
" dressed in a very simple manner, and appeared very little in
" public.

" *3tiò*, IN sonnets 162. and 185. PETRARCH complains, that
" jealousy often deprived him of the pleasure of seeing LAURA.
" Some commentators have indeed supposed this to be meant
" of the jealousy of her parents, watchful over the honour of
" their daughter: but this is a forced and unnatural construc-
" tion; for the term jealousy, *gelosia*, has never been applied to
" the watchful care of parents for the honour of their children.

" *4tò*, PETRARCH, with the intention of celebrating the vic-
" tory which LAURA had gained over love, composed, after her
" death, a poem, entitled The Triumph of Chastity, *Trionfo
" della Castita*. If LAURA had been a virgin, it is clear that he
" would have entitled his poem *Trionfo della Virginita*. In this
" poem, too, he would have given LAURA virgins for her at-
" tendants, and not married women. Now, those who follow
" LAURA to the Temple of Chastity, are LUCRETIA, PENELOPE,
" JUDITH, DIDO, the Greek HIPPO, HERSILIA, &c. all married
" women. The single exception is a Vestal Virgin."

THESE, the author of the *Mémoires* acknowledges, after all, amount to nothing more than strong conjectures. Now follows, says he, something more conclusive.

" *5tò*, PETRARCH, in one of his dialogues with St AUGUSTINE,
" in speaking of LAURA, says, that her body was exhausted by

" frequent childbearing; *corpus ejus crebris partubus exhaustum.*
" It is true, that the word *partubus* is thus abbreviated in the
" manuscript *ptbs*; and, as it was formerly the current opinion
" that LAURA was unmarried, those who had the charge of
" printing the Latin works of the poet, have thought proper to
" interpret the abbreviation *perturbationibus*, and to print it so
" in all the editions of those works: But it ought certainly to
" be read *partubus*, for these good reasons: *first*, That the epithet
" *crebris* means a repetition of acts, and therefore applies better
" to acts of childbearing than to passions. If the author had
" meant the latter, he would have coupled the noun with *multis*,
" instead of *crebris*. But what passions can we suppose to have
" exhausted the constitution of the most prudent and modest of
" women, who led a life so simple and so uniform? In the *next*
" place, Messrs CAPERONNIER, BOUDOT and BEJOT, of the
" King's Library, who must be allowed to be good judges of
" the abbreviations that occur in old manuscripts, have de-
" cided, that *partubus* is the proper reading."

SUCH is the whole of that evidence, drawn by the Abbé de SADE from the works of the poet himself in support of this new hypothesis, that LAURA was a married woman. On this evidence, which, it will be allowed, is of itself extremely inconclusive, I shall now make some remarks. I take the author's arguments in the order in which they stand.

1*mò*, THE words *mulier*, *fœmina*, in Latin, and *donna*, *madonna*, in Italian, are equally applicable to married and to unmarried women. *Mulier* and *fœmina* mark the sex alone, without reference to the state or condition. ISIDORUS, in his *Origines*, l. xi. c. 3. says: " *Dicitur igitur mulier secundum fœmineum sexum, non* " *secundum corruptionem integritatis; nam* EVA *statim facta de* " *latere viri, et nondum contacta a viro, Mulier appellata est, di-* " *cente Scripturâ, Et format eam in mulierem.*" Thus too, in the Roman law, where there is the utmost precision in the use of terms,

terms, *mulier* and *fœmina* are indiſcriminately applied to unmarried and to married women. In the 17th law of the *Codex de donat. ante nuptias*, the title bears; "*Donatio ante nuptias* "*facta mulieri in minori ætate, non indiget inſinuatione*," &c. FABER, in his *Theſaurus*, obſerves, on the word *mulier*; "*Variè* "*accipitur vox; aliquando enim communiter de ſexu dicitur, et* "*omnem ætatem ac conditionem ejus amplectitur; itaque et de* "*puellis uſurpatur*," &c.

IN the ſame manner, *donna* and *madonna* are uſed by the Italians, when ſpeaking either of unmarried or of married women. Every Italian poet terms his miſtreſs *donna* and *madonna*. Thus, ARIOSTO, in the beginning of the 35th canto of the *Orlando Furioſo*, ſays,

Che ſalira per me, Madonna, in cielo;

and in his 1ſt elegy,

Non e aſſai Madonna meſi e anni
La fra ſpeme e timor ſin qui ſoſpeſa?

Thus GUARINI, in the *Paſtor Fido*, att. 1.

La fede in cor di donna, ſe pur fede
In donna alcuna (ch'io no'l ſo) ſi trova, &c.

and again,

Bella donna e gentil, ſollecitata
Da numeroſa ſtuol di degni amanti,
Se d'un ſolo e contenta, e gli altri ſprezza
O non e donna, O ſe pur donna, e ſciocca.

Thus too, BOCCACIO, in the Introduction to the *Decamerone*:

Gratioſe e nobili donne, meco penſando, &c.

A thoufand inftances of the fame kind might be given, to fhew, that this criticifm of the author of the *Mémoires* on the words *mulier*, *fœmina*, *donna* and *madonna*, has no folid foundation.

2dò, THE author of the *Mémoires*, when he fays that, in the age of PETRARCH, the unmarried women were always fimply dreffed, and that the ufe of garlands, of pearls, and of jewels, was peculiar only to fuch as were married, affumes a fact of which there is no evidence. MURATORI, in his twenty-fifth differtation on the drefs of the middle ages, proves, that in the north of Italy, about the time of PETRARCH, the drefs of the women was remarkably fplendid; and he makes no diftinction between the drefs of the married, and of the unmarried women. He quotes a monk, GALVANEUS, who, inveighing againft the luxury of the times, fays: " Mulieres fimiliter in pejus omnia mu-" taverunt. Ipfæ namque ftragulatis veftibus, fcopato gut-" ture et collo redimitæ fibulis aureis, gyrovagantur. Sericis " et interdum aureis indumentis veftiuntur. Crinibus crifpatis " more alienigenarum capite perftringuntur. Zonis aureis fu-" percinctæ, Amazones effe videntur." MURAT. *Antiq. Ital.* tom. ii. p. 417.—If fuch was the fplendid attire of the women in the north of Italy, the Court at Avignon importing thence both its manners and its fafhions, would not, it is probable, be behind their models in drefs, as in every other fpecies of luxury. We have the authority of the Abbé de SADE himfelf for affirming, that, under the pontificate of CLEMENT VI., the drefs of the women at the Court of Avignon was fplendid and luxurious in the extreme, (*Mém de* PET. tom. ii. p. 92.); and that this affectation had reached even the lower claffes, appears from a proclamation, which it was found neceffary a fhort time afterwards to iffue, prohibiting the ufe of gold, filver, ermines or filk, in apparel, to all women, unlefs the relations of the Pope, the wives and *daughters* of the Lord Marefchal and Lord Vicar,

the

the Baronesses and the Ladies of Quality of the city *. Now, from the evidence of this document, extending the exception to the *wives* and *daughters* of persons of a certain rank, we are undoubtedly warranted to infer, that this splendour and luxury of dress of the ladies of Avignon, was common to both the unmarried and married women. We know for certain that LAURA was a woman of noble blood. Of this the poet informs us in numberless passages of his writings. Considering, therefore, her rank and condition, her dress will certainly appear to display no extraordinary magnificence. In the 10th sonnet, *(Se la mia vita)*, the poet says no more than that his mistress wore garlands and green clothes †. In the 162d sonnet, *(L'aura serena,)*, he says, that her hair, which formerly she wore loose, was now braided and adorned with pearls and jewels ‡; a customary distinction of dress, as CASTELVETRO remarks, between young girls *(fanciulle)*, and grown women. Now, if the remark of the Abbé de SADE be just, that this mode of adorning the hair was peculiar to married women, those passages which indicate that LAURA adorned her hair in that manner will prove a great deal more than the Abbé intends they should do: for they will demonstrate that PETRARCH was acquainted with LAURA while

* "Quod nulla domina, seu mulier, cujuscunque conditionis existat, exceptis dominabus de parentela domini nostræ Papæ, et uxorum ac filiarum dominorum Mareschalli et Vicarii, et exceptis etiam dominabus baronissis et majoribus in civitate habitantibus, nunc et in futurum ausa sit portare in aliqua rauba seu veste, aliquem reversum de subtus nec de supra, neque in martis variorum, erminiorum, seu quarumcunque aliarum pellium, seu rerum, sive de auro, de argento, nec de serico." *Præconisationes anni* 1372.—*Mém. de* PET. tom. 2. p. 92.

† *E lassar le ghirlande, e i verdi panni.*—

‡ *E le chiome hor avvolte in perle, e'n gemme,*
Alhora sciolte, &c.

while ſhe was unmarried, as they prove, that he had formerly ſeen her while ſhe wore no ornaments on her head, but appeared with her hair looſe, unbraided, and quite unadorned, as the Abbé ſuppoſes to have been the faſhion of the young unmarried women. Thus, in the 162d ſonnet, above quoted, the poet ſays: " The ſerenity of the air, and the return of ſpring, bring " to my remembrance the time when I firſt felt the power of " love; when I firſt beheld that beautiful countenance, and ſaw " thoſe golden locks looſely waving in the wind, which are now " braided and adorned with pearls and jewels *." And ſo likewiſe, in the 69th ſonnet, P. I. deſcribing the time when he firſt ſaw LAURA, he ſays: " Thoſe golden treſſes were then looſely " ſcattered by the wind, which twiſted them into a thouſand " beautiful ringlets:"

Erano i capei d'oro al'aura ſparſi,
Che 'n mille dolce nodi gli avvolgea.

If, therefore, as the Abbé de SADE maintains, this braiding and adorning of the hair marked the diſtinction between the married and unmarried women in the age of PETRARCH, he muſt admit, on the evidence of thoſe paſſages where her headdreſs is ſo deſcribed, that, at the time when the poet was at firſt acquainted

* *L' aura ſerena, che fra verdi fronde*
Mormorando a ferir nel volto viemme
Fammi riſovvenir, quand' Amor diemme
Le prime piaghe, sì dolce e profonde;
E 'l bel viſo veder, ch' altri m'aſconde
Che ſdegno e geloſia celato tiemme;
E le chiome hor avvolte in perle e'n gemme,
Alhora ſciolte, e ſovra or terſe bionde:
Le quali ella ſpargea sì dolcemente,
E raccogliea con sì leggiadri modi
Che ripenſando ancor trema la mente.—

quainted with LAURA, and became enamoured of her, as ſhe wore her hair in looſe ringlets, ſhe was *unmarried;* a circumſtance deſtructive of his whole hypotheſis: for LAURA de NOVES was married to HUGH de SADE, as the Abbé has ſhewn from his own family documents, in 1325; and PETRARCH ſaw *his* LAURA for the firſt time in 1327. I ſhall leave the Abbé to extricate himſelf from this dilemma the beſt way he can: for my part I ſee no poſſible means of an eſcape.

BUT the author of the *Mémoires* has, on the ſubject of the dreſs of LAURA, either wilfully perverted, or moſt palpably miſunderſtood, his authorities. He has quoted ſonnets 151. and 158. † in proof of that richneſs of apparel, which he argues to be characteriſtical of her being a married woman. The firſt of theſe ſonnets is a compariſon of the poet's miſtreſs to the fabled *phœnix*, which is thus deſcribed by PLINY: "Auri fulgore "circa colla, cætera purpureus, cœruleam roſeis caudam pennis "diſtinguentibus." *Nat. Hiſt.* lib. 10. c. 2.—So PETRARCH, in this ſonnet, deſcribes his beautiful Phœnix; her lovely hair *artleſsly* floating in ringlets about her neck, and thus forming a natural necklace of gold; her ſhoulders covered with a purple garment bordered with azure *; thus, in every point reſembling the famed Arabian bird. The former part of this deſcription, which

* *Queſta Fenice de l'aurata piuma*
Al ſuo bel collo candido, gentile
Forma ſenz' arte un sì caro monile
Ch' ogni cor addolciſce, e 'l mio conſuma:
Forma un diadema natural.——
Purpurea veſta d'un ceruleo lembo
Sparſo di roſe i belli homeri vela;
Novo habito, e bellezza unica e ſola.
Fama nell' odorato, e ricco grembo
D' Arabi monti lei ripone e cela;
Che per lo noſtro ciel sì altera vola.

† SONNETS 152. and 159. of the Venice edition, 1756.

which applies to the hair, is evidently characteriſtic of the utmoſt ſimplicity of female decoration, and therefore lends no aid to the Abbé's laboured argument. The latter part deſcribes only the colour of the garments of LAURA, and might apply equally to a fine lady, and to a country maiden who wore a crimſon cloak with a blue border. As to ſonnet 158. which is quoted as proving that LAURA wore garments embroidered with gold and pearls, our author has fallen into a very groſs miſtake, in giving a literal meaning to what is entirely figurative. The poet is there ſpeaking, not of the dreſs or garments, but of the perſonal qualifications and ſhining accompliſhments of his miſtreſs.

> *Vedi quant' arte dora, e' mperla e' nnoſtra*
> *L' habito eletto.—*

This paſſage CASTELVETRO thus properly interprets: " *Abito* " in queſto luogo ſignifica *corpo*, che e come abito e veſtimen-" to al' anima, il quale e ornato di maravigliofe bellezze e ma-" niere, che egli ſignifica, dicendo, che l'arte lo 'ndora e 'mperla, " e' nnoſtra, come sì ſarebbe una veſte." And ſuch is the interpretation which all the commentators have put on this paſſage.

3tiò, IN ſonnet 162. *, the poet complains, that jealouſy had deprived him of the ſight of LAURA; and in ſonnet 185. † the female companions of LAURA make the ſame complaint: but in neither of the paſſages alluded to is there the ſmalleſt hint that the jealouſy of a huſband was here meant. Unleſs, therefore, the author of the *Mémoires* ſhall ſhew, that there can be no jealouſy except in the breaſt of a perſon who is married, his argument concludes nothing. The word *geloſia*, in Italian, is no more lim ited in its ſignification than theEngliſh word *jealouſy*; both

* *L'aura ſerena.* † *Liete, e penſoſe.*

both meaning the refentment of every fpecies of rivalfhip. In the firft of the fonnets above mentioned, it is moft probable that the poet meant that LAURA's own jealoufy had frequently deprived him of the happinefs of feeing her *. What gives the ftrongeft fupport to this interpretation, is the affociation of difdain with jealoufy:

E 'l bel vifo ch' altri m' afconde
Che fdegno o gelofia celato tiemme.

And we know, from fome particulars of the life of PETRARCH, that LAURA had fufficient caufe both of difdain and jealoufy †. It is in this mortifying feature, of an otherwife moft virtuous and exemplary character, that we are to feek for the true reafon of thofe changes of deportment which LAURA manifefted towards

* ON this paffage CASTELVETRO thus remarks: " Si potrebbe intendere di LAURA che, fdegnata col PETRARCA, gli nafcondeffe il vifo: O, perche foffe innamorata di fe fteffa, aveffe gelofia che il PETRARCA la vedeffe:

Se forfe ogni fuo gioia
Nel fuo bel vifo è folo,
E di tull' altro è fchiva. Canz. 13.

Ma meglio è d'intendere de' parenti,

Dogliofe per fua dolce campagnia
La qual ne toglie invidia e gelofia.— Son. 185."

† WE learn from his familiar letters, that his paffion for LAURA had not reftrained him from the indulgence of a meaner amour, with a woman of low manners and of a difagreeable temper; a paffion, of courfe, in which the heart had no fhare; and that, in confequence of this connection, which was even of fome years duration, and was a fource to him of much difquiet, he had a natural fon and daughter; of the former of whom we find frequent mention made in the courfe of thofe letters.

towards her lover, thofe alternate marks of favour and of cold referve, and that tedious protraction of the final reward of a paffion, unexampled in its ardour and duration.

In the 185th fonnet, where the female companions of Laura complain that envy or jealoufy had deprived them of her company, the expreffion may be meant either of her own jealoufy, as in the former inftance, or more probably, in this place, of the jealoufy of her parents. " *Reftata in cafa per invidia o gelofia de* " *parenti*," fays Castelvetro: and the fame author remarking that fome have fufpected from this paffage that Laura was a married woman, acutely obferves, that the context plainly indicates that the expreffion will not admit of that conftruction: Her companions lament, that they are deprived of her company by that envy or jealoufy which repines at the happinefs of another, as if it were its own misfortune:

Dogliofe per fua dolce compagnia
La qual ne toglie invidia e gelofia
Che d'altrui ben, quafi fuo mal, fi dole.

" The man who is truly jealous," he well obferves, " cannot be " faid to repine at the happinefs of another, *as if it were* his own " misfortune; for, in reality, *it is* his own misfortune."

4*tò*, The author of the *Mémoires* is equally ill-founded in the argument he endeavours to draw from the title of the *Trionfo della Caftita*, as in moft of his other critical remarks. *Caftita* in Italian, *caftitas* in Latin, and *chaftity* in Englifh, are equally applicable to a virgin as to a married woman. Diana is celebrated for her chaftity as well as Penelope. Some of the Doctors have even limited the application of the term *Chaftity* to fuch as are unmarried. " *Caftus et continens*," fays Aquinas, " *fic differunt, quod caftus dicitur ante nuptias, continens vero poft* " *eas*."

But,

BUT, ſays the Abbé de SADE, had LAURA been a virgin, the poet would, in this compoſition, have given her virgins for her attendants, and not married women. Thoſe who follow her to the Temple of Chaſtity, ſays he, are all married women, with the exception of a *Veſtal Virgin*. Here our author is guilty of a groſs miſrepreſentation. In the poem of the *Trionfo della Caſtita*, PETRARCH, ſo far from citing examples only of married women, as LUCRETIA, PENELOPE, &c. ſays:

Io non porria le ſacre benedette
Vergini ch' ivi fur chiuder' in rima.—

"I could not comprehend in rhyme all the ſacred virgins that "were preſent:" and he enumerates the nine muſes,

—CALLIOPE *e* CLIO *con l' altre ſette* *;—

together with VIRGINIA and the Veſtal TUCCIA. We have here an

 example

* I AM well aware of the doubt that has been entertained by certain grave and learned authors, with regard to the *virginity* of the Muſes. As to CALLIOPE, indeed, the matter was paſt a doubt; for her amour with ŒAGRUS, king of Theſſaly, was proclaimed by the birth of a ſon, who made ſufficient noiſe in the world, the famous ORPHEUS; and therefore BUCHANAN has very guardedly expreſſed himſelf, with reſpect to her, in his epigram:

CALLIOPE *longum cœlebs cur vixit in ævum?*
Nempè, nihil doti quod numeraret, erat.

For this lady, though a mother, was certainly *cœlebs*, or unmarried; and this is quite ſufficient to refute our author's aſſertion that the poet had here given LAURA an attendance only of married women. As to the other Muſes, whatever may have been their failings in private, (and every one of them has ſuffered from the breath of ſcandal; ſee *Menagiana*, t. 2.), their ſtate of *celibacy* is authenticated beyond all queſtion; and I muſt in conſcience believe, that PETRARCH had never heard any of thoſe curious and ſecret anecdotes of the lives of thoſe ladies, which the penetrating reſearch example of unpardonable difingenuity in our Abbé, which, occurring in an inftance of eafy detection, muft juftly render fufpicious many of thofe authorities which he pretends to have drawn from private fources, fuch as the archives of his own and other families, where it is impoffible for others to follow him, and inveftigate the truth of his information.

Before I leave this argument I muft obferve, that, by adopting our author's own mode of reafoning, the works of the poet will furnifh us with fimilar evidence, directly deftructive of his hypothefis. If the ftate of Laura, whether married or unmarried, is to be determined from that of her companions or attendants, we find, in many other paffages of the poetical works of Petrarch, that he affigns to her an attendance of virgins. Thus, in the 11th eclogue, in which the companions of Laura lament her death, under the name of Galatea, one of them fays:

> *Addam perpetuos celebret quos mundus honores;*
> Virgineos *addam cœtus, ritusque verendos.*

And in the 3d eclogue, employing his favourite allufion, the *verdant Laurel*, under which he always figures his miftrefs, he fays:

> *Purpurea in ripa, Laurique virentis ad umbram,*
> Virgineam *afpicio, cœlo plaudente choream.*—

5to, The laft of the arguments advanced by the author of the *Mémoires*, which he gives as in a manner conclufive on this point, is that which is drawn from the dialogue with St Augustine, and confifts in the interpretation given to the contracted

fearch and deep erudition of modern authors has brought to light, otherwife he would not have made them the attendants of his Laura, married or unmarried, to the Temple of *Chaftity*.

tracted word *ptbs*, which is found in fome manufcripts of the works of PETRARCH. Our author having frankly enough acknowledged that all the preceding arguments amount only to conjectures, *(ce ne font là après tout, que de très fortes conjectures,)* might certainly have included the laft with equal propriety under the fame denomination. His interpretation of the word *ptbs* is evidently nothing more than conjecture; to fupport which we have only his own opinion, and, as he fays, that of Meffieurs CAPPERONIER, BOUDOT and BEJOT of the King's Library; although, among the *Pièces juftificatives*, we find only the certificate of one of thofe gentlemen (CAPPERONIER) to that effect; and this expreffed with fuch obfcurity and confufion of idea, that we cannot tell what are the characters in the two manufcripts he mentions *. But one thing is plain; before we can admit any conjectural interpretation of this contraction, the Abbé de SADE muft prove, that the two manufcripts which bear this contracted word are the oldeft of all the manufcripts of the writings of PETRARCH, otherwife his argument concludes nothing; for, if the more ancient manufcripts have the word at

* " CERTIFICAT de Monfieur CAPPERONIER, Garde de la Bibliotheque du Roi.

" JE fouffigné, Garde de la Bibliotheque du Roi, certifie, que dans le manufcript du Roi, cotté 6502, contenant un ouvrage de PETRARQUE, intitulé, " *De conflictu* " *curarum propriarum, ad* AUGUSTINUM," fol. 13. cot. 1. on lit, et qu'on doit lire: " Et corpus illud egregium morbis ac crebris partubus exhauftum multum priftini " vigoris amifit;" lefquels mots fe trouvent encore dans le manufcript cotté 6728. cod. 19. pag. 1. où ils doivent être lûs de la même manière. En foi de quoi, j'ai figné le prefent certificat, en l'hotel de la Bibliotheque du Roi, ce 16 Juin 1762.

CAPPERONIER."

It does not appear from this certificate that the two manufcripts mentioned bear any contraction of this word at all; yet thefe are certainly the manufcripts to which the Abbé de SADE here refers as bearing *ptbs*. When M. CAPPERONIER, therefore, declares, *on lit, et on doit lire*, fo and fo, the expreffion is as obfcure and inaccurate, as the decifion is dogmatical and prefumptuous.

at full length, *perturbationibus*, the abbreviation in the later manuſcripts muſt be explained in that manner alone. Now, the Abbé has not taken upon him to maintain, that thoſe two which bear the contraction *ptbs* are the moſt ancient of all the manuſcripts; and his ſilence on that material point is a preſumption of his knowledge to the contrary. But, even were we to allow that thoſe two manuſcripts were actually proved to be the moſt ancient, let us ſee what could thence be concluded. All the other manuſcripts, which we ſhall ſuppoſe were copied from thoſe two moſt ancient ones, and all the editions of the printed works, have interpreted this contracted word, and written it at full length, not *partubus*, as M. de SADE would have it read, but *perturbationibus*. Now, it will not certainly be denied that the writers of thoſe old manuſcripts, and the editors of the oldeſt printed editions,o f the former of whom many were probably cotemporary with the author, and of the latter all lived at no great diſtance of time from his age, were much better able to read and to interpret the abbreviations of the oldeſt manuſcripts, than thoſe critics who have examined them at the diſtance of four hundred years from the time when they were written. Even admitting, therefore, that the Abbé had proved his two favourite manuſcripts to be the moſt ancient of all extant, and that the queſtion were ſimply, What is the moſt natural interpretation to be given to this contraction? we have, in oppoſition to his opinion, and that of his friends of the King's Library, the direct authority of all the other manuſcripts of the works of PETRARCH, and all the printed editions, that the word ought to be read not *partubus* but *perturbationibus*.

BUT I ſhall make the author of the *Mémoires* a ſtill more important conceſſion; and I ſhall even put the caſe, that inſtead of *two* manuſcripts, perhaps out of two hundred, which read the word contracted thus *ptbs*, while the reſt have it at full length *perturbationibus*, the whole exiſting manuſcripts of the author's works had borne

borne the word thus contracted; and that the queſtion were, Whether it ought to be explained *partubus* or *perturbationibus?* This queſtion, it muſt be allowed, would be beſt reſolved by conſidering the ſenſe of the context. Let us now, therefore, ſee what is thence to be drawn. PETRARCH, in the dialogue in queſtion, feigns a converſation between himſelf and St AUGUSTINE, in which the latter is endeavouring to convince him of the impropriety of abandoning himſelf to the influence of a paſſion, whoſe imperious power had enſlaved to itſelf, and rendered ſubordinate, every feeling of his nature. Among other arguments, he urges the folly of ſetting his whole affection on an object of which death might ſo ſoon deprive him. PETRARCH anſwers, that he hopes he ſhall never live to ſee that day; and obſerves, that, in the courſe of nature, LAURA being the younger, ought to ſurvive him. To this St AUGUSTINE replies, that ſuch an event is nevertheleſs the moſt probable: "*quod corpus* "*illud egregium morbis ac crebris ptbs exhauſtum multum priſtini* "*vigoris amiſit.*" Whether ought we, along with all the editors of the printed works of PETRARCH, to explain this contraction by *perturbationibus*, frequent diſquietude or conflicts of mind; or, with the Abbé de SADE, to ſuppoſe it to mean *partubus* or child-bearing? The anſwer immediately made by PETRARCH, muſt reſolve the doubt. "It is probable," ſays St AUGUSTINE, "that LAURA, though the younger of the two, will "ſooneſt be the victim of death; for her conſtitution, by much "ſickneſs, as well as mental inquietude, child-bearing, has loſt a great deal of its former vigour."—"I too," ſays PETRARCH, "have had my ſhare "of mental inquietude, and that more ſevere than hers, and I am "conſiderably more advanced in life: *Ego quoque et curis gravior,* "*et ætate provectior factus ſum.*" If we admit the common reading, the reply is rational and conſonant: "I have ſuffered from "the ſame cauſe, and more than ſhe has:" If we adopt the interpretation

pretation of the Abbé de SADE, the reply is equally abſurd and impertinent to the obſervation that precedes it.

WITH regard to the critical deciſion pronounced by the Abbé on the meaning of the word *creber*, viz. that it imports a repetition of *acts*, and that it cannot with propriety be applied to *paſſions*; it had certainly been proper that he had ſupported this judgment either by ſome authorities of profeſſed grammarians, or examples from claſſical writers. He has done neither; and that for the beſt of reaſons: he had none to produce. *Creber*, as we find from the beſt authorities, is uſed preciſely in the ſenſe of *frequens* or *aſſiduus*; and is therefore, with perfect propriety, applied as well to paſſions as to acts. He obſerves, that had *perturbationibus* been the proper reading, the author would have coupled it with *multis*, and not with *crebris*; a remark betraying ignorance of grammatical preciſion: for who is there that needs to be told that *multus*, applying to number, can with no propriety be employed to denote frequency of repetition?

BUT the author of the *Mémoires* aſks, What paſſions we can ſuppoſe to have exhauſted the conſtitution of the moſt *prudent* and *modeſt* of women, who led a life ſo *ſimple* and ſo *uniform*? To this I anſwer by another queſtion: How can we, ignorant as we are of the private and domeſtic hiſtory of this lady, pretend to ſay what cauſes ſhe might, or might not have had, of anguiſh and diſquiet? How many women of prudence and of modeſty are, from unavoidable circumſtances of ſituation, the victims of mental inquietude; and experience, even in a life of the utmoſt privacy and retirement, the keeneſt anguiſh, from the turbulent paſſions, the malice or the caprice, of thoſe with whom they are connected.

I HAVE now, as I truſt, impartially canvaſſed the whole of thoſe arguments drawn by the author of the *Mémoires* from the

works

works of PETRARCH himſelf, or what may be termed the intrinſic evidence in ſupport of the material part of his hypotheſis, namely, that LAURA was a married woman; nor do I think I preſume too much when I ſay, that I have ſhewn their abſolute inſufficiency to prove that propoſition. The queſtion might therefore be ſafely left here; in the confidence, that an hypotheſis which is newly brought forward * in oppoſition to long eſtabliſhed belief, and the concurring aſſent, for ages, of all who have been converſant with the matter of inquiry, and which is ſhewn to reſt on no baſis, either of hiſtorical evidence or of ſound reaſoning, does not require the confutation of oppoſite proof to lay it in the duſt: But, as I have already had occaſion incidentally to produce ſome of thoſe teſtimonies from the works of the poet himſelf, which tend moſt poſitively to diſprove this hypotheſis, and an abundance of matter of ſimilar import muſt offer itſelf to all who have examined thoſe works with any attention, I am prompted to draw yet a little more from this ſtock of the internal evidence, before I take my leave of the ſubject.

1*mò*. PETRARCH has compoſed 318 ſonnets, 49 *canzoni* or ſongs, and 6 *trionfi*; a large volume of poetry, entirely on the ſubject of his paſſion for LAURA; not to mention a variety of paſſages in his proſe works, where that favourite topic is occaſionally treated, and even diſcuſſed at very great length. In the

 whole

* THIS hypotheſis may certainly be termed a new one; ſince, although TASSONI, and ſome of the commentators on the Sonnets of PETRARCH have, in their obſervations on certain paſſages which the Abbé de SADE produces as proofs of his theory, remarked, that a ſuſpicion might thence ariſe that LAURA was a married woman, none of them have ventured to affirm (as our author) for certain, that ſhe was ſo. On the contrary, VELUTELLO's concluſion, after confeſſing the very imperfect information which could be collected relative to the family, ſtate and condition of LAURA, is: "*Per coſa certa habbiamo da tenere ch' ella non foſſe mai maritata.*"—"We muſt hold it for a point abſolutely certain, that ſhe was never married."

whole of thoſe works, there is not to be found a *ſingle paſſage*, which intimates that LAURA was a married woman. Is it to be conceived that the poet, who has exhauſted language itſelf in ſaying every thing poſſible of his miſtreſs; who mentions not only her looks, her dreſs, her geſtures, her converſations; but her companions, her favourite walks and her domeſtic occupations, would have omitted ſuch capital facts, as her being married, and the mother of many children; married too, as the author of the *Mémoires* aſſerts, to a man who was jealous of her, and who uſed her with harſhneſs and unkindneſs on PETRARCH's account?

2dò. WOULD this harſh and jealous huſband have permitted this avowed admirer of his wife, this importunate gallant, who followed her as her ſhadow wherever ſhe went, and attended her in town and in the country, to ſee her daily, and converſe with her alone, to write to her, to make aſſignations with her, and to ſend her preſents as tokens of his attachment? Yet, that PETRARCH enjoyed all theſe liberties, is evident from numberleſs paſſages of his works*. That the poet and his miſtreſs were wont even to walk together in the public gardens, is evident from the incident alluded to in the 208th ſonnet, *Due roſe freſche:* A friend, who met them together in a garden, taking them both by the hand, preſented each with a roſe, declaring, at the ſame time, that the ſun never ſhone on a truer pair of lovers. And that their paſſion was the common diſcourſe

* SON. 41. *Perch' io t' habbia.* Son. 59. *Quando giunſe.* Canz. 14. *Perchè quel.* Canz. 15. *Volgendo gli occhi.* Son. 49. *Se voi poteſte.* Canz. 12. *Perch' al viſo.* Canz. 4. *Nel dolce tempo.* Son. 19. *Mille fiate.* Canz. 8. *Sì è debile.* *Trionf. di Morte*, cap. 2. &c. &c. In the 42d ſonnet, *(Se col cieco deſir)*, PETRARCH complains that LAURA had failed to keep an appointment that ſhe had made with him, and in which, he had flattered himſelf, he was to be indulged in freely declaring his paſſion.

diſcourſe of the public, appears from many paſſages, where the poet dwells on that circumſtance as a matter of regret:

> ——*Sì come al popol tutto*
> *Favola fui gran tempo, onde ſovente*
> *Di me medeſmo meco mi vergogno.*—' Son. 1.

3*tiò*. PETRARCH, in the 200dth ſonnet, *(Real natura)**, records the following remarkable anecdote. At a brilliant aſſembly and feſtival, given on occaſion of the arrival of a foreign prince at Avignon, LAURA was preſent, along with the moſt diſtinguiſhed ladies of the place. This prince, whom the poet celebrates as a moſt amiable and accompliſhed character, curious to ſee a lady of whom the compoſitions of PETRARCH had given him ſo high an idea, eagerly ſought her out amidſt the crowd, and ſoon diſcovered her by her ſuperior beauty and the gracefulneſs of her demeanour. Approaching her with an air of gentleneſs united with dignity, and making a ſign to the ladies who ſurrounded her to ſtand a little apart, he took her by

Z 2 the

* *Real natura, angelico intelletto,*
Chiar' alma, pronta viſta, occhio cerviero,
Provvidenza veloce, alto penſiero,
E veramente degno di quel petto;
Sendo di donne un bel numero eletto
Per adornar il dì feſto ed altero,
Subito ſcorſe il buon giudicio intero
Fra tanti, e sì bei volti il più perfetto;
L' altre maggior di tempo, o di fortuna
Trarſi in diſparte commandò con mano,
E caramente accolſe a sè quell' una:
Gli occhi, e la fronte con ſembiante umano
Bacciolle sì, che rallegrò ciaſcuna:
Me empiè d' invidia l' atto dolce e ſtrano.

the hand, and (after the faſhion of his country) ſaluted her, by kiſſing her forehead and her eyes: a mark of regard, ſays the poet, which was approved of by all the ladies who were preſent, but which he himſelf beheld with envy. The commentators are not agreed as to the prince of whom this anecdote is recorded. M. de la BASTIE is of opinion that it was ROBERT, King of Naples, who is known to have diſtinguiſhed PETRARCH by many marks of friendſhip and beneficence, and whom the poet has, in various parts of his writings, celebrated with the higheſt eulogy; and this is likewiſe the opinion of BEMBO, DANIEL, and others. The Abbé de SADE, on the other hand, has adduced ſome ſtrong arguments to ſhew, that the prince here alluded to was CHARLES of Luxembourg, ſon of JOHN, king of Bohemia. The diſpute, as to the perſons, is of no conſequence; the anecdote muſt be admitted as true, and it has ever been regarded as highly honourable both for the poet and his miſtreſs. In that light we are aſſured it was conſidered by the ladies who were preſent; and, as is it is no part of the female character, to view with complacency an unmerited preference ſhewn to a rival in beauty or accompliſhments, we muſt hold this as an unequivocal proof, that they conſidered this flattering mark of diſtinction as deſervedly beſtowed, and, of courſe, that they regarded the attachment of PETRARCH and of LAURA as an honourable and virtuous flame. Now, let it be ſuppoſed, with the Abbé de SADE, that this lady, thus highly diſtinguiſhed as the object of the poet's paſſion, had been the wife of a man of rank and character, the mother of a family, is it poſſible to believe that this foreign prince, who is deſcribed as a paragon in every courtly accompliſhment, ſhould have thus openly braved every law of decency and of propriety, and, in a full aſſembly (met to do him honour) have inſulted, not only the huſband of this lady, but every woman of honour or of virtuous character

racter who was prefent? Is it to be conceived, that the hufband of this lady, ftrongly impreffed with the feelings of jealoufy on the fcore of this ardent attachment, as this author himfelf reprefents him to have been, and who, in all probability muft himfelf have witneffed the incident here recorded, fhould have filently and tamely fubmitted to this grofs affront? Is it poffible to figure, that the whole affembly fhould have crowned with their approbation this glaring indignity and violation of decorum?

4to. Would this jealous hufband have not only patiently witneffed the mutual expreffions of this ardent paffion for the fpace of twenty-one years, that his wife was alive; but have complaifantly permitted her gallant, or a friend under his character, to embalm the memory of his miftrefs by a rapturous love-elegy, to be inclofed in her coffin; the laft infult which the honour of a hufband could fuftain? Yet this, we muft believe, if we adopt the hypothefis of the Abbé de Sade: For, if the ftory of the leaden cafket has any truth in it at all, (and its fuppofed truth is the main prop of that hypothefis), this elegy or fonnet muft have been written, either by Petrarch himfelf, or by a friend affuming his character.

5to. An amour of this kind, with a married woman, the mother of a numerous family, under whatever colours this reverend author, in the laxity of the morals of his country may choofe to palliate and difguife it, was in itfelf an offence both againft religion and morality, and muft have been viewed by the poet himfelf in a criminal light. But the general morals of Petrarch were exemplary, his virtue was even of a rigid caft; and, if at any time he was overpowered by the weaknefs of humanity, his mind, naturally of an ingenuous frame, fuffered the keeneft contrition, and prompted to an ample atonement, by a fincere avowal of his fault. In this light, however, he never confiders

confiders his paffion for LAURA. On the contrary, it appears to have been his glory and pride, and to have exalted him equally in his own efteem and in that of others.

Anima——
Da lei ti vien l'amorofo penfero,
Che mentre 'l fegui al fommo ben t' invia,
Poco prezzando quel ch' ogni huom defia:
Da lei vien l'animofa leggiadria,
Ch' al ciel ti fcorge per deftro fentero;
Si ch' i vo già de la fperanza altero. Son. 12.

Hence has my foul her nobleft aims deriv'd,
From that pure flame; hence rais'd her thoughts on high,
Indignant, fpurning what the vulgar train
Of earth-born fpirits prize. O beft of guides,
That chear'ft with hopes, that proudly lift the foul,
And point the path to heaven!———

"In amore meo," fays PETRARCH, in his dialogue with St AUGUSTINE, "nil turpe, nil obfcœnum, nil denique præter magnitudinem culpabilis." *Dial. de contemptu mundi.* "Illa juvenilem animum ab omni turpetudine revocavit, uncoque retraxit, atque alta compulit fpectare." *Ibid.* "Amore acerrimo, fed unico et honefto, in adolefcentia laboravi, et diutiùs laboràffem, nifi jam tepefcentem ignem mors acerba, fed utilis, extinxiffet." *Epift. ad poft.*

To any perfon who is acquainted with the writings of PETRARCH, and efpecially with thofe which were compofed after the death of LAURA, it muft appear the moft bigotted perverfion of ideas, to maintain that they are confiftent with the notion of his cherifhing a paffion for a married woman. I fhall here tranflate a few paffages from thofe latter poems.

IN

In the 289th ſonnet, or the 61ſt of the ſecond part, written after the death of Laura, we find theſe ſtrong expreſſions:

S' honeſto amor, può meritar mercede *, &c.

"If honourable and virtuous love e'er merited a reward, and "if compaſſion can aught avail, I ſhall obtain the recompence "of a conſtancy of affection, which, towards that dear object, "and in the eyes of the world, was as pure as the light of "heaven. Formerly ſhe miſtruſted that affection, and was un- "certain of the end and object of my paſſion. Now ſhe ſees "my heart and inmoſt ſoul; and thence I truſt that in heaven "ſhe now compaſſionates my ſufferings: for oft I behold her "in my dreams, regarding me with looks full of tenderneſs and "pity; and I fondly hope, that when I too ſhall have laid aſide "this garb of mortality, ſhe will welcome me to thoſe bleſt a- "bodes, where all true followers of Christ, and friends to virtue, "ſhall dwell for ever in happineſs." Can any perſon who reads this remarkable paſſage, believe that the love of Petrarch was a criminal and adulterous affection? If he ſtill heſitates on that point,

* *S' honeſto amor può meritar mercede:*
E ſe pietà anchor può quant' ella ſuole,
Mercede havrò; che più chiara che 'l ſole,
A madonna ed al mondo è la mia fede.
Già di me paventoſa, hor ſa, nol crede,
Che quello ſteſſo, ch' or per me ſi vuole,
Sempre ſi volſe; e s' ella udia parole,
O vedea 'l volto, hor l' animo, e 'l cor vede.
Ond' i' ſpero che 'n fin al ciel ſi doglia
Di miei tanti ſoſpiri; e così moſtra
Tornando a me sì piena di pietate:
E ſpero ch' al por giù di queſta ſpoglia
Venga per me con quella gente noſtra
Vera amica di Cristo, *e d' honeſtate.*

point, let him read the following ſtriking apoſtrophe in ſonnet 302.:

Donna, che lieta *, &c.

"O Lady, that now ſtandeſt in the preſence of God; as, ſure
"thy ſpotleſs life has well deſerved that place! O matchleſs paragon of all that is excellent in woman! Now, with the eyes
"of Him who ſees all things, thou beholdeſt my faithful love,
"my pure and virtuous affection; and thou ſeeſt, that towards
"thee my heart felt the ſame emotions while thou wert on
"earth, as now in heaven. O, that, in reward of all my lengthened ſufferings, I ſoon may join thee there!"

In the end of ſonnet 303., immediately following, he thus expreſſes himſelf:

Sol un conforto †, &c.

"My only comfort is, that ſhe who ſees my thoughts, may obtain for me that mercy that I may ſoon be with her."

So,

* *Donna, che lieta col principio noſtro*
Ti ſtai come tua vita alma richiede,
Aſſiſa in alta e glorioſa ſede.—
O delle donne altero e raro moſtro,
Hor nel volto di lui che tutto vede,
Vedi 'l mio amore, e quella pura fede.—
E ſenti, che ver te il mio core in terra
Tal fu, qual hora in cielo.——
Dunque per ammendar la lunga guerra,
Per cui dal mondo a te ſola mi volſi,
Prega, ch' io venga toſto a ſtar con voi.

† *Sol 'un conforto a le mie pene aſpetto*
Ch' ella che vede tutti i miei penſieri
M' impetre gratia ch' i' poſſa eſſer ſeco.

So, likewiſe, in the next ſonnet, 304.*:

"O happy day, when, iſſuing from this earthly priſon, and "throwing off the ſpoils of mortality, burſting from this cloud "of darkneſs into the ſplendour of eternal day, I ſee at once "my God, and the dear object of my love!"

In his hymn to the Virgin, with which he concludes his ſonnets, and which is, perhaps, the moſt perfect of his compoſitions, where he confeſſes all the errors and weakneſſes of his life; and when, from the nature of the ſubject, he muſt have deemed it nothing leſs than impiety to have uttered a falſehood, or even to have palliated or extenuated a crime, he takes merit to himſelf, in the ſight of heaven, for his paſſion for Laura; and thus reaſons with the Bleſſed Virgin, appealing to that clemency which he ſuppoſes her peculiar characteriſtic †:

"O Bleſſed Virgin, paragon of clemency and humanity, "let the example of the Almighty Being incite thee to ſhew "mercy to an humble contrite heart; and if with ſuch ſtrength "and ardour of affection I have been capable of loving a frail "mortal, what mayſt thou not conclude muſt be my devotion "towards

* *O felice quel dì, che del terreno*
Carcere uſcendo, laſci rotta e ſparta,
Queſta mia grave, e frale, e mortal gonna.—
E da sì folte tenebre mi parta,
Voltando tanto ſu nel bel ſereno,
Ch' io veggia il mio Signore, *e la mia Donna.*

† *Vergine umana, e nemica d' orgoglio,*
Del commune principio amor t' induca;
Miſerere d' un cor contrito umile;
Che ſe poca mortal terra caduca
Amar con sì mirabil fede ſoglio,
Che dovrò far di te coſa gentile!— Canz. 8. Part. 2.

" towards Thee, the bright example of all excellence!" In perfect union with theſe ſentiments of the poet himſelf, is that beautiful ſonnet, which Boccacio, recently after the death of Petrarch, compoſed in memory of his departed friend. It is formed on that favourite thought, which is moſt natural to the ſoul on the near proſpect of death;

Or ſe' ſalito, caro Signor mio,
Nel regno al qual ſalir ancora aſpetta
Ogni anima da Dio *a quello eletta,*
Nel ſuo partir di queſto mondo rio.
Or ſe' cola dove ſpeſſo il deſio
Ti tirò già per vedere Lauretta;
Or ſe' dove la mia bella Fiammetta
Siede con lei nel coſpetto di Dio.
Or con Senuccio, *e con* Cino, *e con* Dante,
Vivi ſecuro d' eterno ripoſo,
Mirando coſe da noi non inteſe.
Deh! s' aggrado ti fui nel mondo errante,
Tirami dietro a te, dove gioioſo
Vegga colei che pria d' amor m' acceſe.

" Now art thou gone, my dear maſter, to thoſe bliſsful ſeats, " to which all the ſpirits of the juſt, the elect of God aſpire, " when freed from this abode of guilt. Now haſt thou attain- " ed that heavenly region, which thy longing deſire to ſee thy " departed Laura had made the object of thy moſt earneſt " wiſhes; where my lovely Fiammetta ſits, along with her, in " the preſence of God. Now, with Senuccio, with Cino, and " with Dante, thou liveſt in the bliſsful aſſurance of eternal " reſt; and delighteſt in the contemplation of thoſe things " which ſurpaſs our weak underſtanding. Oh! if I was ever " dear to thee while in this fleeting ſcene of exiſtence, draw me " now to thyſelf, where once more I may be happy in the ſight " of her who was my firſt, my only love!"

6tò.

6to. IN the dialogue above quoted, (*De contemptu mundi, Dial.* 3.), where St AUGUSTINE is introduced reaſoning with the poet, and endeavouring to convince him of the errors of his paſt life, and particularly to diſſuade him from the indulgence of his paſſion for LAURA, to which he was as much a ſlave after her death as he had been during her life, the holy father makes uſe of every argument that can be drawn both from religion and morality: Would he have omitted the ſtrongeſt of all arguments? Would he have forgotten to urge that LAURA was the wife of another; and, conſequently, that his paſſion was a crime both in the ſight of GOD and man? Yet, to this purpoſe, there is not a ſingle word in the whole of that various and laboured argument.

7mo. IT will be in vain for a diſciple of this new hypotheſis to attempt its juſtification, upon the principle that the love of PETRARCH, being entirely of a refined and Platonic nature, might innocently have for its object a married woman, and the mother of a family. The author of the *Mémoires* himſelf abandons that ground of argument *, which, indeed, cannot be maintained in any conſiſtency with thoſe ſentiments which the poet himſelf has avowed in many parts of his works. The love of PETRARCH was no otherwiſe diſtinguiſhed from an ordinary paſſion than by its fervency and duration. He felt for LAURA the ſame emotions, which an ardent but honourable lover feels for a moſt beautiful, amiable and accompliſhed miſtreſs. He admired the graces of her mind, he reverenced her virtues, and he was enamoured of the beauties of her perſon. He owns, that he paſſionately deſired the reward of his love in the poſſeſſion of this treaſure. The poet, who expreſſes himſelf thus rapturouſly in the language of ordinary human love, muſt abandon all pretenſions to a Platonic affection:

* *Mémoires pour la Vie de* PETRARQUE, vol. i. note 21.

Con lei fos' io *, &c.

"Would that I were with her but a single night, alone, in dark-
"ness, where only the stars should behold us; and might that
"night last for ever! Might she never assume a borrowed shape,
"(like DAPHNE), to escape from my arms!"

Deh hor foss' io con vago de la LUNA †, &c.

"Ah that I were along with ENDYMION, the beloved of LUNA,
"laid to rest in some green wood; and that she who consumes
"my fleeting days were there alone with me, or attended only
"by love, for one single night! But might the sun remain for
"ever beneath the waves!"

So in the 58th sonnet,

PIGMALION, *quanto lodar ti dei* ‡, &c.

"How much, O PIGMALION, hadst thou reason to be content
"with

* *Con lei foss' io da che si parte il sole!*
E non ci vedess' altri, che le stelle,
Sol una notte; e mai non fosse l' alba,
E non si transformasse in verde selva
Per uscirmi di braccio.——Canz. 3. *A qualunque*, &c.

† *Deh hor foss' io con vago de la* LUNA
Addormentato in qualche verdi boschi
E questa ch' anzi vespro a me fa sera
Con essa, e con amor in quella piaggia
Sola venisse a stars' ivi una notte!
E 'l di si stesse, e 'l sol sempre ne l' onde!
Sest. 7. Part. I. *Non ha*, &c.

‡ PIGMALION, *quanto lodar ti dei*
Dell' imagine tua, se mille volte
N' avesti quel ch'io sol una vorrei.

" with thy image, who enjoyedſt a thouſand times thoſe de-
" lights, which O that I but could once taſte*."

THESE paſſages leave no room for the ſuppoſition of that refined and Platonic affection, which it is pretended a virtuous young man may, without blame, indulge for the wife of another. I enter not into the queſtion, how far even a theory of this kind is reconcileable to ſtrict morality, or whether that ſpecies of continued attention, that marked eſteem and preference, which at leaſt muſt be rewarded by a correſponding ſympathy and regard for the perſon who expreſſes them, is materially leſs injurious to the ſacred bond of conjugal affection, than a plan of ſeduction purſued from its ordinary motives. A moraliſt might perhaps decide, that where the effect of both is the ſame, the alienation of the affections of a wife; the garb of virtue and of decency, aſſumed by the former, is only a higher aggravation of its criminality. But the diſcuſſion of this queſtion is ſuperfluous, where the ſuppoſition of a Platonic love cannot, as we have ſeen, be admitted.

8*vò*. *Laſtly*, As the love of PETRARCH for LAURA was an honourable and virtuous paſſion, ſo the works of the poet afford ſufficient evidence, that he ardently deſired to be united to LAURA in marriage, and was even in the near proſpect of that happineſs:

Amor con quanto ſforzo oggi mi vinci!
E ſe non ch' al deſio creſce la ſpeme;
I' cadrei morto, ove più viver bramo. Son. 64.

Già

* IT is amuſing to obſerve, how even this paſſage has been ſtrained to admit of an interpretation ſuited to that Platonic affection which ſome of his commentators have wiſhed to aſcribe to the poet. The pleaſures, ſay they, which PETRARCH here expreſſed his deſire of enjoying, were thoſe which would ariſe on finding the picture of LAURA endowed, like PIGMALION'S ivory image, with ſpeech and underſtanding. But they own, at the ſame time, that, as PIGMALION'S enjoyments are generally believed to have been leſs refined, the poet has choſen an unlucky alluſion; and that the obvious ſenſe of the paſſage is *rien moins que Platonique.*

Già incomminciava a prender ſecurtade
La mia cara nemica a poco a poco
De' ſuoi ſoſpetti ; e rivolgeva in gioco
Mie pene acerbe, ſua dolce honeſtade :
Preſſo era 'l tempo dov' amor ſi ſcontra
Con caſtitate ; e a gli amanti è dato
Sederſi inſieme, e dir che lor' incontra. Son. 47. Part. 2.

Tempo era homai da trovar pace o tregua
Di tanta guerra ; ed erane in via forſe. Son. 48. Part. 2.

Tranquillo pórto avea moſtrato amore
A la mia lunga e torbida tempeſta.—
Già traluceva a' begli occhi 'l mio core,
E l' alta fede non più lor moleſta.
Ahi morte ria, come a ſchiantar ſe' preſta
Il frutto di molt' anni in sì poche hore ! Son. 49. Part. 2.

In ſupplement of theſe authorities from the ſonnets of Petrarch, may be added that report which was current at the time, or at leaſt among the earlieſt writers who have given any account of the poet's life, namely, that the Pope, who held Petrarch in the higheſt eſtimation, and to whom he was indebted for ſeveral valuable eccleſiaſtical preferments, was extremely ſolicitous that he ſhould be united in marriage to Laura, and offered to give him, in that event, a diſpenſation for retaining his church-benefices. If the ſtory is true, the Pope of whom it is recorded muſt have been Clement VI., as he is the only one of the Pontiffs, who were Petrarch's coteinporaries, to whom theſe characteriſtics could apply. M. Fleury, in his *Eccleſiaſtical Hiſtory*, is certainly miſtaken when he attributes the propoſition here mentioned to Benedict XII. the predeceſſor of Clement ; for Petrarch owed no favour to that Pontiff, whom he has ſatirized in many parts of his writings, as a

man

man of barbarous difpofitions, mean and fordid propenfities, and whofe grofs ignorance difgraced the high ftation which he occupied. The anecdote is mentioned by SQUARZAFICHI; as it is likewife by one of the oldeft editors of the poems of PETRARCH, in the preface to the Venice edition 1473: though the latter author erroneoufly names the Pope URBAN V. who did not afcend the papal chair till fome years after the death of LAURA. This uncertainty with regard to the perfon is not, however, fufficient to difcredit the fact itfelf, that the Pope, whoever he was, might, from favour to PETRARCH, have earneftly defired to fee him united to the object of his paffion.

THE clerical character of PETRARCH ought not to be confidered as affording any objection to the fuppofition, that he ardently wifhed to be united to LAURA in marriage. Though enjoying ecclefiaftical preferments, he had never accepted of any charge which conferred a care of fouls. He had frequently been folicited with earneftnefs to accept of a bifhopric; but conftantly refufed it, either from a fenfe of his own demerits, when weighed againft the qualities he thought requifite for that facred character, or, more probably, (as he himfelf indeed hints), from a defire to preferve his liberty, and follow, without reftraint, that courfe of life which he found moft congenial to his tafte. It is probable, therefore, that his views with refpect to LAURA had their influence on this determination; fince he was thus at liberty, merely by the facrifice of fome flender pecuniary emoluments, to change his condition at any time he might think proper. Examples of this kind were at that time extremely common; and the ftory above related, if true, is a proof that the Sovereign Pontiffs were even in ufe to difpenfe with the refignation of benefices to their particular favourites in thofe circumftances.

BUT, whatever weight we may be inclined to give to this anecdote, it is, on the whole, fufficient to our purpofe, if while, on the one hand, we have fhewn that there is not the fmalleft

folidity

ſolidity in all that elaborate fabric of argument, which has been brought to prove that LAURA was a married woman, we have proved, on the other, from the whole tenor of the writings of PETRARCH, the only evidence that applies to the matter, that his affection for LAURA was *an honourable and virtuous flame.* On this ſubject the reflection of M. de la BASTIE is equally beautiful, as a ſentiment of morality, and juſt, as an obſervation on human nature: " *Il n'y a que la vertu ſeule qui ſoit capable* " *de faire des impreſſions que la mort n'efface pas.*"

VII.

VII. Description *of an* Extra-uterine Fœtus. *By Mr Thomas Blizzard*, F. R. S. Edin. *Lecturer on Anatomy and Surgery, and Surgeon to the London Hospital.*

[*Read Feb.* 3. 1800.]

MUCH light has been thrown upon many of the moſt important and intricate functions of the animal machine, by accurate inquiries into the proceſſes of nature under extraordinary circumſtances. The doctrine of conception, lately advanced, owes its ſupport, in no ſmall degree, to obſervations made on caſes, in which, from adventitious cauſes, Nature has been led out of her ordinary tract. The following caſe appears to be of ſuch a deſcription; and, as tending further to elucidate the ſubject, may probably be not unacceptable to the learned members of this Society.

The woman, who was the ſubject of the following inveſtigation, was aged twenty-eight, of a robuſt form; had been ſix times pregnant; firſt at the age of nineteen, when ſhe was delivered of a living child; ſhe then had two abortions; of the fourth pregnancy ſhe was delivered of a living child; and of the fifth ſhe miſcarried, about five weeks before her death. On the morning of the day ſhe died, at about half paſt eleven, after having gone through conſiderable fatigue in cleaning her houſe, ſhe was ſeized with a violent pain in the lower part of the abdomen, which continued till her death; ſhe gradually became weaker; her abdomen became more and more ſwollen, and about nine in the evening ſhe died. Having felt extraordinary fenfations for a fhort time before this attack, fhe expreffed to her friends a great defire, that, fhould fhe die, her body might be examined.

The abdomen being opened, there appeared, proceeding from the pelvis, a confiderable quantity of blood, both in a fluid, and in a coagulated, ftate: two meafured quarts were taken from the cavity.

The fource of the hæmorrhage was an interefting point of inquiry. This foon appeared. In the middle of the Fallopian tube of the left fide, nearly at an equal diftance from its fimbriated extremity and its termination in the uterus, a pouch was formed, about the fize of a pigeon's egg, or a little larger, of an oval form, in the middle of which was a rupture, of the bignefs of a fmall quill, through which it was plain the blood had iffued. The parts were removed, for more accurate examination. Having divided the vagina, I was ftruck with the appearance of a quantity of jelly, extending from the os uteri, and alfo with the increafed fize of the uterus, which led to a fufpicion that there had been a recent conception. The nature of the cafe now began to unfold itfelf. It appeared, that an extra-uterine geftation had taken place; that the procefs was going on in the Fallopian tube, the embryo having refted there inftead of paffing to the cavity of the uterus; that the tube had enlarged to the capable extent, and then had burft. The fimbriated extremity of this tube was open as ufual, and its cavity nearly of the natural fize, perhaps a little enlarged. It poffeffed alfo the tortuous form, as is common, till it began to expand to form the pouch. The tube was of its natural fize alfo, in that half towards the uterus, and was alfo pervious; for, quickfilver was introduced by the tube near the uterus, towards the pouch, which paffed readily, and infinuated itfelf between the pouch and its contents. The quickfilver paffed readily alfo into the cavity

cavity of the uterus; ſo that there was no permanent obſtruction to the paſſage of the embryo into the uterus.

HAVING carefully laid open the pouch, it appeared full of a firm coagulum. Upon breaking through this, at the depth of about a quarter of an inch, there appeared a membranous circle, of a lightiſh colour, about the ſize of a ſcarlet bean, and within this another ſimilar membrane preſented itſelf, perfectly diſtinct from the former; probably theſe membranes repreſented the chorion and amnion. Within the latter membrane, nothing could be diſcovered but coagulum. There was no appearance of fœtus; but it muſt be remembered, that only five weeks had elapſed ſince the laſt abortion.

THE uterus was much thickened, being nearly twice its bulk in an unimpregnated ſtate; and, notwithſtanding the hæmorrhage which had occurred, its veſſels appeared conſiderably enlarged. The cervix uteri was entirely filled with a jelly, and the whole ſurface of the cavity of the uterus was lined with a ſoftiſh glutinous ſubſtance, which ſeemed organized, and was much redder than the reſt of the uterus. Changes had been produced in the uterus ſimilar to what are ſeen in ordinary geſtation; its parietes were thickened; its cavity enlarged; its cervix ſhut up with a jelly; and the glutinous effuſion juſt deſcribed might not perhaps improperly be conſidered as a membrana decidua in its early ſtate of formation.

THE ovaria were much veſiculated, particularly the left; in which there was a corpus luteum of conſiderable ſize: in the right ovarium there was no corpus luteum. The veſicles in theſe ovaria were large and much diſtended. Their appearance was exactly ſimilar to that of the ovaria of a young woman I ſome time ago examined, who had died from an accident, after having been married but a few days, in whoſe uterus there were evident marks of impregnation, and in both ovaria, corpora lutea, two or three, I am not certain which, in one ovarium, and one in the other. The corpora lutea, in both cafes, appeared to be formed by an effufion of blood, afterwards coagulated, into a burft veficle. The fection which fhewed the corpus luteum, difcovered its internal fubftance to be dark, with a border of a yellowifh colour, as if formed of coagulating lymph, feparated from craffamentum.

The very early impregnation, after the abortion, in this cafe, is a circumftance that feems entitled to remark. Only five weeks intervened between her laft mifcarriage and her death; and it muft be fuppofed that impregnation happened a confiderable time before her death, from the changes which had taken place. If I might be allowed to venture a conjecture of the caufe of thefe phenomena, Does it not appear that there might have been fome irregular contraction of the Fallopian tube, which is probably mufcular, that caufed the embryo to reft where it did? It was proved there was no permanent caufe of obftruction in the tube.

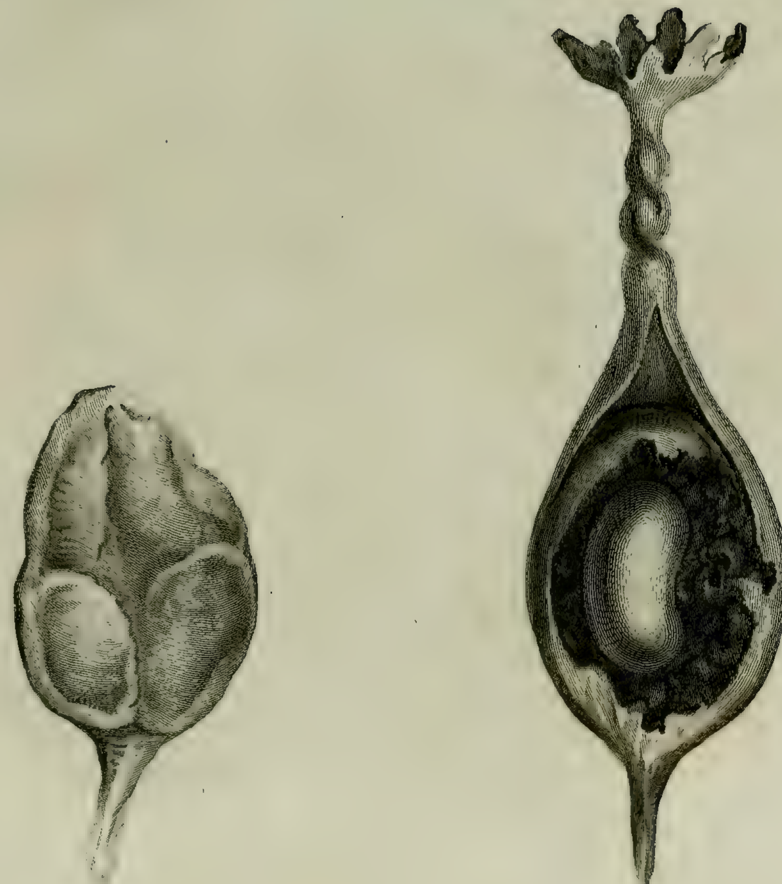
Explanation of the Plates.

Plate II. fhews the uterus and its appendages, with the enlargement in the Fallopian tube, having a burft opening in its middle. The whole is drawn, as nearly as poffible, of the dimenfions of the parts. By the divifion of the uterus may be feen its increafed thicknefs and fize, and an attempt to fhew the jelly occupying the cervix uteri.

Plate III. exhibits two figures: the one fhews the enlargement of the Fallopian tube, and the appearance of its contents as defcribed; the other expreffes the ovarium of the fame fide, with a large corpus luteum.

VIII.

Plate III.



VIII. METEOROLOGICAL ABSTRACT *for the* YEARS 1797, 1798, *and* 1799. *Communicated by JOHN PLAYFAIR*, F. R. S. EDIN. *and Profeſſor of Mathematics in the Univerſity of Edinburgh.*

[*Read at the Meetings in Jan.* 1798, 1799, & 1800.]

IN drawing up this abſtract, I have ſomewhat enlarged the plan followed in thoſe formerly communicated to the Society. In order to repreſent more accurately the progreſs of the ſeaſons, every month is here divided into three parts, and the ſtate of the barometer and thermometer is given for each of theſe diviſions.

IN the tables, therefore, that follow, the firſt column contains the greateſt height of the barometer for each of the above diviſions; the ſecond the leaſt; the third the mean; and the fourth the temperature of the air in the room where the barometer is kept; the fifth and ſixth columns ſhew the greateſt height of the thermometer in the air that was obſerved during the ten days to which the numbers refer; the next three give the mean heights as obſerved at three different times every day, viz. at 8 in the morning, 10 in the evening, and, as nearly as can be judged, when the day is warmeſt, that is, ſome time between mid-day and 3 in the afternoon. The mean of all theſe three is taken for the mean temperature of the day, which being computed for each day, the mean of all theſe mean temperatures is ſet down as the medium temperature of the air for every one of the thirty-ſix thirty-fix divifions of the year. The mean of the three divifions of every month is given in the next column, under the title of the mean temperature of the month.

This arrangement of the meteorological abftract has been adopted, as exhibiting the variations both of the barometer and thermometer, and alfo their mean ftate, without either entering into particulars too minutely, or abftracting from them too much.

It is prefumed that the mean temperatures, which are the points moft difficult to be afcertained, are given with tolerable exactnefs, as they are deduced from three obfervations made every day, of which the firft, that at 8 A. M. is itfelf not far from the medium temperature of the whole day, and the other two are as near as circumftances will allow, to the two extremes of greateft heat and greateft cold.

The barometer of which the heights are here given, is the fame that was ufed formerly, (See *Tranfactions of the Royal Society of Edinburgh*, Vol. IV. p. 213.) ; but in May 1798 its place was changed from Windmill Street to Buccleugh Place, about 130 yards farther to the fouth; 3 feet lower than before, or about 262 above the level of the fea. The thermometer is fhaded from the fun, expofed to the north, and about 30 feet from the ground: At this height it often ftands at 33 or even 34 degrees, when a flight froft is felt on the furface.

METEO-

METEOROLOGICAL TABLE FOR 1797.

		Greatest Height of Barometer.	Least Height of Bar.	Mean Height of Bar.	Mean Temperature of Merc. in Barometer.	Greatest Height of the Thermometer.	Least height of Ditto.	Mean Temp. of the Air 8 A. M.	Mean Temp. of Air when warmest.	Mean Temp. of the Air 10 P. M.	Mean Temp. of every ten days.	Mean Temp. of the Month.	Depth of Rain.	Days of West wind.	Days of East wind.
		Inches.	Inches.	Inches.	Inches.	°	°	°	°	°	°		Inches.		
Jan.	Div. 1.	30.324	29.610	30.0776	48.1	44.5	34.5	39.35	41.10	38.95	39.0			5	5
	—— 2.	30.096	29.096	29.7866	44.8	51.5	29.25	37.97	40.12	38.75	38.94	40.38	1.320	10	
	—— 3.	29.670	28.918	29.4400	48.4	52.0	35.0	42.66	44.50	42.50	43.22			11	
Feb.	Div. 1.	30.330	29.294	29.9302	51.3	52.5	33.0	47.40	49.65	40.30	45.78			10	
	—— 2.	30.126	28.820	29.7742	48.2	52.0	32.0	38.15	42.65	39.85	40.22	43.19	.775	8	2
	—— 3.	30.298	30.022	30.1385	51.9	52.0	34.0	40.94	47.93	41.87	43.58			5	3
March,	Div. 1.	30.106	29.245	29.7442	48.1	53.0	31.0	37.0	42.33	39.0	39.44			5	5
	—— 2.	30.124	29.238	29.6432	48.5	50.25	34.0	39.3	45.75	41.0	42.08	41.85	1.030	2	8
	—— 3.	29.922	29.038	29.5281	49.0	54.5	37.75	42.8	47.0	42.5	44.05			7	4
April,	Div. 1.	30.012	29.315	29.6154	49.75	53.0	39.5	43.12	50.0	42.6	45.24			2	8
	—— 2.	29.942	29.276	29.5272	50.3	55.50	38.25	45.52	52.50	46.0	48.01	46.51	1.460	5	5
	—— 3.	30.104	29.428	29.6442	51.4	60.0	40.50	44.35	50.02	44.5	46.29			5	5
May,	Div. 1.	29.974	28.970	29.4664	52.3	58.5	37.5	46.1	50.12	42.37	46.19			2	8
	—— 2.	29.919	29.220	29.5900	54.9	64.0	39.5	52.0	55.70	51.05	52.92	51.69	2.630	8	2
	—— 3.	30.116	29.310	29.6654	59.7	72.5	45.75	55.68	60.38	51.84	55.97			10	1
June,	Div. 1.	29.984	29.112	29.6400	57.4	63.0	43.5	52.67	58.05	47.70	52.81			7	3
	—— 2.	29.932	29.432	29.7214	58.9	62.5	44.25	53.10	56.75	50.17	53.34	53.65	1.533	1	9
	—— 3.	29.886	29.484	29.7300	59.0	63.0	49.5	54.05	58.72	51.67	54.81			2	8
July,	Div. 1.	30.026	29.186	29.5948	62.4	70.5	53.0	57.92	63.10	57.30	59.77			9	1
	—— 2.	29.838	29.412	29.6188	65.1	72.5	54.0	62.30	66.85	60.50	63.22	60.59	3.375	9	1
	—— 3.	29.832	29.116	29.7218	62.45	66.5	52.0	58.87	61.45	56.09	58.80			6	4
Aug.	Div. 1.	29.764	29.060	29.4368	61.3	65.75	53.5	57.82	63.82	54.80	58.81			10	
	—— 2.	29.710	29.220	29.4527	61.4	69.5	53.0	58.55	62.95	56.00	59.17	58.95	4.320	9	1
	—— 3.	29.920	29.430	29.6640	62.0	65.0	52.0	58.15	62.34	56.06	58.85			11	
Sept.	Div. 1.	29.996	29.222	29.5799	59.3	63.5	47.5	53.65	58.92	51.12	54.23			7	3
	—— 2.	29.664	28.834	29.3406	55.9	61.25	47.5	51.65	56.3	49.85	52.60	53.61	3.265	8	2
	—— 3.	29.722	29.326	29.5330	57.1	63.5	45.75	52.32	58.57	51.1	54.0			6	4
Oct.	Div. 1.	30.026	29.454	29.6810	56.45	59.75	45.25	50.82	57.3	49.5	52.81			8	2
	—— 2.	30.088	29.020	29.5447	54.37	55.0	34.5	46.42	51.1	45.2	47.84	46.18	3.097	10	
	—— 3.	30.088	28.840	29.4912	43.66	47.0	31.5	37.9	43.7	37.8	39.8			5	6
Nov.	Div. 1.	30.270	29.224	29.8077	50.45	55.5	32.5	43.1	51.0	45.3	46.47			8	2
	—— 2.	30.203	29.404	29.7576	48.9	51.0	32.0	39.17	44.45	39.4	41.01	40.02	1.245	10	
	—— 3.	29.704	28.658	29.4469	44.0	43.5	24.0	32.05	32.9	32.67	32.54			7	3
Dec.	Div. 1.	29.924	28.610	29.2714	43.6	44.0	30.0	35.17	37.57	35.07	35.77			7	3
	—— 2.	29.751	28.563	29.0573	46.7	55.0	30.5	40.32	41.57	42.52	41.13	39.87	1.910	10	
	—— 3.	30.104	29.080	29.7187	49.5	52.5	35.25	42.84	42.95	41.66	42.72			10	1
													Total Rain, 25.360	256	109

Mean Temperature of the Year, 48°.04

1797.

R E M A R K S.

There is little in the meteorological hiſtory of this year to be added to what is contained in the table. The winter was very open and mild, with a briſk ſteady wind, for the moſt part from W. to S. S. W. There was but little froſt, and ſcarcely any ſnow.

In March, the eaſt wind began to prevail; a pretty ſmart froſt was felt in the beginning of the month, and ſome ſnow fell on the 6th and 7th; the weather was cold for the ſeaſon till April, when its temperature came up nearly to the mean.

May, except at the beginning, when there were ſome froſty nights, was favourable, and the wind very often from the S. W. June, on the contrary, was cold, with a great prevalence of N. E. wind.

Though in July and Auguſt the weather became much better, yet the general temperature of the ſummer is by no means high; if we reckon the ſeaſon of vegetation from the vernal equinox to the 20th of October, we ſhall find the mean temperature during that time 53°.32, which is under the uſual quantity.

In October there was a great fall of rain, with uncommonly high floods. This was preceded by the wind changing on the 19th from the S. W., in which quarter it had been for ſeveral weeks, to the N. W., accompanied with froſt in the night. On the afternoon of the 20th, the wind came round to N. E. and E., blowing hard, with heavy rain all night. On the 21ſt, the rivers were much out; the bridge over the Tweed at Kelſo was ſwept away by the flood. The barometer, which had fallen very low on the 17th, continued ſo till the 26th.

On the 22d of November, a very ſharp froſt ſet in, with the wind N. and N. W. The thermometer, on the 23d at 10 at night, was at 24°, and nearly the ſame again on the 29th.

On the 4th of December, the wind came to the S. W., which put an end to the froſt; and about the middle of the month, the weather became uncommonly fine and warm for the ſeaſon. The thermometer was for ſome days conſtantly above 50°, and once as high as 55°, and this not at noon, but at night, on the 18th, with a clear ſky, and a high wind from S. S. W. The ſeaſon, however, could not maintain this temperature long; it became colder on the 21ſt, and the year ended with a ſlight froſt and the wind from the N. W.

METEO-

METEOROLOGICAL TABLE FOR 1798.

		Greatest Height of Bar.	Least Height of Bar.	Mean Height of Bar.	Mean Temp. of Merc. in Bar.	Greatest Height of the Thermometer.	Least Height of Ditto.	Mean Temp. of the Air 8 A. M.	Mean Temp. of Air when warmest.	Mean Temp. of the Air 10 P. M.	Mean Temp. of every ten days.	Mean Temp. of the Month.	Depth of Rain.	Days of West wind.	Days of East wind.
		Inches.	Inches.	Inches.	Inch.	°	°	°	°	°	°	°	Inches.		
Jan.	Div. 1.	30.303	29.003	29.7334	44.3	47.5	28.5	33.25	37.82	33.42	34.83			9	1
	—— 2.	29.943	28.785	29.401	46.5	50.5	35.0	39.67	41.65	40.67	40.66	38.68	2.310	8	2
	—— 3.	30.104	28.424	29.4936	48.4	47.75	32.0	40.11	41.5	40.05	40.55			11	0
Feb.	Div. 1.	30.490	29.152	29.968	48.6	50.0	33.75	41.25	44.50	42.65	42.47			9	1
	—— 2.	30.024	28.952	29.5955	48.4	51.5	22.25	36.77	41.62	35.55	37.98	39.05	.800	9	1
	—— 3.	30.043	28.800	29.505	46.1	45.0	30.0	33.84	39.57	36.72	36.71			7	1
March,	Div. 1.	30.023	29.750	29.8330	51.6	54.0	32.0	44.32	49.85	44.12	46.09			7	3
	—— 2.	29.990	28.884	29.6801	47.5	45.25	32.75	34.17	41.0	37.07	37.4	41.15	1.560	4	6
	—— 3.	30.101	29.489	29.7886	49.7	50.0	32.50	38.38	44.11	37.38	9.96			5	6
April,	Div. 1	30.063	28.764	29.4525	50.7	68.5	39.0	47.77	51.11	48.65	49.17			10	0
	—— 2.	30.010	29.255	29.6735	55.2	69.0	40.0	49.0	56.6	46.9	50.80	50.51	1.825	4	6
	—— 3.	30.062	29.734	29.8794	56.7	63.25	40.5	51.17	55.57	48.0	51.58			3	7
May,	Div. 1.	30.081	29.515	29.8611	57.2	02.0	44.0	49.22	55.4	48.45	51.02			2	8
	—— 2.	30.000	29.271	29.6200	58.9	62.5	45.0	51.6	59.1	49.85	58.49	56.21	1.480	8	2
	—— 3.	30.300	29.980	30.1097	62.3	73.0	49.75	58.4	64.77	54.15	59.11			6	5
June,	Div. 1.	30.282	29.461	29.9560	61.5	71.5	50.0	57.9	64.3	55.0	59.1			10	0
	—— 2.	30.145	29.382	29.8000	64.5	71.25	52.25	61.3	67.3	56.9	61.8	62.24	1.865	8	2
	—— 3.	30.041	29.325	29.7956	63.9	73.0	51.75	61.82	66.75	59.05	65.82			9	1
July,	Div. 1.	29.530	29.162	29.3662	63.6	75.0	55.5	62.42	67.45	58.65	62.84			10	0
	—— 2.	29.594	29.088	29.3430	62.6	66.75	53.25	58.52	63.77	55.32	59.2	60.58	2.530	7	3
	—— 3.	29.930	29.380	29.609	62.9	67.25	52.5	59.72	63.10	56.32	59.71			7	4
Aug.	Div. 1.	30.110	29.374	29.7042	64.2	72.25	51.25	60.75	65.02	57.07	60.95			8	2
	—— 2.	29.884	29.364	29.7452	62.1	66.5	49.5	57.8	61.07	55.97	58.28	60.05	2.995	6	4
	—— 3.	30.174	29.451	29.9366	65.2	74.5	50.25	60.2	66.0	56.60	60.93			9	2
Sept.	Div. 1.	29.804	29.062	29.4708	63.0	70.0	49.25	59.1	63.45	54.37	58.97			6	4
	—— 2.	29.888	28.722	29.5516	63.4	63.0	47.75	56.1	60.72	53.77	56.86	55.91	2.515	5	5
	—— 3.	29.728	28.924	29.3570	50.2	65.0	43.0	51.77	55.2	48.72	51.89			8	2
Oct.	Div. 1.	30.110	29.216	29.8207	58.3	62.5	46.75	55.05	58.07	52.32	55.14			7	3
	—— 2.	30.010	29.162	29.4818	56.6	59.5	36.5	46.17	51.02	46.55	47.91	48.69	2.105	8	2
	—— 3	29.611	28.975	29.3176	56.7	52.5	41.0	44.9	48.9	46.02	43.04			7	4
Nov.	Div. 1.	29.780	28.588	28.9600	57.0	55.5	35.5	43.52	47.77	43.65	44.98			8	2
	—— 2.	30.052	29.438	29.7588	52.5	43.0	29.5	37.02	41.3	37.12	39.76	41.39	2.210	5	5
	—— 3.	29.871	28.394	29.2345	52.1	51.5	28.0	37.8	40.97	39.6	39.45			7	3
Dec.	Div. 1.	30.225	28.950	29.6136	53.3	46.5	35.0	37.75	41.4	38.92	39.35			6	4
	—— 2.	30.248	29.050	29.5490	51.9	44.5	28.5	38.92	39.77	38.37	39.02	36.84	1.660	1	9
	—— 3.	30.428	29.408	30.022	48.0	44.0	18.0	32.04	34.04	32.45	32.17			6	5
												Total Rain,	23.835	250	115

Mean Temperature of the Year, 49°.28

1798.

REMARKS.

The climate of this part of our Iſland hardly admits of a finer ſeaſon than that of 1798. The winter, though not altogether without froſt, was on the whole mild and open. In March, indeed, the temperature was rather below the mean; but in April it roſe conſiderably above it, and continued ſo till the end of October.

It is remarkable of this year, that the temperature changed much more regularly than uſual, increaſing gradually till the end of June, and diminiſhing in the ſame manner till the end of December, without any conſiderable retrogradation. The mean temperature is more than 1°.5 above that of an ordinary year; and this difference fell chiefly on the ſummer months. The mean temperature of June, July and Auguſt, was 61°; the thermometer was often above 70, and never ſo low as 49 degrees. The vegetating ſeaſon, reckoned from the 20th of March to the 20th of October, has for its mean temperature 56°.17, conſiderably above the mean of an ordinary year; and, as along with this the rains fell ſeaſonably, the crops of all kinds were very abundant. The fine weather of this year extended over the whole of Britain, and indeed over the greateſt part of Europe.

METEO-

METEOROLOGICAL TABLE FOR 1799.

Month	Div.	Greateſt Height of Barometer.	Leaſt Height of Bar.	Mean Height of Bar.	Mean Temperature of Merc. in Barometer.	Greateſt Height of the Thermometer.	Leaſt height of Ditto.	Mean Temp. of the Air 8 A. M.	Mean Temp. of Air when warmeſt.	Mean Temp. of the Air 10 P. M.	Mean Temp. of every ten days.	Mean Temp. of the Month.	Depth of Rain.	Days of Weſt wind.	Days of Eaſt wind.
		Inches.	Inches.	Inches.	Inch.	°	°	°	°	°	°	°	Inches.		
Jan.	Div. 1.	30.301	29.744	30.0489	51.6	47.0	28.0	35.32	38.42	36.62	36.78			8	2
	—— 2.	29.940	29.046	29.6598	54.5	49.0	38.5	43.27	45.30	44.10	44.18	37.61	.335	10	
	—— 3.	29.656	28.854	29.2906	50.1	43.0	25.5	30.30	33.05	32.57	31.9			8	3
Feb.	Div. 1.	29.858	29.251	29.5582	45.3	40.0	21.25	27.32	31.97	27.10	28.8			2	8
	—— 2.	29.769	28.826	29.2571	52.0	45.5	28.0	35.97	40.09	37.47	38.8	37.60	.964	5	5
	—— 3.	29.814	28.685	29.4205	56.5	57.75	37.5	43.40	48.53	43.60	45.18			8	
March,	Div. 1.	30.148	29.500	29.9034	56.0	54.75	35.5	39.55	43.62	37.45	40.20			3	7
	—— 2.	30.100	29.280	29.5975	55.3	45.0	32.25	36.57	41.08	34.97	37.78	38.80	.695	5	5
	—— 3.	30.050	29.122	29.5236	52.8	49.0	31.25	37.70	40.09	36.13	38.42			0	11
April,	Div. 1.	29.860	29.072	29.4586	49.7	42.5	31.0	34.70	38.67	34.12	35.83			1	9
	—— 2.	30.080	28.666	29.2674	54.4	50.0	37.5	42.67	47.12	42.02	43.90	41.91	2.280	6	4
	—— 3.	29.986	29.170	29.7251	57.0	63.5	36.75	45.32	50.50	42.20	46.0			6	[illegible]
May,	Div. 1.	29.728	29.312	29.5605	55.7	52.0	34.5	42.57	46.62	40.60	43.08			2	8
	—— 2.	30.182	29.162	29.6807	57.1	61.50	37.5	44.57	50.45	44.05	46.02	46.88	3.355	5	5
	—— 3.	30.000	29.185	29.6777	60.5	66.50	44.0	52.40	58.00	49.25	51.55			11	0
June,	Div. 1.	30.164	28.585	29.6402	61.1	73.75	43.0	56.25	60.37	50.72	55.80			6	4
	—— 2.	30.260	29.560	29.9839	61.8	66.50	46.5	52.85	58.40	51.02	54.09	56.36	1.335	2	8
	—— 3.	30.100	29.600	29.8625	62.5	76.50	49.0	57.12	65.17	55.32	59.20			6	4
July,	Div. 1.	29.764	29.214	29.7151	62.3	70.5	53.0	60.85	65.65	56.90	61.13			8	2
	—— 2.	29.676	29.068	29.3556	60.6	66.0	50.0	55.62	60.62	53.45	56.56	58.09	3.125	5	5
	—— 3.	29.776	29.280	29.5629	59.2	67.5	50.75	55.80	59.68	54.34	56.60			4	7
Aug.	Div. 1.	29.884	29.024	29.4773	60.5	66.5	50.5	58.22	59.45	53.90	57.19			3	7
	—— 2.	29.664	28.760	29.2750	60.5	64.5	50.5	56.45	60.62	53.17	56.74	56.81	4.450	6	4
	—— 3.	29.820	29.110	29.5112	59.8	62.0	49.0	55.63	60.20	53.70	56.51			11	0
Sept.	Div. 1.	30.164	29.860	30.0120	62.5	66.25	51.5	55.50	62.00	54.52	57.34			6	4
	—— 2.	29.862	28.850	29.3300	60.3	64.0	50.0	53.32	57.17	52.82	54.44	54.26	4.085	3	7
	—— 3.	29.720	28.912	29.2634	58.0	57.5	44.5	50.47	54.62	48.97	51.02			8	2
Oct.	Div. 1.	29.640	29.010	29.3590	56.0	56.25	38.5	46.97	51.80	45.25	48.01			8	2
	—— 2.	29.786	29.210	29.5028	56.4	56.75	35.0	44.17	52.03	45.75	47.32	47.01	2.035	6	4
	—— 3.	30.012	29.012	29.5438	52.6	55.5	35.5	44.40	48.68	44.1	45.72			5	6
Nov.	Div. 1.	29.542	28.525	29.0798	56.5	52.5	33.0	42.92	46.17	41.0	43.36			9	1
	—— 2.	30.200	28.923	29.4466	56.5	49.75	34.0	39.80	44.75	39.52	41.02	41.70	1.645	10	0
	—— 3.	30.252	29.464	29.8733	54.9	51.5	33.0	38.72	42.27	41.17	40.72			7	3
Dec.	Div. 1.	30.024	28.950	29.5164	50.0	48.5	35.5	40.47	42.27	39.50	40.74			2	8
	—— 2.	30.370	29.610	29.9307	51.9	42.5	28.0	35.32	37.37	36.20	36.29	36.52	1.370	8	2
	—— 3.	30.352	29.792	30.1114	49.7	38.0	18.0	30.61	36.20	30.80	32.53			8	3
													Total Rain, 25.874	211	154

Mean Temperature of the Year, 46°.13

1799.

R E M A R K S.

The year 1799, though it afterwards proved fo unfavourable, began with very mild weather. January was uncommonly pleafant, and the wind generally from the S. W.

In the beginning of February, the wind changed to N. E.; on the 7th, the thermometer ftood at 22°; and the morning of the 9th was remarkable for the moft violent ftorm of wind and fnow that is remembered in this country: the wind was a little fouth of eaft, and the thermometer as low as 25°. The fame day afforded a ftriking example of the viciffitudes in our climate; for the ftorm, which began about one in the morning, having continued till noon, the wind foon after came to the S. W.; a thaw began immediately, and in the evening the thermometer ftood at 40°. The froft, however, foon returned, but without feverity; the weather in the latter end of the month was mild, though the fnow, which had been much drifted on the 9th, ftill lay deep in many places.

On the 3d of March, the wind came to the N. E., and the feries of cold and difagreeable weather began, which extended over almoft all the remainder of the year. The winds, during the whole of March, were dry and parching; the nights frofty, and the mean temperature hardly above that of February.

April was equally unfavourable; it fnowed frequently, and the wind, though often in the weft, was always fo far to the north, that it was never accompanied with warmth. Even at the end of the month there were fcarce any figns of vegetation, and the trees feemed hardly farther advanced than in February.

Through the whole of May the feafon continued extremely backward; the wind, whether in the eaft or the weft, was always far to the north, and the medium temperature of the month about 3° lower than in ordinary years.

June was a little more favourable; and indeed the only part of the feafon that afforded any thing like good fummer weather, was the laft ten days of June, and the firft ten of July. The mean temperature, during that time, was about 60°, and was not fo high, by feveral degrees, for any other period of the fame length in the whole fummer.

From this time the weather became continually colder. There was little funfhine, and a great deal of rain, during the remaining part of July, and the whole of Auguft and September.

The

The month of October was in itfelf tolerable, and nearly of the ufual temperature; but by no means able to make up the deficiencies of the former feafon. The harveft was not generally got in till the end of November, and, in high grounds, hardly till the end of December.

One of the peculiarities of this feafon was, that the eaft wind continued to blow very frequently after the folftice, through the whole fummer, and always from a northern point. The weft winds, alfo, were generally far to the north, which feldom happens in this climate, from the time of the fummer folftice to the fetting in of froft in November or the latter end of October.

The general impreffion which this year has left on every body's mind is, that it was very rainy; and yet it does not appear, from the regifter, that more rain fell in 1799 than in ordinary years. The fact is, that the only rainy months were July, Auguft and September, particularly the two latter; in thefe three the depth of rain was nearly a foot, which is at leaft double of the ufual quantity. This quantity, alfo, fell not in heavy fhowers, with intervals of funfhine, as is ufual in fummer, but in drizzling, long continued rains, with foggy weather, as in winter. Hence the amount of it was very naturally over-rated.

The mean temperature of the whole year is 46°.18, more than 1°.5 below the ufual mean. But the mean temperature of the feafon of vegetation, computed from the 20th of March to the 20th of October, is no more than 51°.27, almoft 5° below that of 1798. This deficiency of temperature may appear at firft fight hardly adequate to that deficiency in the crop which is afcribed to it; but it fhould be confidered, that vegetation fcarcely proceeds at all with a temperature under 40°, fo that this may not improperly be regarded as the point of heat at which vegetation begins, and the boundary, in as much at leaft, as refpects agriculture, between fruitfulnefs and fterility. Now, 56° is the mean temperature of a good feafon, in this country, as we know from the inftance of 1798, and therefore 16° of heat is the whole diftance between the mere germination of vegetables and the fulleft maturity they can attain in our climate. A deficiency of 5°, therefore, which is nearly a third of the whole 16°, muft neceffarily be accompanied with a great fhortcoming in the maturity of all vegetable productions.

Whether the quantity of the crop may be expected to be proportional to the excefs of the mean temperature of the vegetating feafon above 40°, fo that the crop of this year fhould be to that of the preceding as two to three, or if it muft be in a greater or a lefs ratio, may deferve to be more accurately confidered. There is, however, reafon to think, that the variations of the crop, at leaft of the corn crop, will be greater than in proportion to the variations of temperature; for, if the mean heat of the vegetating feafon were to fall as much below that of 1799, as the heat of 1799 did below that of 1798, it would be reduced to 46°, a temperature fo low, as would certainly prevent the ripening of corn altogether.

By

By doubling the deficiency of the heat, therefore, we do a great deal more than double the deficiency of the crop, ſo that the latter varies in a higher ratio than the former.

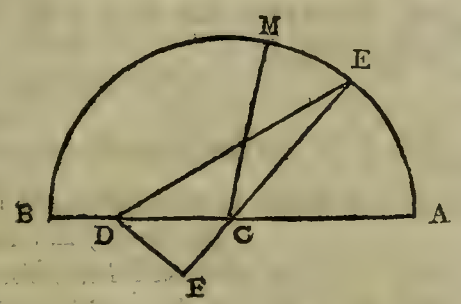
THE limit at which corn will not ripen, is probably higher than 46°, and may perhaps be ſtated at 48°. If it be alarming to think that our climate has, in any caſe, come ſo near to a line, beyond which the labours of the huſbandman are ineffectual, it is comfortable to reflect, that the reſources of the country have been able to provide a remedy even for ſo great an evil, and to avert all its moſt fatal effects.

IX.

IX. *A* New *and* Universal Solution *of* Kepler's Problem. *By James Ivory, Esq. Communicated by John Playfair, Professor of Mathematics and F. R. S. Edin.*

[*Read July* 7. 1800.]

1. KEPLER, having discovered the laws that regulate the motion of a planet in its orbit, proposed the following problem, for determining the true place of a planet at any given time: "To draw a "straight line DE, from an "eccentric point D in the "diameter of a semicircle "AEB, so that the whole se-"micircle may be to the "sector ADE, in a given "ratio."



In resolving this problem, we are to take the quadrature of the circle for granted: and therefore, if C be the centre of the circle, and if the sector ACM be taken, such, that the whole semicircle is to the sector ACM in the required given ratio, the problem may be otherwise enunciated: "To draw a straight "line DE from an eccentric point D, to cut off a sector ADE, "that shall be equal to the given sector ACM."

The given arch AM, or the given angle ACM, is usually called the mean anomaly; and the arch AE, or the angle ACE, the anomaly of the eccentric: the problem, therefore, is reduced

ced to this: "The mean anomaly being given, to find the ano-"maly of the eccentric."

2. Draw the ſtraight line DF at right angles to the diameter paſſing through the point E: Then, ſince the ſector ADE is equal to the ſector ACM, and the ſpace ACE is common to both, the triangle EDC will be equal to the circular ſector ECM: therefore, it is manifeſt, that the ſtraight line DF is equal to the circular arch EM.

Suppose that the radius of the circle is unity; and let $m =$ arch AM, $\mu =$ arch AE, and $\varepsilon =$ eccentricity DC: Then, ſince DF $= \varepsilon$ ſin μ, we ſhall have this equation, expreſſing the relation between the arch of mean anomaly and the arch of eccentric anomaly:

$$m - \mu = \varepsilon \text{ ſin } \mu.$$

3. In the equation juſt found, let us put $m = 2n$ and $\mu = 2\nu$: and, remarking that ſin $\mu =$ ſin $2\nu = 2$ ſin $\nu \times$ coſ ν, we ſhall readily obtain,

$$n - \nu = \varepsilon \text{ ſin } \nu \times \text{coſ } \nu:$$

And, if we further ſuppoſe $e = \varepsilon \times \frac{\text{ſin } (n - \nu)}{n - \nu}$, and, by means of this formula, exterminate ε, we ſhall find

$$\text{ſin } (n - \nu) = e \text{ ſin } \nu \times \text{coſ } \nu.$$

It may be remarked here, for the greater preciſion, that, from the nature of the problem, the arches, m and μ, never exceed 180°; and, of conſequence, the arches n and ν, never exceed 90°.

4. If we conſider e as a known or given quantity, it is evident, that the equation laſt found will no longer have the form of a tranſcendental equation; and that the arch ν will be determined, when the arch n is given, by a finite equation, reſolvable by known methods.

In ſtrictneſs, indeed, we cannot conſider e as a given quantity; for the exact value of e depends upon the arch ν, and cannot be known unleſs the length of that arch were known;

in

in other words, unlefs the problem, of which we are treating, were already refolved. But it is eafy to demonftrate, that e is always very nearly equal to the eccentricity ε; and that, therefore, we may affume $e = \varepsilon$, at leaft for a firft approximation to the value of ν.

FOR it is clear, from the moft elementary principles, that the maximum value of $\text{fin}\, \nu \times \text{cof}\, \nu$ is equal to $\frac{1}{2}$: therefore the arch $n - \nu$, determined by the equation $\text{fin}\,(n - \nu) = \varepsilon \,\text{fin}\, \nu \times \text{cof}\, \nu$, when greateft of all, can never exceed $\frac{\varepsilon}{2}$. It is alfo evident, from the nature of KEPLER's problem, that ε can never be greater than unity; becaufe the point D is fuppofed to be always taken in the diameter, and never without the circle. Therefore, even in the extreme cafe, when $\varepsilon = 1$, the arch $n - \nu$ can never be greater than $\frac{1}{2}$.

BUT fmall arches of a circle are very nearly equal to their fines; a propofition that we may extend, without great error, to all arches not exceeding 30°. Now, we have fhewn, that the length of the arch $n - \nu$ can never exceed $\frac{1}{2}$, and therefore, that arch will always be lefs than the arch of 30°, the fine of which is $\frac{1}{2}$. Therefore, the fraction $\frac{\text{fin}\,(n - \nu)}{n - \nu}$ will always be very nearly equal to unity; and, confequently, the value of e determined by the formula $e = \varepsilon \times \frac{\text{fin}\,(n - \nu)}{n - \nu}$, will, in all cafes whatfoever, differ but little from ε.

ASSUMING, therefore, $e = \varepsilon$, if we denote by ϖ the value of ν correfponding to this value of e, in the equation $\text{fin}\,(n - \nu) = e \,\text{fin}\, \nu \times \text{cof}\, \nu$; we may confider ϖ as a firft approximation to half the arch of eccentric anomaly.

5. HAVING thus found one approximate value of ν, it is eafy to find as many others as we pleafe, by means of the formulas already inveftigated.

For, in the formula $e = \varepsilon \times \frac{\text{ſin}\ (n - \nu)}{n - \nu}$, let ϖ, the firſt approximate value already found, be ſubſtituted for ν, and let ε' denote the value of e that will reſult from the ſubſtitution; or, which is the ſame thing, let $\varepsilon' = \varepsilon \times \frac{\text{ſin}\ (n - \varpi)}{n - \varpi}$: And further, let ε' be ſubſtituted for e in the equation ſin $(n - \nu) = e$ ſin $\nu \times$ coſ ν, and let ϖ' denote the correſponding value of ν: then will ϖ' be a ſecond approximation to the arch ν.

In like manner, ϖ' being ſubſtituted for ν in the formula $e = \varepsilon \times \frac{\text{ſin}\ (n - \nu)}{n - \nu}$, will give a new value of e, denoted by ε'': and, by means of the equation ſin $(n - \nu) = e \times$ ſin $\nu \times$ coſ ν, this new value of e will give a third approximation to the arch ν, denoted by ϖ''. And it is manifeſt, that the ſeries of arches, ϖ, ϖ', ϖ'', &c. approximating to the value of ν, may be continued indefinitely.

6. I now ſay, that the arches ϖ, ϖ', ϖ'', &c. which conſtitute the ſeries of approximations to the value of ν, are alternately too ſmall and too great: that is, the firſt, third, fifth, &c. terms in the ſeries are all leſs; but the ſecond, fourth, ſixth, &c. terms in the ſeries are all greater, than the exact value of ν.

For, if, in the equation ſin $(n - \nu) = e \times$ ſin $\nu \times$ coſ ν, we write ſin n coſ ν — coſ n ſin ν, for ſin $(n-\nu)$, and divide both ſides by ſin $\nu \times$ coſ ν, we ſhall get

$$e = \frac{\text{ſin}\ n}{\text{ſin}\ \nu} - \frac{\text{coſ}\ \nu}{\text{coſ}\ \nu}:$$

in this formula e vaniſhes when $n = \nu$: and, ſuppoſing the arch ν to decreaſe, it is manifeſt that the poſitive part, $\frac{\text{ſin}\ n}{\text{ſin}\ \nu}$, will increaſe, and that the negative part, $\frac{\text{coſ}\ n}{\text{coſ}\ \nu}$, will decreaſe: therefore e will increaſe when ν decreaſes; and the leſs the arch ν is, the greater will be the value of e. This, it is evident, muſt alſo be true, when taken inverſely; that is, the greater e is, the leſs will be the arch ν.

Let

Let us now conſider π, the firſt term in the ſeries of approximate arches; this arch is the value of ν in the equation $\text{ſin}\,(n-\nu)=e\times\text{ſin}\,\nu\times\text{coſ}\,\nu$, when ε is ſubſtituted for e; but, ſince we have $e=\varepsilon\times\frac{\text{ſin}\,(n-\nu)}{n-\nu}$, it is evident, that e is leſs than ε, and conſequently ν will be greater than π.

Again, take π', the ſecond term in the ſeries of approximate arches. This arch is the value of ν in the equation $\text{ſin}\,(n-\nu)=e\times\text{ſin}\,\nu\times\text{coſ}\,\nu$, when ε' is ſubſtituted for e: Now, $\varepsilon'=\varepsilon\times\frac{\text{ſin}\,(n-\pi)}{n-\pi}$, and $e=\varepsilon\times\frac{\text{ſin}\,(n-\nu)}{n-\nu}$; and ſince ν has been ſhewn to be greater than π, it is evident that $n-\pi$ will be greater than $n-\nu$: but the greater arch has to its ſine the greater ratio; therefore the fraction $\frac{\text{ſin}\,(n-\pi)}{n-\pi}$, will be leſs than the fraction $\frac{\text{ſin}\,(n-\nu)}{n-\nu}$: conſequently ε' will be leſs than e; and therefore π' will be greater than ν.

And, in general, if, in the formula $e=\varepsilon\times\frac{\text{ſin}\,(n-\nu)}{n-\nu}$, we ſubſtitute a greater arch for ν, we ſhall have a value of e greater than its true value; but, if we ſubſtitute a leſs arch for ν, we ſhall have a value of e leſs than its true value: but we have demonſtrated, that, in the equation $\text{ſin}\,(n-\nu)=e\,\text{ſin}\,\nu\times\text{coſ}\,\nu$, the greater e is, the leſs will the arch ν be: from which conſiderations the truth of what we have aſſerted above is evident, viz. that the arches π, π', π'', &c. continued indefinitely, are alternately too ſmall and too great.

7. Let us now compare together the alternate terms in the ſeries of approximate arches; and it will not be difficult to perceive, that the firſt, third, fifth, &c. terms, which have been ſhewn to be all leſs than half the arch of eccentric anomaly, continually increaſe; but that the ſecond, fourth, ſixth, &c. terms, which have been ſhewn to be all greater than half the arch of eccentric anomaly, continually decreaſe.

For, it is obvious, that the greateſt value of c, in the formula, $c = \varepsilon \times \frac{\text{ſin}\,(n - \nu)}{n - \nu}$, is when $n - \nu = 0$, in which caſe $c = \varepsilon$: therefore, the arch π will be the leaſt of all the approximate arches, π, π', π'', &c.: therefore π is leſs than π'', the third term in the ſeries. Again, we have $\varepsilon' = \varepsilon \times \frac{\text{ſin}\,(n - \pi)}{n - \pi}$, and $\varepsilon''' = \varepsilon \times \frac{\text{ſin}\,(n - \pi'')}{n - \pi''}$: and ſince π is leſs than π'', it is manifeſt that $n - \pi$ will be greater than $n - \pi''$: conſequently ε' will be leſs than ε'''; whence it follows, that π', the value of ν correſponding to ε', will be greater than π''', the value of ν correſponding to ε'''. Thus, then, we have ſhewn, that the firſt term in the ſeries π, π', π'', π''', &c. is leſs than the third term; but that the ſecond term is greater than the fourth: and, it is clear that, by a ſimilar mode of reaſoning, we may prove that the third term is leſs than the fifth; but the fourth term greater than the ſixth; and ſo on.

8. It has now been demonſtrated, that the arches in the ſeries π, π', π'', &c. are alternately leſs and greater than half the arch of eccentric anomaly; and further, that the terms in the ſeries, leſs than that arch, continually increaſe, while the terms in the ſeries, greater than that arch, continually decreaſe; whence, it is manifeſt, that by computing more and more terms of the ſeries, we ſhall have the value of the arch of eccentric anomaly within narrower and narrower limits. The arches π, π', π'', &c. form a ſeries of approximations, converging to the true length of half the arch of eccentric anomaly, and erring alternately in defect and in exceſs.

And here a queſtion occurs. It may be aſked, Do the terms of the ſeries π, π', π'', &c. converge ſlowly to the arch ſought? or, Do they converge rapidly to it? According to the anſwer that we ſhall be able to give to this queſtion, our method of ſolution is to be reckoned more or leſs perfect and valuable.

We

We have hitherto confidered the feries of approximations to the value of ν to begin with the arch π; but, in effect, the feries may be confidered to begin with the arch n. For, if in the formula $e = \varepsilon \times \frac{\text{fin}\,(n - \nu)}{n - \nu}$, we fubftitute n for ν, the refulting value of e will be ε, to which the arch π correfponds in the equation fin $(n - \nu) = e \times$ fin $\nu \times$ cof ν. It is clear, therefore, that the arch π is derived from the arch n, precifely in the fame way that π' is derived from π; or that any other term in the feries is derived from the term that immediately precedes it.

The error of the arch n, confidered as an approximation to ν, is $n - \nu$: taking the extreme cafe, when $\varepsilon = 1$, (in which cafe the convergency of the feries is evidently the floweft), the length of the arch $n - \nu$, when a maximum, is (Art. 3.) equal to $\frac{1}{2}$, correfponding to 28° 39′ nearly. Therefore the arch n, confidered as an approximation to ν, is very wide of the truth: And, if we can prove that the error of π, the fecond term in the feries, is neverthelefs inconfiderable, we fhall be entitled to conclude favourably with regard to the convergency of the feries.

The error of the arch π is $\nu - \pi$, and we are now to inquire, to what degree of magnitude this arch may attain. For this purpofe let us confider the two equations,

$$(n - \nu) = \varepsilon \times \text{fin}\,\nu \times \text{cof}\,\nu,$$

$$\text{fin}\,(n - \pi) = \varepsilon \times \text{fin}\,\pi \times \text{cof}\,\pi,$$

by means of which the arches ν and π are determined when the arch n is given. It is clear, that the quantities $\varepsilon \times$ fin $\nu \times$ cof ν and $\varepsilon \times$ fin $\pi \times$ cof π are evanefcent, when $\nu = 0$ and $\pi = 0$, and alfo when $\nu = 90°$ and $\pi = 90°$: therefore we fhall have $n = \nu = \pi$, not only when $n = 0$, but alfo when $n = 90°$. It has alfo been fhewn, that the arch ν is greater than the arch π: therefore the quantity $\nu - \pi$ vanifhes, when $n = 0$ and when $n = 90°$, and between thefe two limits it is always pofitive; confequently there is an intermediate value of n, where the arch $\nu - \pi$ will be a maximum.

Since

SINCE $2 \sin \nu \times \cos \nu = \sin 2\nu$, and $2 \sin \pi \times \cos \pi = \sin 2\pi$, the two equations become,

$$n - \nu = \frac{\varepsilon}{2} \sin 2\nu,$$

$$\sin (n - \pi) = \frac{\varepsilon}{2} \sin 2\pi;$$

take the fluxions of these equations, making n, ν, and π variable; then, having brought $\dot{n}$ to stand by itself on one side, we shall find,

$$\dot{n} = \dot{\nu}\,(1 + \varepsilon \cos 2\nu),$$

$$\dot{n} = \dot{\pi}\left(1 + \frac{\varepsilon \cos 2\pi}{\cos (n - \pi)}\right).$$

whence, by equating these two values of $\dot{n}$,

$$\dot{\nu}\,(1 + \varepsilon \cos 2\nu) = \dot{\pi}\left(1 + \frac{\varepsilon \cos 2\pi}{\cos (n - \pi)}\right).$$

Now $\nu - \pi$ is a maximum when $\dot{\nu} - \dot{\pi} = 0$, that is, when $\dot{\nu} = \dot{\pi}$: therefore, if we divide both sides of the preceding equation by the equal quantities $\dot{\nu}$ and $\dot{\pi}$; and further, reject what is common to both sides, and divide the remainders by ε, we shall have, in the case when $\nu - \pi$ is a maximum,

$$\cos 2\nu = \frac{\cos 2\pi}{\cos (n - \pi)}.$$

If we combine this equation with the two equations that express, in general, the relations of n to ν, and of n to π, we shall have three equations sufficient to determine the three arches, n, ν and π, in the case when $\nu - \pi$ is a maximum. But, as one of the equations is transcendental, this could only be done by the method of infinite series, and would lead into very perplexed calculations. We may, however, by an easy formula, determine a limit, which the quantity $\nu - \pi$, when greatest of all, cannot exceed: this will be sufficient for the purpose we have at present in view.

FROM

From the equation $\operatorname{cof} 2\nu = \frac{\operatorname{cof} 2\pi}{\operatorname{cof}(n - \pi)}$, we readily obtain

$$\operatorname{fin} 2\nu = \frac{\sqrt{(\operatorname{cof}^2(n-\pi) - \operatorname{cof}^2 2\pi)}}{\operatorname{cof}(n-\pi)} = \frac{\sqrt{\operatorname{fin}^2 2\pi - \operatorname{fin}^2(n-\pi)}}{\operatorname{cof}(n-\pi)};$$

but $\operatorname{fin}(n - \pi) = \frac{\varepsilon}{2} \operatorname{fin} 2\pi$, therefore, by fubftitution,

$$\operatorname{fin} 2\nu = \frac{\operatorname{fin} 2\pi}{\operatorname{cof}(n-\pi)} \times \sqrt{1 - \frac{\varepsilon^2}{4}}.$$

Multiply both fides by $\frac{\varepsilon}{2}$; and fubftitute, for $\frac{\varepsilon}{2} \operatorname{fin} 2\nu$, its equal $n - \nu$; and for $\frac{\varepsilon}{2} \operatorname{fin} 2\pi$, its equal $\operatorname{fin}(n - \pi)$, and we fhall have,

$$n - \nu = \frac{\operatorname{fin}(n-\pi)}{\operatorname{cof}(n-\pi)} \times \sqrt{1 - \frac{\varepsilon^2}{4}}.$$

Subtract both fides of this equation from $n - \pi$, and, remarking that $\tan(n - \pi) = \frac{\operatorname{fin}(n-\pi)}{\operatorname{cof}(n-\pi)}$, there will refult,

$$\nu - \pi = n - \pi - \tan(n - \pi) \times \sqrt{1 - \frac{\varepsilon^2}{4}}.$$

To fimplify this formula, I put $n - \pi = \varrho$, and $a = \sqrt{1 - \frac{\varepsilon^2}{4}}$: and fo, (in the cafe when $\nu - \pi$ is a maximum) we have

$$\nu - \pi = \varrho - a \tan \varrho.$$

Let us now confider the function $\varrho - a \tan \varrho$: becaufe $a = \sqrt{1 - \frac{\varepsilon^2}{4}}$ is lefs than unity, it is evident that we may take the arch ϱ fo fmall, that $a \tan \varrho$ fhall be lefs than ϱ; and, that there is a value of ϱ fuch that $\varrho = a \tan \varrho$: it is alfo manifeft, that between the limits $\varrho = 0$ and $\varrho = a \tan \varrho$, (in both which cafes $\varrho - a \tan \varrho = 0$) the function $\varrho - a \tan \varrho$ attains a maximum value.

If, therefore, we can prove, that the arch $n - \pi$ is always between the limits $\varrho = 0$ and $\varrho = a \tan \varrho$; it will follow that the maximum value of the function $\varrho - a \tan \varrho$ is greater than the arch $\nu - \pi$, even when that arch is greateft of all.

Now,

Now, ſince ſin $(n - \pi) = \varepsilon$ ſin $\pi \times$ coſ π, it is evident that ſin $(n - \pi)$ can never be greater than $\frac{\varepsilon}{2}$. Alſo, if $\varrho = a \tan \varrho$, we ſhall have ſin $\varrho \angle a \tan \varrho$; therefore $\frac{\sin \varrho}{\tan \varrho} = \cos \varrho \angle a$: therefore, ſince $a = \sqrt{1 - \frac{\varepsilon^2}{4}}$, it is is manifeſt that ſin ϱ is greater than $\frac{\varepsilon}{2}$. Whence it is obvious, that $n - \pi$ is leſs than the arch ϱ, determined by the formula $\varrho = a \tan \varrho$.

It now remains that we determine the maximum value of the function $\varrho - a \tan \varrho$: for this purpoſe, let $y = \tan \varrho$, and ſince $\dot{\varrho} = \frac{\dot{y}}{1 + y^2}$, we ſhall have, by the uſual method,

$$\frac{1}{1 + y^2} - a = 0;$$

whence $y = \tan \varrho = \sqrt{\left(\frac{1}{a} - 1\right)}$.

Therefore, if we take $\tan \varrho = \sqrt{\frac{1}{\sqrt{1 - \frac{\varepsilon^2}{4}}} - 1}$, the arch $\nu - \pi$ can never be greater than $\varrho - a \tan \varrho$.

If we take $\varepsilon = 1$, we ſhall have $\tan \varrho = \sqrt{\left(\frac{2}{\sqrt{3}} - 1\right)}$, whence $\varrho = 21^\circ 28' 14''$ nearly, and $\varrho - \frac{\sqrt{3}}{2} \tan \varrho = .03411$, which is the length of an arch of $1^\circ 57'$ nearly. It is therefore certain, that, even in the extreme caſe, when $\varepsilon = 1$, the arch π cannot differ from half the arch of eccentric anomaly, more than $1^\circ 57'$; a very ſmall error, conſidering that the firſt ſuppoſition of $n = \nu$ is very wide of the truth. We may therefore conclude, that the ſeries π, π', π'', &c. converges to the true length of half the arch of eccentric anomaly with uncommon rapidity.

9. We have now ſhewn, that, by means of the finite equation ſin $(n - \nu) = e$ ſin $\nu \times$ coſ ν, together with the formula $e = \varepsilon \times \frac{\sin (n - \nu)}{n - \nu}$, we may deduce a ſeries of arches, converging very rapidly to half the arch of eccentric anomaly. The rea-

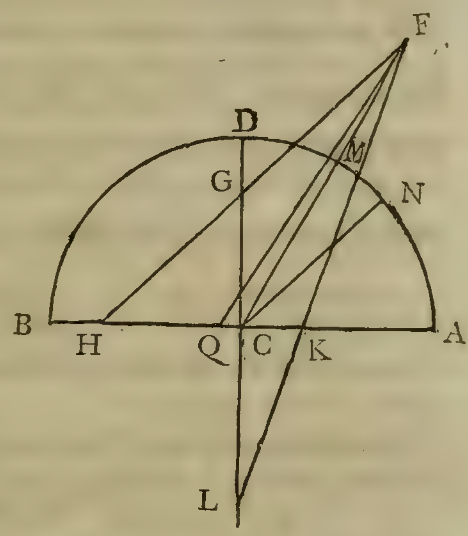
ſonings

ſonings that have led us to this concluſion are quite general, and hold good in every ſtate of the data of the problem. To have a complete and univerſal ſolution of this famous problem, it only remains, that we inveſtigate a rule for computing the arch v, from the equation ſin $(n - v) = e \times$ ſin $v \times$ coſ v, ſuppoſing n and e to be given quantities. This is what we are now to ſet about.

THE given equation, ſin $(n - v) = e \times$ ſin $v \times$ coſ v, is eaſily transformed into this, (Art. 6.)

$$e = \frac{\text{ſin } n}{\text{ſin } v} - \frac{\text{coſ } n}{\text{coſ } v}.$$

Let A D B be a ſemicircle, and let the diameter DC be drawn at right angles to AB: Take the arch AM $= n$, and in CM produced take CF $= \frac{1}{e} \times$ CA: From the point F draw the ſtraight line FGH, ſo that the part GH, intercepted between AB and CD, in the angle DCB, may be equal to CA, the radius of the circle: and, laſtly, through C draw CN, parallel to FH: I ſay, that the arch AN is equal to v.



FROM the two triangles FCH, FCG, we have

$$\text{ſin FHA : ſin FCA : : FC : FH} = \text{FC} \times \frac{\text{ſin AM}}{\text{ſin AN}},$$

$$\text{ſin FGD : ſin FCD : : FC : FG} = \text{FC} \times \frac{\text{coſ AM}}{\text{coſ AN}}.$$

Now, FH — FG = HG = CA, therefore

$$\text{CA} = \text{FC} \times \left(\frac{\text{ſin AM}}{\text{ſin AN}} - \frac{\text{coſ AM}}{\text{coſ AN}}\right);$$

 conſequently,

consequently, since $\frac{CA}{CF} = e$, we get

$$e = \frac{\sin AM}{\sin AN} - \frac{\cos AM}{\cos AN}:$$

therefore, because AM $= n$, it is manifest that AN $= \nu$.

Thus, then, the resolution of the equation $\sin(n - \nu) = e \sin n \times \cos \nu$, coincides with that case of the general problem, "*De inclinationibus*" of the ancients, in which the two given lines are supposed to be straight lines, intersecting one another at right angles. If we take CF $= \frac{1}{\varepsilon} \times$ CA, the arch AN, found by the construction above, will be no other than the arch π, the first term in the series that we have already discussed: and, in like manner, if we take CF successively equal to $\frac{1}{\varepsilon} \times \frac{(n - \pi)}{\sin(n - \pi)} \times CA$; $\frac{1}{\varepsilon} \times \frac{(n - \pi')}{\sin(n - \pi')} \times CA$; $\frac{1}{\varepsilon} \times \frac{(n - \pi'')}{\sin(n - \pi'')} \times CA$; and so on: we may find, by the same construction, the other terms π', π'', π''', &c. of that series.

If, therefore, we are to rest satisfied with a geometrical construction, we may consider Kepler's problem as already resolved. For, it is manifest from what has been proved, that, by means of the known and elementary problem, "*De inclinationibus*," we may, in all cases, approximate to the arch of eccentric anomaly as nearly as may be required. It must be confessed, however, that a construction of this kind, let it be ever so ingenious or elegant, is of no use to the astronomer, who seeks for a rule by which to conduct his calculations, and who will not be satisfied with a speculation of the mind.

10. The problem, "*De inclinationibus*," when the two lines given by position are supposed to be straight lines, is, in general, a solid problem. The geometrical construction cannot be effected, unless by the help of the conic sections; and the solution, by the modern algebra, leads to an equation of the fourth power,

power, or at leaſt to an equation of the third power. It is known, however, that in one particular ſituation of the given point, the problem becomes more ſimple. This happens when the given point is ſituated any where in a ſtraight line, biſecting the angle contained by the two lines given by poſition. In this caſe, the problem becomes a plane problem, and leads to an equation of the ſecond degree only.

SINCE, then, the ſtraight lines AB, CD, interſect one another at right angles, the plane caſe of the problem will happen, when the angle ACM is half a right angle; that is, when n is equal to an arch of $45°$, or when the mean anomaly is a right angle. This caſe deſerves to be particularly conſidered, on account of the ſimplicity of the ſolution it admits of.

WHEN $n = 45°$, it is obvious, that $\text{ſin}\, n = \text{coſ}\, n = \frac{1}{\sqrt{2}}$: and the general equation becomes

$$e = \frac{1}{\sqrt{2}} \times \left(\frac{1}{\text{ſin}\, v} - \frac{1}{\text{coſ}\, v}\right) = \frac{1}{\sqrt{2}} \times \frac{\text{coſ}\, v - \text{ſin}\, v}{\text{ſin}\, v\ \text{coſ}\, v};$$

ſquaring both ſides,

$$e^2 = \frac{1}{2} \times \frac{\text{coſ}^2 v + \text{ſin}^2 v - 2\,\text{ſin}\, v\ \text{coſ}\, v}{\text{ſin}^2 v\ \text{coſ}^2 v} = \frac{1}{2} \times \frac{1 - 2\,\text{ſin}\, v\ \text{coſ}\, v}{\text{ſin}^2 v\ \text{coſ}^2 v},$$

but $\text{ſin}\, 2v = 2\,\text{ſin}\, v\ \text{coſ}\, v$: therefore

$$e^2 = 2 \times \frac{1 - \text{ſin}\, 2v}{\text{ſin}^2\, 2v}.$$

HAVING reduced this formula, we ſhall find

$\text{ſin}^2\, 2v + \frac{2}{e^2}\,\text{ſin}\, 2v = \frac{2}{e^2}$: whence $\text{ſin}\, 2v = \frac{\pm\sqrt{2e^2+1} - 1}{e^2}$.

If we regard the problem, "*De inclinationibus*," independently of any application, and ſuppoſe that e may have all poſſible degrees of magnitude, both theſe values of ſin $2v$ may give ſolutions of the problem. One value, viz. $\text{ſin}\, 2v = \frac{\sqrt{2e^2+1} - 1}{e^2}$, being always leſs than 1, will give a ſolution, whatever be the magnitude of e: it will even give two ſolutions; becauſe ſin $2v$,

 being

being determined in magnitude only, will correſpond to two arches, the one leſs, and the other greater than 90°. Of theſe two arches, the former will give the poſition of the line FH, having the part GH, inſcribed in the angle DCB, equal to CA; and the latter will give the poſition of the line FKL, having the part KL, inſcribed in the angle ACL, equal to CA.

THE other value of ſin $2v$, viz. ſin $2v = \frac{-\sqrt{2e^2+1}-1}{e^2}$, will give no ſolution when e is not greater that 1; becauſe, in that caſe, $\frac{\sqrt{2e^2+1}+1}{e^2}$ is greater than 1: But, for other values of e, when $\frac{\sqrt{2e^2+1}+1}{e^2}$ is leſs than 1, this value of ſin $2v$ will alſo determine the poſition of two ſtraight lines, both lying in the angle ACD, that will ſatisfy the problem.

IN the particular application we have in view, e being never greater than 1, we muſt compute ſin $2v$ by the formula ſin $2v = \frac{\sqrt{2e^2+1}-1}{e^2}$: and all ambiguity will be taken away, by the conſideration that $2v$, or the arch of eccentric anomaly, muſt be leſs than the arch of mean anomaly.

THE formula ſin $2v = \frac{\sqrt{2e^2+1}-1}{e^2}$ is not very convenient in practice: we ſhall obtain another method of computing ſin $2v$, greatly preferable in this reſpect, in the following manner.

RESUME the formula,

$$e^2 = 2 \times \frac{1 - \text{ſin } 2v}{\text{ſin}^2 2v}.$$

Suppoſe ſin $2v = \frac{2 \text{ coſ A}}{1 + \text{coſ A}}$: then, by ſubſtitution, we ſhall find

$$e^2 = \frac{1}{2} \times \frac{(1 - \text{coſ A}) \times (1 + \text{coſ A})}{\text{coſ}^2 \text{A}} = \frac{\text{ſin}^2 \text{A}}{2 \text{ coſ}^2 \text{A}} = \frac{1}{2} \times \tan^2 \text{A}:$$

therefore $\tan \text{A} = e \times \sqrt{2} = e \times \text{ſec. } 45°$.

ALSO,

ALSO, since $\sin 2\nu = \frac{2 \cos A}{1 + \cos A}$; we have $1 - \sin 2\nu = \frac{1 - \cos A}{1 + \cos A} = \frac{\sin^2 \frac{A}{2}}{\cos^2 \frac{A}{2}} = \tan^2 \frac{A}{2}$: now let $2\nu = \mu = 90° - 2\lambda$; then $1 - \sin 2\nu = 1 - \cos 2\lambda = 2 \sin^2 \lambda$: therefore $\sin \lambda = \tan \frac{A}{2} \times \frac{1}{\sqrt{2}} = \tan \frac{A}{2} \times \sin 45°$.

INVERTING this analysis, we derive the following rule for computing the eccentric anomaly, when the mean anomaly is a right angle: Take $\tan A = e \times \sec 45°$; then $\sin \lambda = \tan \frac{A}{2} \times \sin 45°$, and $\mu = 90° - 2\lambda$. This rule would be rigorous and exact, if we could give to e the value it has in the formula $e = \varepsilon \times \frac{\sin \frac{m - \mu}{2}}{\frac{1}{2}(m - \mu)}$: but as this cannot be done, we must be content with approximating to the arch sought as nearly as our purpose may require. We will, therefore, by means of the rule, compute a series of arches, p, p', p'', &c. by successively substituting for e, the values ε, $\varepsilon \times \frac{\sin \frac{m - p}{2}}{\frac{1}{2}(m - p)}$, $\varepsilon \times \frac{\sin \frac{m - p'}{2}}{\frac{1}{2}(m - p')}$, &c. and the series p, p', p'', &c. will converge very quickly to the exact value of the arch of eccentric anomaly, erring alternately in defect and in excess. For the arches here denoted by p, p', p'', &c. are manifestly no other than the double of the arches formerly denoted by π, π', π'', &c. respectively.

THE case of the general problem that we have here resolved, may be thus enunciated: "To draw a straight line from an "eccentric

" eccentric point in the diameter of a ſemicircle, that ſhall di-" vide the ſemicircle into two equal parts."

11. Let us now proceed to conſider the reſolution of the formula,

$$e = \frac{\text{ſin } n}{\text{ſin } \nu} - \frac{\text{coſ } n}{\text{coſ } \nu},$$

ſuppoſing n to be any angle whatever not greater than a right angle.

From the point F, draw the ſtraight line FKL, ſo that the part KL, inſcribed in the angle ACL, may be equal to CA; and put $q =$ angle FKA. Then, by proceeding in a manner ſimilar to what is done in Art. 9. we ſhall eaſily derive this equation,

$$e = \frac{\text{coſ } n}{\text{coſ } q} - \frac{\text{ſin } n}{\text{ſin } q}.$$

Further, let the ſtraight line FQ be drawn to biſect the angle HFK: put $\varphi =$ angle FQA, and $\psi =$ angle KFQ $=$ angle QFH: then, ſince it has already been ſhewn, that $\nu =$ angle FHA, it is obvious that $\nu = \varphi - \psi$ and $q = \varphi + \psi$: whence we ſhall have theſe two equations, for determining the two angles φ and ψ,

$$e = \frac{\text{ſin } n}{\text{ſin } (\varphi - \psi)} - \frac{\text{coſ } n}{\text{coſ } (\varphi - \psi)};$$

$$e = \frac{\text{coſ } n}{\text{coſ } (\varphi + \psi)} - \frac{\text{ſin } n}{\text{ſin } (\varphi + \psi)}.$$

By taking away the denominators, and remarking, that ſin $(\varphi + \psi) \times$ coſ $(\varphi + \psi) = \frac{1}{2}$ ſin $(2\varphi + 2\psi)$ and ſin $(\varphi - \psi) \times$ coſ $(\varphi - \psi) = \frac{1}{2}$ ſin $(2\varphi - 2\psi)$, there will reſult

$$\frac{e}{2} \text{ ſin } (2\varphi - 2\psi) = \text{ſin } n \times \text{coſ } (\varphi - \psi) - \text{coſ } n \times \text{ſin } (\varphi - \psi), \text{ and}$$

$$\frac{e}{2} \text{ ſin } (2\varphi + 2\psi) = \text{coſ } n \times \text{ſin } (\varphi + \psi) - \text{ſin } n \times \text{coſ } (\varphi + \psi).$$

Take the ſum and difference of theſe two equations, and, for the ſums and differences of the ſines and co-ſines, having the ſame co-efficient, ſubſtitute the products that are equal to them; we ſhall have,

$e \sin 2\varphi \cos 2\psi = 2 \sin n \sin \varphi \sin \psi + 2 \cos n \cos \varphi \sin \psi$, and

$e \cos 2\varphi \sin 2\psi = -2 \sin n \cos \varphi \cos \psi + 2 \cos n \sin \varphi \cos \psi$.

Now, $\sin n \sin \varphi + \cos n \cos \varphi = \cos(\varphi - n)$, and

$-\sin n \cos \varphi + \cos n \sin \varphi = \sin(\varphi - n)$: therefore,

$$\left.\begin{array}{l} e \sin 2\varphi \cos 2\psi = 2 \cos(\varphi - n) \sin \psi \\ e \cos 2\varphi \sin 2\psi = 2 \sin(\varphi - n) \cos \psi \end{array}\right\} \text{(A)}$$

If in the ſecond of the equations (A), we write $2 \sin \psi \cos \psi$ for $\sin 2\psi$, and divide by $\cos \psi$, we ſhall obtain $e \cos 2\varphi \sin \psi = \sin(\varphi - n)$: whence $\sin \psi = \frac{1}{e} \times \frac{\sin(\varphi - n)}{\cos 2\varphi}$. But $\cos 2\psi = 1 - 2\sin^2 \psi = 1 - \frac{2}{e^2} \times \frac{\sin^2(\varphi - n)}{\cos^2 2\varphi}$: Subſtitute theſe values of $\sin \psi$ and $\cos 2\psi$ in the firſt of the equations (A), and, after having reduced, there will reſult,

$$e^2 \sin 2\varphi \cos^2 2\varphi - 2\sin^2(\varphi - n) \sin 2\varphi = 2 \sin(\varphi - n) \cos(\varphi - n) \cos 2\varphi.$$

Now, $2\sin^2(\varphi - n) = 1 - \cos(2\varphi - 2n)$, and $2 \sin(\varphi - n) \times \cos(\varphi - n) = \sin(2\varphi - 2n)$: therefore, by properly ordering the terms,

$$\sin 2\varphi - e^2 \sin 2\varphi \cos^2 2\varphi = \sin 2\varphi \cos(2\varphi - 2n) - \cos 2\varphi \sin(2\varphi - 2n).$$

But $\sin 2\varphi \cos(2\varphi - 2n) - \cos 2\varphi \sin(2\varphi - 2n) = \sin 2n$: therefore, if we put $x = \sin 2\varphi$, ſince $\cos^2 2\varphi = 1 - x^2$, our equation will finally become

$$x^3 + \left(\frac{1}{e^2} - 1\right) x = \frac{\sin 2n}{e^2},$$

which equation will ſerve to determine $x = \sin 2\varphi$.

We have ſtill to find the angle ψ: for this purpoſe I reſume the equations (A), and multiplying croſs-wiſe, and dividing by $2e$, there reſults,

$$\sin 2\varphi \sin(\varphi - n) \cos 2\psi \cos \psi = \cos 2\varphi \cos(\varphi - n) \sin 2\psi \sin \psi.$$

For $\sin 2\psi$ write $2 \sin \psi \cos \psi$: and for $2\sin^2 \psi$ write $1 - \cos 2\psi$; then, having properly diſpoſed the terms, we ſhall find

$$\big(\sin 2\varphi \sin(\varphi - n) + \cos 2\varphi \cos(\varphi - n)\big) \cos 2\psi = \cos 2\varphi \cos(\varphi - n).$$

Now,

Now, ſin 2φ ſin $(\varphi - n)$ + coſ 2φ coſ $(\varphi - n)$ = coſ $(\varphi + n)$: therefore

$$\text{coſ}\,(\varphi + n) \times \text{coſ}\,2\psi = \text{coſ}\,2\varphi \times \text{coſ}\,(\varphi - n),$$

whence, as φ is now known, coſ 2ψ will be found by this proportion,

$$\text{coſ}\,(\varphi + n) : \text{coſ}\,(\varphi - n) :: \text{coſ}\,2\varphi : \text{coſ}\,2\psi.$$

Having thus found both the angles φ and ψ, their difference will give the angle ν which is ſought.

In order to render the method of computation, derived from the preceding analyſis, as ſimple and as commodious for practice as the nature of the ſubject will permit, we ſhall change the letters φ and ψ to denote the double of what they have hitherto done; and we ſhall alſo put m for its equal $2n$: This being obſerved, we have the following rule:

1. Let there be formed this cubic equation

$$x^3 + \left(\frac{1}{e^2} - 1\right) x = \frac{\text{ſin}\,m}{e^2},$$

from which x is to be found: then x = ſin φ.

2. State this proportion,

$$\text{coſ}\,\frac{\varphi + m}{2} : \text{coſ}\,\frac{\varphi - m}{2} :: \text{coſ}\,\varphi : \text{coſ}\,\psi,$$

by which the angle ψ will be found.

Then $\varphi - \psi = 2\nu$ = arch of eccentric anomaly.

This rule would be rigorous and exact if we could give to e the value it has in the formula $e = \varepsilon \times \dfrac{\text{ſin}\,\frac{m - \mu}{2}}{\frac{1}{2}(m - \mu)}$: and if, by means of the rule, we compute a ſeries of arches, p, p', p'', &c. by ſucceſſively ſubſtituting for e, the values ε, $\varepsilon \times \dfrac{\text{ſin}\,\frac{m - p}{2}}{\frac{1}{2}(m - \mu)}$,

$\varepsilon \times$

$e \times \frac{\sin \frac{m - p'}{2}}{\frac{1}{2}(m - p')}$, &c.; the ſeries p, p', p'', &c. will converge very quickly to the exact value of the arch of eccentric anomaly, erring alternately in defect and in exceſs. For the arches that we here denote by p, p', p'', &c. are manifeſtly no other than the doubles of the arches formerly denoted by π, π', π'', &c. reſpectively.

12. It is to be remarked, that, ſince e is never greater than 1, the cubic equation in the rule above, is of the form which admits only one real root, ſo that it may either be reſolved by Cardan's rule, or by the ordinary methods of approximation; its root is always poſitive.

It is to be remarked too, that in the ſame cubic equation, $x = 1$, or $\sin \phi = 1$ when $\sin m = 1$; and, conſequently, $\phi = 90°$ when $m = 90°$. Hence, it is eaſy to infer, that the arch ϕ is leſs, or greater than a quadrant, or equal to it, according as the arch m is leſs, or greater than a quadrant, or equal to it. This remark is neceſſary to determine ϕ, when its ſine x is given, on account of the ambiguity of the ſines.

When $m = 90$, or $\sin m = 1$, we ſhall manifeſtly have the caſe of the problem that we before (Art. 10.) conſidered ſeparately. But though we have here $\sin \phi = 1$, and $\phi = 90°$, we ſhall in vain look for a ſolution of this caſe from the general rule, (Art. 11.): becauſe the firſt and third terms in the proportion for computing $\cos \psi$ become evaneſcent. We may, however, deduce the rule of calculation of Art. 10. from the general inveſtigation of Art. 11. in the following manner:

Resume the firſt of the equations (A), writing ϕ, ψ and m, for 2ϕ, 2ψ and $2n$, according to the change made in the notation: viz.

$$e \sin \phi \times \cos \psi = 2 \cos \frac{\phi - m}{2} \times \sin \frac{\psi}{2}.$$

Suppoſe

Suppoſe $\cos\psi = \frac{2\cos A}{1+\cos A}$: then, ſince $1 - \cos\psi = 2\sin^2\frac{\psi}{2}$

we have $2\sin^2\frac{\psi}{2} = \frac{1-\cos A}{1+\cos A} = \frac{\sin^2\frac{A}{2}}{\cos^2\frac{A}{2}} = \tan^2\frac{A}{2}$, therefore

$\sin\frac{\psi}{2} = \tan\frac{A}{2} \times \frac{1}{\sqrt{2}} = \tan\frac{A}{2} \times \sin 45°$: We ſhall alſo have

$2\sin^2\frac{\psi}{2} = \frac{1-\cos A}{1+\cos A} = \frac{(1-\cos A)^2}{1-\cos^2 A} = \frac{(1-\cos A)^2}{\sin^2 A}$; whence

$\sin\frac{\psi}{2} = \frac{1-\cos A}{\sin A} \times \frac{1}{\sqrt{2}}$: Subſtitute now, for $\cos\psi$, its value

$\frac{2\cos A}{1+\cos A}$; and for $\sin\frac{\psi}{2}$, its value $\frac{1-\cos A}{\sin A} \times \frac{1}{\sqrt{2}}$; and we ſhall eaſily obtain,

$$e\sqrt{2} \times \sin\varphi \times \cos A = \cos\frac{\varphi - m}{2} \times \sin A,$$

whence $\frac{\sin A}{\cos A} = \tan A = e \times \frac{\sin\varphi}{\cos\frac{\varphi-m}{2}} \times \sqrt{2}$.

Hence we have another general rule for computing ψ when φ is given, which is this: Take $\tan A = e \times \frac{\sin\varphi}{\cos\frac{\varphi-m}{2}} \times \sec 45°$;

then $\sin\frac{\psi}{2} = \tan\frac{A}{2} \times 45°$.

Taking $m = 90°$, and $\varphi = 90°$, this rule coincides with that already given in Art. 10.: And it may be of uſe, not only when m is exactly a right angle, but alſo when it is very nearly ſo.

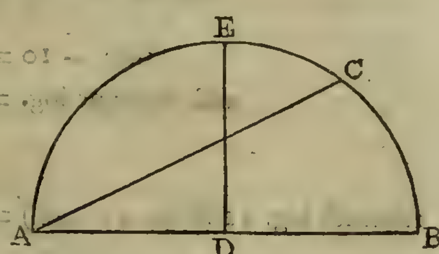
The detail with which we have diſcuſſed the rules and formulas of computation that have been deduced from the analyſis, leaves no difficulty in applying them to practice. Theſe rules are

are sufficient, in all cases whatever, for computing the eccentric anomaly, when the mean anomaly is given. They embrace the problem in its fullest extent, and, in point of universality, nothing more is to be wished for.

THUS, then, we have a general and direct method of determining the motion of a body describing an elliptic orbit, whether the eccentricity of the orbit be small or great. The method is so extensive, as even to comprehend the case, when the elliptic orbit, having become indefinitely flattened, the motion of the body is no longer in a curve, but in a straight line, tending to the centre of forces. (*Vide Prin. Math. lib.* 1. *sect.* 7. *prop.* 32. *et* 36.)

13. IN order to illustrate the method of computation required in the rules that have been investigated, I shall now subjoin two examples. I have selected, for this purpose, two problems relating to the circle, taken from a work of M. EULER, (*Int. in Analys. Inf. lib.* xi. *cap.* 22. *prob.* 4. *et* 5.) where they are resolved by the method of *trial and error*.

EXAMPLE 1. Prob. To draw a chord, AC, from the extremity of the diameter of a semicircle, that shall divide the semicircle into two equal parts.



TAKE D, the centre of the circle, and draw DE perpendicular to AB: It is manifest, that the sector BDE will be equal to the sector BAC; and that BE, being the mean anomaly, BC will be the anomaly of the eccentric. We have here, then, $m = 90^\circ$ and $\varepsilon = 1$: and we must compute by the rule in Art. 10.

 1. To

1. To compute p, the first term in the series, approximating to BC: Since $e = \varepsilon = 1$, we have

$$\tan A = \text{sec. } 45° = 10.1505150,$$

therefore $A = 54° \; 44'$.

Then $\sin \lambda = \tan \frac{A}{2} \times \sin 45°$; and $p = 90° - 2\lambda$.

$$\text{Now log. tan } \frac{A}{2} = 9.7140051$$

$$\text{log. sin } 45° = 9.8494850$$

$$\text{log. sin } \lambda = 9.5634901 = \sin 21° \; 28'.$$

Therefore $\lambda = 21° \; 28''$, and $p = 90° - 2\lambda = 47° \; 4'$; this value of p is less than BC.

2. To compute the second term p', we have $e = \varepsilon \times \dfrac{\sin \frac{m-p}{2}}{\frac{1}{2}(m-p)}$

$= \dfrac{\sin 21° \; 28'}{\text{arc } 21° \; 28'}$ and $\tan A = e \times \sec 45° = \dfrac{\sin 21° 28'}{\text{arc } 21° 28'} \times \sec 45°$.

$$\text{log. sec } 45° = 10.1505150,$$

$$\text{log. sin } 21° \; 28' = 9.5634335$$

$$\text{sum} - 10 = 9.7139485$$

$$\text{add constant log.} = 3.5362739$$

$$13.2502224$$

$$\text{subtract log. } 1288' \; (= 21° \; 28') = 3.1099159$$

$$\text{log. tan } A = 10.1403065$$

therefore $A = 54° \; 5' \; 54''$.

Then,

Then, fin $\lambda = \tan\frac{A}{2} \times$ fin $45°$, and $p' = 90° - 2\lambda$,

$$\log.\ \tan\frac{A}{2} = 9.7080866$$

$$\log.\ \text{fin}\ 45° = \underline{9.8494850}$$

$$\log.\ \text{fin}\ \lambda = 9.5575716 = \log.\ \text{fin}\ 21°\ 9'\ 53'',$$

therefore, $2\lambda = 42°\ 19'\ 46''$, and $p' = 47°\ 40'\ 14''$; this value of p' is greater than BC.

3. For the third term p'', we have $e = \varepsilon \times \dfrac{\text{fin}\dfrac{m - p'}{2}}{\frac{1}{2}(m - p')} = \dfrac{\text{fin}\ 21°\ 10'}{\text{arc}\ 21°\ 10'}$: and tan A $= e \times$ fec $45° = \dfrac{\text{fin}\ 21°\ 10'}{\text{arc}\ 21°\ 10'} \times$ fec $45°$.

$$\begin{aligned} \log.\ \text{fecant}\ 45° &= 10.1505150 \\ \log.\ \text{fin}\ 21°\ 10' &= \underline{\ 9.5576060} \\ \text{fum} - 10 &= 9.7081210 \\ \text{add conft. log.} &= \underline{\ 3.5362739} \\ & \quad 13.2443949 \\ \text{fubtract log.}\ 1270' &= \underline{\ 3.1038037} \\ \log.\ \tan A &= 10.1405912 \end{aligned}$$

therefore $A = 54°\ 6'\ 58''$.

Now fin $\lambda = \tan\frac{A}{2} \times$ fin $45°$, and $p'' = 90° - 2\lambda$;

$$\log.\ \tan\frac{A}{2} = 9.7082530$$

$$\log.\ \text{fin}\ 45° = \underline{9.8494850}$$

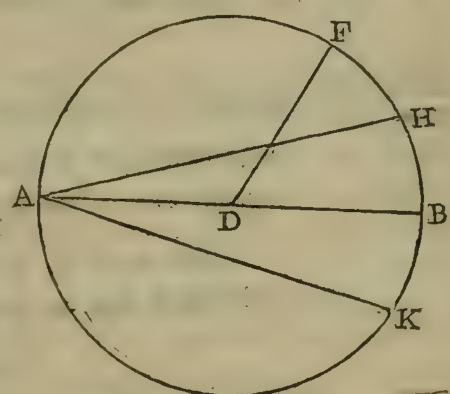
$$\log.\ \text{fin}\ \lambda = 9.5577380 = \log.\ \text{fin}\ 21°\ 10'\ 24''.$$

Therefore $2\lambda = 42°\ 20'\ 48''$, and $p'' = 47°\ 39'\ 12''$; this value of p'' is lefs than BC.

THE

The more accurate value of BC, according to the computation of M. Euler, is $47° \ 39' \ 12'' \ 46'''$: ſo that the laſt approximation is already almoſt exact. This example is well fitted to illuſtrate the convergency of the ſeries, p, p', p'', &c. even in the moſt unfavourable circumſtances.

Example 2. Prob. From a given point A, in the circumference of a circle, to draw two chords, AH and AK, that ſhall divide the circle into three equal parts.



Draw the diameter AB, and, from the centre D, draw the radius DF, making the angle BDF $=$ 60°. Becauſe the ſegment AH is one-third part of the whole circle, it will be two-third parts of the ſemicircle: therefore the ſector HAB will be one third part of the ſemicircle, and will be equal to the ſector BDF. Wherefore it is evident, that BF, being the mean anomaly, BH will be the anomaly of the eccentric: ſo that in this caſe we have $m = 60$, and $\varepsilon = 1$.

1. To compute the firſt term p: we have $e = \varepsilon = 1$, and the cubic equation becomes ſimply, $x^3 = \text{ſin } m$; whence $x = \text{ſin } \varphi = \sqrt[3]{\text{ſin } m} = \sqrt[3]{\text{ſin } 60°}$: therefore

$$\log. \text{ſin } \varphi = 9.9791769,$$
$$\text{and } \varphi = 72° \ 24'.$$

Now $\text{coſ} \frac{\varphi + m}{2} : \text{coſ} \frac{\varphi - m}{2} : : \text{coſ } \varphi : \text{coſ } \psi$,

coſ

$$\text{log. cof } \phi = 9.4805385$$

$$\text{log. cof } \frac{\phi - m}{2} = 9.9974523$$

$$19.4779908$$

$$\text{log. cof } \frac{\phi + m}{2} = 9.6058923$$

$$\text{log. cof } \psi = 9.8720985$$

therefore $\psi = 41° \ 51'$,
confequently $p = \phi - \psi = 30° \ 33'$, which is lefs than BH.

2. For the next term p', we have $e = \varepsilon \times \dfrac{\text{fin } \frac{m - p}{2}}{\frac{1}{2}(m - p)} = \dfrac{\text{fin } 14° \ 43' \ 30''}{\text{arc } 14° \ 43' \ 30''}$; and $x^3 + \left(\frac{1}{e^2} - 1\right) x = \dfrac{\text{fin } m}{e^2}$:

$$\text{log. fin } 14° \ 43' \ 30'' = 9.4051412$$

$$\text{add conft. log.} = 3.5362739$$

$$\text{fum} - 10 = 2.9414151$$

$$\text{fubtract log. } 883'.5 = 2.9462066$$

$$\text{log. } e = \bar{1}.9952085$$

$$\text{log. } \frac{1}{e} = 0.0047915$$

$$2$$

$$\text{log. } \frac{1}{e^2} = 0.0095830, \quad \text{and } \frac{1}{e^2} = 1.022410,$$

alfo $\frac{1}{e^2} - 1 = .022410 = a$. Now, log. fin $m = 9.9375306$,

therefore log. $\dfrac{\text{fin } m}{e^2} = 9.9471136$, and $\dfrac{\text{fin } m}{e^2} = .885347 = b$.

The

The cubic equation therefore becomes $x^3 + a\,x = b$: and it is manifeſt that x is nearly $=$ ſin $72° \; 24'$; and, having corrected this value, by the ordinary method, I find,

$$x = \text{ſin}\,\varphi = .9524420$$

therefore $\varphi = 72° \; 15' \; 31''$.

But, $\text{coſ}\,\frac{\varphi + m}{2} : \text{coſ}\,\frac{\varphi - m}{2} :: \text{coſ}\,\varphi : \text{coſ}\,\psi$,

$$\log.\ \text{coſ}\,\varphi = 9.4839026$$

$$\log.\ \text{coſ}\,\frac{\varphi - m}{2} = 9.9975102$$

$$19.4814128$$

$$\log.\ \text{coſ}\,\frac{\varphi + m}{2} = 9.6071052$$

$$\log.\ \text{coſ}\,\psi = 9.8743076.$$

Therefore $\psi = 41° \; 31' \; 20''$, and

conſequently $p' = \varphi - \psi = 30° \; 44' \; 11''$, which is greater than BH.

M. Euler finds the arch AH $= 129° \; 16' \; 27''$; therefore, BH $= 30° \; 43' \; 33''$: ſo that the ſecond approximation p', differs little more than half a minute of a degree from the truth.

14. The only caſes of Kepler's problem that are intereſting to the aſtronomical obſerver, are the two extreme caſes, when the eccentricity is very ſmall, and when it is very great, approaching to unity. The former of theſe two caſes is that of the planets, all of which deſcribe orbits very little eccentric, and nearly circular; the latter is that of the comets, which, on the contrary, move in very eccentric orbits. The principal object of this paper is accompliſhed in what has already been done; but it will be no improper ſequel, to apply the general method to the two caſes juſt mentioned.

The ſuppoſition that the eccentricity is ſmall, contributes greatly to remove the chief difficulties that occur in the ſolution of of KEPLER's problem. Indeed, it is only this particular cafe of the general problem, that we can confider to have been refolved, hitherto, in a fatisfactory manner. We already poffefs many excellent folutions of this cafe, fome of them deduced from the moft elementary principles, and others obtained by the aid of the higher calculus. To thefe another may be added, derived as a corollary from the general folution contained in this paper, and which will be found not unworthy of the notice of aftronomers.

BECAUSE the two arches denoted by μ and p are the double of the arches ν and π, the arch $\mu - p$, which is the error of p, confidered as an approximation to the eccentric anomaly, will be double of the arch $\nu - \pi$: therefore, it is obvious, (Art. 8.) that the arch $\mu - p$ will never be greater than

$$2\left(\varrho - \tan\varrho\sqrt{1 - \frac{\varepsilon^2}{4}}\right),$$

taking $\tan\varrho = \sqrt{\dfrac{1}{\sqrt{1 - \frac{\varepsilon^2}{4}}} - 1}$.

THE flighteft attention to the nature of this expreffion, is fufficient to evince, that it decreafes very rapidly as ε decreafes. If we evolve it into a feries, proceeding according to the powers of ε, that feries will contain only the third and higher odd powers. Therefore, when ε is fmall, as it is in the cafe of the planets, the amount of the above formula will be inconfiderable. It may even be fo inconfiderable, that the error of p will be of no account in practice, and the firft approximation will give the eccentric anomaly with the requifite exactnefs.

BY means of this formula, I have computed the limit of the error of p, for all the planetary orbits, and have arranged the refults in the following table:

	Value of ε	Limit of the error of p
Mercury,	.205513	1′ 46″.0
Venus,	.006885	0.0
Earth,	.016814	0.0
Mars,	.093088	9.8
Jupiter,	.048077	1.3
Saturn,	.056223	2.1
Georgium Sidus,	.046683	1.2

The inſpection of this table ſhews, that the error of the firſt approximation, obtained by the ſuppoſition of $e = \varepsilon$, in all the planetary orbits, is a very ſmall quantity, and ſuch as may be neglected on moſt occaſions.

It is to be recollected, that the rule we have inveſtigated for computing the eccentric anomaly, would give a rigorous reſult, provided the exact value of e were known. But, as that value cannot be deduced directly from the data, the repetition of the calculation is neceſſary to correct the firſt aſſumed value of e, and to make it approach nearer and nearer to the true value. The method of proceeding that is directed above, viz. to aſſume at firſt $e = \varepsilon$, and from thence to deduce a ſeries of approximations to the arch ſought, is perhaps the only one that will apply univerſally, and in all circumſtances of the problem. But it is to be obſerved, that the reaſoning in Art. 4, 5, and 6. will remain the ſame, provided only that the firſt aſſumed value of e be greater than its true value: and, if the firſt aſſumed value of e be leſs than its true value, the reaſoning will not thereby be eſſentially altered; all the change that will take place, is, that the error of the approximations will now be alternately in exceſs and in defect. Therefore, in applying the general method, if we can take hold of any circumſtances, peculiar to the parti-

cular

cular cafe, from which we can directly deduce a nearer value of e than the fuppofition of $e = \varepsilon$, we will avail ourfelves of fuch circumftances, and will thereby obtain a feries of approximations, converging fafter to the eccentric anomaly.

RESUME the general equation of the arches of eccentric and mean anomalies (Art. 2.) viz.

$$m - \mu = \varepsilon \sin \mu :$$

it is evident, that the lefs ε is, the lefs will be the difference of the two arches m and μ; and that, when ε is fmall, the quantity $\varepsilon \sin \mu$ will be nearly equal to the quantity $\varepsilon \sin m$. Therefore, if we take an arch τ, fuch that $2\tau = \varepsilon \sin m$, it is obvious that we fhall have $\frac{\sin \tau}{\tau} = \frac{\sin \frac{m - \mu}{2}}{\frac{1}{2}(m - \mu)}$ nearly, and, confequently, $e = \varepsilon \times \frac{\sin \tau}{\tau}$ nearly. To fpeak more correctly, the error of the affumption $e = \varepsilon$, will be of the fame order with the third power of the eccentricity; but the error of the affumption, $e = \varepsilon \frac{\sin \tau}{\tau}$, will be of the fame order with the fourth power of the eccentricity *.

 It

* SINCE $m - \mu = \varepsilon \sin \mu$, we have, in feries,

$$\sin \frac{m - \mu}{2} = \frac{\varepsilon \sin m}{2} - \frac{1}{6} \cdot \frac{\varepsilon^3 \sin^3 \mu}{2^3} + \frac{3}{40} \cdot \frac{\varepsilon^5 \sin^5 \mu}{2^5} - \&c.$$

Therefore $\frac{\sin \frac{m - \mu}{2}}{\frac{1}{2}(m - \mu)} = 1 - \frac{\varepsilon^2}{24} \cdot \sin^2 \mu + \frac{3\varepsilon^4}{640} \sin^4 \mu - \&c.$

and $e = \varepsilon \times \frac{\sin \frac{m - \mu}{2}}{\frac{1}{2}(m - \mu)} = \varepsilon - \frac{\varepsilon^3}{24} \sin^2 \mu + \frac{3\varepsilon^5}{640} \sin^4 \mu - \&c.$

fo that the error of the fuppofition $e = \varepsilon$, is manifeftly of the order ε^3.

AGAIN, from the equation $m - \mu = \varepsilon \sin \mu$, we eafily derive $\sin \mu = \sin m - \frac{\varepsilon}{2} \sin 2m$, neglecting the terms above the firft order: and, fubftituting this

It has been ſhewn above, that the error of the firſt approximation, derived from the aſſumption $e = \varepsilon$, is almoſt of no account, as to any real practical purpoſe, in the orbits of all the planets, excepting Mercury: and much more will this concluſion be true of the more exact aſſumption $e = \varepsilon \frac{\text{ſin}\,\tau}{\tau}$.

Let us now conſider the cubic equation which the rule requires to be reſolved: the equation is

$$x^3 + \left(\frac{1}{e^2} - 1\right) x = \frac{\text{ſin}\,m}{e^2},$$

and, in the caſe we are now occupied with, e is ſmall, being nearly equal to the eccentricity. Multiply all the terms of the equation by e^2; write ſin φ for x, and coſ^2 φ for $1 - x^2$; and we ſhall eaſily obtain,

$$\text{ſin}\,\varphi = \text{ſin}\,m + e^2\,\text{ſin}\,\varphi\,\text{coſ}^2\,\varphi.$$

In this formula it is clear, that the term e^2 ſin φ coſ^2 φ is inconſiderable in compariſon of the other two: therefore ſin φ = ſin m nearly; and, conſequently, the two arches φ and m will differ but little from one another. From this conſideration, we readily derive a ſeries of approximations, φ', φ'', φ''', &c. converging very faſt to the exact value of φ; viz.

$$\text{ſin}\,\varphi' = \text{ſin}\,m + e^2\,\text{ſin}\,m\,\text{coſ}^2\,m$$
$$\text{ſin}\,\varphi'' = \text{ſin}\,m + e^2\,\text{ſin}\,\varphi'\,\text{coſ}^2\,\varphi'$$
$$\text{ſin}\,\varphi''' = \text{ſin}\,m + e^2\,\text{ſin}\,\varphi''\,\text{coſ}^2\,\varphi'',$$

and ſo on. The error of the firſt approximation φ', is of the order

this value of ſin μ in the ſeries for e, we have, (neglecting the terms above the order ε^4),

$$e = \varepsilon - \frac{\varepsilon^3}{24}\,\text{ſin}^2\,m + \frac{\varepsilon^4}{24}\,\text{ſin}\,m \times \text{ſin}\,2m;$$

and this value of e is exact, as far as the order ε^4 incluſively.

But the aſſumption $e = \varepsilon \times \frac{\text{ſin}\,\tau}{\tau}$, where $\tau = \frac{\varepsilon\,\text{ſin}\,m}{2}$, being thrown into a ſeries, we get,

$$e = \varepsilon - \frac{\varepsilon^3}{24}\,\text{ſin}^2\,m + \frac{\varepsilon^5}{640}\,\text{ſin}^4\,m - \&c.$$

and therefore the error of this aſſumption is of the order ε^4.

order e^4 or ε^4; that of the fecond approximation is of the order ε^5; and in none of the planetary orbits will it be neceffary to pufh the approximations further than the fecond term of the feries.

THE method of finding the arch φ, that has juft been explained, is very commodious in practice; becaufe the value of $e^2 \sin \varphi \cos^2 \varphi$ is eafily computed by the common tables, when a known arch is fubftituted for φ. But we may, with advantage, apply the method of infinite feries to the refolution of the equation. If we put $\beta = 1 - e^2$, and $z = \frac{\sin m}{\beta^2} \times \frac{e}{\beta}$, we have

$$\sin \varphi = \frac{\sin m}{\beta^2} \times (1 - z^2 + 3 z^4 - 12 z^6 + 55 z^8 - \&c.).$$

THE rule requires ftill another operation, viz. to find the arch ψ. For this purpofe we have the proportion

$$\cos \frac{\varphi + m}{2} : \cos \frac{\varphi - m}{2} :: \cos \varphi : \cos \psi.$$

But, in the cafe we are now confidering, the arch ψ is always fmall: and, on this account, the proportion above is of little ufe in practice, when any degree of accuracy is required. The reafon is, that the common tables are not computed to a fufficient number of figures for determining fmall angles from their cofines. We will, therefore, prefer the other method of computing ψ, given in Art. 12. which is not liable to the fame inconvenience.

THE obfervations we have now made, lead us to the following rule, for computing the anomaly of the eccentric in the orbits of the planets:

1. COMPUTE the arch τ from the formula $2\tau = \varepsilon \times \sin m$.

2. COMPUTE $e = \varepsilon \times \frac{\sin \tau}{\tau}$; and determine the arch φ from the equation $\sin \varphi = \sin m + e^2 \sin \varphi \cos^2 \varphi$.

3. COMPUTE $\tan A = e \times \frac{\sin \varphi}{\cos \frac{\varphi - m}{2}} \times \sec 45^\circ$; and $\sin \frac{\psi}{2} = \tan \frac{A}{2} \times \sin 45^\circ$.

THEN $\mu = \varphi - \psi$.

It

It is to be kept in mind, that the arch found by this rule is only the firſt term of a ſeries of approximations, converging very faſt to the eccentric anomaly: and that, by a repetition of the calculation, a reſult may be obtained, that will ſatisfy the moſt ſcrupulous accuracy. But the rule may be conſidered as exact, as to any real practice, for the orbits of all the planets, excepting Mercury: and, even in the orbit of Mercury, the error will never exceed a few ſeconds. Let it be obſerved, further, that the error of the rule is chiefly in the arch ψ: for the error of ψ is of the ſame order with the error of e; whereas the error of φ is of the ſame order with the error of e^2.

Example. Let it be required to find the eccentric anomaly, correſponding to the mean anomaly $64^\circ\ 37'\ 8''.5$ in the orbit of Mars, ſuppoſing the eccentricity to be $= .093088$.

We have here $m = 64^\circ\ 37'\ 8''.5$, and $\varepsilon = .093088$.

1. To compute τ from the formula $2\tau = \varepsilon$ ſin m;

$$\begin{aligned} \log.\ \varepsilon &= \bar{2}.9688937 \\ \log.\ \text{ſin}\ m &= 9.9559089 \\ \text{conſt}\ \log. &= \underline{3.5362739} \end{aligned}$$

log. 2τ in minutes $= 2.4610765$; $2\tau = 289'$; and $\tau = 2^\circ\ 24'$.

2. Then $e = \varepsilon \dfrac{\text{ſin}\ \tau}{\tau}$; and ſin $\varphi' =$ ſin $m + e^2$ ſin m coſ^2 m.

$$\begin{aligned} \text{Sin}\ \tau &= 8.6219616 \\ \log.\ \varepsilon &= \bar{2}.9688937 \\ \text{conſt.}\ \log. &= \underline{3.5362739} \\ \text{ſum} - 10 &= 1.1271292 \\ \text{ſubtract}\ \log.\ 144' &= \underline{2.1583625} \\ \log.\ e &= \bar{2}.9687667 \\ \log.\ e^2 &= \bar{3}.9375334 \\ \log.\ \text{ſin}\ m &= 9.9559089 \\ 2\ \log.\ \text{coſ}\ m &= \underline{19.2642510} \end{aligned}$$

log. e^2 ſin m coſ^2 $m = \bar{3}.1576933$, and e^2 ſin m coſ$^2 m = .0014378$.

To

To this adding nat. fin $m = .9034776$, we have nat. fin $\varphi' = .9049154$, and $\varphi' = 64° \ 49'$. To have a more correct value of φ, repeat the calculation,

$$\begin{aligned} \text{Log. } e^2 &= \bar{3}.9375334 \\ \text{log. fin } \varphi' &= 9.9566250 \\ 2 \times \text{log. cof } \varphi' &= \underline{19.2578320} \end{aligned}$$

log. e^2 fin φ' cof^2 $\varphi' = \bar{3}.1519904$, whence nat. fin $\varphi = .9048966$, therefore $\varphi = 64° \ 48' \ 33''$.

3. To find ψ, we have $\tan A = e \times \dfrac{\text{fin } \varphi}{\text{cof} \frac{\varphi - m}{2}} \times \text{fec } 45°$;

$$\begin{aligned} \text{log. } e &= \bar{2}.9687667 \\ \text{log. fin } \varphi &= 9.9565982 \\ \text{log. fec } 45° &= \underline{10.1505150} \\ \text{Sum} &= 19.0758799 \\ \text{log. cof} \frac{\varphi - m}{2} &= \underline{9.9999994} \\ \text{log. tan A} &= 9.0758805 = \text{log. tan } 6° \ 47' \ 29'' \end{aligned}$$

But fin $\frac{\psi}{2} = \tan \frac{A}{2} \times$ fin $45°$, therefore

$$\begin{aligned} \text{log. tan } \frac{A}{2} &= 8.7733146 \\ \text{log. fin } 45° &= \underline{9.8494850} \\ \text{log. fin } \frac{\psi}{2} &= 8.6227996 = \text{log. fin } 2° 24' 16''.5. \end{aligned}$$

Therefore $\frac{\psi}{2} = 2°.24'.16''.5$, and $\psi = 4°.48'.33''$, fo that $\mu = \varphi - \psi = 60°$, the eccentric anomaly required.

15. It remains now to confider the cafe of the problem, applicable to comets, where the eccentricity is very great, or nearly equal to unity. Here the general folution may be ufed, and, even without any new fimplification, arifing from the peculiarities of the cafe, will bring out an accurate refult with very little trouble:

trouble. I ſhall give an example in the orbit of the famous comet of 1682.

The comet of 1682, which re-appeared in 1759, according to the prediction of Dr Halley, is the only one of which the period is known with any tolerable degree of certainty. M. de la Lande has fixed the period of this comet at 28070 days: computing from this the mean diſtance from the ſun, by the law of Kepler, that the cubes of the mean diſtances are as the ſquares of the periodic times, we ſhall find, that half the greater axis of the ellipſe deſcribed by the comet, is 18.07575, the mean diſtance of the earth from the ſun being unity. According to the determination of the ſame aſtronomer, the perihelion diſtance, eſtimated in parts of the ſame unit, is 0.5835; conſequently, the diſtance of the focus of the ellipſe from the centre, is 17.49225. Therefore, in the orbit of this comet, the eccentricity, or the quantity ε, is equal to $\frac{17.49225}{18.07575} = 0.96772$ nearly: and we can now aſſign the true place of the comet in the orbit, as well as its true diſtance from the ſun, at any given diſtance of time, from the paſſage over the perihelion or aphelion.

Example. Let it be required to find the anomaly of the eccentric of the comet 1759, (from which the true place and true diſtance from the ſun are derived by eaſy and known rules), 16 days, 4 hours, 44′ before or after the paſſage over the perihelion.

The mean anomaly, correſponding to the given time, is $0^\circ\ 12'\ 27''.83$, reckoned from the perihelion; but as our method requires the mean anomaly to be reckoned from the aphelion, we have $m = 179^\circ\ 47'\ 32''.17$.

1. To compute the firſt approximation to the eccentric anomaly ſought, we have $e = \varepsilon = 0.96772$; hence $\frac{1}{e^2} = 1.0678$; $\frac{1}{e^2} - 1 = .0678 = a$; $\frac{\sin m}{e^2} = .0038715 = b$.

Then,

Then, to find a value of x, from the equation, $x^3 + a\,x = b$: since b is small in comparison of a, it is manifest that x must be a very small fraction; and, consequently, x^3 inconsiderable in respect of $a\,x$: therefore $x = \frac{b}{a} = .05$ nearly: and, having corrected this value by the common method, we shall find,

$$x = \sin\varphi = .0547 = \sin 3^\circ\ 8', \text{ and so } \varphi = 176^\circ 52'.$$

As the first approximation which we are now computing, cannot be exact, even to the nearest minute, it would be fruitless to push the calculation to a great degree of accuracy. For the same reason, I here use the proportion in the general rule, because it requires but one operation for finding ψ, viz. $\cos\frac{\varphi + m}{2}$: $\cos\frac{\varphi - m}{2} :: \cos\varphi : \cos\psi$.

Hence ψ is found $= 3^\circ\ 1'$, and therefore $p = \varphi - \psi = 173^\circ\ 51'$; this is the first approximation to the eccentric anomaly reckoned from the aphelion, and is too small.

2dly, To compute the second approximation, we have,

$$e = \varepsilon \times \frac{\sin\frac{m-p}{2}}{\frac{1}{2}(m-p)} = \varepsilon \times \frac{\sin 2^\circ\ 58'}{\text{arc } 2^\circ\ 58'};\quad \text{whence } \frac{1}{e^2} = 1.06878;$$

$\frac{1}{e^2} - 1 = .06878 = a$; $\frac{\sin m}{e^2} = .0038749 = b$. Therefore, $x^3 + a\,x = b$: but we know, that a near value of x is $.0547$: and, having corrected this value, we shall find, $x = \sin\varphi = .0540430$; therefore, $\sin\varphi = .0540430 = \sin 3^\circ\ 5'\ 52''$, and $\varphi = 176^\circ\ 54'\ 8''$.

As ψ is here a small angle, we must use the method of Art. 12. to find it with the requisite exactness: this gives $\tan A = e \times \frac{\sin\varphi}{\cos\frac{\varphi - m}{2}} \times \sec 45^\circ$, and $\sin\frac{\psi}{2} = \tan\frac{A}{2} \times \sin 45^\circ$; therefore log. $\tan A = 8.8689481$, and $\psi = 2^\circ\ 59'\ 32''$, wherefore $p' = \varphi - \psi = 173° 54' 36''$, which is greater than the anomaly of the eccentric, reckoning from the aphelion, but very near to it: the error not exceeding three or four ſeconds. The eccentric anomaly, reckoned from the perihelion, is therefore nearly $6° 5' 24''$, but greater than this arch.

16. In this method of calculating, though any degree of accuracy may be obtained, yet when the diſtance from the perihelion is very ſmall, the computation may run out to conſiderable length. In ſeeking a remedy for this inconvenience, I ſaw that much advantage might be obtained, by comparing the motion in an eccentric ellipſe with the motion in a parabola, ſince at the perihelion they ſo nearly agree. The reſult of this compariſon will perhaps be thought to make an uſeful addition to the general methods explained above.

Let there be propoſed this geometrical problem: An eccentric ellipſe being given, and likewiſe a parabola, having the ſame perihelion diſtance with the ellipſe; it is required to draw a radius vector in the ellipſe, to cut off a ſector that ſhall be equal to a given ſector of the parabola.

If this problem can be reſolved, the application of it to the preſent reſearch will be eaſy.

Let the radii vectores that cut off the equal ſectors from the ellipſe and the parabola, be reſpectively denoted by ϱ and r; and let v and z be the angles that ϱ and r make with the axes of the curves, reckoned from the perihelia: then, conſidering the two ſectors as variable quantities, we ſhall have

$$\varrho^2 \dot{v} = r^2 \dot{z},$$

for theſe are obviouſly the doubles of the fluxions of the two ſectors.

Let a be the mean diſtance of the ellipſe, and ε the eccentricity: then, from the known property of the curve,

$$\varrho = \frac{a(1 - \varepsilon^2)}{1 + \varepsilon \operatorname{cof} v}.$$

Suppose

SUPPOSE $x = \tan \frac{v}{2}$; then $\operatorname{cof} v = \frac{1 - x^2}{1 + x^2}$: and, if we put p to denote the perihelion diftance, $= a\ (1 - \varepsilon)$, and $\lambda = \frac{1 - \varepsilon}{1 + \varepsilon}$, we fhall obtain, by fubftitution,

$$\varrho = \frac{p\,(1 + \varepsilon)\,(1 + x^2)}{(1 + \varepsilon) + (1 - \varepsilon)\,x^2} = p \times \frac{1 + x^2}{1 + \lambda\,x^2}.$$

Further, from the equation $x = \tan \frac{v}{2}$, we get, $\dot{v} = \frac{2\,\dot{x}}{1 + x^2}$:

Therefore,

$$\varrho^2\,\dot{v} = 2\,p^2 \times \frac{\dot{x}\,(1 + x^2)}{(1 + \lambda\,x^2)^2}.$$

AGAIN, the perihelion diftance of the parabola being, by the fuppofition, equal to the perihelion diftance of the ellipfe, we have, from the nature of the curve,

$$r = \frac{2p}{1 + \operatorname{cof} z} = \frac{p}{\operatorname{cof}^2 \frac{z}{2}}.$$

LET $y = \tan \frac{z}{2}$: then $\frac{1}{\operatorname{cof}^2 \frac{z}{2}} = 1 + y^2$ and $\dot{z} = \frac{2\,\dot{y}}{1 + y^2}$: confequently,

$$r^2\,\dot{z} = 2\,p^2 \times \dot{y}\,(1 + y^2).$$

Now, equating the values of $\varrho^2\,\dot{v}$ and $r^2\,\dot{z}$, that have juft been obtained, and, omitting the common multiplier, $2\,p^2$, there will refult,

$$\dot{y}\,(1 + y^2) = \frac{\dot{x}\,(1 + x^2)}{(1 + \lambda\,x^2)^2};$$

and taking the fluents

$$y + \frac{y^3}{3} = \left\{x + \frac{x^3}{3}\right\} - 2\lambda \times \left\{\frac{x^3}{3} + \frac{x^5}{5}\right\} + 3\lambda^2 \left\{\frac{x^5}{5} + \frac{x^7}{7}\right\} - 4\lambda \left\{\frac{x^7}{7} + \frac{x^9}{9}\right\} + \&c.$$

It is manifeft, that this fluent requires no correction; becaufe

 the

the two angles, v and z, are ſuppoſed to begin to flow together at the perihelia of the curves.

17. According to the ſuppoſition, the eccentricity ε is nearly equal to unity, and conſequently, $\lambda = \frac{1 - \varepsilon}{1 + \varepsilon}$ will be a ſmall fraction: but, as $x \left(= \tan \frac{v}{2}\right)$ increaſes from 0 to ∞, it is obvious, that the fluent, obtained above, will be of uſe in computation only to a certain limit, however ſmall λ may be ſuppoſed to be. For the part of the fluent depending on λ manifeſtly converges by the powers of the quantity λx^2; and therefore, as long as λx^2 is a ſmall fraction, ſo long only can we compute x when y is given, by means of the fluent: but when x has paſſed that limit, the fluent, in the form here given to it, is no longer of any uſe in computation.

But the fluent, although limited in its application by the conſideration juſt explained, will enable us to compute x when y is given, and to determine the angle v of true anomaly in the ellipſe, by means of the angle z in the parabola, for a conſiderable portion of the elliptic orbit lying adjacent to the perihelion, on either ſide. We may therefore deduce from it a ſeries that will ſerve to compute the true place of a comet in the portion of its orbit which it deſcribes during one apparition.

In order to determine the angle v by means of the angle z, we muſt firſt find a value of x in terms of y: and, to avoid too complex calculations, we ſhall neglect the terms multiplied by the powers of λ, higher than the ſquare. The extreme ſmallneſs of λ, in the orbits of all the comets, permits to ſimplify the calculation in this manner, and, nevertheleſs, to obtain a reſult, that will be ſufficiently exact during the time of one apparition.

Neglecting, then, the terms multiplied by the powers of λ higher than the ſquare, we have

$$y + \frac{y^3}{3} = \left\{x + \frac{x^3}{3}\right\} - 2\lambda \left\{\frac{x^3}{3} + \frac{x^5}{5}\right\} + 3\lambda^2 \left\{\frac{x^5}{5} + \frac{x^7}{7}\right\}.$$

Aſſume

Assume $x = y + A\lambda y^3 + B\lambda^2 y^5$,

A and B being indeterminate quantities, not depending on λ: Then, neglecting the quantities which the degree of exactness prescribed permits us to neglect, we shall find,

$x^3 = y^3 + 3A\lambda y^5 + 3(B + A^2)\lambda^2 y^7$,

$x^5 = y^5 + 5A\lambda y^7$,

$x^7 = y^7$.

If now we substitute these values in the equation between x and y, and omit the terms common to both sides, there will result,

$$\begin{aligned} 0 = (A + Ay^2) \times \lambda y^3 &+ (B + By^2 + A^2y^2) \times \lambda^2 y^5, \\ -2\left(\frac{1}{3} + \frac{1}{5}y^2\right) \times \lambda y^3 &- 2(A + Ay^2) \times \lambda^2 y^5, \\ &+ 3\left(\frac{1}{5} + \frac{1}{7}y^2\right) \times \lambda^2 y^5. \end{aligned}$$

Hence

$$A = \frac{2}{3} \times \frac{1}{1+y^2} + \frac{2}{5} \times \frac{y^2}{1+y^2};$$

$$B = \frac{11}{15} \times \frac{1}{1+y^2} + \frac{13}{35} \times \frac{y^2}{1+y^2} - A^2 \times \frac{y^2}{1+y^2}.$$

But, since $y = \tan\frac{z}{2}$; therefore $\frac{1}{1+y^2} = \cos^2\frac{z}{2} = 1 - \sin^2\frac{z}{2}$, and $\frac{y^2}{1+y^2} = \sin^2\frac{z}{2}$: consequently

$$A = \frac{2}{3} - \frac{4}{15}\sin^2\frac{z}{2},$$

$$B = \frac{11}{15} - \frac{254}{315}\sin^2\frac{z}{2} + \frac{16}{45}\sin^4\frac{z}{2} - \frac{16}{225}\sin^6\frac{z}{2}.$$

18. SUPPOSE $v = z + w$; w expressing the difference of the two angles v and z, which, it is obvious, depends on λ, and is to be reckoned of the same order with that quantity: Then $x = \tan\frac{v}{2} = \tan\frac{z+w}{2}$: but $y = \tan\frac{z}{2}$; therefore, according to TAYLOR's theorem, rejecting the quantities that ought to be rejected,

rejected,

$$x = \tan\frac{z+w}{2} = y + \frac{\dot{y}}{\dot{z}} \times w + \frac{1}{2} \cdot \frac{\ddot{y}}{\dot{z}^2} \times w^2.$$

Assume now $w = C \times \lambda + D \times \lambda^2$; C and D being indeterminate quantities, not depending on λ; then, by substitution,

$$x = y + \frac{\dot{y}}{\dot{z}} \times C\lambda + \left\{\frac{\dot{y}}{\dot{z}} D + \frac{1}{2} \cdot \frac{\ddot{y}}{\dot{z}} \times C^2\right\} \times \lambda^2.$$

But $\frac{\dot{y}}{\dot{z}} = \frac{1+y^2}{2}$; and $\frac{1}{2} \cdot \frac{\ddot{y}}{\dot{z}^2} = \frac{y(1+y^2)}{4}$: therefore,

$$x = y + \frac{C}{2} \times (1+y^2) \times \lambda + \left\{\frac{D}{2} + \frac{C^2}{4} y\right\} \times (1+y^2) \times \lambda^2.$$

We must now determine C and D, so that this value of x, and its value already found, may be identical: Thus we have,

$$\frac{C}{2} = A \times \frac{y^2}{1+y^2} \times y,$$

$$\frac{D}{2} + \frac{C^2}{4} \times y = B \times \frac{y^2}{1+y^2} \times y^3;$$

therefore,

$$C = 2A \times \sin^2\frac{z}{2} \times y,$$

$$D = \left\{2B \sin^2\frac{z}{2} - 2A^2 \sin^4\frac{z}{2}\right\} \times y^3,$$

and, taking the values of A and B, (Art. 17.)

$$C = \left\{\frac{4}{3} \sin^2\frac{z}{2} - \frac{8}{15} \sin^4\frac{z}{2}\right\} \times y;$$

$$D = \left\{\frac{22}{15} \sin^2\frac{z}{2} - \frac{788}{315} \sin^4\frac{z}{2} + \frac{64}{45} \sin^6\frac{z}{2} - \frac{64}{225} \sin^8\frac{z}{2}\right\} \times y^3,$$

and finally, if we substitute for the powers of $\sin\frac{z}{2}$, their values in the cosines of the multiples of the arch, we shall find,

$$C = \frac{1}{15} \times \{7 - 6\cos z - \cos 2z\} \times y,$$

$$D = \frac{1}{3150}\{510 - 78\cos z - 341\cos 2z - 84\cos 3z - 7\cos 4z\} y^3$$

Having

Having thus found C and D, we have $v = z + \dot{w} = z + C\lambda + D\lambda^2$: that is,

$$v = z + \frac{1}{15}\left\{7 - 6\ \text{cof}\ z - \text{cof}\ 2z\right\}\lambda\ \tan\frac{z}{2},$$

$$+\frac{1}{3150}\left\{510 - 78\,\text{cof}\,z - 341\,\text{cof}\,2z - 84\,\text{cof}\,3z - 7\,\text{cof}\,4z\right\}\lambda^2\tan^3\frac{z}{2}.$$

19. Having now refolved the problem that was propofed, it remains to apply it to find the true place of a comet in an eccentric orbit. For this purpofe, nothing more is wanting, than to be able to determine the angle z in the parabola, at any given inftant of time, reckoning from the paffage over the perihelion. We fhall here fuppofe, as a matter already known and demonftrated, the theory that is commonly given of a body defcribing a parabolic trajectory round the fun, placed in the focus: and we fhall alfo make ufe of the aftronomical tables that have been computed, for finding the true place in that trajectory when the time is given. It would indeed be eafy for us to deduce the whole of that theory, and to explain the conftruction of the tables, from the fluxional equation,

$$r^2\dot{z} = 2p^2 \times \dot{y}\,(1 + y^2)$$

obtained above: but this would only be to repeat what is already familiar to aftronomers.

SUPPOSE, then, a body to defcribe the given parabola, by its gravitation to the fun placed in the focus; and let us compare the motion of the body in the parabola, with the motion of the comet in the eccentric orbit: If two bodies defcribe different conic fections by the action of a central force, tending to the foci of the curves, and varying inverfely as the fquare of the diftance, it is demonftrated, by the writers on central forces, (*Vide* NEWT. *Prin. Math. lib.* 1. *prop.* 14.) that they will defcribe areas, in the fame time, that are in the fubduplicate ratio of the two parameters: Therefore, the area defcribed by the body in the parabola, in any given time, will be to the area defcribed

by

by the comet, in the ſame time as the ſquare root of the parameter of the parabola to the ſquare root of the parameter of the ellipſe; that is, as $\sqrt{2p}$ to $\sqrt{a(1-\epsilon^2)}$; or as $\sqrt{2p}$ to $\sqrt{p(1+\epsilon)}$; or as $\sqrt{2}$ to $\sqrt{1+\epsilon}$; or, finally, as $\sqrt{1+\lambda}$ to 1. But the area, deſcribed by the comet in the eccentric ellipſe, that is, the ſector of the ellipſe cut off by the radius vector ϱ, is equal to the ſector of the parabola, cut off by the radius vector r: Therefore, the ſector cut off by a radius vector drawn to the body, deſcribing the parabola by the ſolar force, will be to the ſector cut off by the radius vector r, in the proportion of $\sqrt{1+\lambda}$ to 1. Now, in the table of the motion in a parabola, the arguments of the true anomaly are no other than the areas cut off by the radii vectores; or, which is the ſame thing, they are numbers proportional to thoſe areas: Therefore, if the argument of the true anomaly of the body in the parabolic trajectory, found for the given inſtant of time, be diminiſhed in the proportion of $\sqrt{1+\lambda}$ to 1, we ſhall have the argument, which correſponds in the table, to the angle z required.

We have, therefore, this rule for finding the angle z at any given time, by means of the table of the motion in a parabola *: Let d denote the interval between the given time and the paſſage over the perihelion, expreſſed in days; then z will be the angle in the table that correſponds to the argument, $\frac{d}{p^{\frac{3}{2}}} \times \frac{1}{\sqrt{1+\lambda}}$. Having thus found the angle z in the parabola for the given time, we muſt apply to it the *equation* in the formula of Art. 18. to have the true anomaly in the eccentric orbit †.

5. In

* *Vide* Table Generale du Mouvement des Cometes, Aſtronomie de La Lande, tom. iii. p. 335. 2d edit.

† It may be remarked, that the angle z is always leſs than the angle v, and that the equation to be applied to z is always additive.

If

20. In calculating the place of a comet, as seen from the earth, the astronomer has occasion to compute, not only the heliocen- tric

If we compare the motion of the comet in the eccentric orbit, immediately to the motion of the body in the parabolic trajectory, it is obvious, that the angular velocity of the former is less than the angular velocity of the latter, at the perihelia of the curves: therefore, supposing the two bodies to pass over the perihelia together, the body in the parabola will advance before the comet. But, as the radii vectores in the ellipse increase at a slower rate than the radii vectores in the parabola, the angular velocity in the ellipse will increase at a faster rate than the angular velocity in the parabola, in order that the areas described in the same time may preserve their just proportion. Hence it is clear, that the angular velocity of the comet will, in the first place, become equal to the angular velocity of the body in the parabola, after which the former body will gain upon the latter; the difference of the true anomalies will become less and less, and will at last vanish, the two heliocentric places exactly coinciding.

If we denote by v the true anomaly, common to both the ellipse and parabola, when the heliocentric places coincide; and if $x = \tan\frac{v}{2}$, it will not be difficult to deduce, from the reasoning above, the following equation for determining x, viz.

$$\frac{1}{\sqrt{1+\lambda}} \times \left\{x + \frac{x^3}{3}\right\} = \left\{x + \frac{x^3}{3}\right\} - 2\lambda\left\{\frac{x^3}{3} + \frac{x^5}{5}\right\} + 3\lambda^2\left\{\frac{x^5}{5} + \frac{x^7}{7}\right\} - \&c.$$

which is easily reduced to this,

$$0 = \frac{1}{\lambda} \times \left(1 - \frac{1}{\sqrt{1+\lambda}}\right) \times \left\{1 + \frac{x^2}{3}\right\} - 2\left\{\frac{x^2}{3} + \frac{x^4}{5}\right\} + 3\lambda\left\{\frac{x^4}{5} + \frac{x^6}{7}\right\} - \&c.$$

and, if we neglect the quantities multiplied by λ and its powers, we shall have simply,

$$0 = \frac{1}{2} - \frac{x^2}{2} - \frac{2\,x^4}{5},$$

whence $x = \sqrt{\frac{\sqrt{105} - 5}{8}} = 0.8098 = \tan 39°$, nearly.

Therefore $v = 78°$: and, whatever be the eccentricity of the orbit, provided it be very great, the heliocentric place in the ellipse will, at this distance from the perihelion, coincide with the heliocentric place in the parabola: nearer the perihelion, the true anomaly in the ellipse will be less than the true anomaly in the parabola: and, more remote from the perihelion, the true anomaly in the ellipse will be greater than the true anomaly in the parabola. I need not remark, that this conclusion is not to be understood with the utmost rigour; for we have arrived at it by neglecting the quantities multiplied by the small fraction λ and its powers.

tric place, but alſo the diſtance of the comet from the ſun, or the radius vector of the orbit. The formula $\varrho = p \times \frac{1 + x^2}{1 + \lambda x^2}$ found in Art. 16. will furniſh a convenient rule for this purpoſe: For, if we make $\tan u = x \times \sqrt{\lambda} = \tan \frac{v}{2} \times \sqrt{\lambda}$; we ſhall have $\cos^2 u = \frac{1}{1 + \lambda x^2}$, and $\frac{1}{\cos^2 \frac{v}{2}} = 1 + x^2$; and, therefore, alſo

$\varrho = \frac{\cos^2 u}{\cos^2 \frac{v}{2}} \times p$, whence ϱ may be found.

X.

X. Description *of some* Improvements *in the* Arms *and* Accoutrements *of* Light Cavalry, *proposed by the Earl of* Ancram, *Colonel of the Mid-Lothian Regiment of Fencible Cavalry, and* F. R. S. Edin. *to his Excellency Marquis* Cornwallis, *Lord-Lieutenant of Ireland, &c. &c. in a* Letter *to Captain* Taylor, *Military Secretary to his Excellency.*

[*Read, March* 4. 1799.]

SIR, *Hillsbro' Dec.* 23. 1798.

Some time in the month of July 1797, I had a carabine made at Drogheda, of different dimensions, and of a different construction from that of our Light Dragoons: the mode of carrying and of using it is likewise different; and, as I have had considerable experience of this carabine since that period, I take the liberty of recommending it to the notice of his Excellency the Lord Lieutenant, in order that a trial may be made, whether the adoption of such an *arm* might not be of advantage to the Light Cavalry.

The barrel is twenty-one inches in length, (Pl. IV. fig. 1.) and of carabine bore. The weight of the carabine is not more than five pounds. The length of the bayonet-blade is thirteen inches, (fig. 2.) and there is a spring on the ring of it, to prevent the possibility of its flying off the barrel. The touch-hole is of a conical shape; the base of the cone opening into the pan, the summit into the barrel. The breech-plug is made to slope down towards the touch-hole, so that the carabine primes itself, the powder running into the pan by means of two inclined planes, the one formed by the ſhape of the breech-plug, the other by that of the touch-hole. The butt is excavated, ſo as to admit the hand eaſily, and it is ſtrengthened by a plate of iron, which runs from the end of the breech-plug, round to the end of the ſteel-tube which holds the ramrod.

THE advantages which it poſſeſſes over the common carabine, are theſe: It is ſtronger, becauſe it is fortified all round with iron; it is upwards of two pounds lighter; and, although ſeven inches ſhorter in the barrel, it carries as far as the other.

ONE might expect, that as the touch-hole is ſomewhat larger than uſual, there muſt be a greater exploſion through it, and conſequently a diminution of the exploſion through the barrel, as well as of the velocity of the bullet; but the fact is, that the touch-hole, although it be larger at the baſe, it is not much larger at the ſummit, where it opens into the barrel, than the common touch-hole; and, by frequent and accurate experiment, I have found, that, with the ſame quantity of powder, the ſmall carabine throws a ball with a force fully equal, if not ſuperior, to that of the common carabine. Perhaps the circumſtance of the length of the barrel, correſponding better with the diameter of the bullet, may account for this fact, and likewiſe make up for any trifling diminution of the exploſion from the barrel, which may poſſibly be occaſioned by the ſomewhat increaſed ſize of the touch-hole.

As this carabine primes itſelf, it can be loaded on horſeback, at ſpeed, or in the dark, or in windy weather, and under the cloak when it rains. I have never known it to miſs priming; and, by way of experiment, I have fired it upwards of twenty times ſucceſſively, and have always found, on opening the pan, that it was as well primed at the laſt, as at the firſt ſhot. It can be fired eaſily five times in a minute, with the aſſiſtance of a ſecond ramrod, (fig. 3.) which,

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when the dragoon is mounted, hangs to his carabine-belt, or to a button placed below the right ſhoulder. By means of the iron-plate, and the excavation in the butt, the dragoon is enabled to act on the defenſive againſt the ſabre, by parrying, in caſe of neceſſity, with the uſual protects eſtabliſhed in the ſword-exerciſe, and, with the bayonet fixed, he is likewiſe enabled to act on the offenſive, by giving point, as well againſt infantry as cavalry. The carabine is alſo uſed with ſo much eaſe and accuracy as a piſtol, as to ſuperſede the neceſſity of encumbering the dragoon with one. When he diſmounts, it is thrown over the left ſhoulder, ſlung by means of a ring faſtened to the handle of the butt; and the muzzle is ſlipped into a ſmall leathern bucket, attached to the belt, a little above, and behind the right hip-bone. In this poſition, it is carried without impeding the movements of the ſword-arm.

It is to be obſerved, that a powder ſomewhat finer granulated than the common Government powder, is required for the uſe of this carabine; and I have found, on inquiry, that the expence of granulation is a mere trifle.

The carabine is placed with the muzzle downwards, behind the right thigh of the dragoon, in a long holſter, (fig. 4.) which reaches above the lock and protects it, and which is faſtened at the bottom to a girth, by means of a ring and ſtrap, and at the top by a loop to the back-part of the ſaddle. It might be faſtened on before, in the manner in which dragoons uſually fix their buckets, but, by the above method, it is not only out of the way of the action of the ſword-arm, but it is of no more encumbrance to him than the piſtol, and can be drawn and returned with greater facility, there being no flounce to the holſter, which is trumpet-mouthed, in order to render this operation the eaſier when performed at ſpeed. The hammer is likewiſe raiſed as high as the top of the cock, as a guard, in order to prevent the poſſibility of the piece cocking itſelf, which ſo often happens to

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the piſtol when ſuddenly returned, and to the carabine or muſket, when bruſhing through an hedge or other incloſure. The facility of drawing and returning the carabine, alſo renders the uſe of the carabine belt and ſwivel to the dragoon when mounted unneceſſary; a matter of no ſmall importance, as the carabine when ſlung almoſt diſables the ſword-arm from acting. A ſmall priming flaſk, (fig. 5.) containing about two ounces of powder, made of horn, which hangs from the left ſhoulder below the right elbow, will be found of great convenience, by ſaving the breaking of a freſh cartridge, if in windy or wet weather the piece ſhould happen to miſs fire.

I HAVE likewiſe to add, that, owing to the number of accidents which troop horſes are ſubject to, from the weight, inconvenience, bad conſtruction, and inſufficiency of our dragoon ſaddle, I have made ſome alteration on the common huſſar-ſaddle, which, I cannot help flattering myſelf, corrects, in a great meaſure, the defects of both. The common huſſar-ſaddle is ſhorter than ours; it raiſes the ſeat of the rider conſiderably above the horſe's back. The ſtirrup-leathers are likewiſe removed further back. Owing to theſe circumſtances, the huſſar ſits very high and erect on his horſe; he appears rather to reſt on his fork than on his ſeat, his balance inclining forward. The principal advantages of this ſaddle are, that it fits all horſes equally well, and that the back-bone of the animal is perfectly free and untouched. The diſadvantages of it are, that, by placing the horſeman ſo high above the horſe, the ſecurity of his ſeat is affected, becauſe, the nearer a man ſits to his horſe's back, the firmer is the graſp which he is able to make with his thighs, and the leſs likely he is to loſe his balance; and although in a flat open country the inconvenience of the huſſar-ſeat may not be ſo much felt, yet in a hilly or incloſed one, and where leaping is neceſſary, the rider is continually expoſed to be thrown forward on his horſe's neck; beſides which, there is

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a conſiderable degree of trouble and time required in ſaddling, for, after the ſaddle is put on, which is little more than a tree, the ſheep-ſkin or cloth is to be thrown over and faſtened.

The alteration I have made conſiſts in removing the cantle about two inches back, and the ſtirrups one inch forward; by depreſſing the ſeat to within one inch of the horſe's back, and by making it ſomewhat broader. By this means there is ſtill a current of air running below the ſaddle, the dragoon ſtill retains the manege-ſeat, and all the material advantages of the huſſar-ſaddle are preſerved. The ſaddle is covered with leather; it is infinitely ſtronger, poſſeſſes every uſeful property of our dragoon-ſaddle, and weighs five pounds leſs.

In general, I cannot help obſerving, that the ſabre ought to be conſidered as the right-arm of light cavalry, the carabine or piſtol merely as an auxiliary, to be uſed when the ſword is rendered uſeleſs, or in a woody, moraſſy, or incloſed country, where a light horſeman is unable to come to cloſe quarters with his adverſary. A light dragoon cannot be too lightly clad, armed and accoutred; for, as activity and expedition are his principal qualifications, he muſt not be encumbered by any unneceſſary weight. A light dragoon and his horſe ſhould be conſidered as one animal: his arms muſt be ſuch as he can uſe with eaſe and dexterity when mounted; at the ſame time, provided it can be done without any additional encumbrance or inconvenience, there is no reaſon why they ſhould not be adapted as much as poſſible to his defence when diſmounted; but, if you attempt to make him equal at the ſame time to an huſſar, and to an infantry man, you inevitably render him inferior to either.

With the carabine deſcribed above, I think the piſtol totally unneceſſary. A dragoon can only load and fire one *arm* at a time in action; another is ſuperfluous; it only gives him additional trouble, and his horſe additional weight; beſides which, when

when he dismounts, he is obliged to leave his piſtol in his holſter, ſo that one half of his arms remains with his horſe.

I CONFESS I am ſanguine enough to conceive, that this *arm* might be in general uſe for the light cavalry, or at leaſt, that to every regiment of cavalry, both light and heavy, according to its ſtrength, a light ſquadron or troop ſhould be attached, armed as above, to act relatively to the reſt of the regiment, as the light infantry do to the battalion companies; and who ſhould be conſtantly exerciſed in all detached duties, ſuch as are performed by ſkirmiſhers or flankers, ſcouts, vedettes, &c. and who, by being thus lightly armed and accoutred, are better fitted for the alert and active operations which are required in theſe ſervices.

I have the honour to be,

SIR,

&c. &c. &c.

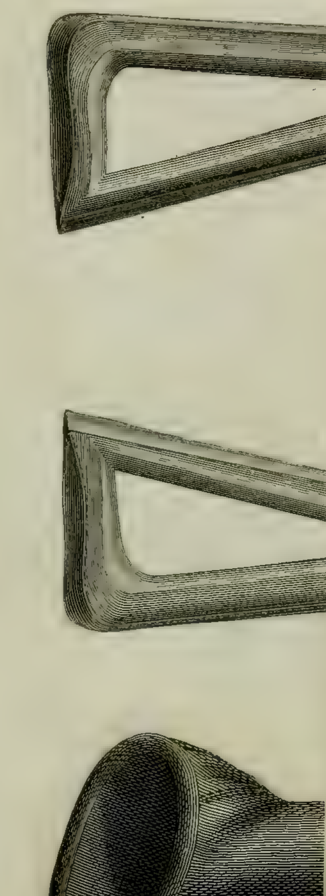
ANCRAM,
Col. M. L. F. C.

To Captain Taylor,
Military Secretary to his Excellency
the Lord-Lieutenant, &c. &c.
Dublin Caſtle.

Note. SINCE writing the above, I have found it more convenient to remove the holſter and carabine from behind the right thigh, to the fore-part of the ſaddle, where a dragoon uſually fixes his bucket, having obſerved, that at cloſe files, the holſters are, in the former poſition, more liable to be rubbed and injured.

. IV.

p. 252.



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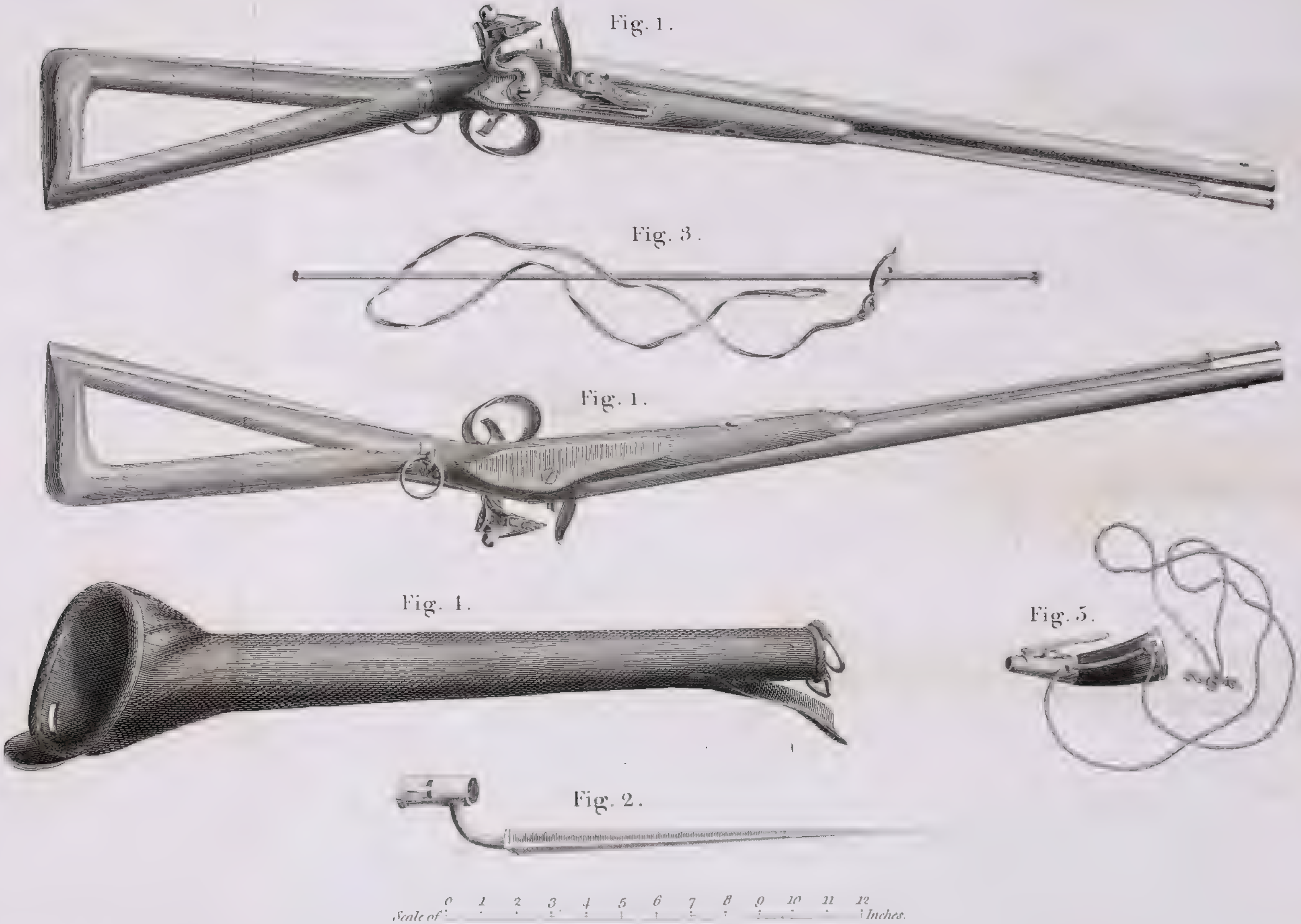

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XI. A New Method *of expressing the* Coefficients *of the* Development *of the* Algebraic Formula $(a^2+b^2-2ab\cos\varphi)^n$, *by means of the Perimeters of two Ellipses, when n denotes the Half of any Odd Number; together with an* Appendix, *containing the* Investigation *of a* Formula *for the* Rectification *of any* Arch *of an* Ellipse. By *Mr William Wallace, Assistant-Teacher of the Mathematics in the Academy of Perth.*

[*Read Jan.* 11. 1802.]

1. IN calculating the effect of the mutual action of two planets upon each other, it has been found necessary to develop the algebraic formula $(a^2+b^2-2ab\cos\varphi)^n$ into a series of this form, $A+B\cos\varphi+C\cos 2\varphi+D\cos 3\varphi+$ &c. Here a and b denote the distances of the planets from the sun; φ denotes the angle of commutation; and the values of n, more immediately the subject of consideration, are $-\frac{3}{2}$, and $-\frac{5}{2}$.

The determination of the coefficients A, B, C, &c. in these cases, appears to have been considered as a matter of difficulty by the mathematicians who first applied to the solution of the problem; for they found, that although it was only necessary to compute the first two coefficients A and B, the rest being easily derived from them, yet it did not appear that they could be expressed in finite terms, nor even by means of circular arches, or by logarithms. Recourse was therefore had to other methods, and chiefly to the method of infinite series; but

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as the ſeries which moſt readily occurred to them, converged in ſome caſes ſo ſlowly as to be in a manner uſeleſs, no ſmall degree of analytical addreſs has been found neceſſary, either to render it more convergent, or to find the ſum of a competent number of its terms, with a moderate degree of labour.

2. But in conſidering the ſubject, it has occurred to me, that although we cannot expreſs the values of the coefficients in finite algebraic terms, nor even by means of circular arches, or by logarithms, yet when n is the half of an odd number, either poſitive or negative, we may always expreſs them by means of the proportion which the perimeters, or ſemi-perimeters, of two ellipſes bear to thoſe of their circumſcribing circles. The problem may therefore be reduced to the rectification of the circle and ellipſe, and mathematicians know that ſuch reduction is conſidered as the next degree of reſolution, in point of ſimplicity, to our being able to effect the ſolution by means of circular arches, or by logarithms only.

3. It is well known that we can eaſily obtain a fluxionary expreſſion for each of the coefficients A, B, C, &c. in the equation
$(a^2+b^2-2ab\cos\varphi)^n = A + B\cos\varphi + C\cos 2\varphi + D\cos 3\varphi + \&c.$;
for if each ſide of the equation be multiplied by $\dot{\varphi}$, and the fluent taken, we get

$$\int\dot{\varphi}(a^2+b^2-2ab\cos\varphi)^n = A\varphi + B\sin\varphi + \tfrac{1}{2}C\sin 2\varphi + \tfrac{1}{3}D\sin 3\varphi + \&c.$$

Let us now take the fluent generated, while φ from 0 becomes a ſemicircle, then ſin φ, ſin 2φ, ſin 3φ, &c. all vaniſh; ſo that, putting π to denote half the perimeter of a circle, of which the radius is 1, we get,

$$\pi A = \int\dot{\varphi}(a^2+b^2-2ab\cos\varphi)^n.$$

In like manner, if we multiply each ſide of the aſſumed equation

equation by $2\dot{\varphi}\cos\varphi$, and take the general equation of the fluents, we get

$$\int 2\dot{\varphi}\cos\varphi(a^2+b^2-2ab\cos\varphi)^n = B\varphi+(2A+C)\sin\varphi+\tfrac{1}{2}(B+D)\sin 2\varphi+\&c.;$$

therefore, when $\varphi=\pi$,

$$\pi B=\int 2\dot{\varphi}\cos\varphi(a^2+b^2-2ab\cos\varphi)^n.$$

Again, if we multiply both sides of the same equation by $2\dot{\varphi}\cos 2\varphi$, and take the relation of the fluents as before, we get

$$\pi C=\int 2\dot{\varphi}\cos 2\varphi(a^2+b^2-2ab\cos\varphi)^n.$$

4. PROCEEDING in this way, we might get a fluxionary expression for each of the remaining coefficients D, E, &c. but this is not necessary, for they may be all found from the first two, A and B, and from one another, as we have already observed; and the scale of their relation has been determined as follows *:

LET the fluxions of the logarithms of each side of the assumed equation

$$(a^2+b^2-2ab\cos\varphi)^n=A+B\cos\varphi+C\cos 2\varphi+\&c.$$

be taken, and the whole be divided by $\dot{\varphi}$; also, for the sake of brevity, let us put Δ for $\frac{a^2+b^2}{ab}$; thus we get

$$\frac{-2n\sin\varphi}{\Delta-2\cos\varphi}=\frac{B\sin\varphi+2C\sin 2\varphi+3D\sin 3\varphi+\&c.}{A+B\cos\varphi+C\cos 2\varphi+D\cos 3\varphi+\&c.}$$

LET the numerator of each side of this equation be now multiplied by the denominator of the other side, substituting $\sin(p+1)\varphi+\sin(p-1)\varphi$ for $2\cos\varphi\sin p\varphi$, and, $\sin(p+1)\varphi-\sin(p-1)\varphi$ for $2\sin\varphi\cos p\varphi$, then, putting the result $=0$, we get

Kk2 $+\Delta B$

* Traité du Calcul Differentiel et du Calcul Integral, par LACROIX; vol. ii. page 120.

$$\left.\begin{array}{l}+\Delta B\\-2C\\+2nA\\-nC\end{array}\right\}\sin\varphi\left.\begin{array}{l}+2\Delta C\\-B\\-3D\\+nB\\-nD\end{array}\right\}\sin 2\varphi\left.\begin{array}{l}+3\Delta D\\-2C\\-4E\\+nC\\-nE\end{array}\right\}\sin 3\varphi\left.\begin{array}{l}+4\Delta E\\-3D\\-5F\\+nD\\-nF\end{array}\right\}\sin 4\varphi\ \&c. = 0;$$

hence we readily derive the following series of equations :

$$C = \frac{\Delta}{n+2}B + \frac{2n}{n+2}A$$

$$D = \frac{2\Delta}{n+3}C + \frac{n-1}{n+3}B$$

$$E = \frac{3\Delta}{n+4}D + \frac{n-2}{n+4}C$$

$$F = \frac{4\Delta}{n+5}E + \frac{n-3}{n+5}D$$

&c. &c.

5. But besides being able to determine the remaining coefficients of the development of the formula $(a^2+b^2-2ab\cos\varphi)_n$, where the exponent is n, by means of the first two A and B, we can also determine all the coefficients of the development of $(a^2+b^2-2ab\cos\varphi)^{n-1}$, where the exponent is $n-1$, by means of the same two coefficients A and B; and by them we can determine the coefficients when the exponent is $n \pm q$, where q denotes any whole number whatsoever *.

For let us assume

$$(a^2+b^2-2ab\cos\varphi)^{n-1} = A' + B'\cos\varphi + C'\cos 2\varphi + D'\cos 3\varphi + \&c.$$

Then,

$$(a^2+b^2-2ab\cos\varphi)^n\left\{\begin{array}{l}=(a^2+b^2-2ab\cos\varphi)(A'+B'\cos\varphi+C'\cos 2\varphi+\&c.)\\ = A + B\cos\varphi + C\cos 2\varphi + D\cos 3\varphi + \&c.\end{array}\right.$$

From these two values of $(a^2+b^2-2ab\cos\varphi)^n$, by due reduction, and

* Traité du Calcul Differentiel et du Calcul Integral, par Lacroix; vol. ii. page 120.

and ſubſtitution of Δ for $\frac{a^2+b^2}{ab}$, and of $\operatorname{cof}(p+1)\varphi + \operatorname{cof}(p-1)\varphi$ for $2\operatorname{cof}\varphi\operatorname{cof}p\varphi$, we get the following equation,

$$\left.\begin{array}{r}\Delta A' + \Delta B' \\ -2A' \\ -B' - C' \\ -\frac{A}{ab} - \frac{B}{ab}\end{array}\right\}\operatorname{cof}\varphi \left.\begin{array}{r}+\Delta C' \\ -B' \\ -D' \\ -\frac{C}{ab}\end{array}\right\}\operatorname{cof}2\varphi \left.\begin{array}{r}+\Delta D' \\ -C' \\ -E' \\ -\frac{D}{ab}\end{array}\right\}\operatorname{cof}3\varphi, \text{ \&c.} = 0.$$

HENCE it follows, that,

$$B' = \Delta A' - \frac{A}{ab}$$

$$C' = \Delta B' - 2A' - \frac{B}{ab}$$

$$D' = \Delta C' - B - \frac{C}{ab}$$

$$E' = \Delta D' - C - \frac{D}{ab}$$

$$\text{\&c.} \qquad \text{\&c.}$$

IT yet remains to determine A′, the firſt coefficient; but that, as well as B′, may be readily found by conſidering that we have already found (Art 4.)

$$C = \frac{\Delta}{n+2}B + \frac{2n}{n+2}A;$$

therefore, ſubſtituting $n-1$ for n, and C′ for C, for the very ſame reaſon we get

$$C' = \frac{\Delta}{n+1}B' + \frac{2n-2}{n+1}A'.$$

BY comparing together this value of C′, and the values of B′ and C′ as given in the laſt ſeries of equations, and ſubſtituting for Δ its value $\frac{a^2+b^2}{ab}$, we get,

$$A' = \frac{a^2+b^2}{(a^2-b^2)^2}A + \frac{(n+1)ab}{n(a^2-b^2)^2}B$$

$$B' = \frac{4ab}{(a^2-b^2)^2}A + \frac{(n+1)(a^2+b^2)}{n(a^2-b^2)^2}B.$$

6. IT

6. It is obvious, that if we had fuppofed A′ and B′, the firft two coefficients of the feries $A' + B' \operatorname{cof} \varphi + C' \operatorname{cof} 2\varphi + \&c.$ known, we could thence have determined A and B the firft two coefficients of the feries $A + B \operatorname{cof} \varphi + C \operatorname{cof} 2\varphi + \&c.$; fo that, in either cafe, the remaining coefficients C′, D′, &c. C, D, &c. which depend upon the firft two in each feries, would alfo be known.

Hence it follows, that while on the one hand we can proceed from the cafe in which the exponent is n, to thofe cafes in which the exponents are $n - 1$, $n - 2$, &c. fo, on the other hand, we can proceed from the fame cafe, to others in which the exponents are $n + 1$, $n + 2$, and thus we may go on, according to a defcending or afcending fcale, as far as we pleafe.

7. I now proceed to the chief object of inquiry in this paper, namely, to find convenient geometrical expreffions for the firft two coefficients A and B, in fome particular cafe, where the exponent n is the half of an odd number; and I felect that in which $n = -\frac{3}{2}$, becaufe of its importance in phyfical aftronomy; but it is evident from what has been already fhewn, that from hence we may determine the coefficients, in the cafes of $n = -\frac{5}{2}$, &c. and alfo $n = -\frac{1}{2}$, $n = +\frac{1}{2}$, &c.

Let us therefore affume this equation,

$$(a^2 + b^2 - 2ab \operatorname{cof} \varphi)^{-\frac{3}{2}} = A + B \operatorname{cof} \varphi + C \operatorname{cof} 2\varphi + \&c.;$$

or, putting, for the fake of brevity, $\varepsilon = \frac{b}{a}$, where b is fuppofed to be lefs than a,

$$(1 + \varepsilon^2 - 2\varepsilon \operatorname{cof} \varphi)^{-\frac{3}{2}} = a^3 (A + B \operatorname{cof} \varphi + C \operatorname{cof} 2\varphi + \&c.)$$

Then, from what has been already fhewn, (Art 3.) we have

$\pi a^3 A$

$$\pi a^3 \mathrm{A} = \int \frac{\dot{\varphi}}{(1+\varepsilon^2 - 2\varepsilon \operatorname{cos} \varphi)^{\frac{3}{2}}};$$

$$\pi a^3 \mathrm{B} = \int \frac{2\dot{\varphi} \operatorname{cos} \varphi}{(1+\varepsilon^2 - 2\varepsilon \operatorname{cos} \varphi)^{\frac{3}{2}}};$$

the fluents to be confidered as generated, while φ increafes from o to π.

8. LET us affume ψ fuch an arch, that

$$\operatorname{Sin} \psi = \frac{\operatorname{fin} \varphi}{\sqrt{1+\varepsilon^2 - 2\varepsilon \operatorname{cos} \varphi}};$$

from this affumption it is evident that when $\varphi = 0$, then $\psi = 0$, and when $\varphi = \pi$, then ψ alfo $= \pi$.

TAKING now the fluxion of each fide of our affumed equation, we get

$$\dot{\psi} \operatorname{cos} \psi = \frac{\dot{\varphi} \operatorname{cos} \varphi}{\sqrt{1+\varepsilon^2 - 2\varepsilon \operatorname{cos} \varphi}} - \frac{\varepsilon \dot{\varphi} \operatorname{fin}^2 \varphi}{(1+\varepsilon^2 - 2\varepsilon \operatorname{cos} \varphi)^{\frac{3}{2}}}$$

and by reducing the fractional quantities to a common denominator, and fubftituting $1 - \operatorname{cof}^2\varphi$ for $\operatorname{fin}^2\varphi$, the fame equation becomes

$$\dot{\psi} \operatorname{cos} \psi = \frac{\dot{\varphi}(1 - \varepsilon \operatorname{cos} \varphi)(\operatorname{cos} \varphi - \varepsilon)}{(1+\varepsilon^2 - 2\varepsilon \operatorname{cos} \varphi)^{\frac{3}{2}}}.$$

BUT $(1 - \varepsilon \operatorname{cof} \varphi)(\operatorname{cof} \varphi - \varepsilon)$, the numerator of the latter fide of the equation, may be otherwife expreffed, thus,
$\frac{1}{4\varepsilon}\left\{(1-\varepsilon^2)^2 - (1+\varepsilon^2 - 2\varepsilon \operatorname{cof} \varphi)^2\right\}$; hence it follows that

$$\dot{\psi} \operatorname{cos} \psi \begin{cases} = \dfrac{\dot{\varphi}\left\{(1-\varepsilon^2)^2 - (1+\varepsilon^2 - 2\varepsilon \operatorname{cos} \varphi^2)\right\}}{4\varepsilon(1+\varepsilon^2 - 2\varepsilon \operatorname{cos} \varphi)^{\frac{3}{2}}} \\ = \dfrac{(1-\varepsilon^2)^2 \dot{\varphi}}{4\varepsilon(1+\varepsilon^2 - 2\varepsilon \operatorname{cos} \varphi)^{\frac{3}{2}}} - \dfrac{1}{4\varepsilon} \dot{\varphi} \sqrt{1+\varepsilon^2 - 2\varepsilon \operatorname{cos} \varphi}; \end{cases}$$

therefore, $\dfrac{\dot{\varphi}}{(1+\varepsilon^2 - 2\varepsilon \operatorname{cof} \varphi)^{\frac{3}{2}}} = \dfrac{4\varepsilon \dot{\psi} \operatorname{cof} \psi}{(1-\varepsilon^2)^2} + \dfrac{1}{(1-\varepsilon^2)^2} \cdot \dot{\varphi} \sqrt{1+\varepsilon^2 - 2\varepsilon \operatorname{cof} \varphi}$,

and, taking the fluents when $\varphi = \pi$,

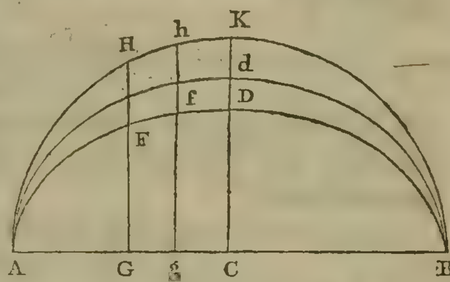
$$\pi a^3 \mathrm{A} = \frac{1}{(1-\varepsilon^2)^2} \int \dot{\varphi} \sqrt{1+\varepsilon^2 - 2\varepsilon \operatorname{cos} \varphi}.$$

LET

Let us now ſubſtitute $2\,\text{coſ}\,\frac{^2\varphi}{2} - 1$, for its equal, $\text{coſ}\,\varphi$, and the fluxionary expreſſion $\dot{\varphi}\sqrt{1 + \varepsilon^2 - 2\varepsilon\,\text{coſ}\,\varphi}$ becomes $(1 + \varepsilon)\dot{\varphi}\sqrt{1 - \frac{4\varepsilon}{(1+\varepsilon)^2}\,\text{coſ}\,\frac{^2\varphi}{2}}$; ſo that if we aſſume e ſuch, that $e^2 = \frac{4\varepsilon}{(1+\varepsilon)^2}$, we alſo get

$$\pi.a^3A = \frac{1}{(1+\varepsilon)(1-\varepsilon)^2}\int\dot{\varphi}\sqrt{1 - e^2\,\text{coſ}\,\frac{^2\varphi}{2}}.$$

Let ADB be an ellipſe, of which the ſemi-tranſverſe axis $AC = 1$, and excentricity $= e$, and let AKB be a circle, having AB, the tranſverſe axis, for its diameter; let FG, any ordinate to the tranſverſe axis, meet the circle in H; then, if we put the arch $AH = \frac{\varphi}{2}$, it is well known that $\frac{1}{2}\dot{\varphi}\sqrt{1 - e^2\,\text{coſ}\,\frac{^2\varphi}{2}}$ will denote the fluxion of the elliptic arch AF; therefore, when $\varphi = \pi$, the fluent of $\dot{\varphi}\sqrt{1 - e^2\,\text{coſ}\,\frac{^2\varphi}{2}}$ will evidently be expreſſed by ADB, the ſemi-perimeter of the ellipſe: Let us denote the ſemi-perimeter by (E) and we have $a^3\pi A = \frac{(E)}{(1+\varepsilon)(1-\varepsilon)^2}$; hence, and by ſubſtituting $\frac{b}{a}$ for ε, we get



$$A = \frac{1}{(a+b)(a-b)^2} \times \frac{(E)}{\pi};$$

and becauſe e, the excentricity of the ellipſe, has been aſſumed $= \frac{2\sqrt{\varepsilon}}{1+\varepsilon}$, and $\varepsilon = \frac{b}{a}$; therefore the excentricity $= \frac{2\sqrt{ab}}{a+b}$; and the ſemi-conjugate axis $= \frac{a-b}{a+b}$.

9. Having now found a geometrical expreſſion, ſufficiently ſimple, for A, I proceed to inveſtigate a ſimilar expreſſion for the

the next coefficient B; which, as we have already ſeen (Art. 7.) is equal to $\frac{1}{\pi a^3}\int\frac{2\dot{\phi}\operatorname{cof}\phi}{(1+\varepsilon^2-2\varepsilon\operatorname{cof}\phi)^{\frac{3}{2}}}$.

FROM our aſſumed equation, $\operatorname{fin}\psi=\frac{\operatorname{fin}\phi}{\sqrt{1+\varepsilon^2-2\varepsilon\operatorname{cof}\phi}}$, we have found $\dot{\psi}\operatorname{cof}\psi=\frac{\dot{\phi}(1-\varepsilon\operatorname{cof}\phi)(\operatorname{cof}\phi-\varepsilon)}{(1+\varepsilon^2-2\varepsilon\operatorname{cof.}\phi)^{\frac{3}{2}}}$, (Art. 8.); but, ſince from the ſame equation it appears that

$$\operatorname{cof}\psi=\sqrt{1-\operatorname{fin}^2\psi}=\frac{\operatorname{cof}\phi-\varepsilon}{\sqrt{1+\varepsilon^2-2\varepsilon\operatorname{cof}\phi}};$$

therefore, $\frac{\dot{\psi}(\operatorname{cof}\phi-\varepsilon)}{\sqrt{1+\varepsilon^2-2\varepsilon\operatorname{cof}\phi}}=\frac{\dot{\phi}(1-\varepsilon\operatorname{cof}\phi)(\operatorname{cof}\phi-\varepsilon)}{(1+\varepsilon^2-2\varepsilon\operatorname{cof}\phi)^{\frac{3}{2}}}$;

and, dividing both ſides by $\frac{\operatorname{cof}\phi-\varepsilon}{\sqrt{1+\varepsilon^2-2\varepsilon\operatorname{cof}\phi}}$,

$$\dot{\psi}=\frac{\dot{\phi}(1-\varepsilon\operatorname{cof}\phi)}{1+\varepsilon^2-2\varepsilon\operatorname{cof}\phi}. \qquad (\alpha)$$

AGAIN, from our aſſumed equation, we have $\varepsilon^2\operatorname{fin}^2\psi=\frac{\varepsilon^2(1-\operatorname{cof}^2\phi)}{1+\varepsilon^2-2\varepsilon\operatorname{cof}\phi}$, and therefore,

$$\sqrt{1-\varepsilon^2\operatorname{fin}^2\psi}=\frac{1-\varepsilon\operatorname{cof}\phi}{\sqrt{1+\varepsilon^2-2\varepsilon\operatorname{cof}\phi}}. \qquad (\beta)$$

LET the product of the correſponding ſides of the equations (α) and (β) be taken, and we get

$$\dot{\psi}\sqrt{1-\varepsilon^2\operatorname{fin}^2\psi}=\frac{\dot{\phi}(1-\varepsilon\operatorname{cof}\phi)^2}{(1+\varepsilon^2-2\varepsilon\operatorname{cof}\phi)^{\frac{3}{2}}}.$$

Now we have found, (Art. 8.) $\varepsilon\dot{\psi}\operatorname{cof}\psi=\frac{\dot{\phi}(1-\varepsilon\operatorname{cof}\phi)(\varepsilon\operatorname{cof}\phi-\varepsilon^2)}{(1+\varepsilon^2-2\varepsilon\operatorname{cof}\phi)^{\frac{3}{2}}}$, therefore, taking the ſum of theſe two equations, it follows that

$$\dot{\psi}\sqrt{1-\varepsilon^2\operatorname{fin}^2\psi}+\varepsilon\dot{\psi}\operatorname{cof}\psi\left\{\begin{aligned}&=\frac{(1-\varepsilon^2)\dot{\phi}(1-\varepsilon\operatorname{cof}\phi)}{(1+\varepsilon^2-2\varepsilon\operatorname{cof}\phi)^{\frac{3}{2}}}.\\&=\frac{(1-\varepsilon^2)\dot{\phi}}{(1+\varepsilon^2-2\varepsilon\operatorname{cof}\phi)^{\frac{3}{2}}}-\frac{\varepsilon(1-\varepsilon^2)\dot{\phi}\operatorname{cof}\phi}{(1+\varepsilon^2-2\varepsilon\operatorname{cof}\phi)^{\frac{3}{2}}}.\end{aligned}\right.$$

Taking now the fluents, when ψ and $\varphi = \pi$, and ſubſtituting $\pi a^3 A$ for $\int \frac{\dot{\varphi}}{(1 + \varepsilon^2 - 2\varepsilon \operatorname{coſ} \varphi)^{\frac{3}{2}}}$, alſo $\frac{1}{2}\pi a^3 B$ for $\int \frac{\dot{\varphi} \operatorname{coſ} \varphi}{(1 + \varepsilon^2 - 2\varepsilon \operatorname{coſ} \varphi)^{\frac{3}{2}}}$, we get, $\int \dot{\psi} \sqrt{1 - \varepsilon^2 \operatorname{ſin}^2 \psi} = (1 - \varepsilon^2)\pi a^3 A - \frac{1}{2}\varepsilon(1 - \varepsilon^2)\pi a^3 B$.

Let AdB be another ellipſe, having its ſemi-tranſverſe alſo $= 1$, but its excentricity $= \varepsilon$; let its conjugate axis meet the circle in K, and let gf, an ordinate to the tranſverſe axis, meet the circle in h; then, if the arch Kh be denoted by ψ, the fluxion of the elliptic arch df will be expreſſed by $\dot{\psi}\sqrt{1 - \varepsilon^2 \operatorname{ſin}^2 \psi}$; therefore, when $\psi = \pi$, we have $\int \dot{\psi}\sqrt{1 - \varepsilon^2 \operatorname{ſin}^2 \psi}$ equal to AdB, half the perimeter of the ellipſe. Let us put (E′) for the ſemi-perimeter of this ſecond ellipſe, and our laſt equation becomes

$$(E') = (1 - \varepsilon^2)\pi a^3 A - \frac{1}{2}\varepsilon(1 - \varepsilon^2)\pi a^3 B,$$

and, by proper reduction, and ſubſtitution of $\frac{b}{a}$ for ε, we finally get

$$B = \frac{2a}{b} A - \frac{2}{b(a^2 - b^2)} \times \frac{(E')}{\pi};$$

and, ſince the excentricity of this other ellipſe is $\frac{b}{a}$, its ſemi-conjugate axis $= \frac{\sqrt{a^2 - b^2}}{a}$.

Thus we have reduced the determination of A and B, the two firſt coefficients of the ſeries, and upon which all the remaining coefficients depend, to the rectification of the ellipſe: now this is a problem which we can readily reſolve, by means of infinite ſeries, in every caſe that can poſſibly occur.

10. I next obſerve, that in determining the coefficients A, B, &c. we are not confined to the two ellipſes juſt now inveſtigated; for, inſtead of them, we may ſubſtitute other two, having their excentricities as great or as ſmall as we pleaſe. This peculiarity of our ſolution depends upon a very curious relation which

which is known to ſubſiſt between indefinite arches, as well as between the whole perimeters of any three contiguous terms of a ſeries formed by an infinite number of ellipſes, the axes, or excentricities of which have among themſelves a very remarkable connection.

LET E, E′, E″, E‴, &c. denote the ſemi-perimeters of a ſeries of ellipſes; e, e', e'', e''', &c. their excentricities, and c, c', c'', c''', &c. their conjugate axes; let theſe ellipſes be ſo related to each other, that $e' = \frac{1-c}{1+c} = \frac{1-\sqrt{1-e^2}}{1+\sqrt{1-e^2}}$, $e'' = \frac{1-c'}{1+c'} = \frac{1-\sqrt{1-e'^2}}{1+\sqrt{1-e'^2}}$, $e''' = \frac{1-c''}{1+c''} = \frac{1-\sqrt{1-e''^2}}{1+\sqrt{1-e''^2}}$, &c. Then we have the following ſeries of equations *.

$$(1+e')E - (2+c')\ E' + c'\ (1+c')\ E'' = 0;$$
$$(1+e'')E' - (2+c'')\ E'' + c''\ (1+c'')\ E''' = 0,$$
$$(1+e''')E'' - (2+c''')\ E''' + c'''(1+c''')\ E^{\text{IV}} = 0,$$
&c. &c. &c.

THIS ſeries of equations may be continued backwards by putting $E^{\backprime}$, $E^{\backprime\backprime}$, &c. for the ſemi-perimeters of ellipſes of which the excentricities are $e^{\backprime}$, $e^{\backprime\backprime}$, &c. and ſemi-conjugate axes $c^{\backprime}$, $c^{\backprime\backprime}$, and ſince $e = \frac{1-\sqrt{1-e^{\backprime 2}}}{1+\sqrt{1-e^{\backprime 2}}}$, $e^{\backprime} = \frac{1-\sqrt{1-e^{\backprime\backprime 2}}}{1+\sqrt{1-e^{\backprime\backprime 2}}}$, &c.; therefore, $e^{\backprime} = \frac{2\sqrt{e}}{1+e}$, $e^{\backprime\backprime} = \frac{2\sqrt{e^{\backprime}}}{1+e^{\backprime}}$, &c.

11. FROM the foregoing ſeries of equations, it appears, that any ellipſe of the ſeries $E^{\backprime\backprime}$, $E^{\backprime}$, E, E′, &c. may be expreſſed by means of any other two ellipſes of the ſame ſeries: for the number of equations is always two leſs than the number of ellipſes; and therefore, having aſſumed any number of equations, we

* SEE a Memoir upon the Compariſon of Elliptic Arcs, by LEGENDRE, in the Memoirs of the Royal Academy of Sciences for 1786. See alſo the Appendix to this Paper.

can, by means of them, exterminate all the ellipſes, except three, and thus obtain a ſingle equation, expreſſing a relation between any three ellipſes we may chooſe to retain.

Now, in conſidering the ſeries of quantities $e^{\backprime\backprime}$, $e^{\backprime}$, e, e', e'', &c. which denote the excentricities of the ellipſes, it will readily appear, that in continuing the ſeries forward, they rapidly diminiſh; for ſince $e' = \frac{1-c}{1+c} = \frac{1-c^2}{(1+c)^2} = \frac{e^2}{(1+c)^2}$, it is evident, that however ſmall the conjugate axis of any one of the ellipſes may be, the excentricity of the next ellipſe in the ſeries will be expreſſed by a fraction leſs than the ſquare of that which expreſſed the excentricity of the former; but if the conjugate axis be nearly equal to the tranſverſe, then the excentricity of the next ellipſe is only a little greater than one-fourth of the ſquare of the excentricity of the former ellipſe.

Let us ſuppoſe, for example, $e = .5$, then it follows that $e' = .071797$, $e'' = .001292$, and e''' is ſo ſmall, as not to admit of being expreſſed by fewer than ſix cyphers between the decimal point and firſt ſignificant figure to the right hand; and, conſequently, the ellipſe of which it is the excentricity, differs not ſenſibly from a circle.

Hence alſo it follows, that the ſeries of excentricities e, $e^{\backprime}$, $e^{\backprime\backprime}$, &c. continued in the oppoſite direction, muſt approach rapidly towards 1, which is their limit; and if we ſuppoſe $e = .5$, as before, we ſhall have $e^{\backprime} = .942809$, $e^{\backprime\backprime} = .999566$; as to $e^{\backprime\backprime\backprime}$, it differs ſo little from the tranſverſe axis, that the ellipſe, of which it denotes the excentricity, may be conſidered as a ſtrait line. Thus it appears, that the rectification of any ellipſe may be reduced to the rectification of two other ellipſes, either conſiderably more excentric than itſelf, or conſiderably leſs excentric.

12. It is now to be obſerved, that the two ellipſes by which we have expreſſed the coefficients of the development of the formula $(a^2 + b^2 - 2ab \cos\varphi)^n$, have the ſame relation between

themſelves,

themſelves, that any two adjoining ellipſes in the ſeries E`, E, E′, &c. have to each other.

For, e denoting the excentricity of the ellipſe (E), and ε that of the ellipſe (E′), we have found $e^2 = \frac{4\varepsilon}{(1+\varepsilon)^2}$, (Art. 8.) therefore, $\sqrt{1-e^2} = \frac{1-\varepsilon}{1+\varepsilon}$, and conſequently $\varepsilon = \frac{1-\sqrt{1-e^2}}{1+\sqrt{1-e^2}}$, ſo that we may change the ſymbols (E) and (E′) for E and E′, and put e' for ε; and, ſince we have the two equations

$$(1+e)\,E` - (2+c)\,E + c\,(1+c)\,E' = 0,$$
$$(1+e')\,E - (2+c')\,E' + c'\,(1+c')\,E'' = 0,$$

it will immediately follow, that if any of the two ellipſes E and E′, (by which we have expreſſed the coefficients A, B, &c.) have ſuch a degree of excentricity, as to be unfavourable to numerical calculation, we can expreſs that ellipſe by means of the other one, and a third ellipſe, which may be more or leſs excentric than either of the other two ellipſes, juſt as we pleaſe.

13. I shall now give, in the form of practical rules, the ſubſtance of the preceding inveſtigations for determining the firſt two coefficients of the development of the formula $(a^2+b^2-2ab\cos\phi)^n$, in the caſe when $n = -\frac{3}{2}$; but from theſe, and from what has been delivered in Articles 4. and 5., it is eaſy to determine any number of the coefficients, when n is the half of any whole number, poſitive or negative.

To compute A and B in the equation

$$(a^2+b^2-2ab\cos\phi)^{-\frac{1}{2}} = A + B\cos\phi + C\cos 2\phi + \&c.$$

1. Find E half the perimeter of an ellipſe, of which the ſemi-tranſverſe axis $= 1$, the ſemi-conjugate $= \frac{a-b}{a+b}$, and therefore, the excentricity $= \frac{2\sqrt{ab}}{a+b}$.

2. Also

2. Also, find E′ half the perimeter of another ellipſe, of which the ſemi-tranſverſe axis $= 1$, the ſemi-conjugate axis $= \frac{\sqrt{a^2 - b^2}}{a}$, and excentricity $= \frac{b}{a}$.

Let π denote 3.14159, &c.

$$\text{Then, } A = \frac{1}{(a+b)(a-b)^2} \cdot \frac{E}{\pi}.$$

$$B = \frac{2a}{b} A - \frac{2}{b(a^2 - b^2)} \cdot \frac{E'}{\pi}.$$

If either of the ellipſes E, E′, were judged to be of ſuch a form, as that the ſeries for its rectification converged too ſlowly, then another ellipſe, either more or leſs excentric, might be ſubſtituted for it thus:

Let e and c denote the excentricity and ſemi-conjugate axis of the ellipſe E, and e' and c' the ſame things of the ellipſe E′.

3. Compute $e^{\backprime} = \frac{2\sqrt{e}}{1+e}$, and $c^{\backprime} = \frac{1-e}{1+e}$; then $e^{\backprime}$ and $c^{\backprime}$ will denote the excentricity and ſemi-conjugate axis of an ellipſe more excentric than either E′ or E.

4. Or, compute $e'' = \frac{1-c'}{1+c'}$, and $c'' = \frac{2\sqrt{c'}}{1+c'}$; then e'' and c'' will denote the excentricity and ſemi-conjugate axis of an ellipſe leſs excentric than either of the ſame ellipſes.

5. Compute $E^{\backprime}$ half the perimeter of the ellipſe, of which the excentricity is $e^{\backprime}$.

6. Or, Compute E″ half the perimeter of the ellipſe, having its excentricity $= e''$.

$$\text{Then, } E \begin{cases} = \frac{1+e}{2+c} E^{\backprime} + \frac{c(1+c)}{2+c} E'. \\ = \frac{2+c'}{1+e'} E' - \frac{c'(1+c')}{1+e'} E''. \end{cases}$$

$$E' \begin{cases} = \frac{2+c}{c(1+c)} E - \frac{1+e}{c(1+c)} E^{\backprime}. \\ = \frac{1+e'}{2+c'} E + \frac{c'(1+c')}{2+c'} E''. \end{cases}$$

14. THERE are two series, which, so far as I know, are the best adapted to the rectification of the ellipse, of any hitherto published; because, while both are well suited to a moderate degree of excentricity, the one converges with great rapidity when the excentricity is considerable, and the other when it is small.

THE first of these series, which appears to have been originally given by Mr EULER *, converges by the powers of the semi-conjugate axis, and may be expressed, so as to exhibit the law of the series, as follows:

LET the semi-transverse axis $= 1$,

the semi-conjugate axis $= c$.

Then, $\frac{1}{4}$ of the perimeter of the ellipse is equal to

$$1 + \left(c^2 + \frac{3}{4}\cdot\frac{1}{2}c^4 + \frac{3.5}{4.6}\cdot\frac{1.3}{2.4}c^6 + \frac{3.5.7}{4.6.8}\cdot\frac{1.3.5}{2.4.6}c^8 + \&c.\right) \text{hyp. log.} \frac{2}{\sqrt{c}}$$

$$- c^2 \cdot \frac{1}{4}$$

$$- \frac{3}{4}\cdot\frac{1}{2}c^4\left(\frac{7}{12} - \frac{1}{2.3.4}\right)$$

$$- \frac{3.5}{4.6}\cdot\frac{1.3}{2.4}c^6\left(\frac{13}{20} - \frac{1}{2.3.4} - \frac{1}{4.5.6}\right)$$

$$- \frac{3.5.7}{4.6.8}\cdot\frac{1.3.5}{2.4.6}c^8\left(\frac{19}{28} - \frac{1}{2.3.4} - \frac{1}{4.5.6} - \frac{1}{6.7.8}\right)$$

$$- \&c.$$

THE other series was given by Mr IVORY, in a very ingenious paper upon the same subject we have been just now considering †. It converges by the difference of the axes, divided by their sum; so that if we suppose the semi-axes, as before, to be denoted by 1 and c; and put $d = \frac{1-c}{1+c}$, and π for 3.14159, &c. Half the perimeter

$$= \frac{\pi}{1+d}\left\{1 + \frac{1^2}{2^2}d^2 + \frac{1^2.1^2}{2^2.4^2}d^4 + \frac{1^2.1^2.3^2}{2^2.4^2.6^2}d^6 + \frac{1^2.1^2.3^2.5^2}{2^2.4^2.6^2.8^2}d^8 + \&c.\right\}$$

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* IN a tract entitled "Animadversiones in Rectificationem Ellipsis," which forms part of the second volume of his Opuscula.

† Transactions of the Royal Society of Edinburgh, vol. iv.

I SHALL now give a third feries, which, fo far as I know, is new; and which, befides poffeffing the fingular advantage of converging rapidly in every cafe of excentricity that can poffibly occur, has other properties that render it peculiarly well fitted to the purpofe for which it is wanted in this paper.

LET 1 denote the femi-tranfverfe axis, and e the excentricity of an ellipfe, as before.

Find $e' = \frac{1-\sqrt{1-e^2}}{1+\sqrt{1-e^2}}$, $e'' = \frac{1-\sqrt{1-e'^2}}{1+\sqrt{1-e'^2}}$, $e''' = \frac{1-\sqrt{1-e''^2}}{2+\sqrt{1-e''^2}}$, &c.

Compute $P = (1+e')(1+e'')(1+e''')$ &c.

and $Q = \frac{e}{2} + \frac{ee'}{2.2} + \frac{ee'e''}{2.2.2} + \frac{ee'e''e'''}{2.2.2.2} +$ &c.

Half the perimeter $= \pi P(1 - eQ)$.

IT is evident from the form of this laft feries, that if e be confidered as the excentricity of E, one of the ellipfes by which we have expreffed the coefficients A, B, (Art. 13.) it prefently follows, that e', the next quantity that occurs in the feries, will denote the excentricity of E′, the other ellipfe. Now e', e'', &c. are the fame functions of e, that e'', e''', &c. are of e'. Hence it follows, that if

$$P' = (1+e'')(1+e''')(1+e^{\text{iv}}), \text{ \&c.}$$

$$Q' = \frac{e'}{2} + \frac{e'e''}{2.2} + \frac{e'e''e'''}{2.2.2} + \text{ \&c.}$$

we get, $E' = \pi P'(1 - e'Q')$.

BUT in comparing together the values of P and P′, alfo Q and Q′, it appears that $P' = \frac{P}{1+e'}$, and $Q' = \frac{2Q}{e} - 1 = \frac{e(1+e')^2}{2e'}Q - 1$, fo that the ratios of the ellipfes E and E′ to their common circumfcribing circle, (which are what we want in the computation of the coefficients A and B) may be expreffed thus:

$$\frac{E}{\pi} = P(1 - eQ).$$

$$\frac{E'}{\pi} = P(1 - \frac{e}{2}(1+e')Q).$$

15. THE

15. THE method of determining the coefficients, which is derived from this laſt ſeries for the rectification of the ellipſe, therefore, ſeems better than ſuch as would be afforded by either of the other two ſeries given in this paper. For, beſides conſidering that there is no quantity required for the computation of $\frac{E'}{\pi}$, which was not previouſly wanted in the computation of $\frac{E}{\pi}$, it appears that the logarithms of the quantities e', e'', &c. alſo $1+e'$, $1+e''$, &c. may be all derived from e, and from each other, with great facility, by means of the common trigonometrical tables, in the following manner.

LET α be ſuch an arch that $\sin\alpha = e$, then $\cos\alpha = \sqrt{1-e^2}$, and conſequently $e' = \frac{1-\sqrt{1-e^2}}{1+\sqrt{1-e^2}} = \frac{1-\cos\alpha}{1+\cos\alpha} = \tan^2\frac{\alpha}{2}$, alſo $1+e'$ $= \sec^2\frac{\alpha}{2}$. Again, by taking α' ſuch that $\sin\alpha' = e'$, we find, in like manner, $e'' = \tan^2\frac{\alpha'}{2}$, and $1+e'' = \sec^2\frac{\alpha'}{2}$; and ſo on we may go, as far as may be neceſſary. Hence it follows, that

$$P = \sec^2\frac{\alpha}{2}\cdot\sec^2\frac{\alpha'}{2}\cdot\sec^2\frac{\alpha''}{2}, \text{ \&c.}$$

$$Q = \frac{\sin\alpha}{2} + \frac{\sin\alpha\cdot\sin\alpha'}{2\cdot 2} + \frac{\sin\alpha\cdot\sin\alpha'\cdot\sin\alpha''}{2\cdot 2\cdot 2} + \text{\&c.}$$

and ſince we are provided with the logarithms of $\sec\frac{\alpha}{2}$, $\sec\frac{\alpha'}{2}$, &c. alſo $\sin\alpha$, $\sin\alpha'$, &c. the arithmetical calculation of P and Q will be very eaſy.

I MUST obſerve that this laſt method of determining the coefficients A and B, in the caſe of the exponent $n = -\frac{3}{2}$, by means of the infinite product $P = (1+e')(1+e'')(1+e''')$ &c. and the ſeries $Q = \frac{e}{2} + \frac{ee'}{2.2} + \frac{ee'e''}{2.2.2} +$ &c. coincides in the reſult with the method given by Mr IVORY, for determining the ſame coefficients, when the exponent is $-\frac{1}{2}$, in his very ingenious paper upon that ſubject, which we have already quoted : his ſolution, however, is obtained without any reference to the ellipſe.

I MIGHT now ſhew the application of the different methods here inveſtigated, by calculating the values of A and B in ſome particular caſe, as, for example, in the caſe of Venus and the Earth ; but as theſe methods have been ſufficiently detailed, ſo as to be eaſily underſtood by any perſon ſkilled in arithmetical calculation, I ſhall conclude this paper, and give, in the form of an Appendix, the inveſtigation of a new formula for the rectification of any elliptic arch whatever ; but from which, in the particular caſe of that arch becoming a quadrant, we immediately derive the third and laſt formula mentioned in this paper, for finding the ſemi-perimeter of an ellipſe.

APPEN-

APPENDIX, containing the Inveſtigation of a Formula for the Rectification of any Arch of an Ellipſe.

1. IT is now generally underſtood, that by the rectification of a curve line, is meant, not only the method of finding a ſtraight line exactly equal to it, but alſo the method of expreſſing it by certain functions of the other lines, whether ſtraight lines or circles, by which the nature of the curve is defined. It is evidently in the latter ſenſe that we muſt underſtand the term *rectification*, when applied to the arches of conic ſections, ſeeing that it has hitherto been found impoſſible, either to exhibit ſtraight lines equal to them, or to expreſs their relation to their co-ordinates, by algebraic equations, conſiſting of a finite number of terms.

WITH reſpect to the rectification of the circle and parabola, there ſeems to remain but little farther to be deſired. The determination of any arch of the former of theſe curves is a problem which ſo often occurs, and its ſolution is rendered ſo eaſy, by the aid of trigonometrical tables, that formulæ, involving circular arches, are conſidered as nearly of the ſame degree of ſimplicity, as if they involved only algebraic functions of ſtraight lines; and as to the latter curve, it is well known that the formula expreſſing its length is compoſed of two parts, the one an algebraic, and the other a logarithmic function of the co-ordinates, ſo that, by means of a table of logarithms, we can quickly aſſign the numerical value of any portion of the curve.

2. THE indefinite rectification of the ellipſe, or hyperbola, cannot, however, be effected by formulæ of ſuch ſimplicity as thoſe which expreſs indefinite arches of the two former curves; for the algebraic equations which define their nature, and from which we derive the formulæ for their rectification, are more complex than the equations which define the nature of the circle and parabola. But it is to be obſerved, that whatever difficulty there may be in the rectification of the ellipſe and hyperbola, it is now confined entirely to the ellipſe; for by one of the moſt happy applications that has ever been made of the modern analyſis to geometry, it has been diſcovered, that the rectification of any hyperbolic arch whatever may always be reduced to the rectification of two elliptic arches *. Thus, it appears, that whatever facility the following ſeries may afford for the rectification of the ellipſe, it muſt alſo be underſtood to extend the ſame to the rectification of the hyperbola, and to every problem, for the ſolution of which the rectification of either of theſe curves is neceſſary.

3. BUT to proceed with the inveſtigation of our formula, let us ſuppoſe that φ and φ' are two arches of a circle, ſo related to each other, that

$$\frac{\text{ſin } 2\varphi}{\sqrt{1+e'^2+2e'\text{coſ } 2\varphi}} = \text{ſin } 2\varphi',$$

where e' denotes an invariable quantity.

THEN it is evident, that when $\varphi = 0$, φ' is alſo $= 0$, and while φ increaſes from 0 to a quadrant or $\frac{\pi}{2}$, φ' will alſo increaſe from 0 to $\frac{\pi}{2}$.

TAKING now the fluxion of our aſſumed equation, we get,

$$\frac{\dot{\varphi}\text{ coſ } 2\varphi}{\sqrt{1+e'^2+2e'\text{ coſ } 2\varphi}} + \frac{e'\dot{\varphi}\text{ ſin }^2 2\varphi}{(1+e^2+2e'\text{ coſ } 2\varphi)^{\frac{3}{2}}} = \dot{\varphi}'\text{ coſ } 2\varphi;$$

But

* THIS diſcovery was made by Mr LANDEN, who publiſhed it firſt in the Philoſophical Tranſactions for 1775, and afterwards in his Mathematical Memoirs.

But $\frac{\sin^2 2\varphi}{1 + e'^2 + 2e' \cos 2\varphi} = \sin^2 2\varphi'$, therefore also

$$\frac{\dot{\varphi} \cos 2\varphi}{\sqrt{1 + e'^2 + 2e' \cos 2\varphi}} + \frac{e'\dot{\varphi} \sin^2 2\varphi'}{\sqrt{1 + e'^2 + 2e' \cos 2\varphi}} = \dot{\varphi}' \cos 2\varphi'.$$

Let us now reduce the terms on the left-hand side of the first fluxionary equation to a common denominator, and we find

$$\frac{\dot{\varphi}(1 + e' \cos 2\varphi).(e' + \cos 2\varphi)}{(1 + e'^2 + 2e' \cos 2\varphi)^{\frac{3}{2}}} = \dot{\varphi}' \cos 2\varphi'.$$

But from our assumed equation, we find

$$\frac{e' + \cos 2\varphi}{\sqrt{1 + e'^2 + 2e' \cos 2\varphi}} = \cos 2\varphi'.$$

Hence, and from the last fluxionary equation, we have

$$\frac{\dot{\varphi}(1 + e' \cos 2\varphi)}{1 + e'^2 + 2e \cos 2\varphi} = \dot{\varphi}'.$$

Again, taking the square of both sides of our assumed equation, and multiplying by e'^2, we have $\frac{e'^2(1 - \cos^2 2\varphi)}{1 + e'^2 + 2e' \cos 2\varphi} = e'^2 \sin^2 2\varphi'$, and therefore

$$\frac{1 + e' \cos 2\varphi}{\sqrt{1 + e'^2 + 2e' \cos 2\varphi}} = \sqrt{1 - e'^2 \sin^2 2\varphi'}.$$

Let each side of the last fluxionary equation be now divided by the corresponding side of the last equation; thus we find

$$\frac{\dot{\varphi}}{\sqrt{1 + e'^2 + 2e' \cos 2\varphi}} = \frac{\dot{\varphi}'}{\sqrt{1 - e'^2 \sin^2 2\varphi'}};$$

and by comparing this result with the second fluxionary equation, we also find

$$\frac{\dot{\varphi} \cos 2\varphi}{\sqrt{1 + e'^2 + 2e' \cos 2\varphi}} + \frac{e'\dot{\varphi}' \sin^2 2\varphi'}{\sqrt{1 - e'^2 \sin^2 2\varphi'}} = \dot{\varphi}' \cos 2\varphi'.$$

Let us next assume $e^2 = \frac{4e'}{(1 + e')^2}$, then it follows that $\sqrt{1 - e^2} = \frac{1 - e'}{1 + e'}$, and $e' = \frac{1 - \sqrt{1 - e^2}}{1 + \sqrt{1 - e^2}}$; if we now substitute $1 - 2 \sin^2 \varphi$ for $\cos 2\varphi$, and e^2 for $\frac{4e'}{(1 + e')^2}$, in the expression $1 + e'^2 + 2e' \cos 2\varphi$, it is transformed to $(1 + e')^2 (1 - e^2 \sin^2 \varphi)$, so that, after due

reduction

reduction, our two last fluxionary equations may be otherwise expressed, thus:

$$\frac{\dot{\varphi}}{\sqrt{1 - e^2 \sin^2 \varphi}} = (1 + e') \frac{\dot{\varphi}'}{\sqrt{1 - e'^2 \sin^2 2\varphi'}},$$

$$\frac{\dot{\varphi} \sin^2 \varphi}{\sqrt{1 - e^2 \sin^2 \varphi}} = \frac{e'(1+e')}{2} \cdot \frac{\dot{\varphi}' \sin^2 2\varphi'}{\sqrt{1 - e'^2 \sin^2 2\varphi'}} + \frac{1}{2} \cdot \frac{\dot{\varphi}}{\sqrt{1 - e^2 \sin^2 \varphi}} - \frac{1 + e'}{2} \dot{\varphi}' \cos 2\varphi'.$$

Let us, for the present, denote the fluents of the fluxions which enter these two equations, as follows:

$$P = \int \frac{\dot{\varphi}}{\sqrt{1 - e^2 \sin^2 \varphi}}, \qquad P' = \int \frac{\dot{\varphi}'}{\sqrt{1 - e'^2 \sin^2 2\varphi'}},$$

$$N = \int \frac{\dot{\varphi} \sin^2 \varphi}{\sqrt{1 - e^2 \sin^2 \varphi}}, \qquad N' = \int \frac{\dot{\varphi}' \sin^2 2\varphi'}{\sqrt{1 - e'^2 \sin^2 2\varphi'}};$$

and the relation of the fluents will be expressed thus:

$$P = (1 + e') P',$$

$$N = \frac{e'(1 + e')}{2} N' + \frac{P}{2} - \frac{1 + e'}{2.2} \sin 2\varphi'.$$

4. Now, remarking that these two equations have been obtained, by assuming

$$e' = \frac{1 - \sqrt{1 - e^2}}{1 + \sqrt{1 - e^2}}, \text{ and } \sin 2\varphi' = \frac{\sin 2\varphi}{\sqrt{1 + e'^2 + 2e' \cos 2\varphi}} = \frac{\sin 2\varphi}{(1 + e')\sqrt{1 - e^2 \sin^2 \varphi}};$$

let us farther assume two similar equations, thus,

$$e'' = \frac{1 - \sqrt{1 - e'^2}}{1 + \sqrt{1 - e'^2}}, \ \sin 4\varphi'' = \frac{\sin 4\varphi'}{\sqrt{1 + e''^2 + 2e'' \cos 4\varphi'}} = \frac{\sin 4\varphi'}{(1 + e'')\sqrt{1 - e'^2 \sin^2 2\varphi'}},$$

where it is evident that when $\varphi' = 0$, φ'' is also $= 0$, and that while φ or φ' increases from 0 to $\frac{\pi}{2}$, φ'' will also increase from 0 to $\frac{\pi}{2}$.

If we take the fluxions of this second assumed equation, and reason in all respects as in Article 3., we shall arrive at a similar conclusion, that is, putting P'' for $\int \frac{\dot{\varphi}''}{\sqrt{1 - e''^2 \sin^2 4\varphi''}}$, and N'' for $\int \frac{\dot{\varphi}'' \sin^2 4\varphi''}{\sqrt{1 - e''^2 \sin^2 4\varphi''}}$,

P' =

$$P' = (1 + e'')\,P'',$$

$$N' = \frac{e''(1+e'')}{2}\,N'' + \frac{P'}{2} - \frac{1+e''}{2.4}\,\text{fin}\,4\phi''.$$

If by means of thefe two equations, and the two fimilar equations found in laft Article, we exterminate P′ and N′, we get

$$P = (1+e')\,(1+e'')\,P'',$$

$$N = e'e''\frac{(1+e')\,(1+e'')}{2\;.\;2}N'' + \left\{\frac{1}{2} + \frac{e'}{2.2}\right\}P - \frac{1+e'}{2.2}\,\text{fin}\,2\phi' - \frac{e'(1+e')\,(1+e'')}{2\,.\,2\,.\,2\,.\,2}\,\text{fin}\,4\phi''.$$

5. Let us now affume two feries of equations, fuch, that all the terms of each feries may be fimilar to one another, and to the equations which we have already affumed, (Art. 3. and 4.) or fo that

$$e''' = \frac{1-\sqrt{1-e''^2}}{1+\sqrt{1-e''^2}},\ \text{fin}\,8\phi''' = \frac{\text{fin}\,8\phi''}{\sqrt{1+e'''^2+2e'''\text{cof}\,8\phi''}} = \frac{\text{fin}\,8\phi''}{(1+e''')\sqrt{1-e''^2\,\text{fin}^2\,4\phi''}},$$

$$e^{\text{IV}} = \frac{1-\sqrt{1-e'''^2}}{1+\sqrt{1-e'''^2}},\ \text{fin}\,16\phi^{\text{IV}} = \frac{\text{fin}\,16\,\phi'''}{\sqrt{1+e^{\text{IV}2}+2e^{\text{IV}}\text{cof}\,8\phi'''}} = \frac{\text{fin}\,16\,\phi'''}{(1+e^{\text{IV}})\sqrt{1-e'''^2\,\text{fin}^2\,8\phi'''}},$$

&c. &c.

each feries being fuppofed to proceed, according to the fame law, as far as we pleafe; then, by due confideration of the quantities fin $8\phi'''$, fin $16\phi^{\text{IV}}$, &c. it appears that when $\phi'' = 0$, all the remaining arches ϕ''', ϕ^{IV}, &c. are alfo each $= 0$, and while ϕ'' increafes from 0 to $\frac{\pi}{2}$, the remaining arches alfo increafe from 0 to $\frac{\pi}{2}$; fo that, upon the whole, while ϕ, the firft arch, increafes from 0 to a quadrant, each of the remaining arches ϕ', ϕ'', ϕ''', &c. will alfo increafe from 0 to a quadrant.

6. Let us next fubftitute for the fluents as before

$$P''' = \int\frac{\dot\phi'''}{\sqrt{1-e'''^2\,\text{fin}^2\,\phi'''}},\ \text{\&c.}\qquad N''' = \int\frac{\dot\phi'''\,\text{fin}^2\,8\phi'''}{\sqrt{1-e'''^2\,\text{fin}^2\,8\phi'''}},\ \text{\&c.}$$

then, following the fame analogy as in Articles 3 and 4. we have

P″ =

$$P'' = (1 + e''')\,P''',$$
$$P''' = (1 + e^{iv})\,P^{iv},$$
&c. &c.
$$N'' = \frac{e'''(1+e''')}{2}N''' + \frac{P''}{2} - \frac{1+e'''}{2\,.\,8}\,\text{fin}\,8\phi''',$$
$$N''' = \frac{e^{iv}(1+e^{iv})}{2}N^{iv} + \frac{P'''}{2} - \frac{1+e^{iv}}{2\,.\,16}\,\text{fin}\,16\phi^{iv},$$
&c.

and thus we may proceed as far as we pleafe. If thefe equations be now combined with the equations fimilar to them which we have already found (Art. 3. and 4.), and the quantities N′, P′; N″, P″; N‴, P‴, &c. be gradually exterminated in the fame way as in obtaining the equations of Art 4. we get the following feries of equations:

$$P \begin{cases} = (1+e')\,P' \\ = (1+e')(1+e'')\,P'' \\ = (1+e')(1+e'')(1+e''')\,P''', \end{cases}$$
&c.

$$N \begin{cases} = e'\frac{(1+e')}{2}N' + \frac{1}{2}P - \frac{(1+e')}{2}\,\text{fin}\,2\phi', \\ = e'e''\frac{(1+e')(1+e'')}{2\,.\,2}N'' + \left\{\frac{1}{2}+\frac{e'}{2.2}\right\}P - \frac{1+e'}{2\,.\,2}\text{fin}\,2\phi' - \frac{e'(1+e')(1+e'')}{2\,.\,2\,.\,2\,.\,2}\text{fin}\,4\phi'', \\ = e'e''e'''\frac{(1+e')(1+e'')(1+e''')}{2\,.\,2\,.\,2}N''' + \left\{\frac{1}{2}+\frac{e'}{2.2}+\frac{e'e''}{2.2.2}\right\}P - \frac{1+e'}{2.2}\,\text{fin}\,2\phi' \\ \quad - \frac{e'(1+e')(1+e'')}{2\,.\,2\,.\,2\,.\,2}\text{fin}\,4\phi'' - \frac{e'e''(1+e')(1+e'')(1+e''')}{2.2\,.\,2\,.\,2\,.\,2.2}\text{fin}\,16\phi'''. \end{cases}$$
&c.

If we fuppofe thefe values of P and N to be continued indefinitely, it is evident that the former will be expreffed by the product of an infinite number of invariable factors, and by the limit to which the feries of fluents P′, P″, P‴, &c. conftantly tends; and the latter by another infinite product, and two infinite feries, which involve alfo P, and the limits to which the

the series of fluents N', N'', N''', &c. and the series of arches φ, φ', φ'', &c. both constantly tend.

7. Now in order to investigate these limits, I observe, in the first place, that the quantities which constitute the series e, e', e'', e''', &c. converge very fast towards 0; for, since $e' = \frac{1-\sqrt{1-e^2}}{1+\sqrt{1-e^2}} = \frac{e^2}{(1+\sqrt{1-e^2})^2}$, it is evident that e must always be a fraction less than 1, and that e' must be less than the square of that fraction; indeed, if e be small, e' will be nearly $= \frac{e^2}{4}$. In the same manner it appears that e'' is between the limits of e'^2 and $\frac{e'^2}{4}$, so that e'' is less than e^4, but greater than $\frac{e^4}{64}$; and proceeding in this manner, we find e''' less than e^8, e^{iv} less than e^{16}, and so on, the exponents of e forming the geometrical progression, 2, 4, 8, 16, 32, &c.

Thus it appears, even without taking into account the denominators of the powers of e, that the rapidity with which the quantities e, e', e'', &c. converge towards 0 is indeed very great.

8. I observe, in the next place, that the terms which constitute the series of arches $\varphi, \varphi', \varphi'', \varphi'''$, &c. also converge very fast to a certain limit. For since we have assumed such a relation between the arches that

$$\sin 2\varphi' = \frac{\sin 2\varphi}{\sqrt{1+e'^2+2e'\cos 2\varphi}},$$

$$\sin 4\varphi'' = \frac{\sin 4\varphi'}{\sqrt{1+e''^2+2e''\cos 4\varphi'}},$$

$$\sin 8\varphi''' = \frac{\sin 8\varphi''}{\sqrt{1+e'''^2+2e'''\cos 8\varphi''}}$$

&c.

it is evident, from what has been said in the last Article, that the denominators of the fractions, which are equal to $\sin 2\varphi'$, $\sin 4\varphi''$, $\sin 8\varphi'''$, &c. converge quickly towards unity, so that the ratios of $\sin 2\varphi$ to $\sin 2\varphi'$, of $\sin 4\varphi'$ to $\sin 4\varphi''$, of $\sin 8\varphi''$ to $\sin 8\varphi'''$, &c.

 approach

approach very faſt to the ratio of equality, which is their limit; and the ſame will alſo be true of the ratios which every two adjoining arches of the ſeries $\phi, \phi', \phi'', \phi'''$, &c. have to each other.

Let us denote the limit, to which theſe arches continually approach, by θ, and it will preſently appear, that the ſame arch θ is alſo the limit of the ſeries of fluents $\int \frac{\dot{\phi}}{\sqrt{1-e^2 \text{ſin}^2 \phi}}$, $\int \frac{\dot{\phi}'}{\sqrt{1-e'^2 \text{ſin}^2 2\phi'}}$, $\int \frac{\dot{\phi}''}{\sqrt{1-e''^2 \text{ſin}^2 4\phi''}}$, &c. or of P, P′, P″, &c. ſo that we have ultimately

$$P = \theta(1+e')(1+e'')(1+e'''), \text{ \&c.}$$

9. In the ſame way it appears that the ſeries of fluents $\int \frac{\dot{\phi} \text{ ſin}^2 \phi}{\sqrt{1-e^2 \text{ſin}^2 \phi}}$, $\int \frac{\dot{\phi}' \text{ſin}^2 2\phi'}{\sqrt{1-e'^2 \text{ſin}^2 2\phi'}}$, $\int \frac{\dot{\phi}'' \text{ſin}^2 4\phi''}{\sqrt{1-e''^2 \text{ſin}^2 4\phi''}}$, &c. or N, N′, N″, &c. continually approaches to $\int \dot{\theta} \text{ ſin}^2 n\theta$, where n denotes a number indefinitely great; but in this caſe $\int \dot{\theta} \text{ ſin}^2 n\theta = \int \dot{\theta} \left(\frac{1}{2} - \frac{1}{2} \text{coſ } 2n\theta\right)$ is equal to $\frac{\theta}{2}$, a finite quantity; now if we remark that this quantity, which enters the value of N, (Art. 6.) is multiplied by the two infinite products $e'.e''.e'''$. &c. and $\frac{1+e'}{2} \cdot \frac{1+e''}{2} \cdot \frac{1+e'''}{2}$, &c. both of which are evidently equal to 0, it appears that the quantity $e' e'' e'''$, &c. $\times \frac{(1+e')}{2} \frac{(1+e'')}{2} \frac{(1+e''')}{2} \text{ \&c.} \times \frac{\theta}{2}$ is to be rejected.

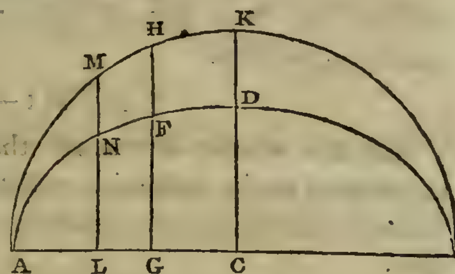
10. Finally, with regard to the quantity N itſelf, or $\int \frac{\dot{\phi} \text{ ſin}^2 \phi}{\sqrt{1-e^2 \text{ſin}^2 \phi}}$, it is evidently equal to $\frac{1}{e^2} \left\{ \int \frac{\dot{\phi}}{\sqrt{1-e^2 \text{ſin}^2 \phi}} - \int \dot{\phi} \sqrt{1-e^2 \text{ſin}^2 \phi} \right\}$; now if we put $1 = AC$, the ſemi-tranſverſe axis of an ellipſe, $e =$ the excentricity, and $\phi = HK$, any arch of the circumſcribing circle, intercepted between the conjugate axis, and FG an ordinate to the tranſverſe axis, it is well known that

that $\dot{\varphi}\sqrt{1 - e^2 \text{fin}^2\varphi}$ expreſſes the fluxion of the elliptic arch FD; let us denote that arch by z, and we have $N = \frac{P - z}{e^2}$; ſo that upon the whole we find

$$\frac{P - z}{e^2} = \begin{cases} +\left(\frac{1}{2} + \frac{e'}{2.2} + \frac{e'e''}{2.2.2} + \frac{e'e''e'''}{2.2.2.2} + \&c.\right) P \\ -\left(\frac{1+e'}{2.2}\text{fin}\,2\varphi' + \frac{e'(1+e')(1+e'')}{2\,.\,2\,.\,2\,.\,2}\text{fin}\,4\varphi'' + \frac{e'e''(1+e')(1+e'')(1+e''')}{2\,.\,2\,.\,2\,.\,2\,.\,2\,.\,2}\text{fin}\,8\varphi''' + \&c.\right. \end{cases}$$

and from this equation the manner of expreſſing z, by means of the infinite product P, and two infinite ſeries, is obvious.

11. I SHALL now collect into one point of view the reſult of the preceding analyſis, in the form of a practical rule for finding the length of any arch of an ellipſe.



LET $1 = AC$ the ſemi-tranſverſe axis,

$e =$ the excentricity,

$z = DF$ any arch of the ellipſe, reckoned from the extremity of the conjugate axis.

$\varphi = HK$, an arch of the circumſcribing circle, intercepted between the conjugate axis and FG, an ordinate to the tranſverſe.

COMPUTE the quantities e', e'', e''', &c. fin $2\varphi'$, fin $4\varphi''$, fin $8\varphi'''$, &c. from theſe two ſeries of equations,

$$e' = \frac{1 - \sqrt{1 - e^2}}{1 + \sqrt{1 - e^2}}, \qquad \text{fin}\,2\varphi' = \frac{\text{fin}\,2\varphi}{(1 + e')\sqrt{1 - e^2\,\text{fin}^2\varphi}},$$

$$e'' = \frac{1 - \sqrt{1 - e'^2}}{1 + \sqrt{1 - e'^2}}, \qquad \text{fin}\,4\varphi'' = \frac{\text{fin}\,4\varphi'}{(1 + e'')\sqrt{1 - e'^2\,\text{fin}^2 2\varphi'}},$$

$$e''' = \frac{1 - \sqrt{1 - e''^2}}{1 + \sqrt{1 - e''^2}}, \qquad \text{fin}\,8\varphi''' = \frac{\text{fin}\,8\varphi''}{(1 + e''')\sqrt{1 - e''^2\,\text{fin}^2 4\varphi''}},$$

$$e^{\text{IV}} = \frac{1 - \sqrt{1 - e'''^2}}{1 + \sqrt{1 - e'''^2}}, \qquad \text{fin}\,16\varphi^{\text{IV}} = \frac{\text{fin}\,16\varphi'''}{(1 + e^{\text{IV}})\sqrt{1 - e'''^2\,\text{fin}^2 8\varphi'''}}.$$

&c. &c.

 THE

The quantities e', e'', e''', &c. approach very faſt to 0, and the arches φ', φ'', φ''', &c. approach to a certain limit, which let us denote by θ.

Compute alſo theſe three quantities,

$$P = (1 + e')(1 + e'')(1 + e''') \text{ \&c.}$$

$$Q = \frac{e}{2} + \frac{ee'}{2.2} + \frac{ee'e''}{2.2.2} + \frac{ee'e''e'''}{2.2.2.2} +, \text{ \&c.}$$

$$R = \frac{e(1+e')}{2.2} \text{ſin} 2\varphi' + \frac{ee'(1+e')(1+e'')}{2.2.2.2} \text{ſin} 4\varphi'' + \frac{ee'e''(1+e')(1+e'')(1+e''')}{2.2.2.2.2.2} \text{ſin} 8\varphi''' + \text{\&c.}$$

The elliptic arch $z = \theta P(1 - eQ) + eR$.

When φ becomes a quadrant, the ſines of $2\varphi'$, $4\varphi''$, $8\varphi'''$, &c. are evidently each 0, ſo that, putting $\pi = 3.14159$, &c. we have in this caſe, $\theta = \frac{\pi}{2}$, and, putting E to denote the whole elliptic quadrant, we have

$$E = \frac{\pi}{2} P(1 - eQ).$$

Now it is worthy of remark, that the expreſſion $P(1 - eQ)$ is common to the indefinite arch z and the whole quadrant E; hence it follows that the indefinite arch may be alſo expreſſed thus,

$$z = \frac{2\theta}{\pi} E + eR.$$

12. From this laſt formula we may derive many of thoſe curious relations which are known to ſubſiſt between certain aſſignable elliptic arches, as alſo between theſe arches and the whole elliptic quadrant *. For example, we may hence deduce that very remarkable property of the ellipſe, which is commonly known by the name of Fagnani's theorem, namely, that an elliptic arch, reckoned from the extremity of either axis being ſuppoſed given, another arch, reckoned from the extremity of the other axis, may be found, by a geometrical conſtruction, ſuch

* The properties of the ellipſe here alluded to have been explained by Euler, and ſome of them have alſo been obſerved by Landen.

ſuch, that the difference of theſe arches ſhall be equal to a certain aſſignable ſtraight line.

For, let $(z) = DN$, another elliptic arch, reckoned from the extremity of the tranſverſe, and $(\varphi) = KM$, the correſponding arch of the circumſcribing circle; then, if we find (φ'), (φ''), (φ'''), &c. a ſeries of arches ſimilar to the ſeries φ', φ'', φ''', &c. (Art. 11.) or ſuch that

$$\text{ſin } 2(\varphi') = \frac{\text{ſin } 2(\varphi)}{(1+e')\sqrt{1-e^2 \text{ ſin}^2(\varphi)}},$$

$$\text{ſin } 4(\varphi'') = \frac{\text{ſin } 4(\varphi')}{(1+e'')\sqrt{1-e'^2 \text{ ſin}^2 2(\varphi')}},$$

$$\text{ſin } 8(\varphi''') = \frac{\text{ſin } 8(\varphi'')}{(1+e''')\sqrt{1-e''^2 \text{ ſin}^2 4(\varphi'')}},$$

&c.

and put (θ) to denote the limit of the arches (φ), (φ'), (φ''), &c. alſo,

$$(R) = \frac{e(1+e')}{2 \cdot 2}\text{ſin}\,2(\varphi') + \frac{ee'(1+e')(1+e'')}{2 \cdot 2 \cdot 2 \cdot 2}\text{ſin}\,4(\varphi'') + \frac{ee'e''(1+e')(1+e'')(1+e''')}{2 \cdot 2 \cdot 2 \cdot 2 \cdot 2 \cdot 2}\text{ſin}\,8(\varphi''') + \&c.$$

it follows, that $(z) = \frac{2(\theta)}{\pi} E + e(R)$.

Now if we ſuppoſe the arches φ and (φ) to be ſuch that $\varphi' + (\varphi') = \frac{\pi}{2}$, then it follows that $\text{ſin } 2\varphi' = \text{ſin } 2(\varphi')$, and $\text{ſin } 4\varphi' = -\text{ſin } 4(\varphi')$, and ſince

$$\text{ſin } 4\varphi'' = \frac{\text{ſin } 4\varphi'}{(1+e'')\sqrt{1-e'^2 \text{ ſin}^2 2\varphi'}},$$

$$\text{alſo, ſin } (4\varphi'') = \frac{\text{ſin } 4(\varphi')}{(1+e'')\sqrt{1-e'^2 \text{ ſin}^2 2(\varphi')}},$$

we have $\text{ſin } 4\varphi'' = -\text{ſin } 4(\varphi'')$. Now when $\varphi' = 0$, then $(\varphi') = \frac{\pi}{2}$; but when $\varphi' = 0$, it appears (Art. 5.) that $\varphi'' = 0$, and alſo, that when $(\varphi') = \frac{\pi}{2}$, then $(\varphi'') = \frac{\pi}{2}$; therefore, when $4\varphi'' = 0$, $4(\varphi'') = 2\pi$. Again, while φ' increaſes from 0 to $\frac{\pi}{4}$, (φ') will decreaſe from $\frac{\pi}{2}$ to $\frac{\pi}{4}$; hence, and from the two laſt equations, it

follows,

follows, that in the same circumstances $4\varphi''$ will increase from 0 to π, and $4(\varphi'')$ will decrease from 2π to π; and because that while φ' increases from $\frac{\pi}{4}$ to $\frac{\pi}{2}$, (φ') diminishes from $\frac{\pi}{4}$ to 0, it will also follow, that while $4\varphi''$ increases from π to 2π, $4(\varphi'')$ will decrease from π to 0; and since during these changes of the value of the arches $4\varphi''$, $4(\varphi'')$, we have always $\sin 4\varphi'' = -\sin 4(\varphi'')$, it is evident that $4\varphi'' + 4(\varphi'') = 2\pi$, or $\varphi'' + (\varphi'') = \frac{\pi}{2}$.

AGAIN, from the two equations, $\sin 8\varphi''' = \frac{\sin 8\varphi''}{(1+e'')\sqrt{1-e''^2 \sin^2 4\varphi''}}$, $\sin 8(\varphi''') = \frac{\sin 8(\varphi'')}{(1+e'')\sqrt{1-e''^2 \sin^2 4(\varphi'')}}$, and from the consideration that $\varphi'' + (\varphi'') = \frac{\pi}{2}$, we find $\sin 8\varphi''' = -\sin 8(\varphi''')$; and hence, by reasoning as before, we also find $\varphi''' + (\varphi''') = \frac{\pi}{2}$; so that, upon the whole, we obtain the two following series of equations:

$$\begin{array}{ll}
\sin 2\varphi' = +\sin 2(\varphi') & \varphi' + (\varphi') = \frac{\pi}{2}, \\
\sin 4\varphi'' = -\sin(4\varphi'') & \varphi'' + (\varphi'') = \frac{\pi}{2}, \\
\sin 8\varphi''' = -\sin(8\varphi'''), & \varphi''' + (\varphi''') = \frac{\pi}{2}. \\
\&c. & \&c.
\end{array}$$

and therefore ultimately $\theta + (\theta) = \frac{\pi}{2}$.

Now we have the elliptical arch DF, or $z = \frac{2\theta}{\pi}\mathrm{E} + e\mathrm{R}$;

also the arch DN, or $(z) = \frac{2(\theta)}{\pi}\mathrm{E} + e(\mathrm{R})$.

Hence $\mathrm{DN} + \mathrm{DF} = z + (z) = \frac{2\{\theta+(\theta)\}}{\pi}\mathrm{E} + e\{\mathrm{R}+(\mathrm{R})\}$; but, from the two preceding series of equations, it is evident that $\frac{2\{\theta+(\theta)\}}{\pi} = 1$, and $e\{\mathrm{R}+(\mathrm{R})\} = \frac{e^2(1+e')}{2}\sin 2\varphi'$; therefore $\mathrm{DN} + \mathrm{DF} = \mathrm{E} + \frac{e^2(1+e')}{2}\sin 2\varphi'$ and $\mathrm{AN} - \mathrm{DF} = \frac{e^2(1+e')}{2}\sin 2\varphi'$ $= \frac{e^2 \sin 2\varphi}{2\sqrt{1-e^2\sin^2\varphi}} = \frac{e^2 \sin\varphi \,.\, \cos\varphi}{\sqrt{1-e^2\sin^2\varphi}}$.

THUS

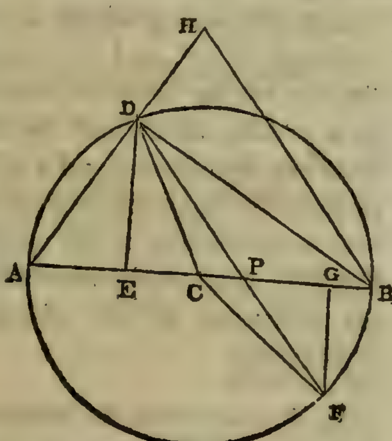
Thus we have two elliptic arches, reckoned from the extremities of the axes, the difference of which is equal to a certain assignable straight line.

13. Having found this very curious relation between the elliptic arches AN and FD, let us next investigate the relation between the corresponding circular arches AM, HK, by means of the equation $\phi' + (\phi') = \frac{\pi}{2}$.

Let $\psi =$ AM the arch of the circumscribing circle, between the extremity of the transverse axis, and the ordinate LNM, then $\text{cos}\,\psi = \text{sin}(\phi)$, and $\text{sin}\,2\psi = \text{sin}\,2(\phi)$; and since $\text{sin}\,2\phi' = \text{sin}\,2(\phi')$, also $\text{sin}\,2\phi' = \frac{\text{sin}\,2\phi}{(1+e')\sqrt{1-e^2\text{sin}^2\phi}}$, and $\text{sin}\,2(\phi') = \frac{\text{sin}\,2(\phi)}{(1+e')\sqrt{1-e^2\text{sin}^2(\phi)}}$, it follows, that to determine the relation between ϕ and ψ, we have

$$\frac{\text{sin}\,2\phi}{(1+e')\sqrt{1-e^2\text{sin}^2\phi}} = \frac{\text{sin}\,2\psi}{(1+e')\sqrt{1-e^2\text{cos}^2\psi}},\ \text{or,}\ \frac{\text{sin}\,2\phi}{\sqrt{1+e'^2+2e'\text{cos}2\phi}} = \frac{\text{sin}\,2\psi}{\sqrt{1+e'^2-2e'\text{cos}2\psi}}.$$

Let AB be the diameter of a circle, of which AC the radius $= 1$; let the arch AD $= 2\phi$, and BF (taken in the opposite semicircle) $= 2\psi$, take CP $= e'$, join PD, PF, CD, CF, and draw DE and FG perpendicular to the diameter; then DE $= \text{sin}\,2\phi$, and EC $= \text{cos}\,2\phi$; also FG $= \text{sin}\,2\psi$, and GC $= \text{cos}\,2\psi$.



From the elements of geometry we have

$$\text{PD}^2 \begin{cases} = \text{DC}^2 + \text{CP}^2 + 2\text{PC}\,.\,\text{CE}, \\ = 1 + e'^2 + 2e'\,\text{cos}\,2\phi; \end{cases}$$

$$\text{also PF}^2 \begin{cases} = \text{FC}^2 + \text{CP}^2 - 2\text{PC}\,.\,\text{CG}, \\ = 1 + e'^2 - 2e'\,\text{cos}\,2\psi. \end{cases}$$

therefore,

therefore, $\frac{\text{ſin } 2\varphi}{\sqrt{1 + e'^2 + 2e'^2 \text{ coſ } 2\varphi}} = \frac{DE}{PD}$, and $\frac{\text{ſin } 2\psi}{\sqrt{1 + e'^2 - 2e' \text{ coſ } 2\psi}} = \frac{FG}{PF}$, hence it appears that $\frac{DE}{PD} = \frac{FG}{PF}$, ſo that the triangles PED, PGF, are ſimilar; and, as it is obvious, from what has been already ſhewn, that the arches AD, BF, are either both leſs or both greater than quadrants, it follows that PD and PF lie in the ſame ſtraight line. Join AD and BD, and draw BH parallel to DF, meeting AD in H, then AP : PB :: AD : DH :: tan ABD : tan DBH or tan BDF; that is, $1 + e' : 1 - e' :: \tan\varphi : \tan\psi$; therefore, $\tan\psi = \frac{1 - e'}{1 + e'} \tan\varphi$. But if we denote the ſemi-conjugate axis of the ellipſe by c, we have $e' = \frac{1 - c}{1 + c}$ (Art 11.); and therefore $c = \frac{1 - e'}{1 + e'}$; ſo that we at laſt derive from our formula, the following very remarkable theorem, firſt obſerved by Count Fagnani.

Let AD be a quadrant of an ellipſe, of which the ſemi-tranſverſe axis $= 1$ *, and AK a quadrant of the circumſcribing circle; let $c =$ the ſemi-conjugate axis, and $e =$ the excentricity; let $\psi =$ AM, an arch of the circle, reckoned from the extremity of the tranſverſe, and $\varphi =$ HK, an arch of the circle, reckoned from its interſection with the conjugate axis; draw ML and HG perpendicular to the tranſverſe axis, meeting the ellipſe in N and F; then, if the arches ψ and φ be ſuch, that $\tan\psi = c \tan\varphi$, the difference of the elliptic arches AN, FD, is equal to $\frac{e^2 \text{ ſin } \varphi \text{ coſ } \varphi}{\sqrt{1 - e^2 \text{ ſin }^2\varphi}}$.

14. It would not be difficult to ſhew, that by means of our formula, other theorems ſimilar to that of Fagnani might be inveſtigated; but as theſe, I believe, have been pointed out by other writers, and as this paper has extended to a greater length than was at firſt intended, I paſs them over for the preſent, and proceed,

* See Fig. page 279.

proceed in the next place to remark, that although in general the quantity R be expreffed by an infinite feries, yet there are certain values of the arch φ, fuch, that the feries will terminate; and when this happens, the limiting arch θ can be found exactly; fo that in each of thefe cafes, we obtain a finite equation expreffing a relation between the whole elliptic quadrant, and a particular arch of it.

FOR we have already obferved (Art. 5.), that while φ increafes from 0 to a quadrant, or $\frac{\pi}{2}$; each of the arches φ', φ'', φ''', &c. increafes from 0 to a quadrant; fo that when $\varphi = \frac{\pi}{2}$, then fin $2\varphi'$, fin $4\varphi''$, fin $8\varphi'''$, &c. are each $= 0$, and $\theta = \frac{\pi}{2}$; now in the very fame way it appears, that while $2\varphi'$ increafes from 0 to $\frac{\pi}{2}$, each of the arches $2\varphi''$, $2\varphi'''$, &c. increafes from 0 to $\frac{\pi}{2}$; fo that, in this cafe, fin $2\varphi' = 1$, and fin $4\varphi''$, fin $8\varphi'''$, and all that follow are each $= 0$; hence alfo the limiting arch $\theta = \frac{\pi}{4}$; and to determine the value of φ, we have this equation $\frac{\text{fin } 2\varphi}{\sqrt{1+e'^2+2e'\text{cof} 2\varphi}} = \text{fin } 2\varphi' = 1$, from which we find cof $2\varphi = -e'$.

15. REASONING in this way, we find that the feries R will confift of a finite number of terms, and the limit θ be exactly affignable in innumerable other cafes, namely, when any one of the following feries of equations takes place:

cof $2\varphi = -e'$, the limit θ being then $=$	$\frac{\pi}{4}$,
cof $4\varphi' = -e''$, - - - -	$\frac{\pi}{8}$,
cof $8\varphi'' = -e'''$, - - - -	$\frac{\pi}{16}$,
&c.	&c.

AND fince, by means of the feries of equations given in Article 11, we can determine φ, fo that each of the above equa-

 tions

tions shall successively have place, it appears, that we can assign an indefinite number of elliptic arches, reckoned from the extremity of the conjugate axis, such, that their relation to the whole elliptic quadrant shall be expressible by a finite equation. But we have seen (Art. 13.) that, corresponding to each of these arches, there is another, reckoned from the extremity of the transverse axis, such, that their difference may be expressed by a certain straight line; therefore, it appears that we can assign innumerable arches, reckoned from the extremity of either axis, possessing each the remarkable property of having their relation to the whole elliptic quadrant expressible by a finite algebraic equation *.

16. For example, let us suppose $\cos 2\varphi = -c'$, and therefore $\sin^2\varphi = \frac{1+c'}{2} = \frac{1}{1+c}$, then, since $\sin 2\varphi' = 1$, and $\sin 4\varphi''$, $\sin 8\varphi'''$, &c. are each $= 0$, we have $c\mathrm{R} = \frac{c^2(1+c')}{2 \cdot 2} = \frac{c'}{1+c'} = \frac{1-c}{2}$; now we have also $\theta = \frac{\pi}{4}$, therefore $z = \frac{\mathrm{E}}{2} + \frac{1-c}{2}$; hence we derive the following very curious proposition:

Let the semi-axes of an ellipse be 1 and c; take a straight line in the transverse axis, from the centre towards either vertex, $= \frac{1}{\sqrt{1+c}}$, and at the extremity of that line, between the centre and vertex, draw an ordinate to the transverse

* Hence, by the way, it appears, that instead of the semi-perimeters of two ellipses which we have used in the preceding paper, for expressing the coefficients of the development of $(a^2 + b^2 - 2ab\cos\varphi)^n$, we may substitute any two of an indefinite number of elliptic arches, and certain algebraic functions of the axes of these ellipses; therefore, the different infinite series, which may be used to express the coefficients A and B, are really innumerable.

verſe axis, meeting the curve, and dividing the elliptic quadrant into two parts, the greateſt of which is that next to the vertex of the conjugate axis; then, if E denote the whole elliptic quadrant, the greateſt elliptic arch ſhall be equal to $\frac{E+1-c}{2}$, and the leaſt $= \frac{E-1+c}{2}$; alſo the difference of the arches $= 1-c$ = difference of the ſemi-axes; and the ſame concluſion is readily drawn from the theorem given in Art. 13. by aſſuming $\varphi + \psi = \frac{\pi}{2}$.

17. ANOTHER obſervation I have to make upon our Formula is, that we may hence deduce that relation between indefinite arches of three certain ellipſes, which is mentioned at Article 10. of the preceding paper, and which was firſt obſerved by Mr LEGENDRE; but as the only uſe we have made of that property was in the caſe of thoſe arches being quadrants, or ſemiperimeters, I ſhall, for the ſake of brevity, treat only of that particular caſe.

SUPPOSING, therefore, the quantities e', e'', e''', &c. to be related to each other as already expreſſed in Art. 11. we have found, that if

$P = (1+e')(1+e'')(1+e''')$, &c. and $Q = \frac{e}{2} + \frac{ee'}{2.2} + \frac{ee'e''}{2.2.2} +$ &c.

and E denote the quadrant of an ellipſe, of which the excentricity is e, then $E = \frac{\pi}{2} P(1-eQ)$.

IN like manner, if E′ denote the quadrant of another ellipſe, of which the excentricity is e', and we put

$P' = (1+e'')(1+e''')(1+e^{\text{IV}})$, &c. alſo $Q' = \frac{e'}{2} + \frac{e'e''}{2.2} + \frac{e'e''e'''}{2.2.2} +$ &c.

for the very ſame reaſon $E' = \frac{\pi}{2} P'(1-e'Q')$.

O o 2 BUT

But by comparing together the values of P and P′, alſo of Q and Q′, it appears that $P = (1 + e')P'$, and $Q = \frac{e}{2}(1 + Q')$; ſo that, by ſubſtituting theſe values of P and Q in the formula $E = \frac{\pi}{2}P(1 - eQ)$, and remembering that $e^2 = \frac{4e'}{(1+e')^2}$, we alſo get $(1 + e')E = \frac{\pi}{2}P'(1 + e'^2 - 2e'Q')$; and if, by means of this equation, and the equation, $E' = \frac{\pi}{2}P'(1 - e'Q')$, we exterminate Q′, it will be found that

$$2E' - (1 + e')E = (1 - e'^2)\frac{\pi}{2}P'.$$

Let us now put E″ to denote the quadrant of a third ellipſe of which the excentricity is e''; then, if we alſo aſſume

$P'' = (1+e''')(1+e^{\text{iv}})(1+e^{\text{v}})$, &c., and $Q'' = \frac{e''}{2} + \frac{e''e'''}{2.2} + \frac{e''e'''e^{\text{iv}}}{2.2.2} +$ &c.

and conſider that the ſecond and third ellipſes are related to each other exactly in the ſame manner as the firſt and ſecond, we get an equation ſimilar to the laſt, namely,

$$2E'' - (1 + e'')E' = (1 - e''^2)\frac{\pi}{2}P'';$$

now ſince $P'' = \frac{P'}{1 + e''}$, we can readily, by means of theſe two equations, exterminate P′; accordingly we find

$$(1+e')(1-e'')E - \left\{2(1-e'')+(1-e'^2)(1+e'')\right\}E' + 2(1-e'^2)E'' = 0.$$

Thus we have got an equation expreſſing a very remarkable connection between three elliptic quadrants, the excentricities being e, e', and e''. But this equation may be rendered more ſimple in its form; for if we put c' for the ſemi-conjugate axis of the ſecond ellipſe, we have $e'' = \frac{1 - c'}{1 + c'}$, (Art. 11.) and therefore, $1 + e'' = \frac{1 - e''}{c'}$; now $1 - e'^2 = c'^2$, therefore $(1 - e'^2)(1 + e'') = (1 - e'')c'$; hence alſo we have $2(1 - e'^2) = (1 - e'')c'(1 + c')$, ſo that by ſubſtituting for $(1 - e'^2)(1 + e'')$ and $2(1 - e'^2)$ their values, and

and dividing the whole equation by $(1 - e'')$, we at laſt get

$$(1 + e')E - (2 + e')E' + e'(1 + e')E'' = 0,$$

the very equation which we propoſed to inveſtigate.

18. THE laſt remark I ſhall make at preſent, upon the formula given in this appendix for the rectification of the ellipſe, relates to the quickneſs of its convergency; and this, it appears, will in every caſe be very great. For we have ſeen (Art. 7.) that e' is leſs than the ſquare of the excentricity, e'' leſs than its fourth power, e''' leſs than its eighth power, and ſo on, proceeding according to the geometrical progreſſion 2, 4, 8, 16, 32, &c.; therefore, if only the three quantities e', e'', e''', be computed, together with the correſponding arches φ', φ'', φ''', and ſubſtituted in the formula $z = \theta P(1 - eQ) + eR$, rejecting all the factors of P, and all the terms of the ſeries Q and R, which involve the quantity e^{IV}, &c. the part of the formula thus rejected will be multiplied by the 32d power of the excentricity; but if the terms involving e^{IV} be taken in, the terms rejected will be multiplied by the 64th power, and ſo on; thus it appears, that the method here given for the rectification of the ellipſe is applicable to every caſe of excentricity, and to every length of an arch that can poſſibly occur in calculation. Now theſe are advantages which no other individual ſeries, with which I am acquainted, poſſeſſes; for the common ſeries, which converges by the powers of the excentricity, is of no uſe when the excentricity is great, and it is then neceſſary to have recourſe to a ſeries converging by the powers of the leſſer axis. But the ſeries of this form, which is commonly known, converges only for a certain portion of the elliptic arch, and beyond that portion it diverges; ſo that if it be required to rectify an elliptic arch beyond a certain limit, it is neceſſary to find the length of the whole quadrant, and thence, by the aid of FAGNANI's theorem, to find the length of the arch which was propoſed.

19. I SHALL

19. I SHALL conclude this appendix with an example of the application of our formula; and in the determination of the logarithms of e', e'', &c. $1+e'$, $1+e''$, &c. by means of the trigonometrical tables, I ſhall avail myſelf of the remarks which have been made for that purpoſe at Art. 15. of the preceding paper. As to the quantities ſin $2\phi'$, ſin $4\phi''$, &c. it is eaſy to ſee, from their relation to each other, that their logarithms may be readily determined by means of the ſame tables.

EXAMPLE. It is required to determine the length of an elliptic arch, when e, the excentricity, $=\sqrt{\frac{1}{2}} = .7071068$, and ϕ, the arch of the circumſcribing circle $= 70°$, the ſemi-tranſverſe being ſuppoſed $= 1$.

1. CALCULATION of the logarithms of e', $1+e'$, &c.

		Logarithms.
$e =$	ſin $\alpha =$ ſin $45°, 0', 0''$,	$\bar{1}.8494850$
$e' = \tan^2\frac{\alpha}{2} =$ ſin $\alpha' =$ ſin	$9°, 52', 45''.41.$	$\bar{1}.2344486$
$1+e' =$ ſec $^2\frac{\alpha}{2}$	- - - - -	0.0687694
$e'' = \tan^2\frac{\alpha'}{2} =$ ſin $\alpha'' =$ ſin	$0°, 25'. 40''.7$	$\bar{3}.8733008$
$1+e'' =$ ſec $^2\frac{\alpha'}{2}$	- - - - -	0.0032320
$e''' = \tan^2\frac{\alpha''}{2}$	- - - - -	$\bar{5}.1444734$
$1+e''' =$ ſec $^2\frac{\alpha''}{2}$	- - - - -	0.0000060

BECAUSE e^{iv} is nearly equal to one-fourth of the ſquare of e''', (Art. 7.) it is evident that e^{iv}, $1+e^{\text{iv}}$, &c. may be all neglected.

2. CALCULATION

2. CALCULATION of the logarithms of fin $2\varphi'$, fin $4\varphi''$, &c.

	Logarithms.
$\text{fin}\,\varphi$	$\bar{1}.9729858$
e	$\bar{1}.8494850$
$e\,\text{fin}\,\varphi = \text{fin}\,\beta$	$\bar{1}.8224708$
$\sqrt{1 - e^2\,\text{fin}^2\varphi} = \text{cof}\,\beta$	$\bar{1}.8735073$
$1 + e'$	0.0687694
$(1+e')\sqrt{1 - e^2\,\text{fin}^2\varphi}$	$\bar{1}.9422767$
$\text{fin}\,2\varphi$	$\bar{1}.8080675$
$\text{fin}\,2\varphi' = \text{fin}\,132°, 45', 49''.3$	$\bar{1}.8657908$
e'	$\bar{1}.2344486$
$e'\,\text{fin}\,2\varphi' = \text{fin}\,\beta'$	$\bar{1}.1002394$
$\sqrt{1 - e'^2\,\text{fin}^2 2\varphi'} = \text{cof}\,\beta'$	$\bar{1}.9965270$
$1 + e''$	0.0032320
$(1+e'')\sqrt{1 - e'^2\,\text{fin}^2 2\varphi'}$	$\bar{1}.9997590$
$\text{fin}\,4\varphi'$	$\bar{1}.9986754$
$\text{fin}\,4\varphi'' = \text{fin}\,265°, 57'. 15''.$	$\bar{1}.9989164$
e''	$\bar{3}.8733008$
$e''\,\text{fin}\,4\varphi''\ \ \text{fin}\,\beta''$	$\bar{3}.8722172$
$\sqrt{1 - e''^2\,\text{fin}^2 4\varphi''} = \text{cof}\,\beta''$	$\bar{1}.9999879$
$1 + e'''$	0.0000060
$(1+e''')\sqrt{1 - e''^2\,\text{fin}^2 4\varphi''}$	$\bar{1}.9999939$
$\text{fin}\,8\varphi''$	$\bar{1}.1484707$
$\text{fin}\,8\varphi''' = \text{fin}\,531°, 54'. 30''.$	$\bar{1}.1484768$

the

the next term, fin $16\varphi^{\text{iv}}$, being multiplied by e^{iv}, a very fmall fraction, is on that account to be neglected, as well as all the terms which follow it, for fimilar reafons.

To determine θ, we have $\varphi' = 66°.\ 22'.\ 54''$.

$$\varphi'' = 66°.\ 29'.\ 18''.75$$
$$\varphi''' = 66°.\ 29'.\ 18''.75$$
&c.

Hence it appears that $\theta = 66°.\ 29'.\ 18''.75 = 1.1604440$.

3. Computation of $\theta P(1 - eQ)$.

		Logarithms.
$\frac{e^2}{2}$	$= .2500000$	$\bar{1}.3979400$
$\frac{e^2 e'}{2.2}$	$= .0214466$	$\bar{2}.3313586$
$\frac{e^2 e' e''}{2.2.2}$	$= .0000801$	$\bar{5}.9036294$
$\frac{e^2 e' e'' e'''}{2.2.2.2}$	$= .0000000$	$\overline{10}.7470728$
eQ	$= .2715267$	
	$1.$	
$1 - eQ$	$= .7284733$	$\bar{1}.8624137$
$1 + e'$	- -	0.0687694
$1 + e''$	- -	0.0032320
$1 + e'''$	- -	0.0000060
θ	- -	0.0646242
$\theta P(1 - eQ)$	$= .9978042$	$\bar{1}.9990453$

4. Computation

4. Computation of eR, and thence of z the arch required.

$$
\begin{array}{rll}
 & & \text{Logarithms.} \\
\frac{e^2(1+e)}{2\,.\,2}\,\text{fin}\,2\varphi' = & +.1075153 & \bar{1}.0314702 \\
\frac{e^2 e'(1+e')(1+e'')}{2.2\,.\,2\,.\,2}\,\text{fin}\,4\varphi'' = & -.0063127 & \bar{3}.8002164 \\
\frac{e^2 e' e''(1+e')(1+e'')(1+e''')}{2.2.2.\,.\,2\,.\,2\,.\,2}\,\text{fin}\,8\varphi'' = & +.0000017 & \bar{6}.2210236 \\
\hline
e\text{R} = & .1012043 & \\
\theta\text{P}(1 - e\text{Q}) = & .9978042 & \\
\hline
\text{The elliptic arch } z = & 1.0990085 &
\end{array}
$$

It appears from the preceding calculation, that the length of the elliptic arc would in this cafe have been found with nearly the fame degree of accuracy, if inftead of computing the three quantities e', e'', e''', alfo fin $2\varphi'$, fin $4\varphi''$, and fin $8\varphi'''$, we had only computed two of each feries, viz. e' and e'' of the one, and fin $2\varphi'$, fin $4\varphi''$, of the other feries; or, fuppofing all the three quantities of each feries to have been computed to a fufficient number of places of figures, it is evident that we might thence have found the length of the arch to feveral more places of figures than we have here made ufe of.

XII. Chemical Analysis *of an* Uncommon Species *of* Zeolite. *By Robert Kennedy,* M. D., F. R. S., F. A. S., *and Fellow of the Royal College of Physicians, Edinburgh.*

[*Read Jan.* 11. 1802.]

THE zeolite ſubjected to the following experiments, poſſeſſes ſome of the diſtinguiſhing properties common to other ſtones of the ſame claſs, but differs in certain reſpects from any variety with which I am acquainted. I found it, more than three years ago, in the baſaltic rock on which the Caſtle of Edinburgh is built; and it was incloſed within a maſs of prehnite.

The colour of this zeolite is in ſome parts nearly white, in others greyiſh white. It is compoſed entirely of ſtraight fibres, arranged in bundles or maſſes of different ſizes, all the fibres of each maſs converging towards a common point. The whole ſpecimen is an aggregate of theſe maſſes, the baſes of which are in contact with the prehnite, and are impreſſed with the form of its ſurface, which is rounded, or botryoidal. The croſs fracture preſents the irregular and ragged ends of the broken fibres, termed the *hackly* fracture by Mr Kirwan.

Although the ſhape and arrangement of the fibres appear plainly to be the effects of cryſtallization, yet I have not been able to trace a perfectly regular, determinate form in any of them. However, when the ſtone is broken, ſome of them can be readily ſeparated longitudinally, in a pretty entire ſtate, and ſeem in moſt

 inſtances

inftances to be four-fided and rectangular. Their length is from half an inch to two inches; but their thicknefs does not exceed one-fortieth or one-fiftieth part of an inch. None of thefe fibres can be broken acrofs, fo as to prefent an even furface; for they break irregularly, and become divided at the point of fracture into very minute fpiculæ, which alfo affume a fomewhat rectangular fhape.

The fmall fpiculæ or fibres are tranfparent and colourlefs, with a confiderable degree of luftre; but the unbroken part of the ftone poffeffes lefs luftre than the feparate fpiculæ, and much lefs tranfparency, from a want of compactnefs, and from the effect of many minute cracks.

Its hardnefs is not eafily determined, on account of brittlenefs; but when a piece of it is rubbed againft glafs, though the fibres crumble down very quickly, yet the glafs is flightly fcratched at the fame time. Small fragments of it can be broken with the fingers, or crufhed by preffure, into very flender fpiculæ, which are fharp, and apt to penetrate the hands when touched. Although the cohefion of its component parts be fo weak, yet it bends, and yields in fome meafure before it breaks, and is not eafily ground to powder in a mortar.

I found the fpecific gravity of different pieces of the fpecimen, taken in diftilled water at the temperature of 60°, to vary from 2.643 to 2.740.

This ftone has the property of appearing luminous in a dark place, both by friction and by heat. A very flight degree of friction produces this effect; for a perfon can eafily diftinguifh a phofphoric light, even if he draws his finger acrofs it. When ftruck with a hammer in fuch a manner that fmall fragments ar driven off, they appear luminous in paffing through the air, and continue to fhine for a moment after falling on the ground; and

and a hard body drawn over it leaves a track of light, which remains a ſecond or two viſible. When a piece of the ſtone is pounded quickly in a mortar, a ſtrong light is emitted; but after being wholly reduced to powder, it no longer ſhines.

THE light which it gives by ſlight friction, is fully equal to that produced by two quartz pebbles, rubbed or ſtruck againſt each other ſtrongly.

SMALL fragments of this zeolite placed on a piece of hot iron, or of clay, alſo become luminous, and ſhine with nearly as much brightneſs as common blue fluor does, when heated in the ſame manner. By being made red-hot, however, it is deprived of the property of giving light afterwards by heat, though it ſtill appears faintly luminous by friction. In all theſe experiments it is a reddiſh white light which it emits, accompanied at times with reddiſh momentary kind of flaſhes.

IT can be melted without difficulty into glaſs. When a ſmall piece of it is heated gradually with the blowpipe, it firſt becomes white and opaque; a number of fiſſures are then formed, by which its bulk appears ſomewhat increaſed; and after the flame is made ſufficiently intenſe, it melts into a globule of colourleſs glaſs, the tranſparency of which is imperfect on account of many minute air-bubbles. Although thus fuſible by ſtrong heat, yet a very low degree of ignition ſcarcely affects it. A piece of the ſtone, after having been put into a crucible, and expoſed for a ſhort time to a fire which made it juſt viſibly red-hot, was not altered in appearance, and had not loſt more than $\frac{1}{200}$ part of its weight. In a temperature, however, about 20° of WEDGWOOD, I found that a fragment of this zeolite became opake, and more eaſy to pulverize than in its natural ſtate; at 90° it did not melt, but was glazed on the ſurface, adhered to the crucible, and had begun to ſink down; and at 120° melted into an imperfect glaſs of a light greeniſh yellow colour,

deficient

deficient in tranſparency, which acted ſtrongly on the crucible.

In temperatures ſufficiently high, it loſes from 4½ to 5 *per cent.* of its weight; but to produce this effect, the heat muſt be equal at leaſt to 20° or 25° of Wedgwood. Part of the volatile matter thus driven off is carbonic acid; for the zeolite, when reduced to powder, and mixed with acids, produces a ſlight effervefcence. I found, by three different experiments, that the loſs of weight after the effervefcence was about 3 *per cent.*; conſequently, the remaining 2, or 2½ parts, may be preſumed to conſiſt of moiſture.

This ſubſtance produces a jelly, as moſt other zeolites do, with the ſtronger acids. When ground to powder, and mixed with the ſulphuric, nitric, or muriatic acid, the mixture becomes a firm jelly in a few minutes, provided the acids are not much diluted, or uſed in too great quantity. That which is formed by the action of the nitric or the muriatic acid, is nearly tranſparent; but the ſtone contains ſo much lime, as will preſently be ſhown, that with the ſulphuric, the jelly is white and opake, on account of the ſulphat of lime which is generated.

The prehnite in which it was incloſed, as already mentioned, is of a light green colour, with ſome luſtre, and a conſiderable degree of tranſparency. It gives fire with ſteel, can be readily melted with the blowpipe, and froaths up much before fuſion.

Having made theſe preliminary experiments reſpecting the general properties of the zeolite, I ſubjected a portion of it in the next place to analyſis, to aſcertain of what earths it was compoſed, and found that it conſiſted almoſt wholly of ſilex and lime, with a certain proportion of ſoda. The following were the methods by which it was analyzed.

1. One hundred grains, reduced to fine powder in a mortar of flint, were mixed with 500 grains of muriatic acid, and 1000 grains

grains of diſtilled water. The powder was immediately attacked by the acid; ſome heat, and momentary effervefcence were produced, and in a few minutes the ſilex appeared floating in a looſe, and very divided ſtate. The mixture being heated on a ſand-bath, became in a ſhort time a thin tranſparent jelly. I expoſed this jelly to a gentle heat (leſs than 200° of FAHRENHEIT), till it became nearly dry. Water was then poured on it, and the mixture, after being digeſted for ſome time, was filtered. The undiſſolved part collected on the filter, when ſufficiently waſhed, was dried and heated red-hot for a quarter of an hour. It weighed 51½ grains, was perfectly white, and proved on examination to be pure ſilex, unmixed with any other earth.

2. THE filtered ſolution, which was tranſparent and nearly colourleſs, after being ſaturated with cauſtic ammonia, depoſited a ſmall quantity of a browniſh white precipitate. Having collected this precipitate on a filter, and waſhed it, I expoſed it for a few minutes to a red heat. It then weighed one grain, and had become brown, and was found to conſiſt of half a grain of oxyd of iron, and half a grain of argil.

3. IN the next place I added a few drops of ſulphuric acid to the ſolution, to diſcover if it contained any barytes or ſtrontian. The acid produced no precipitate. The ſolution was then evaporated to a ſmall quantity, and boiled with carbonate of ammonia. Some carbonate of lime was thrown down, which, after being waſhed and expoſed for a few minutes to a very low red heat, weighed 57 grains. To drive off the carbonic acid, and aſcertain the real quantity of lime which this precipitate contained, I put it into a ſmall crucible of Corniſh clay, which was incloſed within a Heſſian crucible, and heated it for three hours. The fire was raiſed ſlowly at firſt, but was afterwards increaſed to 80° of WEDGWOOD. The lime adhered

ſlightly

ſlightly, and weighed $31\frac{1}{2}$ grains. By treating it with ſulphuric acid, I detected ſome ſlight traces of magneſia, the reſt being wholly converted into ſulphate of lime.

4. The different earths having been thus ſeparated from the ſolution, it was evaporated to dryneſs, for the purpoſe of obtaining the ſoda, which I had learned by previous experiments would be wholly taken up with the other ſoluble parts of the zeolite, when decompoſed by acids. After the evaporation, a white ſalt remained, which was heated gradually in a crucible to volatilize the muriate of ammonia, formed in the courſe of the analyſis. When white vapours ceaſed to riſe, the heat was increaſed to redneſs, and a ſalt was left which weighed 17 grains. On re-diſſolving it in a ſmall quantity of water, and boiling the ſolution with carbonate of ammonia, a minute portion of carbonate of lime was precipitated, which weighed one grain; equal to about half a grain of pure lime. I obtained the ſalt by a ſecond evaporation; and after it had been expoſed again to a very low red heat, it weighed 16 grains, and conſiſted wholly of muriate of ſoda.

According to Mr Kirwan's experiments, 16 parts of muriate of ſoda in cryſtals, contain 8.5 parts of ſoda; but the 16 parts above mentioned, by having been dried in a red heat, and conſequently freed from water of cryſtallization, would contain ſomewhat more than 8.5 parts of ſoda. However, the proportion of ſoda in 100 parts of the zeolite, may be ſtated at 8.5 parts, which, though probably rather leſs than the real quantity, muſt be very nearly correct.

Having finiſhed the experiments juſt deſcribed, I analyzed the zeolite a ſecond time; and made uſe of nitric acid, for the purpoſe of aſcertaining whether any muriatic acid entered into its compoſition. By the teſt of nitrate of ſilver, I found that ſome

ſome traces of the muriatic acid could be diſtinguiſhed, although the quantity was very ſmall. With regard to the proportion of the earths, the reſults of the ſecond analyſis correſponded almoſt exactly with the former.

I ALSO expoſed ſome of the zeolite to the action of the ſulphuric acid, in the following manner, with the view of obtaining the ſoda only, the earths being diſregarded. One hundred grains *, reduced to fine powder in the flint mortar, were mixed with 250 grains of ſulphuric acid, diluted with twice its weight of water. Some heat and very ſlight efferveſcence were produced, and the mixture ſoon became thick and gelatinous. It was then evaporated ſlowly to dryneſs in a ſand-bath, in a cup of Chineſe porcelain; and the dry maſs was pulverized and boiled for half an hour with water, and filtered. Having waſhed the undiſſolved reſiduum ſufficiently, I boiled the filtered ſolution with carbonate of ammonia, which precipitated ſome earthy matter. After this had been ſeparated by filtration, the ſolution was evaporated to dryneſs by a gentle heat; and the ſaline maſs left was put into a crucible, and heated ſlowly to redneſs. A white ſalt remained in the crucible, which weighed 19 grains. To free this ſalt from any remains of earthy ſalts which might be mixed with it, I diſſolved it in a ſmall quantity of water, added ſome carbonate of ammonia, and boiled the mixture for a few minutes, by which means a ſlight earthy precipitate was thrown down. This being ſeparated as before, the ſalt was again collected by evaporation, and heated to redneſs. It now weighed

* I MADE uſe of ſmall quantities only of the zeolite in all theſe experiments; becauſe, as the ſpecimen appears to be the only one of the kind which has been found, I wiſhed to preſerve as much of it entire as poſſible.

weighed $17\frac{3}{4}$ grains, and was found on examination to be pure ſulphate of ſoda *.

By Mr Kirwan's eſtimation, $17\frac{3}{4}$ parts of dry ſulphate of ſoda contain nearly 8 parts of alkali; conſequently, from 100 parts of the zeolite there have been obtained by the proceſs laſt deſcribed, 8 parts of ſoda. This reſult correſponds with the former, in which the alkali was collected in the ſtate of muriate of ſoda, as nearly as can be expected in ſuch experiments; and it muſt be remembered, that the proportions of the component parts of neutral ſalts are not aſcertained with preciſion.

According to the different experiments now detailed, 100 parts of this zeolite contain,

Silex (No. 1.) - -	51.5
Lime (No. 3. and 4.) -	32
Argil (No. 2.) - -	.5
Oxyd of iron (No. 2.) - -	.5
Soda, about - - -	8.5
Carbonic acid and other volatile matter,	5.
	98.

with ſome traces of magneſia and muriatic acid.

The

* The experiments which ſhewed that ſoda was the alkaline baſis of this ſalt, have not been ſtated here; becauſe I formerly gave a deſcription of the methods uſed in examining the ſame ſalt, in the paper on Whinſtone and Lava, publiſhed in Part I. of this volume, (p. 85.). To avoid unneceſſary repetition, I beg leave to refer to that paper, both with regard to the manner in which I endeavoured to determine the purity of the ſaline matter, and alſo that of the ſilex, and ſome other earths.

THE ſtone which has now been deſcribed reſembles ſome of the varieties of Tremolite mentioned by SAUSSURE *, in the property of giving a phoſphoric light by friction. Its ſpecific gravity alſo is ſomewhat greater than that of the ordinary kinds of zeolite, as ſtated by mineralogiſts. Excepting in theſe particulars, however, it has the principal characters of a zeolite; for example, in its internal compoſition, in having been found in a whin rock adhering to prehnite, and in producing a jelly with acids. Tremolites have a higher ſpecific gravity than this ſtone, are more infuſible, and are conſiderably different in their compoſition †. Beſides, ſuch kinds of tremolite as I have examined cannot be decompoſed by acids, even when boiling, and muſt be heated with potaſh or ſoda before their component parts can be ſeparated; but the ſubſtance in queſtion is completely decompoſed by acids, like the greater number of zeolites, in a very few minutes, and without the aſſiſtance of heat. For theſe reaſons, it appears to me to be a zeolite.

* Voyages dans les Alpes, 1923.

† KIRWAN's Mineralogy, vol. I. p. 278. Traité de Mineralogie par HAUY, tom. 3. p. 151 and 227.

XIII. Disquisitions *on the* Origin *and* Radical Sense *of the* Greek Prepositions. *By JAMES BONAR*, F. R. S. Edin.

[*Read Dec.* 19. 1803, *Jan.* 16. & *Feb.* 20. 1804.]

ETymological refearches have often been branded as trifling, occupying the mind in ufelefs labour, and contributing little to the progrefs of fcience, tafte, or beneficial knowledge. The juftice of this treatment may well be queftioned. That this fpecies of ftudy, like many others, has frequently been directed to unimportant objects, cannot be denied ; but it by no means follows, that this is the fault of the fubject, or that from fuch refearches, however purfued, nothing valuable can be the refult. If by etymological inveftigations, even in their moft limited form, effential aids are obtained for facilitating our acquaintance with the writings of antiquity,—if fuch inquiries have often proved one of the moft certain means for attaining accuracy of language and precifion of ideas,—if by following them out on an extenfive fcale, affiftance might probably be gained for elucidating fome of the obfcurities of early hiftory, —and if from this fource valuable materials may at all times be drawn for tracing the progrefs of the intellectual powers, and unfolding the laws of thought through the phenomena of its expreffions,—if thefe, and perhaps other merits, muft be allowed to etymological and grammatical difquifitions, they are furely fufficient to refcue from contempt this clafs of inquiries. If to all this we can add, what I believe will pretty generally be found to hold true, that thoſe who affect moſt to deſpiſe this branch of ſtudy, have ſeldom any thing better to ſubſtitute in its place, a ſufficient vindication will, I hope, be afforded, for employing a little literary leiſure in the culture even of this apparently unpromiſing field.

These conſiderations may, I truſt, then, plead my apology for taking up the attention of the Society at this time with ſome etymological hints and conjectures,—not, I own, directed towards the more extenſive tracts in which profounder ſcholars often wiſh to range, but confined within the narrow limits of a particular branch of a particular language,—a language, however, juſtly admired for copiouſneſs and elegance, and a branch of it which at different times has been thought worthy the attention of critics of no inferior note.

By the ingenious and elaborate reſearches of recent etymologiſts, the ſtructure of language has been made to aſſume a ſimpler dreſs than what grammarians had once thought proper to inveſt it with. The *parts of ſpeech*, as they are technically termed, have been reduced from their numerous claſſes to two; the connecting particles, uſually termed and ſuppoſed indeclinable, have been completely excluded from their ſeparate rank; and by tracing out the genealogical relations of words, all have been fairly ſhown to belong to one or other of the claſſes of Nouns or Verbs *.

The

* Even this claſſification of words might be ſtill farther ſimplified, and either the noun or the verb ſhewn to be the root of all language. Etymologiſts have differed

The germ of this theory may be found in the early Engliſh etymologiſts Junius and Skinner; but with them it was carried no farther than the analyſis of certain individual words, without being advanced to any ſpecies of complete and general ſyſtem. A few of the French grammarians ſeem to have made ſome approaches to the ſame ſyſtem; but, it muſt be owned, that it is to the Dutch etymologiſts Schultens, Ten Kate, and above all, Hemsterhuis, and the diſciples of the Hemſterhuſian ſchool, Valknær, Linnep and Scheide, that we owe the firſt complete developement of this new and ſubtle theory of language. The labours of the Dutch etymologiſts, however, for a long time obtained but little notice: even the ſyſtem they had thus ingeniouſly conſtructed, remained almoſt unknown beyond the immediate limits of their own ſchool. In France, Ance de Villoisin appears to have had ſome imperfect acquaintance with it. In this country, I do not know if it had been heard of at all, till the ſame theory was exhibited in a no leſs conſpicuous than amuſing form, by the author of the *Diverſions of Purley*. When, in that lively work, the ſyſtem was laid down, and at the ſame time ſucceſsfully applied to the analyſis of many of the Engliſh particles, our grammarians and etymologiſts at length began to queſtion the old ſyſtem of grammatical arrangement in the claſſification of words. The new theory gradually gained ground, and now the truth of it ſeems to be almoſt generally admitted, though much ſtill remains to be done in the way of its particular application *.

Satisfied

fered in opinion to which of the two that place is to be aſſigned. It is unneceſſary to enter into that queſtion at preſent, though I may afterwards take occaſion to point out ſome reaſons which induce me to think, that tracing language to its firſt elements, we ſhall probably find that it is from the Verb the whole has gradually branched out.

* Though I have thus ſtated, what, I think, cannot be diſputed, that the theory of Horne Tooke, and that of the Dutch etymologiſts, is eſſentially the ſame,

I

Satisfied of the truth of this ſyſtem, and perſuaded, that by following it out as far as poſſible, the beſt foundation will be laid for a juſt inveſtigation of the ſtructure of language, it occurred to me, that the general principles thence afforded, might be applied with advantage, as a guide for analyſing at leaſt one claſs of connective particles in a language which appears to offer peculiar facility in ſuch a reſearch. Should this be found to ſucceed in one language, it muſt facilitate ſimilar inveſtigations in others, and every fact eſtabliſhed in the progreſs of the inquiry, may be regarded as both illuſtrating and confirming the general ſyſtem.

The Greek language certainly poſſeſſes many advantages for ſuch an analyſis. It is a language of regular ſtructure; its roots formed within itſelf, in which, conſequently, the original ſtamina, with their ſubſequent ramifications, may, by accurate inveſtigation, be ſatisfactorily traced. The claſs of connectives in this language upon which this experimental inveſtigation is propoſed at preſent to be made, is the *Prepoſition*, a claſs of words of conſiderable importance in the ſtructure of every language, at the ſame time ſo cloſely connected with nouns, that their mutual relations may be marked with leſs difficulty than is found in analyſing ſome of the others.

Much,

I wiſh not to be underſtood as aſſerting that the one was borrowed from the other. Similar ideas may have occurred to both without any communication. The Dutch etymologiſts certainly claim the priority in point of time, but the praiſe of originality cannot upon that account be denied to either. Dr Beddoes, in a long note appended to his *Obſervations on the Nature of Demonſtrative Evidence*, has taken great pains to prove the originality of Horne Tooke. This point need not be diſputed; but if it was to be made a ſubject of controverſy, it was ſurely unneceſſary, in diſcuſſing it, to throw out, as the Doctor has done, a number of contemptuous, and by no means well-founded ſarcaſms, in depretiation of the labours of the Dutch etymologiſts, and diſparagement of claſſical literature in general. Obſervations of that nature are as little adapted to add force to an argument, as to do credit to the author.

Much, I know, has already been done towards facilitating ſuch an inveſtigation. The ſucceſſive labours, firſt of Dr Moor, after him of more recent grammarians, above all the elucidations of the learned and acute Profeſſor of Greek in the Univerſity of this City, have both opened the proper track of inquiry, and cleared away much of the rubbiſh, with which the jejune and trifling *minutiæ*, the diſtinctions without a difference and repetitions of examples without a connecting principle, introduced by Stephens, Hoogeveen and Vigerus, had blocked up the avenues to all ſuch diſquiſitions. The profounder reſearches of Linnep and Scheide have thrown much additional light upon many particulars before involved in obſcurity, and their etymological deductions, though in many inſtances imperfect, contain valuable materials for analyſing the real ſtructure and ramifications of the Greek language *. The track being once marked out, and the path in a great meaſure cleared, progreſs becomes comparatively eaſy; little more is neceſſary than to follow out the line where thoſe who preceded appear to have ſtopped ſhort too ſoon.

In analyſing the prepoſitions of any language, one general principle may be aſſumed as certain, That to every prepoſition [illegible] one

* The *Analogia Græca* of Linnep contains a general developement of the ſyſtem adopted in the Hemſterhuſian ſchool. This was followed by the *Etymologicon Linguæ Græcæ*, the joint production of Linnep and Scheide, in which the principles of this analogy are applied for analyſing all the primitives of the Greek tongue. The merit of both muſt be admitted; both will be found worthy the attention of any one who wiſhes to inveſtigate the ſubject of Greek analogy. From the latter work, in particular, I have derived much aſſiſtance in the preſent diſquiſition, though it has frequently proved leſs ſatisfactory than might have been expected. The analyſis of the words is too general and indefinite: many of the primitive roots are explained as merely denoting *motion;* an idea ſo vague, that unleſs limited in ſome ſpecific mode, it could afford little aid in our progreſs to a ſyſtematic analyſis of the principles of the language.

one primary radical idea was originally affixed,—that this idea was for the moſt part taken from ſenſible objects,—and that from this radical ſenſe all the ſecondary applications may either immediately or circuitouſly be traced. In the inveſtigation of this radical ſenſe of each of the prepoſitions, I think it will likewiſe appear, that all of them were originally either nouns or participles, moſt of them verbal adjectives, at firſt uſually joined with ſome common ſubſtantive to complete the ſenſe; which ſubſtantive, by uſe, came at length to be dropped, as unneceſſary to be expreſſed, being immediately implied and underſtood. From this it will likewiſe follow, that in the junction of theſe words with other nouns, the primitive rule of conſtruction by which they were joined in ſentences, was that which is termed the *genitive or ablative abſolute*; a conſtruction of ſentences, which though ſtigmatized, and perhaps not unjuſtly, by Lord Monboddo, as lame and gaping, yet was probably of extenſive uſe in the early ſtages of human ſpeech, when bare co-exiſtence of phenomena or events, (the preciſe idea denoted by this mode of conſtruction), was more attended to than that mutual relation and dependence, the gradual diſcovery of which afterwards gave riſe to more compact and connected forms of expreſſion.

Proceeding upon theſe principles, we may poſſibly be able to diſcover the radical ſenſe of each prepoſition, as well as ſome of the leading circumſtances by which, in time, many of them came to be applied, to denote ideas apparently remote from their original meaning. The minuter ramifications to which theſe ſignifications afterwards branched out, it would lead far beyond the bounds of a ſingle paper to attempt inveſtigating.

In conducting ſuch an analyſis, each prepoſition muſt be ſeparately examined,—a minute and tedious proceſs, perhaps, but the only one by which the object can be fairly accompliſhed. No particular arrangement being at all neceſſary for our purpoſe,

I

I ſhall be ſatisfied to take them as they occur in the order of the alphabet.

Αμφὶ.

The near relation which this prepoſition bears to the adjective ἄμφω, *both*, and the adverb ἄμφὶς, *on both ſides*, evidently points out an identity of origin as well as affinity of meaning. All three appear to be immediately derived from the obſolete verb ἄμω, to *embrace* or *graſp* *. From ἄμω it is probable came the verbal adjectives ἀμος and ἀμις, ſignifying *graſped* or *embraced*. From many examples in the more ancient Greek writers, it appears to have been a frequent practice either to add to words the termination φι, or ſimply to inſert the letter φ before the termination. Of this laſt, the inſtances are not unfrequent in Homer and Hesiod, as ὄχεσφι for ὄχεσι, μέσφα for what appears to have been originally μέσα, and ἔννηφι for what I ſhould ſuſpect had been originally ἔννηι or ἔννη †. By a ſimilar inſertion, ἀμος and ἀμις became ἄμφος and ἄμφις, both of them ſignifying, as before, *graſped* or *embraced*. Αμφω, the dual number of ἄμφος, thus exactly expreſſed *two objects, graſped, embraced or united*, the preciſe idea denoted by the term *both*. To ἄμφις, expreſſing *graſped*, ſome common word denoting *place, object*, or the like, being at firſt uſually joined, and afterwards in common uſe omitted, as being univerſally underſtood, ἄμφις and its oblique caſe ἀμφὶ came to ſignify literally *place or object graſped, embraced, or comprehended.*

* The root of the common Greek verb ἀμάω, *to reap*, (literally to *gather together* the fruits of the earth), and of the Latin verb *amo*, to love, originally denoting to *graſp at* or ſtrive to obtain.

† Perhaps the φ thus inſerted was originally the Æolic digamma; a letter common in the moſt ancient Greek writings, and it ſeems probable, pronounced not unlike the φ.

Hence the different applications of ἀμφὶ are eaſily deducible.

1. *About or round a place or object.*

Ἀμφὶ πόλιος οἰκέουσι, (Herodot.) "they dwell—place graſped "or comprehended by their dwellings—the city,"—"round "about the city."

——— ἀμφὶ δέ ποσσὶν ἔχε πτερόεντα πέδιλα, (Hesiod.) "and "had the winged ſandals,—object graſped by them—his feet," "—round his feet."

——— ἀμφὶ δ' ἄρ' ὤμοισιν βάλετο ξίφος. Homer.

"And threw the ſword,—place graſped or embraced by the "ſword-belt,—his ſhoulders,"—"round his ſhoulders."

Ὡς δ' ὅτ' εν ουρανῷ ἄστρα φαεινὴν ἀμφὶ σελήνην
Φαίνετ' ἀριπρεπέα. Homer.

"As in heaven the ſplendid ſtars appear,—object included or "comprehended in their line of appearance,—the refulgent "moon,"—"round the refulgent moon."

Sometimes, inſtead of directly *round about*, ἀμφὶ, by an elliptical mode of phraſeology, expreſſed only *near about*;

———— ἤριπε δ' ἀμφ' αυτῷ,

"he fell—place of falling—the ſpace including or comprehend-"ing him,"—"near about him, or cloſe beſide him."

2. *Among.*

———— ἀμφὶ σφίσι πένθος ὀρώρει. Quint. Smyrn.

"ſorrow aroſe—objects included in its ſphere of action,—ob-"jects embraced in it—them,"—"among them."

3. *Upon.*

—— κάββαλεν ἄνδρα κατα χθόνός, ἀμφὶ δ' ἄρ αὐτῷ
Εζετω. Quint. Smyrn.

"He threw down the man to the ground, and ſat,—object "graſped by his ſitting—him,"—"upon him."

4. Figuratively—*about* or *concerning.*

Ἀμφὶ ἄστρων γραφὴ. Lucian.

"a treatiſe—ſubject comprehended—the ſtars,"—"concerning "the ſtars."

And

And immediately related to this meaning,—FOR or ON ACCOUNT OF.

——— ἱππῆες ἔχον πόνον, ἀμφὶ δ' ἀέθλοις
Δῆριν ἔχον καὶ μόχθον. HESIOD.

"The horsemen were labouring and in earnest contention—object grasped in their contention—the prizes;"—"object in which their contention centred—the prizes."

Ὡς δὲ λέοντε δύω ἀμφὶ κταμένης ἐλάφοιο
Ἀλλήλοις κοτέοντε. HESIOD.

"As two lions irritated at each other—object grasped in their irritation—object in which their irritation centered—a slain hind."

In every one of these, as well as in all its various applications, we find ἀμφὶ, a noun, assuming, from its use, the character of what is termed a preposition.

Αμφὶ, so far as I can find, is a preposition either not at all or very seldom to be met with in the writings of the Greek geometers, περί, being the word generally made use of by them for *about*. The reason I take to be, that this science, relating chiefly to lines and surfaces, ἀμφὶ, which did not immediately, but only indirectly, denote these, was less proper for their purpose than περὶ, which, as we shall afterwards see, literally and radically meant *a boundary*.

This preposition ἀμφὶ has not been naturalized in Latin *; instead of it *circa* and *circum* came into use, evidently the feminine and neuter genders of the old adjective *circus*, the masculine of which we find in common use as a substantive noun, denoting a *round body*, the primitive of *circulus* a circle. *Circa* and *circum*, therefore, properly denote *a line going round a place or object*.

 Here,

* Except in the inseparable preposition *am*, denoting *about*, a mutilation of ἀμφὶ, or rather of the original word before the φ was inserted.

Here, as in many other inftances, we find an example of an idea expreffed with equal propriety either by a noun or its correlative. The Greeks, in ufing ἀμφὶ, kept in view that an object was included; the Latins, in ufing *circa* and *circum*, and the Greeks themfelves, too, in ufing περὶ, had in their eye that there was a boundary containing.

Ανὰ.

Ανὰ is one of thofe prepofitions which, partly from the obfcurity of its primitive root, and partly from its varied and fomewhat anomalous mode of application, prefents confiderable difficulty in the inveftigation of its primary and radical fenfe. LINNEP fuppofed it to mean *preffure upward;* an idea which it certainly does very frequently denote, but into which all the others cannot be fairly refolved. In Profeffor DALZEL's *Fragmenta*, the proper fignification of it is faid to be *per*, *through*, in which opinion STEPHENS coincides. But to this idea, though fo refpectably fupported, I cannot altogether accede. *Through* is undoubtedly one, and a very common meaning of ανὰ, but, like the former, can only be ranked as one of its fecondary applications. Dr MOOR, I apprehend, has come neareft the true radical fignification of ἀνὰ; he explained it as expreffing "*the* "*lines of direction traced backwards ;*" a fenfe not very remote from what appears to be the original one.

Ανα, in the immediate form of a noun, does not, I believe, occur in the Greek writers now extant; but from its cognates and derivatives, it is not difficult to difcover the traces of its former meaning. Ανὰ, as an adverb, is common, expreffing *a circumftance far back,—removed backwards* from us, whether in place or time: αν occurs in compofition in a number of words, with the fenfe as it were of *contradiction* or *reverfal* of the main idea, as ἀνάνδρος, *unmanly*, ἀνόσιος, *impious*, ἀνόρατος, *invifible* : ἄνευ, a derivative from the fame root, generally carries a fimilar force of re-

verfing

verſing the idea denoted by the word with which it is connected.

In all theſe different cognates, we find the idea of *reverſing* or *tracing backward* implied; and APOLLONIUS SOPHISTA, in his *Lexicon Homericum,* mentions this as the primitive idea denoted by the prepoſition *ἀνὰ* *. Here, then, I think, we may fix the radical ſenſe of the word. *Ἀνὰ* appears evidently to be a noun, having the literal ſignification of *reverſed*, *turned back*, or *traced backwards;* and, if neceſſary, ſome common and obvious word, ſuch as *direction*, *poſition*, or the like, may be ſuppoſed to be implied and underſtood; but this will ſeldom be required †.

WE may, therefore, give the radical meaning of *ἀνὰ* as *back*, *backward*, *reverſed*, or *traced back*. Thus, in XENOPHON, *ἀνὰ ποταμὸν διεκομίσαν,* " they bore them,—the river being traced backwards in their bearing," *i. e.* bore them againſt the ſtream, or up the river.

FROM this radical meaning, the other applications will be found naturally to ariſe.

1. VERY frequently *ἀνὰ* ſignifies *up* or *upward*, becauſe *down* or *downward* being the natural and uſual direction which bodies take when left to themſelves, *up* is conſequently the natural direction reverſed or traced back, and therefore properly expreſſed by *ἀνα.*

Ὡς εἰπὼν ἀνὰ νηὸς ἔβην, ἐκέλευσα δ' ἑταίρους
Αὐτούς τ' ἀμβαίνειν, ἀνὰ τέ πρυμνήσια λῦσαι. HOMER.

" Having thus ſpoken, I aſcended the veſſel, and ordered my " companions

* *Ἀνὰ.* Κυρίως μὲν ἡ πρόθεσις, ἐν τούτοις
Ἀφρῷ μορμύροντα ἰδὼν, ἀνὰ τ' ἔδραμ' ὀπίσσω.
APOLLON. SOPHIST. ſub voce *Ἀνα.*

† PARKHURST's idea of the derivation of this prepoſition from a Hebrew word ſignifying *an anſwer*, coincides pretty nearly with the idea here ſtated. The Hebrew word for an anſwer may probably have originally denoted *a rebound.*

"companions to go up (or on board) and to unloose the "cables;"—I went—direction of my going—the vessel—in the order of position the reverse of what is usual with bodies, *i. e.* upwards; I ordered my companions to loose the cables—line in which they loosed them—tending upwards, or reversing the natural tendency of bodies.

——— ——— ὑψὸς' ἀείρας
Θῆκεν ἀνὰ μυρίκην. HOMER.

"Having raised them aloft, he placed them on a tamarisk,"—placed them—point of placing them—a tamarisk;—situation—the reverse of what is the usual tendency of bodies,—that is, *up* or *aloft*.

2. As the notion of a return or reversal always of necessity presupposes the idea of a prior movement or position to which this is opposed, ἀνὰ, from expressing only the second, came naturally to be extended to denote both together. Hence we often find it applied to express the idea of *backwards and forwards, up and down;* that is, course backward, proceeded by course forward, course upward proceeded by position downward, or, as justly and accurately stated in Mr DALZEL's *Fragmenta,* "motum "huc et illuc—itionem et reditionem—sursum et deorsum." Ἀνὰ τὰ ὄρη πλανᾶσθαι (XENOPH.) "to wander—course of wander-"ing—the mountains;—first forwards—then this reversed or "traced backwards,"—hence up and down the mountains,—*over* the mountains.

——— ——— μνησάσθε δὲ θούριδος ἀλκῆς
Νῆας ἀνὰ γλαφυράς. HOMER.

"Keep in mind your impetuous courage—by the hollow ships,"—going along the ships—backwards and forwards—first forwards,—then that course traced back again.

It

It is from this application that ἀνὰ has often come to be uſed for *through* or *over*,—through the mountains, or over the mountains, equivalent to up and down—backward and forward.

3. Ανὰ is joined with nouns of number, to denote *regular diſtribution*. Στρατιώτας ἐξίεναι κελεύει ἀνὰ πέντε, (Vigerus), "he orders "the ſoldiers to go out by fives,"—to go out five—then traced back again,—to go out till they came to five, and then begin or trace back the reckoning again. Κιννάμωμου καὶ νάρδου ἀνὰ οὐγκίαν μίαν, (Vigerus), "of cinnamon and nard, an ounce each,"—of cinnamon an ounce,—then trace back the ſame reckoning for the nard.

4. Sometimes, though but rarely, ανα is uſed to denote *according to*. Λαλεῖν ἀνὰ τὸν αυτὸν λόγον (Clemen. Alex.) "to ſpeak— "the ſame word traced backward,"—according to the ſame word.

Under theſe different heads, all the applications of ανὰ may, I ſhould think, either directly or indirectly be arranged. In all of them we are warranted in regarding it as a noun; and though it cannot be denied, that the varied application and obſcure origin of this prepoſition may ſometimes occaſion difficulty in marking its exact progreſs or ramification, this does not at all detract from the truth of the general propoſition. Here, as in many ſimilar caſes, the following obſervation of Linnep will be found to hold good. "It is not to be expected, that the analo- "gy of any language can be perfectly traced, from the want of "many primitive words, which, having gone into diſuſe, are not "now to be met with, as the moſt ancient compoſitions in the "language have probably periſhed. This defect is ſtill increa- "ſed by the careleſſneſs of lexicographers, who have not been "attentive even to preſerve the old words that were to be met "with at the time they wrote." Hence, under every root, we need not be ſurpriſed to find blanks in the analogy which no ingenuity or conjecture can now poſſibly ſupply.

Αντὶ.

Αντὶ.

Αντὶ is explained both by LINNEP and SCHEIDE, as one of the cafes of an obfolete noun αυς, in the genitive ἄντος, fignifying *front* or *face*. In this etymology they appear to be well founded; for though the noun ἀνς itfelf is fallen into difufe, yet we ftill find fufficient remains to indicate its former exiftence. Αντα, evidently its accufative cafe, is in frequent ufe as an adverb, to exprefs *before,—in prefence, in face of:* ἄντην, probably the accufative cafe of a cognate noun, is likewife ufed adverbially in the fame fenfe with ἄντα: the adjectives ἐναντίος and ἀντίος*, and the adverb αντικρυς, both of them immediate derivatives, exprefs alfo in *prefence*, in *front of:* the verb ἄντεσθαι is explained by HESYCHIUS as fynonymous with λιτανεύειν, to fupplicate, *i. e.* to *fall down before*, or in *front of*, in *prefence of*. To all this may be added, that the Latin prepofition *ante* (evidently the fame word radically with the Greek ἀντὶ †), uniformly expreffes *before*, equivalent to *in front;* and in the fame language we find a fubftantive noun *antæ*, (a plural from the fingular *anta*,) denoting the

* THAT ἀντίος fignifies literally *in front*, the following trait in the Spartan epigram, which defcribes the body of THRASYBULUS carried back from the battle, clearly afcertains:

> Ἑπτὰ πρὸς Αργείων τραύματα δειξάμενος·
> Δεικνὺς ἀντία πάντα.

"having received feven wounds,—fhowing them all *in front*."

† THE Latin language is well known to be derived from the Greek; but it fhould always be attended to, that it is the *Æolic* dialect of the Greek which muft be regarded as the immediate parent of the Latin. Hence many of the words tranfplanted from the Greek to the Latin are found to differ from the common dialect, but to refemble very clofely the correfponding word in the Æolic.

the poſts *ſet up before the entry*, that is, the *fronting part* of a houſe.

Front or *face*, then, appears to be the radical ſenſe of ἀντὶ.

Ιϛάμενος ἀντὶ θύρης, "ſet or placed—front the door,"—fronting the door—before the door.

Αντ' ἠελίοιο τετραμμένος, (Hesiod), "turned—front the ſun,"—fronting the ſun *.

Though *front* was the original and radical meaning of ἀντὶ, yet it is to be obſerved, that the immediate and direct uſe of the word, in this ſenſe, ſeems to have been in a great meaſure ſuperſeded by ſome other prepoſitions. It is in the ſecondary applications, chiefly, ariſing out of this primary idea, that we find ἀντὶ employed by the Greek writers. Theſe ſecondary applications are principally four,—*oppoſition, compariſon, preference* and *ſubſtitution.*

1. *Opposition*,—becauſe fronting or facing is both the natural attitude of direct hoſtility, and the moſt obvious mode of retarding or counteracting what we wiſh to prevent.

'Αντ' ἀνδρὸς ἴτω, (Homer), "go—object fronting or meeting you in face—the man,"—againſt the man;" ἀντιλέγω τοῦτον λόγον, "I contradict that ſpeech,"—"I ſpeak in face of that "ſpeech."

2. *Comparison*,—becauſe placing objects ſo as to make them front or face each other, is the moſt common and ready mode of enabling us to compare and contraſt them. 'Αργυριόν ἀντὶ βοῶν, "ſilver compared with oxen,"—ſilver—object put in front or contraſted—oxen. Βασιλεὺς ἀντὶ μυρίων ἐϛι ϛρατιωτῶν,—"a king "is comparable to ten thouſand ſoldiers,"—"a king is—object "ſet in front to be compared—ten thouſand ſoldiers."

* In this phraſe, which occurs, 'Εργα καὶ 'Ημέραι (line 725) it is plain that αντι can mean nothing but *in front.*

3. *Preference*,—becaufe what is moft valuable is conceived and fpoken of as occupying the front or foremoft place. Αντὶ χρήματων ἑλέσθαι τήν δόξην χρῆ, (Isocr.) "glory fhould be taken in "preference to riches,"—glory fhould be taken as an object, holding the place in front of, or in a more honourable rank,—than riches. Ὁν ἐγώ φημί ἀντὶ παντῶν τῶν ἐνθάδε ἀγῶνων εἶναι, (Pla-"to), which I aver to be the beft of all contefts,"—which I aver to be a conteft—holding the front or firft rank of all contefts.

—————— —————— κάλλος,
Αντ' ἀσπίδων ἁπασῶν,
Αντ' ἐγχέων ἁπάντων. Anacr.

"Beauty—preferable to all fhields—preferable to all lances,"—"beauty—holding a rank in front of, or before all fhields and "all lances *."

4. *Substitution*,—for the reafon juftly ftated in Mr Dalzel's *Fragmenta*; "verifimillimum eft ἀντὶ primitus ufurpatum fuiffe "ad defignandum alterum poni *contra* alterum, id quod fieri fo-"lebat antiquiffimis temporibus, quum merces effent commu-"tandæ, unde facillime transferebatur ad indicandum ipfam "mercium commutationem vel quodlibet fuffectum in locum "alterius." Ἀντ' ἀγάπης μῖσος εὗρασθαι, "inftead of, or in return "for love, to find hatred,"—to find hatred—when the object put in front of it was love. Ειρήνη ἀντὶ πολέμου (Thucyd.) "peace in-"ftead of war,"—peace—object fet in front, for which it is to be taken in exchange—war.

From this laft application of ἀντὶ, it has come to be generally ufed by the Greek writers in fpeaking of commodities fold, to exprefs their price. Examples are unneceffary.

5. From

* Such phrafes are frequently refolved, by fuppofing ἀντί, when it fo occurs to denote *comparifon* or *contraft* only: but this appears to be infufficient; for though all preference implies contraft, all contraft does not imply preference.

5. From a particular view of the primitive idea of fronting or facing, ἀντὶ, when both preceded and followed by the ſame noun, came ſometimes to denote *addition* or *accumulation;* becauſe, when two quantities of the ſame commodity were placed fronting each other, this could not be for the purpoſe of exchange, but rather that the one ſhould be added to the other, to complete a maſs. Hence ſuch phraſes as ἀνίας ἀντὶ ἀνίαων, "ſorrows—in front—ſorrows,"—ſorrows added to ſorrows,—accumulated ſorrows; χάριν ἀντὶ χάριτος, "grace—in front—grace," grace added to grace,—abundant grace. In Engliſh we have adopted a ſomewhat ſimilar idiom, only inſtead of employing the word denoting *front*, we uſe its correlative *after*,—ſaying "day *after* day,"—"grief *after* grief."

Απὸ.

The Greek prepoſition ἀπὸ, and our Engliſh prepoſition *from*, are univerſally allowed to be nearly equivalent in ſenſe; and probably it may be found, that the origin of both has been in a ſomewhat ſimilar line. That ἀπὸ, like other prepoſitions, was originally a noun, there can be little doubt; evident traces of the cognates and derivatives of this noun being ſtill to be met with. Thus, beſides the prepoſition ἀπὸ, we find in uſe ἀπὸ as an adverb, denoting *far off*, with its derivative ἄπωθεν *, *from far;* and though the adjective ἄπος, *diſtant* or *remote*, appears to have become obſolete, yet its comparative ἀπώτερος, *farther*, and ſuperlative ἀπώτατος, *fartheſt* or *moſt remote*, are frequently found. The word ἄπος, likewiſe as a ſubſtantive nouns, occurs once, and, I believe, but once, (Eurip. *Phœniſſ.*), and the lexicographers

* Απὸ,—ἀντὶ τῶ ἄποθεν.

"Πολλὸν γὰρ ἄπο πλυνεί εισι πόλησς.

"For the ciſterns are at a great diſtance from the town."

Apollon. Sophist. in voce.

phers ſeem at a loſs what to make of it. The Greek ſcholiaſt conjectures it to have been equivalent to ὕψος, *height*, and thence transferred to denote μῆκος, *length*, a ſtrange circuitous mode of application, much more naturally reſolved by underſtanding the noun απος as a cognate of the adjective ἄπος, and hence immediately denoting length or diſtance. It may be added, that there is in common uſe a verb ἄπτω, to *tie*, and ἄπτομαι, to *touch:* the root of both theſe, I ſhould conjecture, muſt have been a verb ἄπτω or ἄπω, ſignifying to ſtretch, as it is not eaſy, except upon this ſuppoſition, to reconcile the different meanings of the active with thoſe of the paſſive or middle form of the verb.

These different cognates, then, ſeem all to point to the primitive meaning of ἀπὸ, and mark it out as an adjective denoting *diſtant* or *remote*. Some common noun denoting place or point, being, as in other caſes, underſtood or implied, ἀπὸ came to be uſed in the literal radical ſenſe of *diſtant point,—remote point.* From being thus frequently uſed to expreſs a point, more remote than any other in view, the ſignification was extended to denote likewiſe the *extreme point.*

This primitive radical ſenſe ἀπὸ retained in a number of caſes; but upon this primitive meaning there came, it ſhould ſeem, to be very ſoon engrafted a ſecondary application, which in time, in great meaſure, ſuperſeded the former. In any courſe or progreſs, it is evident, that, in the nature of things, either the commencement or the concluſion may be reckoned the extreme or remoteſt point of the line; but in the common uſe of language it will be found, that it is the former which moſt generally and readily occurs as the point moſt remote from the ſpeaker. Hence the *moſt remote or extreme point*, is often conſidered as the ſame with *the point of departure;* and the word which at firſt denoted the one, came readily to be applied to expreſs alſo the other. Such appears to have been the caſe with

απε

απο among the Greeks. From expreſſing originally the moſt remote or extreme point only, it came to be moſt generally uſed to ſignify, what in the language of navigators is called the *point of departure*, and hence, by an eaſy tranſition, the *point of origin*, the *point of commencing.*

Agreeably to this idea, all the uſes of *ἀπὸ* may be reſolved, by a reference either to the primary or to the ſecondary application of the word.

We find *ἀπὸ*, then, as ſignifying *from*, applied,

1. To *ſpace* or *corporeal objects.* Ἀπὸ δοθέντος σημείου γραμμὴν ἀγαγεῖν, (Euclid.), "to draw a line from a given point,"— "extreme or remote point of drawing,"—or, "point of departure or commencement of drawing it,—the given point."— Ὡρμᾶτο ἀπὸ Σάρδεων, (Xenoph.), "he marched from Sardes,"— "extreme point,—or point of his departure,—Sardes." Ἀπὸ τοῦ τείχεος μάχεσθαι, "to fight from the wall,"—"extreme point of "fighting,"—or "point of outſet of the fighting,—the wall."— Ἀπ' ομμάτων Βαλών με, (Anacr.) "darting at me from the eyes," "—extreme point of darting,—the eyes."

2. To *time.* Ἀπὸ τοῦ νῦν πειρασομεν, "from the preſent time we "ſhall endeavour,"—"we ſhall endeavour,—point of outſet of "our endeavours,—the preſent time." Ἀπὸ δὲ τοῦ πολέμου τῆς τῶν κοινῶν διοικήσεως ἑαυτὸν ὑπεξείλετο, "immediately after the war, he "withdrew from public buſineſs,"—"he withdrew from public "buſineſs,—point of outſet of his withdrawing,—or extreme "point of his withdrawing,—the war." Ὁ Σάβινος κουφότερος ἀπὸ ἐτῶν ἑπτὰ ἐπιτήδειος, (Galen), "the lighter Sabine wine is fit for "uſe from ſeven years old,"—"the wine is fit for uſe,—point "of outſet of this fitneſs,—ſeven years." Ἀπὸ σημείου ἑνὸς ἐπιστρέψαι τὰς ναῦς, (Thucyd.), "to turn the ſhips at one ſignal,"— "to turn the ſhips,—point of outſet of the turning,—one ſignal."

3. To

3. To the *relations* of objects, and chiefly to their relations as effects from a caufe either producing or modifying them. Αἰσθόμενος οὐκ ἀπὸ τοῦ βελτίστου ταῦτα πράττοντα τὸν ἀδελφόν, (DION. HALIC.), " perceiving that his brother was acting thus, not from " the beft difpofition,"—" acting,—point of outfet,—or origin " of acting,—not the beft difpofition." Δοκεῖ ἑτέρως δὲ τοῖς ἀπὸ τῆς στοᾶς, ἑτέρως δὲ τοῖς ἀπὸ τῆς ἀκαδημιάς, (LUCIAN.), " it appears in " a different light to the ftoic and the academic philofopher," " —to perfons from the porch and from the academy,"—" per- " fons proceeding,—point of departure,—the porch and the aca- " demy."

4. IN all thefe different applications of ἀπὸ, which are the moft common, the fecondary fenfe appears to predominate ; but ἀπὸ is fometimes found in ufe, where the original meaning of *remote* or *diftant point* directly prevails. This takes place in regard to material objects, in fuch phrafes as the following : Ὡς μέν τοί γε ἀπὸ τῆς γῆς ἐγένοντο, (XIPHIL.), " when they arrived at a diftance from land,"—" when they arrived—remote or diftant point,— " the land."

——— ἀπὸ χθονὸς εὐρυοδείης
Ἐς δίφρον θῆκαν. HESIOD.

" They placed him in the chariot away from the earth,"—" pla- " ced him—diftant or remote point,—the earth."

——— ἀπὸ δὲ γλαυκῶπις Ἀθηνὴ
Ἔγχεος ὁρμὴν ἔτραπ' ὀρεξαμένη ἀπὸ δίφρου. HESIOD.

" MINERVA turned away the force of the fpear,—warding it " from the chariot,"—" turned the force of the fpear,—warding " it,—fo as to be remote from the chariot,—remote or diftant " point,—the chariot."

IT is in this manner that ἀπὸ is likewife ufed to denote a *negation ;* the one object being fuppofed to be fet at fuch a di- ftance ſtance from the other, that they ceaſe to be in any manner connected. Απέδειξεν ὅτι παντάπασιν αυτῷ ἀπὸ θυμȣ̃, (THEMIST.) "he "ſhewed that it was altogether contrary to his deſire,"—"ſhow-"ed that it was ſo conſtituted, that it was a point entirely di-"ſtant or remote in reſpect to his deſire." Ουκ ἀπὸ γνώμης ῆν, (JULIAN. AUG.), "it was not contrary to his mind,"—"it was "not in the ſituation that it was a remote or diſtant point,—in "regard to his ſentiments." Ὁ ἀπὸ τῆς πρεσβείας, (VIGERUS), "one who has finiſhed his embaſſy,"—"who is no longer an "ambaſſador,"—"who is now in ſuch a ſtate as to be remote "or diſtant in reſpect of the embaſſy."

THE Latin prepoſitions *a*, *ab*, it is well known, are merely the prepoſition απο, in a mutilated form; the radical meaning is nearly, if not entirely the ſame, being ſometimes *remote point*, when *a* or *ab* is uſed to expreſs *from*; ſometimes *point of departure* or *outſet*, when uſed to ſignify *by*. *Abs*, which has the ſame ſenſe with *a* or *ab*, is the adjective ἄπος in a contracted form. *Absque*, which is juſt *abs* with the relative ſubjoined, takes the ſenſe of *remote or diſtant point* only, and hence ſignifies *without*. Thus, "epiſtola absque argumento,"—"a letter without an ar-"gument;"—"a letter,—remote,—in reſpect to which is argu-"ment." "Absque te victoria eſſet,"—"the victory would be "(obtained) without you,"—"the victory,—you being remote "in reſpect to which,—though you were remote or at a di-"ſtance,—would be obtained."

IN theſe different ſenſes of the Greek prepoſition απο, we trace a very ſimilar analogy with what appears in the correſponding Engliſh prepoſition *from*. This Engliſh prepoſition HORNE TOOKE will have to be the Saxon *frum*, *beginning*. But from ſome of the very examples given by himſelf, and ſtill more from others that might have been adduced, it is eaſy to ſee how forced and unſatisfactory the analyſis of many ſentences on this ſuppoſition muſt be. A different origin muſt, therefore, be ſought

ſought for the word. *From*, in Saxon, is *fram* and *fra*. This laſt appears plainly the root, and it is as evidently a deſcendant of the Gothic *fairra*, from. Now this Gothic *fairra* was undoubtedly alſo the root of our adjective *far*, equivalent to diſtant or remote; and from this root proceeds likewiſe the German *Fremde*, a ſtranger. Here, then, we have the ſame analogy which has been already traced out in the Greek; the root of our Engliſh *from* being an adjective expreſſing *remote*, and the prepoſition of courſe meaning juſt—*remote point*, or *diſtant point*.

It may be added, upon the ſubject of ἀπὸ, that this prepoſition, or rather its aſpirated form αφ, is, according to the beſt etymologiſts, the root of the Engliſh adverb *off*, originally *aff*; and the preciſe meaning of this is *diſtant* or *remote*,—*off*, as is well known, being quite a different word, and from a different ſource, from the prepoſition *of*, a word radically and properly denoting *claſs*, *kind* or *ſpecies*, though the two have not always been ſufficiently diſtinguiſhed by lexicographers.

Διὰ.

There can be little doubt, I think, that the prepoſition διὰ muſt have been cloſely related to the verb δίω. Now δίω in common uſe ſignifies to *drive out*, or *drive away*. From many of its cognates and derivatives, however, it is evident, that it muſt have ſignified alſo to *divide*, to *ſplit*, or to *cleave*. The original radical idea expreſſed by it, therefore, from which both theſe took their riſe, may fairly be conjectured to have been to *bore*, to *pierce*, to *penetrate*. From this verb δίω appears to have come a verbal adjective διος, δια, διον, ſignifying, from the verb, *pierced* or *penetrated*, ſometimes DIVIDED; and, when applied to ſurfaces or ſpaces, by a very natural and obvious reſtriction, *paſſed over*, or *croſſed*. To the neuter plural, or feminine ſingular of this adjective, ſome common word, ſuch as γϱαμμὴ, χώϱα, σημεῖα, or the like, being at firſt uſually joined, as in other caſes, and after-

wards

wards in practice omitted, διὰ came to be used alone for *line, space, point* or *object pierced, penetrated, divided, passed over,* or *crossed.*

From this the various applications of the word, as it appears in the form of a preposition, took their rise.

Διὰ, as applied to *space*, was used to denote:

1. and most usually, THROUGH.

Πέλεκυς ἄσι διὰ τȣ̃ δένδρȣ,—" the axe goes through the tree," "—the axe goes,—*object penetrated,—the tree.*" Κύκλος κύκλον διὰ τῶν πόλων τέμνῃ, (Theod. Spær.), " Let one circle cut another " through the poles,"—" *points pierced or penetrated in cutting,*— " *the poles.*" Ἐπίπεδον ἐκβεβλημένον διὰ τοῦ κέντρȣ τᾶς γᾶς καὶ τοῦ ἁλίȣ̃, (Archimed.), " a plane drawn through the centre of the " earth and the sun,"—" drawn,—*points pierced or penetrated in* " *drawing it,*—the centre of the earth and the sun."

——— αἴγλη δι' αἰθέρος ἵκε. Homer.

" The gleam came through the air,"—" *medium penetrated in its* " *coming,—the air.*"

2. Across.

Δι' ἁλὸς φέρονται,—" they advance across the sea,"—" they " advance,—*object or place passed in advancing,—the sea.*"

> Ὅς τε διὰ Θρήκης ἱπποτρόφȣ εὐρέϊ πόντῳ
> Ἐμπνεύσας. Hesiod.

" Which blowing through Thrace, famed for rearing horses,— " on the wide sea,"—" blowing,—*place passed or crossed in blow-* " *ing, in order to reach the sea.*" Ἐπορεύετο δι' Αἰγύπτȣ εἰς Λιβύην, " —he marched through Egypt to Libya,"—" marched,—place " crossed or passed in marching to Libya,—Egypt."

The only difference between these two applications of διὰ consists in this,—that, in the former, the internal part of the object is underſtood to be penetrated or divided, in the latter, the ſurface only; but the idea of penetration is uniformly adhered to.

3. BETWEEN.

Διὰ χειρὸς ἔχειν τὰς ἡνίας, (PLUTARCH), "to hold the reins in "his hand, that is, between the two parts of his hand,"—"to "hold the reins,—*object divided, or, as it were, penetrated by the* "*reigns*,—his hand."

——— εὖτ' ἂν πρῶτα φύγῃς ὀλοὰς διὰ πέτρας. APOLLON.

"When thou art paſſed ſafe between or through the deſtructive "rocks,"—"*objects as it were divided or penetrated in paſſing*,— "the deſtructive rocks."

4. DISTANCE OF INVERVAL.

Ἵππος διὰ τριῶν ϛαδίων ἔτρεχε, "the horſe run to the diſtance of "three ſtadia,"—"the horſe run,—*ſpace penetrated or divided in* "*running*,—the extent of three ſtadia." Εκ τῶν πύργων ὄντων δι' ὀλίγȣ, "from the towers which were at a ſmall diſtance from "each other,"—"towers ſituate,—*ſpace penetrated or divi-* "*ded by the line reaching them,—a ſmall ſpace,—a ſmall inter-* "*val.*"

Διὰ was applied likewiſe to *time* as well as to ſpace. In this mode of application, it expreſſed ſometimes *through*, ſometimes *after*; that is, the action was conſidered as either actually paſſing, or as having juſt paſſed, an interval of time. The analogy between ſpace and time is ſo obvious, that the reaſon of uſing διὰ is this way can require no elucidation. As referring to time, then, διὰ denotes:

1. THROUGH.

Σκόπειν διὰ νυκτὸς καί ἡμέρας, (PLATO), "to deliberate through "night and day,"—"to deliberate,—*interval or ſpace of time* "*paſſed in deliberating*,—night and day."

Διὰ

Δια νυκτὸς εγκοθεύδων. - - ANACREON.

"Sleeping through the night,"—"time paſſed in ſleeping,—the "night."

2. AFTER.

Δια δυεῖν ἡμέρων ἀποχέοντες τὸ πρῶτον ὕδωρ, (DIOSCOR.), "pouring "off the firſt water after two days,"—"pouring off the water, "—*interval of time paſſed before pouring it off*,—or paſſed in its "previous ſtate,—two days." For it is to be obſerved, that when *διὰ* is thus made to denote *after*, it always implies, that the ſtate of the tranſaction then exiſting, is the reſult of the preceding action, with which *διὰ* is joined. *Δια τρίων ἡμέρων ἦλθε*, "after three days he came,"—"interval paſſed in coming,—"three days,"—intimating that three days were ſpent in travelling; whereas, had *μετα* been uſed for *after*, no ſuch previous connection would have been implied; the three days might have been occupied with an action bearing no relation to his coming.

BY a ſecond remove from the primitive meaning of the word, *διὰ* came alſo to be applied to expreſs CAUSATION in all its forms. In this mode of application, it appears to have been extenſively employed to mark, indifferently, the *efficient*,—the *inſtrumental*,—the *formal*,—the *material*,—the *procuring*, and the *final* cauſe. The idea, in all of them, was evidently taken from the uſe of *διὰ* to expreſs an interval paſſed, an intervening object penetrated, or an intermediate ſpace croſſed. A cauſe, of whatever kind, naturally preſented itſelf to the mind as a paſſage or medium between the bare conception or deſign of an action and its entire completion. An example of each ſpecies of cauſe, will ſhow, in this uſe of *διὰ*, the conſtant reference to its radical meaning.

THE *efficient*. *Δι' οὗ ἐλάβομεν χάριν*, (*Rom.* i. 5.), "through "whom we received grace,"—"we received grace,—medium of "its conveyance to us,—him."

Τοὶ μὲν δαίμονές εισι Διὸς μεγάλȣ διὰ βȣλάς. HESIOD.

"They become demons through the will of mighty JOVE,"—"they become demons,—medium by which this is effected,—the "will of JOVE."

Δι' ὅν ευνάζετ' ἀνία. ANACR.

"Through or by which ſorrow is lulled to reſt,"—"ſorrow is "lulled to reſt,—medium of doing this,—it."

THE *inſtrumental.* Ὤλετο διὰ πυρὸς, "it was deſtroyed by fire," "—it was deſtroyed,—medium paſſed in order to its deſtruction, "—fire,"—or, "medium of conveyance for the deſtructive ener"gy,—fire." Κινȣμένης τῆς ἄμμου διὰ τὸ μέγεθος τῶν πνευμάτων, THEOPHRAST.), "the ſand being agitated through or by the vio"lence of the winds,"—"the ſand being agitated,—medium or "paſſage to its agitation,—the violence of the winds."

THE *formal.* Τορνεύεται καὶ γλύφεται διὰ τὸ μαλακὸν, (THEOPHRAST.), "it is carved and ſculptured by reaſon of its being "ſoft,"—"it is carved and ſculptured,—ſtate paſſed to fit it for "being ſo,—ſoftneſs."

THE *material.* Δι' ἄλφιτου πεποίημενος, "made of flour,"—"made,—intervening ſtate paſſed previous to the making,—the ſtate of flour."

THE *procuring* or *meritorious cauſe.*

Ἔχω ἃ ἔχω διὰ σὲ κ' ȣκ ἄλλον βροτῶν. SOPHOCL.

"What I poſſeſs, I poſſeſs by your merit, (or by your favour), "and by that of no other mortal,"—"what I poſſeſs, I poſſeſs, "—medium of its reaching or paſſing to my poſſeſſion,—your "merit or favour."

THE *final,* (nearly related to the preceding) cauſe. Δι' Ἀθηναίȣς μαχήσεται, "he will fight on account of the Athenians,"—"he

"he will fight,—medium to bring about his fighting,—the pro-
"ſpect of benefiting the Athenians."

> Ου τόσον ἀθανάτων ἀγέμεν σπεύδουσι θυηλὰς
> Όσσον αγειρομένων διὰ κάλλεα παρθενικάων. MUSÆUS.

"They haſten, not with the view of celebrating religious rites "in honour of the gods, but for the ſake (or on account) of the "beauty of the damſels who aſſemble on theſe occaſions;"—"they haſten,—medium or paſſage to induce them to do ſo,—"the beauty of the damſels,—the proſpect of meeting the aſſem-"bled damſels."

IN ſeveral of theſe applications of διὰ to cauſation, particularly the laſt, it muſt be admitted, that the mode of expreſſion is elliptical and circuitous; in all of them, however, it is eaſy to trace, either immediately or remotely, the radical ſenſe of διὰ as a noun, modified by uſe into a prepoſition.

Εις.

THIS prepoſition, as well as ἐν, was ſuppoſed by Dr MOOR to have been originally the numeral adjective εἱς, μια, ἐν, both prepoſitions, denoting, as he conceived, *the one ſpace;* that is, the ſpace immediately adjoining the object ſpoken of. This idea, though ingenious, appears clogged with inſurmountable difficulties. Εἰς and ἐν are by no means ſo nearly the ſame in meaning as this derivation would make them. LINNEP derives εἰς from the verb ἑω, *to ſend*,—a derivation undoubtedly forced and unnatural; and SCHEIDE makes εἰς a contraction of εἰσω, *within*, evidently reverſing the analogical order of the language, εἰς being in fact the parent of εἴσω.

NONE of theſe deductions being at all ſatisfactory, for explaining the various uſes of εἰς, we muſt have recourſe to ſome other origin of the word; and one better adapted to the purpoſe may,

may, I think, be found. There is in common uſe a defective verb ἧμαι, ſignifying *to ſit*. From the analogy of the language, it is clear that this muſt have been the perfect paſſive of an obſolete verb ἕω, ſignifying *to ſet*, of which the perfect paſſive would conſequently ſignify, " I have been ſet," or " I am ſet," or what is equivalent, " *I ſit*." Now, in the regular declenſion of this verb ἕω, εἷς would occur as the participle of one of the aoriſts paſſive, naturally and literally therefore ſignifying *ſet down*, or what is equivalent to this, any thing in the act of ſitting, reſting, or ſome ſimilar attitude. Here, as in the other prepoſitions, ſome common word being underſtood, εἰς might come directly and literally to expreſs *ſitting place*, *reſting place*, *ſtopping place*.

WITH this ſenſe, all the uſes of εἰς exactly coincide.

Ἡ εἰς τὰς παραλλήλους εὐθείας εὐθεῖα ἐμπίπτουσα, (EUCLID), " the " ſtraight line falling on parallel ſtraight lines,"—" the ſtraight " line falling,—reſting place,—parallel ſtraight lines." Ἐπεστράφη ὁ κανὼν ἐς τὰν ἅλιον, (ARCHIM.), " let the ruler be turned towards " the ſun,"—" let the ruler be turned,—reſting or ſtopping " point of its turning,—the line of the ſun.

——— ἤλυθον εἰς Τροίην. HOMER.

" I came to Troy,"—" I came,—ſtopping place,—Troy."

Εἰς τοὺς Ἕλληνας χρυσὸν εκόμισε, " he cauſed money be ſent to " the Greeks,"—" he cauſed money be ſent,—ſtopping place,— " place where the ſending reſted,—the Greeks. Ἔβαλλε λίθον εἰς τεῖχος, " he threw a ſtone at or againſt the wall,"—" he threw a " ſtone,—ſtopping place,—the wall." Εἰς τὴν ἐκκλησίαν καθέζομαι, " I ſet myſelf down in the aſſembly,"—" I ſet myſelf down,— " ſitting place,—the aſſembly." Κυρος πάρην εἰς τὸ αυτό χωρίον, (XENOPH.), " CYRUS was preſent at, or in the ſame place,"— " CYRUS was preſent,—reſting place,—the ſame place."

Ναῦται

Ναῦται εἰς Ἑλίκην τε καὶ ἀστέρας Ὠρίωνος
Ἔδρακον ἐκ νηῶν. APOLLON. *Rod.*

"The ſailors from the veſſels looked towards the Bear and the "ſtars of Orion,"—"the ſailors looked,—reſting place of their "looking,—place where their view reſted,—the Bear and Orion." Εἰς τριακόσιους ἐγένοντο συνωμόται, "the conſpirators amounted to "300,"—"the conſpirators proceeded in number,—point where "the numeration ſtopped,—300." Ἐις ἠελίον καταδύντα δαίνυνται, "they feaſt till ſunſet,"—"they feaſt,—ſtopping point of their "feaſting,—ſunſet." Εἰς δύναμιν ἐργάζεται, "he works to the ut-"moſt of his power,"—"he works,—ſtopping point,—his "ſtrength,—point where his exertion ſtops,—the point where his "ſtrength ſtops." Ἐγκλήματα ἐς τοὺς Ἀθηναίους, "complaints "againſt the Athenians,"—"complaints,—object where they "reſted,—the Athenians." Ἐπαινεῖται εἰς τὸ κάλλος, "he is prai-"ſed for his beauty,"—"he is praiſed,—point where the praiſe "reſts,—his beauty."

In all theſe various applications of εἰς, we find the ſame radical idea of *ſtopping place* or *reſting place* exiſts; and, in like manner, it would be eaſy to trace the ſame primitive ſenſe in every phraſe or ſentence where this prepoſition occurs.

Ἐκ.

In the *Etymologicum Parvum* of GREGORY, the derivation of εκ is properly ſtated from the verb ἔκω, "I yield, give way, or "quit my place." This prepoſition, therefore, appears to be a mutilated verbal noun, of ſimilar import with the verb, and may therefore be exactly rendered *quitted*, or *place quitted*, and, by a very obvious tranſition, *ſtate quitted*. This radical meaning correſponds with that of the Engliſh word *out*, which ſeems a branch

branch from the ſame root with the French *ôter* or *oſter*, to take *away* or *remove*.

UNDER this radical and original ſenſe of *ἐκ*, all its different ſignifications may be properly arranged.

1. PLACE.

Ἐκ Πύλȣ ἔλθών.—— HOMER.

" Coming from Pylos,"—" coming,—place quitted,—Pylos." Πολὺν ἐκ ποταμȣ̃ χρυσὸν ἀφύσσαμενος, " drawing much gold from or " out of the river,"—" drawing much gold,—place quitted,— " the river." Λαᾶς ἐξ ὄρεος τμήθεις, " a ſtone cut out of the " mountain,"—" cut out,—place quitted,—the mountain."

2. TIME. Ἐκ τῆς ὑπατείας τέθνηκε, " he died after the conſul- " ſhip,"—" time left,—period quitted,—the conſulſhip." Ἐκ τȣ̃ δεῖπνου ἀνεχώρησεν, " he went away after ſupper,"—" he went " away,—time quitted,—the time of ſupper."

3. STATE. Ἐξ εἰρήνης πολεμεῖν, (THUCYD.), " out of peace to go " to war,"—to go to war,—ſtate quitted,—peace."

UNDER this idea, ἐκ is often applied to the materials from which a thing is made, becauſe the object formed is, by an obvious analogy, conſidered as having quitted the maſs of unformed matter to aſſume its new ſtate. Ποτήριον ἐκ χρύσȣ, " a cup of " gold,"—" a cup made,—ſtate quitted,—the maſs of unformed " gold." Ἐκ τριῶν εὐθειῶν τρίγωνον συστήσασθαι, (EUCLID). " from " three ſtraight lines to conſtruct a triangle,"—" to conſtruct a " triangle,—ſtate or form quitted,—three unconnected ſtraight " lines." Λίθων ἐξ ὧν τὰ ἀγάλματα ποιȣ̃σι, (THEOPH.), " ſtones, of " which ſtatues are made,"—" ſtatues are made,—ſtate quitted, " —maſs of unformed ſtone."

Ἐκ and ἀπὸ are frequently uſed indiſcriminately. From the radical ſenſe of the two, ſuch an idiom is perfectly admiſſible; for as ἐκ ſignifies *the place quitted*, and ἀπὸ *the remote point*, or

point.

point of departure, it is plain, that the idea implied in the one may often be precifely the fame with that conveyed by the other.

Εν.

I HAVE already taken notice of Dr MOOR's idea, that εἰς and ἐν were, both of them, parts of the numeral adjective εἷς, μια, ἑν, an unfatisfactory derivation, by no means conveying the radical fenfe of either prepofition. As we fought the origin of εἰς from another quarter, the fame, I apprehend, muft be done with regard to ἐν. LINNEP's derivation of ἐν, from the verb εἰμὶ, *fum*; and SCHEIDE's fuppofition, that it is a participle from the verb ἕημι, *immitto*, are certainly inadmiffible; the former giving no determinate fenfe to the prepofition, and the latter being particularly faulty, as requiring the prepofition itfelf, or one of fimilar import, to be joined to the verb, before it could acquire the fenfe there affigned to it.

Εν, then, I think, may be juftly deduced, and, indeed, muft be actually derived, from the verb εω, or εννύω, *to clothe*, and thence *to cover*. By direct proceffion from this root, ἐν, whether a verbal fubftantive, or, as is not improbable, a contracted participle, retains the fignification of the verb; and its radical fenfe may therefore be fet down as *clother*, *coverer*, and, by generalizing this idea, *container* or *comprehender*. Let us try how far its various applications may be refolved by this idea.

THE moft natural and common application of ἐν, is to PLACE, and that in different modes. Εν χερσὶν ἔχω, "I have it in my "hands,"—"I have it,—container,—my hands." Λίθος ἐν τῃ ἄκρα τῇ Ερινεάδι καλȣμένη πολὺς, (THEOPHR.), "abundance of "ftone in the promontory called Erineas,"—"abundance of "ftone,—container,—the promontory called Erineas." Παραλληλόγραμμα ἐν ταῖς αυταῖς παραλλήλοις, (EUCLID), "parallelograms "between the fame parallels,"—"parallelograms,—container,—

 "the

"the same parallels." Παραλληλόγραμμον συστήσασθαι ἐν τῇ δοθείσῃ γωνίᾳ, (EUCLID), "to construct a parallelogram,—container,—a "given angle,"—"a parallelogram formed with a given angle." Ἐν τῷ οἴκῳ ἐκαθέζετο, (JOHN xi. 20.), "she sat in the house,"— "container,—the house." Ἐλαχίστη ἐν τοῖς ἡγεμόσιν Ιȣδα, (MAT. ii. 6.), "least among the chiefs of Judah,"—"least of those,— "container or comprehender,—the chiefs of Judah,—of those "whom the title Chiefs of Judah comprehends." Κατέβαινεν ἐν τῇ κολυμβήθρᾳ, (JOHN v. 4.), "he descended into the pool,"—"he "descended,—container of him after his descent,—the pool." Ἐλθεῖν ἐν Ἑλλάδι, "to come into Greece,"—"to come,—contain-"er after the coming,—Greece." In these two last examples, and others of a similar nature, the intermediate step is, as it were, overlooked, being understood as necessarily implied in the completion of the action. In the English preposition *into*, the expression is more full, though the order of the two prepositions of which that word is compounded, is, as it were, inverted,—*to* implying the termination of the motion by which a place is reached, and *in* the position of the object, after having been fixed at its place of destination.

Ev is also applied to TIME; and in this application the same resolution of it may be made. Ev ἑπτὰ ἡμέραις ἀποθνήσκουσιν, (HIPPOCR.), "they will die within seven days,"—"they will die,— "period comprehending the time of their dying,—seven days." Ev σπονδαῖς ἐποίησε, "he did it during the truce,"—"he did it,— "container of the time of doing it,—the truce."

Ev is likewise used in expressing MODES OF ACTION. Ev πέλταις καὶ ἀκόντιοις μαχόνται, (XENOPH.), "they fight" (as usually rendered) "*with* shields and darts,"—"they fight in the way of "shields and darts,"—"they fight,—container of their mode of "fighting,—the use of shields and darts." Ἀποκτεῖναι ἐν ῥομφαίᾳ καὶ ἐν λιμῷ, (APOC. vi. 8.), "to kill in the way of sword and famine,"—

"mine,"—"to kill,—container of the mode of killing,—the ope-"rations of ſword and famine."

Lastly, ἐν is ſometimes employed, when relation is ſignified. Ἐν ἐμοὶ ἐστὶ, "it is in my power,—within my reach,"—"it is,—"container or comprehender,—me,—container of this object,—"the line of my agency."

By a particular mode of phraſeology, the Greeks ſometimes appear to have made uſe of ἐν, where we might have ſuppoſed ἐκ or ἀπὸ ſhould have been employed. Ἐν δένδρῳ κόπτειν κλάδον, "to cut "a branch from the tree,"—"to cut a branch,—container of the "place where it was cut,—the tree." In our mode of expreſſion, *from* would have been uſed, as denoting the point of departure. In the Greek phraſeology, it was ſufficient to mention the place where the action took effect.

The Greek prepoſition ἐν, it may juſt be remarked in paſſing, appears evidently the primitive of the Latin prepoſition *in*, and, through it, of the ſame prepoſition in moſt of the modern European languages. But, from ſome of the preceding examples, it will alſo appear, that the primitive word was made uſe of in a more extended ſenſe than is to be found in its various deſcendants.

Ἐπί.

This prepoſition is one of thoſe which Dr Moor has brought forward as a principal illuſtration of his ingenious theory of the variations of the force of the Greek prepoſitions, according to the different caſes of their connected nouns. Some particular examples, it is certain, ſeem to ſupport this theory, but others as ſtrongly militate againſt it; ſo that I muſt regard it as by no means ſufficiently eſtabliſhed: but whether true or not, does not much concern the ſubject of the preſent inquiry; for, even admitting the truth of this theory. it goes very little way in aſſiſting us to develope the radical ſenſe of the prepoſitions.

In the prepoſition we are now conſidering, though affording one of the beſt illuſtrations of Dr Moor's ideas, the primitive ſenſe muſt be ſought for, not according to his hypotheſis, but by a fair examination of its root and cognates.

The root of ἐπί appears to lie in the verb ἔπω, a verb occurring in different paſſages of the earlier Greek writers, in its ſimple form, and of pretty general uſe in a compound ſtate. Ἔπω is rendered by Stephens *to work, to handle, to follow;* ſometimes *to hurt*, and ſometimes *to warm;* and frequently this verb is uſed by Homer to expreſs *rubbing, furbiſhing*, and *ſcouring*. All theſe different ſenſes are, I think, eaſily reducible to the idea of *preſſing* or *cloſe touching*, which may therefore be taken as the radical ſenſe of ἔπω. Ἔπω, the root, having this radical meaning, its cognate and derivative ἐπί, muſt expreſs the ſame idea; and as ἐπι appears to be a mutilated verbal adjective, with its ſubſtantive underſtood, it is properly and literally *object preſſed, adhered to*, or *object touched cloſely;* and this ſignification it retains whether applied to *place, time*, or *relation*.

1. As applied to *place*.

Ἡ σφαῖρα κυλίνδεται ἐπί τῆς τράπεζης, "the ball rolls upon the "table,"—"the ball rolls,—object preſſed by it, or adhered to "in ſo doing,—the table." Ὁ λίθος πίπτει ἐπί τήν γῆν, "the ſtone "falls upon the ground,"—"the ſtone falls,—object preſſed or "adhered to by its fall,—the ground."

Ὀλέσω δέ πολέας ἐπί νηυσὶν Ἀχαιων. Homer.

"to deſtroy many of the Greeks at or beſide the ſhips,"—"to "deſtroy many,—place or objects cloſe touched in doing ſo,— "the ſhips." Καθίζεσθαι ἐπί τήν ἑστίαν, "to ſit down at or beſide the "hearth,"—"to ſit down,—place touched, or cloſely adhered to "in ſitting,—the hearth." Εὐθεῖα ἐπ' εὐθεῖαν σταθεῖσα, Euclid), "a ſtraight line ſtanding upon a ſtraight line,"—"a ſtraight

" line ſtanding,—place or object adhered to, preſſed, or cloſe " touched in its ſtanding,—another ſtraight line." 'Επ' Αἰγυπτȣ ῥέειν *, (THUCYD.), " to flow towards Egypt,"—" to flow,— " place adhered to, preſſed, or touched in its flowing,—Egypt."

2. As applied to *time*.

Τίς πρῶτος; τίς δὲ ἐπὶ πρώτῳ; EURIP.

" who firſt, and who next after the firſt?"—" who firſt,—and " who adhering to, preſſing, or cloſe-touching the firſt,"— " juſt after the firſt?"

Ὄχνη ἐπ' ὄχνῃ γηράσκει. HOMER.

" pear grows old after pear,"—" pear grows old,—event cloſe- " touched in point of time,—the growing old of another pear."

——— ὅσσον ὁλκὰς ἐπὶ τρίτον ἦμαρ ἀνύσσῃ. DION. GEOG.

" As much as a ſhip of burden would paſs in three days,"— " would paſs,—period adhered to, touched or reached in this " paſſage,—the third day."

3. As applied to *relation*, *poſſeſſion* or *occupation*.

Τῶν ὄντων τὰ μὲν ἐστιν ἐφ' ἡμῖν, τὰ δὲ οὐκ ἐφ' ἡμῖν, (EPICT.), " Of " things ſome are in our power, others not,"—" ſome are ſo " conſtituted, that the object adhered to, preſſed, or cloſe-touch- " ed by them, is the line of our power, or the line of our reach." 'Επὶ τοῖς γεγενημένοις χαλεπῶς φέρειν, " to be diſpleaſed in conſe- " quence of what has taken place,"—" to be diſpleaſed,—point " or

* THIS example is directly againſt Dr MOOR's hypotheſis, that ἐπὶ with the genitive denotes *motion upon*, and with the *accuſative*, *motion directed upon*; here it has the latter ſignification with the *genitive*. The ſame thing holds in many other inſtances, in the beſt and moſt accurate Greek writers.

" or object which the displeasure touches, or adheres to,—the " things that have happened." Ὁ ἐπί τῶν δημοσίων λόγων, " the " officer over the public accounts,"—" the person so occupied, " —that the public accounts are the object touched or adhered " to in his occupation *."

Οἵτινες ὕμνους ἐπὶ μὲν θαλίαις,
Επί τ' εἰλαπίναις, καὶ παρὰ δείπνοις,
Εὕροντο. EURIPID.

" Who invented songs as an accompaniment at festivals, at con- " vivial entertainments, and at feastings,"—" songs,—object or " event which they are made to adhere to, touch, or closely ac- " company,—festivals and entertainments."

IT would be tedious to go through the multifarious quotations brought together by STEPHENS and VIGERUS, as various meanings of ἐπὶ in the Greek writers; all of them may with little difficulty be resolved, either in a direct or secondary sense, into the idea of being *pressed, adhered to, touched closely, handled,* or some equivalent circumstance.

Κατὰ.

IN tracing the origin of κατὰ, a difficulty occurs from the want of a radical verb, corresponding properly in signification with the sense of this preposition. The difficulty, however, is removed, when we take into view the system of cognates, as evidently found to exist in the Greek language †. From the facts

* PERHAPS, in this and similar phrases, there is a reference to the secondary sense of ἕπω, " to handle or work upon,—hence to manage;—the same seems to hold in such expressions as ὁ ἐπί τῆς πόλεος, " the governor of the city,"—" he who " is so placed—that the object handled or managed by him is the city."

† SOME observations on this subject have been thrown into an Appendix, being too long for a note.

facts eftablifhed by this fyftem, it is afcertained, that in thefe primitive roots, one fignification belonged to verbs where the radical confonant was the fame, though the vowels were varied. That thus, for inftance, the roots καω and κεω originally denoted the fame idea, though, in the progrefs of the language, the traces of one of the roots might be loft. Now the verb κεω muft evidently have been the prefent indicative active, whence the defective verb in common ufe, Κεῖμαι, "I lie," originally was formed, this laft being the perfect paffive, with the augment omitted. Upon this analogy, κεω, and of courfe its cognate καω, fignified to *lay*, or *lay down*; and κατὰ, a verbal noun, proceeding from this root, had originally the literal meaning of *laying place*, *lying place*, or place where an object was lying or laid down. This fenfe we fhall find it ftill retains, in its different applications, in the form of a prepofition.

Εὐθεῖα τέτμηται κατὰ τὸ Δ σημεῖον, (EUCLID), "the line is cut at "the point D,"—"the line is cut,—laying place, or place where "the cut lies,—the point D." Συμπιπτέτωσαν κατὰ τὸ K, (EUCLID), "let them coincide at K,"—"let them coincide,—point "where the coincidence lies,—K."

Κατὰ ῥωπήϊα πυκνὰ κέιμεθα. HOM. *Od.*

"We lie,—laying place, or place where we were laid,—the thick "bufhes,"—"among the thick bufhes."

Τα μὲν δασόμεθα κατὰ σφῖσι. APOLLON.

"Thefe things we will divide among them,"—"thefe things we "will divide,—place where the divifion lies,—them." Κατὰ βοῤῥεαν ἑστηκώς, (THUCYD.), "ftanding or fituate toward the north," "—fituate,—point where the fituation lies,—the north."

THIS fenfe of κατὰ correfponds precifely with what is ufually attributed to it when joined with the accufative, viz. *appofition*; and

and, if juſtly analyzed, the ſame will be found to hold when it is joined with the genitive, though, with that caſe, κατὰ is commonly ſaid to expreſs *oppoſition*. Thus, κατὰ σκοπȣ̃ τοξεύειν, (HERODIAN), "to ſhoot at a mark,"—"to ſhoot,—lie of the "ſhot,—or place where the ſhot was made to lie,—the mark." In thoſe caſes where κατὰ falls to be tranſlated as denoting *oppoſition*, the oppoſition unqueſtionably lies in the connected verb, not in the prepoſition.

Κατὰ γῆς πίπτειν. DION. HALICAR.

"To fall againſt the ground,"—"to fall,—lie or direction of the "fall,—place where the falling lay,—the ground." But in moſt inſtances where κατὰ occurs with the genitive, no direct oppoſition is intimated. Thus,

Κατ' ὀφθαλμῶν κέχυτο ἀχλὺς. HOMER.

"A miſt was ſpread, (not againſt, but) *before* his eyes,"—"a "miſt was ſpread,—place where its operation lay,—his eyes."

FROM this primary ſenſe of κατὰ, two ſecondary ſenſes appear to have branched. In the firſt place, any thing laid down evidently occupies a certain ſpace, in the ſame poſition or direction with the object upon which it lies. Upon this idea, κατὰ came frequently to be uſed to expreſs *the line along which any thing has its poſition, courſe, or direction*. Hence it may frequently be rendered *along*. Εἶσι κατ' ὄρεος, "he goes along the mountain,"—"he goes,—direction of his going,—lie of his courſe,—the "mountain." Ἔρχονται κατὰ τῆς ὁδȣ̃, or κατὰ τὴν ὁδὸν, "they "come along the road,"—"they come,—direction or lie of their "coming,—the road." Κατὰ τȣ̃ τεῖχεος κρέμασαι, "to ſuſpend "along the wall,"—"to ſuſpend,—direction in which the ob"ject was ſuſpended,—the wall." By the obvious analogy between place and time, κατὰ in this mode of application was

ſometimes

ſometimes transferred to expreſs *during*, as it were, *along a particular period.*

Κατὰ νύκτα πεπλάνημαι. ANACR.

"I wander during the night,"—"I wander along the period of "night." Sometimes, too, by an extenſion of this ſignification to *modes of relation*, κατὰ came to be uſed to denote *according to*, that is, in the ſame direction with another. Κατ' εἰκόνα τοῦ κτίσαντος, (*Coloſſ.* iii. 10.), "according to the image of the maker,— "formed,—direction of the form,—the image of the maker." The Latin prepoſition correſponding to this was *ſecundum*, evidently the gerund of *ſequor*, and ſignifying *following*. The analogy in the formation of the prepoſitions in the two languages was pretty ſimilar.

AGAIN, another ſecondary ſenſe which came to be affixed to κατὰ, was *down*. This application of the word aroſe, either, as Dr MOOR ſuppoſes, becauſe *down* being the natural direction which bodies take when left to themſelves, the word which denoted direction ſimply, was applied to ſignify this natural tendency; or, what is fully as probable, becauſe an object *lying* or *laid* down, occupies the loweſt poſition of which it is ſuſceptible. In whichever of the two ways it happened, it is certain, that a very common uſe of κατὰ is to ſignify *down*. Ὥρμησε κατὰ τοῦ κρημνοῦ, "he ruſhed down the ſteep,"—"he ruſhed down,—place "where the movement lay,—the ſteep,—direction,—that which "bodies take when left to themſelves,"—"or that which puts "them in the loweſt place they can occupy.

——— κατὰ δέ σφι κελαινὸν;
Αἷμ' ἀπελείβετ' ἔραζ'. HESIOD.

"The black blood flowed down them to the ground,"—"the "blood flowed,—place where the movement lay,—them,—di-

 "rection,—

"rection,—that which bodies take when left to themſelves,— "or that which puts a body in the loweſt place it can occu- "py."

Μετὰ.

THIS is one of the Greek prepoſitions which aſſume ſignifications apparently ſo remote, that, at firſt view, it is not eaſy to reduce them to one radical ſenſe, however confidently we may reſt upon the principle, that there muſt have been ſuch a radical ſenſe, from which the different ſignifications branched out. Μετὰ, it is well known, is commonly uſed in three different meanings; with the genitive denoting *with*, with the dative *among*, with the accuſative *after*. The analogy between the two former is ſufficiently obvious; but the coincidence of either of them with the third ſeems at firſt unaccountable. Upon examining the word, however, by the principles of etymology, perhaps this apparent incongruity may be accounted for.

Μετὰ appears to be an immediate derivative from the obſolete verb *μεω*, *I go*; a verb in common uſe in Latin, though in great meaſure diſuſed in Greek. Μετὰ being thus an immediate deſcendant from a verb ſignifying *to go* or *travel*, its primary ſignification moſt probably was *a way-poſt*, *a way-director*; a ſenſe nearly the ſame with what it ſtill retains in Latin, *meta*, *a goal*. From this ſenſe of way-poſt or way-director, *μετὰ* would ſoon be transferred to expreſs *a conductor of the way*, or *a guide*, of whatever kind this might be. Our own word *guide* ſeems to have been formed by a ſimilar analogy. It is *guida* in Italian, and *guia* in Spaniſh; which laſt clearly demonſtrates the origin of the whole to have been the Latin word *via*, "a way."

EXPLAINING *μετὰ*, then, by *guide* or *conductor*, its different uſes may be eaſily reſolved. With the genitive it denotes a conductor or guide who accompanies us, or whom we accompany; hence

hence it is ufually tranflated *with*. With the accufative it denotes a guide or conductor, who goes before us, or whom we follow; hence it is ufually rendered *after* *. With the dative it denotes a plurality of conductors, and that we are between or amidft them; hence it commonly ftands for *among*. Thus the three fignifications of *with*, *among*, and *after*, take their rife; agreeably to which we may analyze a few of the cafes in which μετὰ occurs, and thefe will fufficiently prove the juftice of the explanation.

1. Μετὰ, with the *genitive*, commonly denoting *with*. Thus, μετὰ Πλάτωνος ἀδικεῖν, "to do injuftice with PLATO,"—"to do in-"juftice,—conductor PLATO."

——— χαῖται ἐρρώοντο μετὰ πνοιῆς. HOMER.

"Their manes were fhaken with the wind,"—"their manes "were fhaken,—conductor the wind." Μεθ' ἡμῶν ποιήσασθαι τὸν "ἀγῶνα, "to engage in the conteft with us, or on our fide,"— "to engage in the conteft,—conductor in the conteft,—us." Ὁ ποίησας τὸ ἔλεος μετ' αὐτοῦ, (*Luk.* x. 37.), "he that fhowed "mercy on him,"—"that fhewed mercy,—guide or direction of "the channel in which the mercy was to flow,—him."

It is to be obferved, that, between a guide or conductor whom we accompany, and a mere companion in the way, the difference is very often hardly perceptible. It was very natural, therefore, in their mode of applying μετὰ, to flide from the one to the other. Hence μετὰ with the genitive, in a variety of inftances, denotes little more than a mere conjunction, or accompaniment, the ftrict idea of guiding or directing being thrown

 into

* AFTER, though the common, is not the univerfal fenfe of μετὰ with the accufative; fometimes, with this cafe, it fignifies the fame as with the dative, Βάκτρον ἦν αὐτῷ μετὰ χεῖρας, (HERODIAN), "he had a ftaff *between* his hands."

into the ſhade. Σώφρονες μετ᾽ ανδρείας, καὶ ἀνδρείοι μετὰ σώφροσυνης, "temperate with fortitude, and brave with temperance,"—"temperate,—companion, fortitude,—and brave,—companion, "temperance." Τὸ ΑΕΚ τρίγωνον μετὰ τῦ ΚΖΓ τριγώνȣ, (EUCLID.), "the triangle AEK, with the triangle KZΓ,"—"the triangle "AEK companion or accompanier the triangle KZΓ."

Μετὰ βάρβιτων ἄειδων. ANACR.

"Singing with the lyre,"—"ſinging,—accompaniment,—the "lyre."

2. Μετὰ with the *dative*, ſignifying uſually *among* or *between*.

Μετὰ πρωτοῖσι πονεῖτο. HOMER.

"He was buſy among the foremoſt,"—"he was buſy,—conduc-"tors or companions ſurrounding,—the foremoſt."

Οὖτιν᾽ ἐγὼ πύματον ἔδομαι μετὰ οἷς ἑταροισιν. HOMER.

"I will devour Noman the laſt among his aſſociates,"—"I "will devour Noman laſt,—conductors or companions in de-"ſtruction,—his aſſociates." Τὸν μὲν μετὰ χερσὶν ἐρυσάτο, "he "caught him between the hands,"—"he caught him,—guides "in catching him, or objects on either ſide of,—the hands." Δεινὸν δ᾽ ἐςί θανεῖν μετὰ κύμασι, "it is a terrible thing to die by "or among the waves,"—"to die,—conductors to death, or ob-"jects ſurrounding in death,—the waves."

3. Μετὰ with the *accuſative*, uſually rendered *after*.

Πρόσθε μὲν ἱππῆες, μετὰ δέ νέφος εἵπετο πεζῶν. HOM. *Il.*

"firſt the horſe,—and after them followed a cloud of foot-ſol-"diers,"—"firſt the horſe,—and a cloud of foot-ſoldiers follow-"ed,—conductors or guides,—them." Οἱ δὲ μετὰ τȣ̃τȣς ὀκτὼ, (ARCHIMED.), "the eight after them,"—"the eight coming,—"conductor preceding them,—them." Μετὰ τὸν πόλεμον ἥκειν, (PLATO),

(PLATO), "to come after the war,"—"to come,—leader or pre-"deceffor in point of time,—the war." Μετὰ τὴν θλίψιν τῶν ἡμερῶν ἐκείνων ὁ ἥλιος σκοτισθήσεται, (LUK. xxiv. 29.), "after the tribu-"lation of thefe days the fun fhall be darkened,"—"the fun "fhall be darkened,—conductor preceding this event,—the tri-"bulation of thefe days."

Οἷοί καὶ Δαναοῖσιν ἀριστῆες μετέασι καὶ μετ' Ἀχιλλῆα. HOM.

"What chiefs there are among the Greeks even after ACHIL-"LES,"—"what chiefs there are,—perfons attending them, "or accompanying them,—the Greeks,—even conductor pre-"ceding them,—ACHILLES."

As any object we are in purfuit of, and feek to obtain, may with great propriety be faid to be the conductor that we follow, μετὰ with the accufative is often ufed to exprefs fuch an intention or action.

Ὅς με μετ' ἀπρήκτους ἔριδας καὶ νείκεα βάλλει. HOM. *Il.*

"Who drives me in purfuit of vain quarrels and contefts,"—"who drives me,—object to which I am driven,—or which I am "made to follow,—vain quarrels and contefts."

——— τήν ποτε Νηλεύς,
Γῆμεν ἑὸν μετὰ κάλλος.

"Whom NELEUS married on account of her beauty,"—"whom "NELEUS married,—object of his purfuit in doing fo,—or ob-"ject by which he was led,—her beauty."

Ζεὺς γὰρ ἐπ' ὠκεανὸν μετ' ἀμύμονας Αἰθιοπῆας,
Χθιζὸς ἔβη μετὰ δαῖτα. HOM. *Il.*

"For JUPITER was gone the day before towards the ocean, "among the virtuous Ethiopians, to hold a feftival,"—"JUPI-

"TER,

"TER was gone,—conductors in respect to place,—the Ethio- "pians,—conductor in respect of object in going,—to hold a "festival."

Μετὰ having often the sense of *after*, frequently transfers that sense to the words with which it is joined in composition. It thus, in many cases, comes to express a change having taken place, an event different from the one that preceded it. Νοέω, "I consider." Μετανοέω, "after one mode of thinking I go to "another,—I change my mode of thinking,—I repent." Τίθημι, "I place." Μετατίθημι, "after having placed the objects in one "mode, I pass to another mode of placing them,—I transpose." When μετὰ in composition denotes participation, this is nothing more than a transferring of its other signification *among* or *with*. Ἔχω, "I have," μέτεχω, "I have along with,—I partake."

WHETHER alone or in composition, therefore, μετὰ appears to be always a substantive noun, denoting guide, conductor, or leader in the way.

Παρὰ.

IT seems to be allowed by the most accurate Greek grammarians, that the radical meaning of παρὰ, in one modification or other, is *besides*. This, I think, brings us pretty near the primitive idea which it was used to express. LINNEP derives it from the adjective παρὸς, *before*; and SCHEIDE, both παρὰ and παρὸς, from the verb πάω, *to press*. Both these, however, appear rather wide of the proper sense of the preposition. Παρὰ, in fact, appears to be an old substantive noun, denoting precisely *side* or *flank*. It must be admitted, that no direct examples of παρὰ, in this form of a noun, are to be found, at least I have not been able to meet with any such; but the sense of the word, when it occurs as a preposition, whether by itself or in composition, sufficiently indicates its original form; and though, as a noun, it does

does not itſelf occur, yet in ſome of its immediate derivatives, traces of the ſame ſignification remain. Thus, in Greek, we have παρειά, a *cheek*, and in Latin we find the adjective *par*, "equal," both evidently deſcendants from this root; the former denoting the ſide parts of the head, the latter an object correſponding ſide for ſide with another. As, therefore, the Engliſh prepoſition *beſide*, is nothing but a contracted form of *being ſide*, that is, being the ſide of an object; ſo the Greek prepoſition παρά, beſide, may juſtly be ſet down as a noun ſignifying *ſide* or *flank;* and from the different aſpects under which an object may be viewed, as occupying the ſide of another, the different applications of παρά take their riſe.

In the mathematical writers this is plain. Παρὰ τὴν δοθεῖσαν εὐθεῖαν παραλληλόγραμμον παραβαλεῖν, (Euclid), "to conſtruct a "parallelogram *upon* (as commonly rendered) a given ſtraight "line,"—"to conſtruct a parallellogram,—ſide of it,—a given "ſtraight line." And in compoſition, παραπληρώματα παραλληλογράμμου, "the *complements* of a parallelogram," as commonly rendered, mean exactly the *ſide fillers*.

In writers of a different claſs the ſame meaning appears. Κολοιὸς παρὰ κολοιὸν ἱζάνει, (Aristot.), "daw ſits beſide daw,"—"daw ſits,—ſide,—daw."

——— παρὰ κρόταφων τε παρειάς. Homer.

"The cheeks beſide the temples,"—"the cheeks,—ſide,—the "temples."

——— αγορὴν ἡ παρὰ νηυσι τέτυκτο. Hom. *Od.*

"The market-place which was formed beſide the ſhips,"—"which was formed,—ſide or flank of the market-place,—ob-"ject by which the market-place was flanked,—the ſhips."

Βῆ

Βῆ δ' ακεών παρὰ θῖνα πολυφλοίσβοιο θαλάσσης. HOM. *Il.*

"He went in ſilence beſide the ſhore of the far reſounding "ocean,"—"he went,—ſide of his courſe, the far reſounding "ocean."

FROM this radical meaning of παρὰ, the tranſition was ſhort and eaſy to apply it to expreſs a particular *ſide or party*, when any oppoſition was ſpoken of. Ἰέναι παρὰ τῷ Τισσαφέρνει, (XENOPH.), "to go over to TISSAPHERNES,"—"to go,—ſide to "which he goes,—that of TISSAPHERNES." Ἐςί κ̀ παρ' ἐμοί τις ἐμπειριά, (DEMOST.), "there is alſo ſome experience on my "ſide,"—"there is ſome experience alſo,—ſide me."

ANOTHER tranſition, perhaps equally eaſy, was to apply παρὰ in expreſſing ſome *particular view* taken of an object. Here, as before, the idea is derived from *ſide*; the ſubject being ſuppoſed to have ſeveral ſides on which it may be viewed, and the one ſpoken of the particular one immediately in view; as,

Θεός ἐδοκᾶν παρὰ τὸ μεγέθος τῶν πεπραγμενῶν. LUCIAN.

"I ſeemed a god for the greatneſs of my actions,"—"I ſeemed "a god,—ſide on which I was viewed,—the greatneſs of my ac-"tions."

IT was natural, too, to apply παρὰ, when *contraſt or compariſon* in any manner was to be denoted; the two objects contraſted being conſidered as placed ſide by ſide, for the purpoſe of more accurate obſervation. Παρὰ τά ςυφρὰ ὁ οἶνος γλυκύτερος, (ARISTOT.), "wine is ſweeter contraſted with bitter things,"—"wine is ſweeter, bitter things being ſet by its ſide."

Ἔχει τὶν' ὀγκόν Ἀργος Ἑλληνῶν παρὰ. EURIP. *Phon.*

"Argos has ſome ground of boaſting beſide the Grecians,"—"object ſet by its ſide,—the other Grecians." Ὡραῖος κάλλει παρὰ

παρὰ τὺς υἱὺς τῶν ανθρώπων, (*Psalm* xlv. 2.), "fair among the chil-"dren of men,"—"fair,—objects placed beside,—the children of "men."

We find παρὰ also applied in two different modes, which at first sight might be deemed opposite, but, rightly explained, are found to accord. It is used sometimes to express that a thing falls short of, sometimes that it goes beyond, the object with which it is connected. Τεσσαράκοντα παρὰ μίαν, (2 *Cor.* xi. 24.), "forty bating one,"—"as far as one on this side of forty *." Παρὰ τὸν ποταμὸν ἔφευγον, "they fled beyond or to the farther side "of the river,"—"side where their flight terminated,—the ri-"ver." Παρὰ, in both these cases, radically means *at the side of*, and nothing more; the particular side, whether nearer or more remote, being left to be discovered from the connection of the sentence †.

With the genitive, παρὰ pretty frequently, though not universally, is made to signify *from at*, or *from beside* an object. Παρὰ βασιλέως πορευόμενον, "coming from beside the king." In such phrases where it is evident a complex sense is affixed to the word, I suspect a compound preposition has originally been in use, viz. παρὲκ, a preposition still frequently met with, and exactly expressing this idea. By degrees, it is probable the εκ came to be dropped, the meaning in most cases being sufficiently plain from the connection. Such omissions may be found in every language, and, to those accustomed to the idiom, they occasion no ambiguity.

Παρὰ in composition frequently denotes *some deviation from* the exact idea expressed by the connected word. Λογίζομαι, "I "reckon;"

* This, I believe, however, is a mode of expression not very common.

† "Duas significationes contrarias patitur," says Vigerus, "*supra* et *infra*," but does not attempt by any explanation to reconcile this apparent contradiction.

"reckon;" παραλογίζομαι, "*I reckon false, I misreckon.*" The idea here is plain: the primitive word is supposed to mark a direct course, and παρὰ, signifying *side*, marks, that instead of holding this direct course, the person goes to a side.

We find, then, παρὰ, in all its different applications, retaining its original meaning of a substantive noun, denoting *side*, in some one particular point of view.

Περὶ.

This is a preposition of more uniform and simple application than the preceding. It is evidently an immediate cognate of πέρας, a *boundary*, and I should conjecture it to be the dative of this very noun, contracted from πέρατι and πέραι to περὶ. Its precise meaning, therefore, is *circumference*, a word equivalent to *entire boundary*. Hence, it is easy to see, how ἀμφὶ and περὶ come to have nearly the same signification; the former expresses *the object contained;* the latter, its correlative, *the containing boundary*. Περὶ, therefore, may be rendered, in general, *circumference* or *boundary*, or *bounder*.

It is thus that the sense of it is to be resolved.

——— περὶ σπέους. HOMER.

"Round the den,"—"bounding or surrounding—the den."

——— ἔνδυνε περὶ στήθεσσιν χιτῶνα. HOM. *Od.*

"He put a tunic round his breast,"—"he put a tunic, forming "the boundary of his breast."

——— περὶ ῥοδέοισιν ἔερση τήκεται. APOLLON. *Rhod.*

"The dew distils round the roses,"—"the dew distils,—a circumference to the roses,"—"forming a circumference to the "roses." Τῶν περὶ τὴν διάμετρον παραλληλογράμμων, (EUCLID), "of

" of parallelograms round the diameter,"—" parallelograms,— " boundary of the diameter,—forming the boundary of the dia- " meter." Περὶ τρίγωνόν κύκλον περιγράψαι, (EUCLID), " to de- " fcribe a circle round a triangle,"—" to defcribe a circle,—a " circumference or boundary of the triangle."

THOUGH ἀμφὶ and περὶ, as commonly ufed, are almoft fynonymous, yet we fometimes find the poets, to make their expreffion more copious, join both prepofitions with the fame noun.

——— Ἡμᾶς δ' ἀμφὶ περὶ κρήνην ἱερὰς κατὰ βωμὰς,
Ἔρδομεν. HOMER.

" We were facrificing round about the fountain and the facred " altars,"—" we were facrificing,—place comprehended by us, " —the circumference of the fountain and the facred altars."

Περὶ, fignifying circumference or boundary, is fometimes applied to denote *near;* the furrounding object, in this cafe, being fuppofed to go about the other without precifely touching it. Ἐπίπτε περὶ τὸ δένδρον, " he fell near the tree,"—" he fell,—in the " fpace bounding or boundary of the tree."

IT is chiefly when applied to time that this laft fenfe of περὶ occurs. Περὶ μεσημβρίαν ἦλθε, " he came about noon,"—" he " came,—time in which his coming happened,—the period fur- " rounding noon,—not actually at it."

IT was by an eafy and obvious tranfition from the radical fenfe, that περὶ was made to exprefs *concerning* or *about* a fubject, meaning the fubject which the thought or the difcourfe comprehends or forms the boundary of. Γογγυσμὸς πολὺς περὶ αὐτȣ̃ ἦν, (*Jo.* vii. 12.), " they murmured much concerning him,"— " they murmured,—the murmur comprehending him." Περὶ τῇ πόλει δεδιέναι, (THUCYD.), " to be afraid for or concerning " the city,"—" to be afraid,—object which the fear included or " comprehended,—the whole city."

Περὶ ſometimes, both ſimply and in compoſition, denotes *ſuperiority*, plainly from the idea, that the object which forms a boundary or circumference to another, muſt of courſe exceed it in bulk or greatneſs.

———— Ἐςὶ περὶ νόον βροτῶν. LUCIAN.

"It is beyond the comprehenſion of mortals,"—"it is ſuch that "it is capable of going round, and conſequently exceeds the "comprehenſion of mortals."

It was obſerved, in the remarks upon ἀμφὶ, that in Latin *circa* and *circum* expreſſed the idea of *round* or *round about*. Theſe are both nouns, preciſely ſimilar in their force and meaning to περὶ.

Πρὸ.

Πρὸ is ſuppoſed by LINNEP to be the ſame with πρὸς, and by SCHEIDE to be a part of the adjective πάρος, *before*. The former appears evidently erroneous, the two prepoſitions, though ſo nearly related in ſound, being in fact very different, as we ſhall find, both in origin and meaning. The latter idea, however, that πρὸ is a part of παρος contracted, ſeems extremely probable. The adjective πάρος, ſignifying properly *fore*, is ſtill in uſe. In ſpeech this word, like many others, might ſuffer an abbreviation of the firſt ſyllable, and be pronounced πρὸς inſtead of πάρος. From this contracted adjective we have the comparative πρότερος, *prior*, and the ſuperlative πρότατος, contracted into πρῶτος, *foremoſt* or firſt. Of this adjective πρὸς, the prepoſition πρὸ appears to be the dative, ſignifying, therefore, with a ſubſtantive underſtood as in other inſtances, the *fore part*, *fore object*, or *object in front*; and as it was always followed by the genitive caſe of the ſucceeding noun, and the genitive caſe in Greek has uſually the ſame force with the particle *of* in Engliſh, viz. *ſpecies* or *kind*,

kind, the Greek prepoſition πρὸ denotes preciſely *fore object in reſpect to.*

Thus we find, in reſpect to *place*. 'Εϛηκε πρὸ τȣ͂ πυλῶνος, "he "ſtood before the door,"—"he ſtood,—front object in reſpect "to him,—the door."

In reſpect to *time*. 'Εγένετο πρὸ τȣ͂ πολέμȣ, "it happened "before the war,"—"it happened,—being a fore event in "reſpect to the war."

In reſpect to *preference*. 'Αρετὴ πρὸ χρήματων, "virtue be-"fore riches,"—"virtue, an object in ſelection preceding riches."

In reſpect to *defence or protection*. Πρὸ τῶν παίδων ϗ τῶν γυναικῶν μάχεσθαι, "to fight in defence of wives and children,"—"to fight,—being the front object in reſpect to your wives and "children."

In transferring this original word πάρος from the Greek, the Latins have been ſo accurate as to form two prepoſitions from the primitive adjective; *præ*, derived from the feminine πρη, and retaining the literal meaning of *fore* or *before*; and *pro*, derived from the maſculine or neuter, uſed chiefly in the ſecondary, but more extended ſenſe, anſwering to the Engliſh *for*, *on account of*.

I may add here, that our Engliſh prepoſition *for*, appears to have a ſimilar origin with the Greek πρὸ. In the Ἔπεα Πτερόεντα, indeed, a different ſource is aſſigned to the Engliſh *for*; it is ſaid to be an abbreviation of the Saxon *farina*, a *cauſe*. Had Lord Monboddo or Mr Harris aſcribed to it ſuch a genealogy, how would Horne Tooke and Dr Beddoes have ridiculed the fancy of originating a prepoſition from the abſtract and metaphyſical idea of cauſation. In fact, this derivation is far-fetched, and it evidently required no ſmall ſtraining to twiſt ſome of the applications of *for* into the line of cauſe and effect *. Without going ſo far, a plain and obvious

origin

* Thus,—"This may do *for* once, but not *for* ever,"—"*cauſe* once,—or *cauſe* "ever," would give hardly any ſenſe. "It is good *for* nothing,"—what mean-

ing

origin of it may be found in the word *fore*; and upon examination I think it will be found, that *for* in Englifh, and πϱὸ in Greek, and *pro* in Latin, as well as the German *vor* or *für*, means, in its radical fenfe, *pofition before*; with this difference only, that in Latin, Englifh, and German, the word denoting the fore object generally follows the prepofition, but in Greek precedes it. In Englifh, therefore, the prepofition *for* may be always taken as meaning fimply *object*. Subftitute *object* in all the phrafes in which the author of the *Diverfions of Purley* has put *caufe* as the meaning of *for*, and it is eafy to obferve how naturally and exactly the fentences may be refolved.

Πϱὸς.

Πϱὸς is evidently a prepofition of very extenfive application, and in inveftigating its original meaning, a variety of opinions have prevailed. Dr Moor refolved it into πϱὸ and εἰς, and fuppofed that it denoted properly *in the fore fpace of*. Scheide afcribes to it a meaning fomewhat fimilar, deducing it from πάϱος, *before*. Both thefe derivations, however, are unfatisfactory. In fome of the modes in which πϱὸς is ufed, the idea of *before* may indeed be traced; but, in others, it cannot, I think, without the moft violent ftraining, be difcovered. It will be neceffary, therefore, to look out for a different origin, and one carrying with it an idea more confonant to the fignification and ufes of the prepofition. This, I think, can only be found in the noun

ing could here be affigned to—"*caufe*, nothing?" "He is tall *for* his age,"—"*caufe*, his age," would denote fomething quite different from what is intended. "*For* a good harveft, a good fummer is neceffary,"—"*caufe*, a good harveft," would completely invert the meaning, making the effect the caufe. "He lived "there *for* twenty years,"—"*caufe*, twenty years," would be unintelligible. In thefe inftances, taking *for* to mean *object placed before*, or *object in view*, the fenfe is obvious. The fame may be faid of fuch phrafes as the following: "He is a "good man *for* ought I know,"—"he is a good man,—*object*, or put in objection, "ought I know," an eafy and plain refolution; whereas, "*caufe*, ought I know," would be extremely forced, if not altogether unmeaning.

noun *πεϱας*, *a termination*, *limit*, or *terminating boundary*. This noun in the genitive forms *πεϱάτος*, which, according to the common analogy and practice of the language, would be gradually contracted into *πεϱάος* and *πεϱῶς*, and lastly abbreviated into *πϱῶς* or *πϱὸς*. If this deduction be correct, (and I think it bears the marks of truth), the radical sense of the preposition *πϱος* must be exactly *termination*, or *terminating in*. From this idea all its different senses may naturally be traced.

In the mathematical writers, we find *πϱὸς* retaining its primitive meaning. *Ἁι πϱὸς τῇ βάσει γωνίαι*, (Euclid, L. 1. p. 4.), "the angles at the base,"—"the angles,—termination,—the "base,—terminating in the base." *Τὴν πλευϱὰν πϱὸς ταῖς ἴσαις γωνίαις*, (Euclid, Lib. 1. pr. 26.), "the side *betwixt* the equal "angles," (as commonly rendered), "the side,—termination,— "the equal angles,—the side whose terminations are the equal "angles." *Πϱὸς τὰς τῶν κύκλων πεϱιφεϱείας εὐθεῖαι πϱοςβλήθωσιν*, (Theod. Lib. 2.), "let straight lines be drawn to the periphery "of the circle,"—"let straight lines be drawn,—termination,— "the periphery of the circles."

The application of *πϱὸς* to other sensible objects, is resolvable in a somewhat similar manner.

——— *πϱὸς μὲν ἁλὸς Κᾶϱες*. Homer.

"The Carians at the sea,"—"the Carians,—boundary the sea, "whose country is bounded by it."

——— *ποτὶ πὸλιος πέτετ' αἰεί*. Homer.

"He still continued flying towards the city,"—"he continued "flying,—termination of his flight,—the city."

Ἁι μὲν πϱὸς βοϱέαο καταιβαταὶ ἀνθϱώποισιν. Homer.

"Those to the north are passable for men,"—"those,—termina- "tion

"tion the north,"—or "boundary,—the north,"—are paſſable "for men."

——— νῆας ποτὶ σπιλάδεσσιν ἔαξαν,
Κύματα. HOMER.

"The waves daſhed the ſhips againſt the rocks,"—"daſhed the "ſhips,—termination or terminating point of the daſhing,—the "rocks." Πρὸς κέντρα λακτίζειν, (*Acts* ix. 5.), "to kick againſt "the ſpikes,"—"to kick,—termination of the action,—the "ſpikes."

FROM ſenſible objects, πρὸς was transferred to objects of different deſcriptions, where the original idea denoted by it was meant to be expreſſed or implied. Οὐ πρὸς ὑμῶν ἐστι, (ISOC.), "it "does not belong to you,"—"it is not a thing whoſe termina-"tion is you." Οὐ νομίζουσιν τὴν αρετὴν πρὸς τῷ σφετέρῳ ἀγαθῷ πεφυκέναι, (XENOPH.), "they do not reckon virtue for their good," "—they do not reckon virtue as terminating in their good."

Πρὸς Διὸς εισὶν ἅπαντες. HOMER.

"All are from JOVE,"—as commonly rendered,—"all refer to "JOVE as the centre in which they terminate." Πρὸς ἀνδρῶν ἔχων τὴν συγγενείαν καὶ οὐ πρὸς γυναικῶν, (DEMOST.), "having relationſhip "on the male, not the female ſide,"—"having his relationſhip, "—termination or boundary,—point to which it is to be tra-"ced back,—the male, not the female ſide." Πρὸς τοῖς εἰρημένοις, "in addition to what has been ſaid,"—"boundary or termina-"ting point to which the ſucceeding remark reaches,—what "has been ſaid."

——— πρὸς αἴσχεσιν ἄλγεα πάσχει. HESIOD.

"He endures afflictions along with diſgraces,"—"he endures "afflictions,

"afflictions,—bounding point,—point to which they reach,—
"disgraces."

We find πρὸς frequently applied to denote that one object is affected, regulated or governed by another; because, in the regulating object, we find the boundary which limits and defines the idea in view. Ἐὰν πρῶτον πρὸς δεύτερον τὸν αὐτὸν ἔχῃ λόγον, (EUCLID), "if the first have the same ratio to the second,"—"if "the first have the same ratio,—defining boundary of the ratio, "—the second." Ποιήσας πρὸς τὸ θέλημα, (*Luke* xii. 47.), "having acted according to his will,"—"having acted,—boundary or limits within which the action was confined,—his "will."

IN this manner may πρὸς, varied and anomalous as it appears, be traced through all its applications to one determinate idea.

IN support of the opinion now stated, that *boundary*, *limit*, or *termination*, was in fact the radical sense of this preposition, it may be added, that among the most ancient Greek writers, the poets, the word ποτὶ was used as synonymous with πρὸς. This is commonly said to be according to the Doric dialect; but so great a change of the word does not, I think, fall within the usual rules of formation of that or any other dialect of the language. The real cause of the interchange I suspect to have been, that ποτὶ and πρὸς, though two quite different words, came to have the same meaning affixed to them in the following manner. Πρὸς, as we have seen, is a contracted case of πέρας, a termination or boundary; ποτὶ, again, seems to be just the Doric dative of πὺς, "the foot," for ποδὶ, agreeably to the common analogy of that dialect. That the word signifying the extremity of the human body should be extended in use to denote a termination in general, was perfectly natural, as it can hardly be doubted, that in many instances the names of the parts of the human body were transferred by analogy to express the different forms of position in sensible objects of every description.

Σὺν.

Σὺν, in regard to its application, is one of the ſimpleſt of the Greek prepoſitions; its radical and primitive meaning being in every caſe eaſily perceptible. Its origin appears to be in the verb σύω, to *ſew*, or to *join together*,—a verb which though obſolete and out of uſe in Greek, being ſupplanted by the ſynonymous word ῥάπτω, has, however, been retained unchanged in Latin, a circumſtance which warrants the inference, that in the earlier ſtages of the Greek language, when the Æolic, the immediate parent of the Latin, was the common dialect, σύω, in the ſenſe of *joining*, was in general uſe. Of this verb σύω, συν ſeems to have been a contracted participle paſſive; and thus in its radical meaning denotes *joined*, *object* or *circumſtance joined*, the analogy being the ſame with what has been ſhewn in the Ἔπεα Πτερόεντα, to take place in regard to the Engliſh prepoſition *with*, a part of the verb *withan* to join.

From this radical ſenſe of σὺν all its various uſes naturally take their riſe, all of them implying junction in place, time, or acting. Ἐρχόμεθα ἡμεῖς σὺν σοί, (*John* xxi. 3.), "we come with "you,"—"we come,—object joined,—you." Τὸ ὑπό τῆς ὅλης σὺν τῇ προσκειμένῃ, (EUCLID), "the ſquare deſcribed by the "whole line, together with the part added,"—"the whole line, "—object joined thereto,—the added part." Σὺν πᾶσι τούτοις ἄλλο κακόν ἐγένετο, "together with all theſe another misfortune "happened,—"another misfortune happened,—circumſtance "joined,—all theſe before enumerated." Σὺν τῷ δειπνεῖν ἀπῆλθε, "he went away in time of ſupper,"—"he went away,—event "joined in time to his going away,—ſupper."

Σὺν ᾧ καὶ Ὕλας κίεν. APOLLON.

"With whom went HYLAS."—"HYLAS went,—perſon joined "him."

"him." Σὺν σοὶ μάχοιμην, "I would fight with your aid,"—"I would fight,—perſon joined to me in fighting,—you." Ὥς οἱ εἴη σὺν Θεῷ εἰρημένον, (HERODOT.), "that it was ſpoken to "him by divine authority,"—"that it was ſpoken,—circum-"ſtance joined thereto,—divine authority."

Ὑπέρ.

THE only two remaining prepoſitions in Greek are ὑπὲρ and ὑπὸ. Theſe are evidently derived from the ſame root, and in their radical ſenſe denote the ſame idea, though, by a different mode of applying it, their common acceptation has the appearance of being nearly oppoſite to each other. The original of both was the adjective ὕπος, ſignifying *high*. Of this adjective, though fallen into diſuſe, there are many immediate derivates and cognates to be found: Ὕπος, *high* or *ſublime*, *height* or *elevation*; ὑψηλὸς, *lofty*; and directly ariſing from the primitive word, the comparative ὕπερος, contracted from ὑποτερος, *higher*, and the ſuperlative ὕπατος, contracted from ὑποτατος, *higheſt* or *ſupreme*. From the comparative ὕπερος, comes the prepoſition ὑπὲρ, radically denoting a *higher object*, hence commonly uſed to expreſs *placed over* another. The analogy of the Engliſh prepoſition was ſimilar; *over* being the comparative of the adjective *op* or *ob*, aſpirated into *ov*, whence by a regular formation came the comparative *over*.

Ὑπὲρ, then, appears to have originally denoted, that one object was *higher* in reſpect of, or in compariſon with another with which it was connected; and, in this caſe, the noun to which the radical force of the prepoſition refers, is the one that *precedes* it in the ſentence, differing in this from moſt of the other prepoſitions, and in particular, as we ſhall immediately ſee, from its cognate ὑπὸ, which generally refers to the one that comes *after*.

 Taking

Taking ὑπὲρ, then, as denoting ſimply *higher in reſpect of*, we ſhall find all its different uſes ſatisfactorily explained.

1. *Over* or *above*, in place:

Στῆ δ' ὑπὲρ κεφαλῆς. HOMER.

"He ſtood over his head,"—"he ſtood,—higher in reſpect of "his head,—higher than his head." Ὑπὲρ τῶν κήπων ȣ̃ρος κᾶται, (HERODOT.), "the keeper lies above the gardens,"—"the keep-"er lies,—higher in reſpect of the gardens."

2. *Superior to*, in point of operation. Τὰ ὑπὲρ ἡμᾶς ȣ̓δὲν πρὸς ἡμᾶς, *(Proverb)*, "the things beyond or above us are no-"thing to us,"—"the things,—higher in regard to their effects "than us, are nothing to us."

——— ὑπὲρ μοῖραν. HOMER.

"Superior to fate,—controlling fate,"—"higher in reſpect to "operation than fate."

3. *Beyond*, from the uſual analogy, by which the ſpeaker conſiders himſelf as the centre of obſervation, and conſequently expreſſes himſelf as if the more remote a place was from him it became as it were higher in the circle in reſpect to him. Αἰθίοπιας τῆς ὑπὲρ Αἰγύπτȣ, (THUCYD.), "Æthiopia which is beyond E-"gypt,"—"Æthiopia, which is farther from the centre of ob-"ſervation, conſequently higher in the circle, in reſpect to E-"gypt."

4. *In defence of*, becauſe a thing which defends another covers it from aſſault, and what covers is naturally accounted *higher*. Ὑπὲρ σȣ̃ μάχομαι, "I fight in defence of you,"—"I "fight to cover you, conſequently in fighting am higher than "you."

5. *In room of*. In this application of ὑπὲρ, I ſuſpect there is an ellipſis of ſome ſuch word as τόπȣ, *place*; ὑπὲρ here implying as

it

it were *over the place from which another is removed*, the primitive idea of *higher* being ftill, though more remotely, retained. Ὑπὲρ σοῦ διακονεῖ, " he ferves in room of thee,"—" he ferves,—coming " over the place from which you have been removed,—or which " you have left unoccupied."

6. *Concerning*, by a fimilar analogy to what takes place with us when we fay, *to think over*, *to talk over* a fubject, the thought or difcourfe being fuppofed, as it were, to cover the fubject thus thought or fpoken of. Ὅσα ὑπὲρ τῆς εἰρήνης κατεψεύσατο μοῦ, (DEMOST.), " what he falfely charged me concerning the peace,"— " what he charged,—fubject of the charge covering or going " over the peace."

Thus, in all the different applications of ὑπὲρ, we find the radical idea denoted by it is *higher*, in fome cafes more directly, in others extended by analogy to exprefs fome particular view in which one object either is made to be, or is confidered as being higher than another.

Ὑπό.

IN analyzing ὑπὲρ, it was found to be the comparative from the adjective ὕπος. Of this adjective the prepofition ὑπὸ appears to be the dative, with fome common word denoting place or pofition underftood. In the application, however, of ὑπὸ and ὑπὲρ as prepofitions, the fignification of the two, though originating from the fame root, was reverfed; in the one cafe, the attribute being referred to the noun preceding; in the other, to its correlative, the noun following. In thefe, therefore, there is no contradiction, but merely a diverfity in the mode of application. *High* and *low*, *above* and *under*, are merely relative terms. When one object is *low* or *under* in reference to another, this laft, of confequence, comes to be *high* or *above* in reference to the firft. Such a pofition of two objects, therefore, may be equally expreffed by faying, that the firft is above the fecond, or the

the ſecond is under the firſt. In the prepoſition ὑπὲρ, the Greeks uſed the one mode of expreſſion; in the prepoſition ὑπὸ the other. In the Engliſh prepoſitions by which theſe are rendered, a ſimilar analogy in both caſes takes place.

As the adjective ὕπος, therefore, originally and literally ſignified *high*, the prepoſition ὑπὸ has radically the ſame meaning, and to this primary idea its various uſes have a direct reference.

1. LITERALLY and moſt uſually, it denotes *under* or *below* in point of *place*, from the analogy already explained. Καὶ τὸν ὑπὲρ γῆς κ̀ ὑπὸ γῆν χρυσὸν, "the gold both above and under the "ground,"—"the gold,—higher in reſpect of the ground, and "the gold in regard to which the ground is higher."

Ἅμα κάτεκειντο, ὁ μὲν ἄνω, ὁ δ' ὑπ' αὐτὸν. LUCIAN.

"They ſat down together, the one above, the other below him, "(ZENO),"—"they ſat down together, in ſuch a poſition, that "the one was above ZENO,—and ZENO was high—in regard to "the other."

Ἄλλο μὲν ἐξεσάωσεν ὑπ' ἰλὺος. APOLLON.

"He ſaved one ſandal from under the mud," "he ſaved one "ſandal,—place quitted,—the ſituation in which the mud was "high in regard to it."

Ἔνδον ἔμιμνε πίθε ὑπὸ χείλεσιν. HESIOD.

"It remained within, under the edges of the veſſel,"—"it re-"mained within, in ſuch a ſituation that the edges of the veſſel "were high, or in a high poſition in regard to it."

BY an eaſy extenſion of this meaning, ὑπὸ came to denote that one object was in any way covered or hid by another. Καὶ μιν κατακρύπτει ὑπὸ τὴν θύραν, (HERODOT.), "he conceals him behind "the door,"—"he conceals him in ſuch a poſition that the door

"covers

" covers him from view,—consequently, in that sense, may be " said to be above or high in regard to him."

2. It is applied in some particular cases to denote a portion of time. The idea seems to have been expressed elliptically in such cases, something more than bare duration being implied; an intimation being also given, that the portion of time so marked out was distinguished by some property conducive to the action.

——— ἦλθε ὑπὸ νυκτι. Apollon.

" He came *under* night," *i. e.* " under cover of night." Ἀπέβη δὲ ὑπὸ σκότυ, " he went off under darkness,"—" under cover of " darkness." Here the primitive idea of ὑπὸ is still discernible, though more remotely marked; the properties which distinguish night or darkness being reckoned the cover or protector of the coming or going off. By a similar analogy arises our English expression *under* night.

3. It sometimes expresses the relation of *superiority* and *subjection*. The connection with the primitive idea is here perfectly obvious. Αἴγυπτος δὲ πάλιν ὑπὸ βασιλέος ἐγένετο, " Egypt came " again under the government of a king,"—" Egypt came to be " again in the situation in which a king was the high or govern- " ing power in the state." Ὑπὸ Πέρσησιν ἐςὶ Αἴγυπτος, (Herod.), " Egypt is under the dominion of the Persians,"—" Egypt is in " such a situation that the Persians are high in regard to it." Ἔχων ὑπ' ἐμαυτὸν ςρατιώτας, (*Math.* viii. 9.), " having soldiers un- " der me,"—" having soldiers in such a state that I am high in " regard to them."

4. Ὑπο is not unfrequently applied to denote the *agent*, *instrument*, or *directing cause* of an action. This application probably arose from the idea, that as in the agent, instrument or directing cause, there was plainly implied some sort of power or dominion

over

over the action, these fell, therefore, to be viewed as the superior or higher objects in regard to it.

Ἔζεε δε χθὼν πᾶσα, κ̀ οὐρανὸς ἠδὲ θάλασσα,
Ῥιπῆ ὑπ' αθανάτων. HESIOD.

"All the earth, and heaven, and sea, were put in commotion under the force of the onset of the immortals,"—"the onset being the acting or presiding cause,—hence the higher power in regard to the effect."

Κέατο τεθνηῶτες ὑπὸ βλοσυροῖσι λέεσι. HESIOD.

"They lay dead by means of,—or under the force of,—furious lions,"—"the lions being the cause or instruments of the death, the power whence the death originated,—hence the higher power in relation to it." Ὑπὸ τῶν δικαςῶν ἐκπεσεῖν, "to go into banishment by the sentence of the judges,"—"to go into banishment,—the judges being the presiding power,—the superior power in regard to that effect." Ἀποθανεῖν ὑπό τῶν πολεμίων, "to fall by means of the enemy,"—"the enemy being the superior power in relation to that event."

In all its various applications, then, we find ὑπὸ a noun, retaining its primitive force; in some cases, it must be owned more remotely, but in all sufficiently plain to enable us to trace it by the principles of just grammatical theory.

Such, then, are the various prepositions in Greek, analyzed according to what I conceive the true principles of philosophical grammar. In the course of the investigation, it may be observed, no great weight has been laid on the particular cases of the

the nouns with which the different prepofitions are joined, and which, in technical language, they are faid to govern. In laying down the grammatical principles of the language, this is undoubtedly a matter of fome importance; but in inveftigating the origin and radical fenfe of the prepofitions, little light, I apprehend, can be derived from that circumftance. Even in regard to their fignification and ufe, it is far from being a certain or accurate guide. Dr Moor has indeed conftructed an ingenious theory about the mode of their application, deduced from the cafes they are found to govern; but the principles of this theory are in many cafes contradicted by the ufes of the prepofitions in the moft accurate Greek writers *. And, indeed, I fufpect the theory

* Dr Moor fuppofes, for inftance, that the following diftinction in the ufe of the prepofitions, with the different cafes, may be regarded as conftant and certain. With the *genitive*, he thinks, that the peculiar relation denoted by the prepofition is reprefented as in a ftate of reft or continued junction with the object: with the *accufative*, as in a ftate of tendency towards: with the *dative*, as in a ftate of junction under fome particular modification. Thus, he fays, επι with the genitive denotes reft or fituation upon; with the accufative, motion directed upon; with the dative, fome particular mode in which one object may be faid to be upon another. He applies the fame theory to ὑπὸ, and fome of the other prepofitions governing different cafes. But many examples might be brought in direct oppofition to this theory. Thus, in Thucydides, ἐπὶ Θρακῆς ϛρατείαν πάρασκευάζεται, "he prepares an "expedition,—*directed upon* or againft Thrace." 'Επὶ Αἰγυπτου ῥέειν, "to flow *to*-"*wards* Egypt." In Homer we find,

——— ὄυτω μοὶ ἐυκλεὴη εἴη επ' ἀνθρώπους.

"So may my fame be eftablifhed or exift among men,—refting upon or among "men." In like manner, we find ὑπὸ with the genitive denoting *tendency* under.

Ἱκάνε ὑπὸ Πριάμοιο. Quint. Smyrn.

"He comes towards the prefence,—or under the view of Priam." The fame prepofition occurs with the accufative very frequently, denoting reft or fituation under. Καταχρύπτει ὑπὸ τὴν θύραν, (Herod.), "he conceals it, refting under or "behind the door."

——— κατεκεῖντο

ory itſelf is too refined and philoſophical to be admitted as juſt in the uſe of a language modified by general uſe, and adapted to the common converſe of men. I have, therefore, all along, rather declined reſting on ſuch a ground, conceiving it in general to be uncertain and fallacious. I wiſhed, in tracing the real ſenſe and origin of the prepoſitions, to proceed, if poſſible, by a different, and what appeared to me a more ſatisfactory route. How far the track I have choſen has enabled me to reach the object in view, muſt be left to the judgment of thoſe beſt acquainted with the principles of the language.

APPENDIX.

—— κατέκειντο ὁ δὲ ὑπ' αὐτον. LUCIAN.

" They ſat down,—the one reſting or ſituate under him." Theſe examples, and many more might be produced, ſeem to prove the fallacy of the ingenious Profeſſor's theory, and ſhow that the Greeks were by no means ſo philoſophically accurate in the uſe of their prepoſitions as he ſuppoſed.

APPENDIX.

IN the outfet of the Inquiry, it was obferved, that the Greek language is a language of regular ftructure, forming its roots within itfelf. Following out this idea, I have, in different parts of the inveftigation, had recourfe to the line of cognates and derivatives, as the fureft aids for conducting us to the radical fenfe of a word, where the immediate root appeared to have fallen into difufe, or the fignification had diverged confiderably from the primitive idea. The grounds upon which I have thus proceeded, may require elucidation.

THE regular fyftem of Greek analogy upon which I have refted fo much, though actually, I am perfuaded, founded in nature and in truth, has been rejected by many, and even treated with ridicule by fome, no mean proficients in the language. The fanciful notions which a few zealous enthufiafts for the perfection of the Greek tongue have engrafted on the general theory, and the imperfect manner in which even its more temperate advocates feem to have underftood or explained it, have drawn upon the fyftem a degree of difcredit, which, if fairly examined, it will hardly, I think, be found to merit. HEMSTERHUIS and his fcholars, who appear to have firft developed it, were deficient as well in tracing this analogy through the minuter ramifications

 of

of the language, as in pointing out its real foundation in the natural progrefs of the human intellect, and the gradual formation of the figns of thought. They contented themfelves with merely giving the general outlines, and taking the facts as they found them occur in the actual ftate of the language. Lord MONBODDO, credulous in all things, and anxious to raife our admiration of the Greeks, maintained the ftrange conceit, that the Greek language was entirely a language of art, invented by men of profound fcience, who, in order to conftruct it, firft formed certain words as the roots from which the whole was to branch out; and then, by certain nicely regulated adjuncts of letters or fyllables, fabricated new words themfelves, and enabled others to form them as they were wanted. Hence, faid he, a complete and regular fyftem of analogy could not fail to take place, every part of the language being fo exactly adapted by thefe men of fcience to the whole and to every other part. Unfortunately, his Lordfhip neglected either to inform us where and when thofe fcientific architects of the language appeared, or to point out the rules of their procedure in this regular fabrication of words: he went no farther than to ftate the fuppofed primitive radicals they had formed, leaving to future inquirers to find out, as they beft could, their fubfequent ramifications. The ingenious author of the appendix to DAWES' *Mifcellanea Critica*, appears to have had a jufter idea of the manner in which fuch an inquiry ought to be conducted. In that appendix, he has given a general ftatement of the Greek radicals, announcing, at the fame time, a farther defign of an entire work, on the origin, progrefs, and connection of ideas, as difcoverable in the formation and ftructure of the Greek tongue. A few fpecimens of this projected analyfis are likewife fubjoined. This promifed work, it is to be regretted, has not yet appeared. There cannot be a doubt that it will contain many curious and valuable obfervations; though, from the fpecimens given in the above appendix, I muft own, that

that ingenious as many of the obſervations undoubtedly are, there ſeems reaſon to queſtion whether the analyſis may not, after all, be found too vague and general to lead to a ſatisfactory reſult.

Though I am ſenſible of theſe deficiencies in the manner in which the different writers I have mentioned have exhibited the ſyſtem of Greek analogy, I cannot but think, at the ſame time, that the ſyſtem itſelf, if fairly delineated, is by no means an arbitrary creature of fancy, but has a real and ſolid foundation in the nature and progreſs of the human intellect. Connected as this ſubject is with many parts of the foregoing diſquiſitions, I hope it will not be going too far out of my way to ſubjoin to this paper ſome remarks in ſupport of that opinion, together with a few curſory hints, both of the grounds on which I conceive this ſyſtem of analogy to reſt, and the mode in which it might probably be followed out with advantage, to elucidate the real nature and progreſs of the language. Such a detail will form the beſt explanation of my reaſons for truſting to this guide ſo much as I have done, in the progreſs of theſe inquiries into the radical ſenſe of the prepoſitions.

In entering upon this diſcuſſion, we may without heſitation adopt in part the Hemſterhuſian ſyſtem of Greek analogy, as detailed by Linnep, and partially caught by Lord Monboddo; a ſyſtem which may be regarded as an approximation to the truth, though requiring much both of correction and addition to render it complete.

According to this ſyſtem, the radical primitives in the Greek language are the five duads *αω*, *εω*, *ιω*, *οω*, and *υω*;—all of them verbs, and all ſignifying the ſame thing, viz. the general idea of *motion*, "*I move*," "*I make to move*," or "*I ceaſe motion;*" by the addition of conſonants to theſe duads, the ſignification is modified and defined in any manner that may be required for expreſſing the various ideas and deſires of the mind. Hence every

every Greek word may be traced to a verb as its immediate primitive, and all verbs ultimately found to emanate from one or other of theſe duads, by the regular addition or inſertion of the conſonants.

ONE obvious defect in this ſyſtem is, that it leaves unexplained altogether the mode in which the various conſonants are uſed for modifying the general idea denoted by the duads. Hence, were we even to admit the hypotheſis as true, by far the greater part of the mechaniſm of language would ſtill remain to be inveſtigated.

ANOTHER eſſential defect ſeems to be, that this ſyſtem ſuppoſes, contrary to all probability, that the abſtract idea of motion unmodified was the root of all language, conſequently of all the conceptions of the intellect; and that it was not till this idea was formed, that the various kinds and modes of it came to be known and named. The progreſs of nature ſeems to be the reverſe: individual acts are firſt obſerved and named, and from theſe individual acts all general notions come to be formed.

THE ſyſtem is farther erroneous in this, that it ſuppoſes theſe five duads to be ſignificant and radical words; whereas it is evident, that the ω in each can be nothing but the termination of the verb; and theſe terminations of the verb, nothing elſe than the remains of a pronoun, coaleſced with the verb with which it had been formerly connected.

IN ſo far then, I apprehend, we ſhall find this ſyſtem of analogy defective and erroneous; yet, at the ſame time, it conducts us a certain length upon the right road, and is therefore by no means undeſerving of attention.

THE natural theory upon which I am inclined to think that a juſt ſyſtem of analogy, not for the Greek language alone, but for every primitive language whatever, might be formed, is the following: Language having been formed and employed for the communication of our feelings, whether of pleaſure or pain, objects

jects were therefore named, (in whatever manner the language originated), from their qualities, as discovered in their actings or effects in regard to us. Hence it follows, that the verb is the root of all language. The sounds by which these feelings were conveyed, were the shortest possible, and would therefore be all monosyllables, consisting probably of one consonant and one vowel sound only. As the vowel sounds are often uncertain and variable in their enunciation, not only in different languages, but in different dialects of the same language, they must therefore have been frequently interchanged in common speech, and no variation in these sounds could be supposed to affect the expression of the particular idea meant to be conveyed. It was to the consonant sounds alone, in which the diversity was fixed and obvious, that recourse was necessarily had, as the radical and uniform signs by which the diversity of ideas or feelings was to be expressed. Each consonant, therefore, had a particular signification, which it always retained, with whichever of the vowel sounds it was united; the vowel sounds serving only as the necessary means of enunciating the consonants; and it being consequently a matter of indifference which of the vowel sounds were thus employed.

Here, then, we may look for the actual roots of language, traced to their remote and primitive origin; simple monosyllabic sounds, containing one consonant only, enunciated indiscriminately by any of the vowel sounds, at the arbitrary option of the speaker.

In applying this theory to the structure of the Greek language, the Hemsterhusian system of Greek analogy comes opportunely to our assistance, at once supporting and supported by the general observations now thrown out. The duads *αω*, *εω*, *ιω*, *οω*, and *υω*, from which, according to this theory, all the words in the language emanate, are in fact, after removing the *ω*, (which, as already mentioned, is an adventitious termination), nothing

but

but the vowel founds ferving for the enunciation of the confonants. It is altogether an error, therefore, to ftate thefe as of themfelves fignificant: but the framers of the analogical theory were mifled by this circumftance, that as the vowel founds were neceffary for the enunciation of the confonants; as all founds were meant to exprefs fome fpecies of action; and as all action neceffarily implies motion, or the ceffation of motion; it feemed therefore not unnatural, that the neceffary component of the fignificant found fhould be fuppofed to denote the equally neceffary component of the action or idea fignified.

Having afcertained the application of the vowel founds, the next object is to confider the application of the confonants. In regard to this, it may be obferved, that whatever is thought of the hypothefis of the duads, it at leaft implies nothing abfurd or improbable to fuppofe, that when men began to name objects from their action, as experienced in their effects, the fame or fimilar feelings would naturally be expreffed by the fame or nearly fimilar founds; that the expreffive part of thefe founds was the confonant, and that, confequently, when the defire to indicate a feeling or idea once fignified recurred anew, this would be done by a repetition of the fame confonant found which had firft been employed to make it known. Hence it will follow, that each different confonant, when enunciated, would foon come to denote a particular range of ideas, agreeing among themfelves in fome common quality, diftinct from what were expreffed by any of the others. What particular clafs of ideas each confonant was to denote, was purely arbitrary and accidental. This, therefore, can never be afcertained by theoretical conjecture: if it be known at all, it muft be traced by the obfervation of facts, as they actually occur in the exifting primitive languages.

Whether, by fuch a mode of inveftigation, the actual formation of language could be fully and fatisfactorily developed, I cannot pretend to determine. I muft own, however, that it feems ſeems to me not improbable, that by proceeding upon ſuch a ſyſtem to analyſe any primitive language, ſome curious and intereſting facts regarding the formation of ſpeech and the progreſs of thought might be diſcovered.

From a ſlight and imperfect ſurvey of the Greek, (which may juſtly be conſidered as an original language *), in its primitive radicals, it appeared to me that ſomething of ſuch a regular analogy was to be found in it; that each of the different conſonants, united with the vowel ſounds in the form of the duads, was employed to expreſs ſome one general idea, which might be traced in various ramifications through all the words emanating from that primitive root. The radical ideas, announced by the different conſonants, ſtruck me as ſomewhat of the following nature.

B Impulſe, or impulſive force.

Γ Expulſion, extruding force.

Δ Piercing force, or connected with that, dividing force.

Z Expanſion or expanſive force, force diffuſing itſelf round on every ſide.

K Laying force, the force by which one object is made to lie upon or occupy a determinate place.

L Abraſion or abraſive force, the force by which objects are ſtripped, ſmoothed or pared.

 M Con-

* I do not here enter at all into the diſpute about the origin of the Greek language from the Hebrew, through the medium of the old Pelaſgic. In fact, ſuch a derivation does not affect the ſtructure of the language as complete within itſelf; for this derivation, if real, was not partial, but total: it was not the engrafting of parts upon a language already formed, but a tranſplantation of the whole, in its native form; ſo that not only the branches, but the roots, with all their natural ramifications, were carried to, and eſtabliſhed in, a new and ſomewhat different ſoil. In reaſoning, therefore, on the ſtructure of the language, the Greek may juſtly be reckoned an original language, forming its roots within itſelf; and the other words from theſe roots, by a regular progreſſion.

Μ Compreſſion, conjoining or compreſſive force.
Ν Cleaving or ſplitting force.
Ξ Strong or violent friction.
Π Adheſion, adheſive force.
Ρ Fluency, flowing force.
Σ Tremor, ſhaking force.
Τ Tenſion, ſtretching force.
Φ Eruptive force, force by which interpoſed obſtacles are broken through.
Χ Opening or disjoining force, force by which two objects once united are made to ſeparate.
Ψ Smooth or leſs violent friction.

If it be objected, that ſome of the ideas thus ſuppoſed to be denoted by the radical components of the language, are too abſtract and metaphyſical to be allowed ſuch a place, I would obſerve, that in ſtating theſe general ſignifications, I by no means ſuppoſe the general idea to have been in view when the various words were formed; but that the generalization aroſe merely from the repeated application of the ſame ſound to expreſs individual objects and individual feelings, concurring in the ſame common quality; for in fact it is rather the expreſſion than the idea which has a metaphyſical aſpect, moſt of the ideas being derived from objects in their nature ſufficiently obvious and frequently occurring.

Should it be thought abſurd to ſuppoſe that all the numerous ideas which ſpeech is intended to expreſs, could ever originate and ſpread out from the few and ſcanty ſources now laid down, I ſhall only reply, that if we can trace in fact the whole body of a copious and expreſſive language, as emanating from theſe primitive roots, and can find every idea borrowing its expreſſion ultimately from one or other of theſe radical ſounds, we ſhall have an argument from fact of the truth of the ſyſtem, which no reaſoning from improbabilities can poſſibly overturn.

After

After all, I by no means wiſh to be underſtood as delivering theſe ſignifications with confidence: they are thrown out as conjectures merely, made upon a curſory inveſtigation, which may probably never be purſued farther. But though theſe particular conjectures may be erroneous, the ſyſtem, I am inclined to believe, holds ſo much of truth, that not only in the Greek, but in every primitive language, if the roots were fully inveſtigated, ſome common idea, ſome common force, might be traced out in the radical uſe of every individual conſonant. A theory founded on ſuch an inductive proceſs, would be by no means chimerical. Within the bounds of a particular language, etymology is a pretty ſafe guide: it is only when raſhly applied to various and diſcordant languages that it is ready to bewilder and miſlead.

Were it poſſible thus to trace, with any degree of preciſion, in the roots of different primitive languages, the power and force of the ſeveral letters, as employed in their radical words, and from theſe to follow out the ſubſequent ramifications of each through all the component parts, an excellent track would thereby be opened for inveſtigating the procedure and advances of the human intellect in the arrangement of ideas and the formation of language. A compariſon of theſe primitive roots would likewiſe enable us beſt to determine, whether any, and what, degree of affinity exiſted among the languages to which they belong. Could the various original languages of the globe be brought into ſuch a point of compariſon, the much-agitated queſtion, Whether all are derived from one? might be eaſily reſolved. Probable as the opinion of their common origin muſt be allowed to be *à priori*, and ſtrengthened as it undoubtedly is by many ſtriking proofs of actual coincidence, ſtill to eſtabliſh it completely, ſuch an evidence *à poſteriori* ſeems to be wanting, as a radical analyſis, founded upon the plan now mentioned, would alone be competent to furniſh.

XIV. Experiments *and* Observations *upon the* Contraction *of* Water *by* Heat *at* Low Temperatures. *By Thomas Charles Hope*, M. D. F. R. S. Ed. *Professor of Chemistry in the University of Edinburgh.*

[*Read 9th January* 1804.]

TO the general law, that bodies are expanded by heat, and contracted by cold, water at the point of congelation, and for some degrees of temperature above it, seems to afford a very singular and curious exception.

The circumstances of this remarkable anomaly have been for some time believed to be the following:

When heat is applied to water ice cold, or at a temperature not far distant, it causes a diminution in the bulk of the fluid. The water contracts, and continues to contract, with the augmentation of temperature, till it reaches the 40th or 41st degree. Between this point and the 42d or 43d, it suffers scarcely any perceptible change; but when heated beyond the last-mentioned degree, it begins to expand, and increases in volume with every subsequent rise of temperature.

During the abstraction of caloric, the peculiarity in the constitution of water equally appears. Warm water, as it cools, shrinks, as other bodies do, till it arrives at the temperature of 43° or 42°. It then suffers a loss of two degrees without any alteration of density. But when farther cooled, it begins to dilate

late, and continues to dilate, as the temperature falls, till congelation actually commences, whether this occurs as ſoon as the water reaches the 32°, or after it has deſcended any number of degrees below it.

SUPPOSING this peculiarity of water to be eſtabliſhed, it muſt appear, indeed, a very odd circumſtance, that heat ſhould produce contraction in this fluid, while it cauſes expanſion in other bodies; and no leſs ſtrange, that within one range of temperature it ſhould contract, and in another expand, the very ſame ſubſtance. Before a deviation from ſo general a law ſhould be received as matter of fact, the proofs of its exiſtence ought to be clear and indiſputable.

THE experiments hitherto publiſhed, from which this ſingularity has been deduced, have all of them been performed upon water contained in inſtruments ſhaped like a thermometer glaſs, and conſiſting of a ball with a ſlender ſtem; and the expanſive or contractive effects of heat and cold have been inferred, from the aſcent or deſcent of the fluid in the ſtem.

To ſuch experiments it has been objected, that the dimenſions and capacity of the inſtrument undergo ſo much change, from variation of temperature, that it is difficult, if not impoſſible, to determine how much of the apparent anomaly ought to be imputed to ſuch changes, and that it is not improbable that the whole of it may be aſcribed to them.

THE object of this paper, which I have now the honour to read to the ſociety, is to prove by a ſet of experiments, conducted in a manner altogether different, that the common opinion is founded in truth, and that water preſents itſelf as that ſtrange and unaccountable anomaly which I have already deſcribed.

IT is worth while, before detailing my experiments, to give a ſhort account of thoſe obſervations which led to the diſcovery of the fact, and which in ſucceſſion have extended our knowledge of it, as well as of thoſe obſervations which have at different periods been offered to diſcredit, and to bring it into doubt.

THE

THE firſt obſervation relative to this ſubject was made by Dr CROUNE, towards the cloſe of the 17th century, while engaged in inveſtigating the phenomena of the great and forcible, though familiar, expanſion which happens to water at the inſtant of freezing; a matter which occupied, in a conſiderable degree, the attention of his fellow-members of the Royal Society of London in the earlier years of that inſtitution.

I SHALL relate in his own words his firſt obſervation: "I filled "a ſtrong bolt-head about half-way up the ſtem with water, a day "or two before the great froſt went off, marking the place where "the water ſtood; and placing it in the ſnow on my leads, "while I went to put ſome ſalt to the ſnow, I found it above "the mark ſo ſoon, that I thought the mark had ſlipt down, "which I preſently raiſed to the water, and as ſoon as ever I "mixed the ſalt with the ſnow, the water roſe very faſt, about "one-half inch above it. I took up then the glaſs, and found "the water all fluid ſtill: it was again ſet down in the ſalt and "ſnow; but when I came about an hour after to view it, the "ball was broke, and the water turned to hard ice, both in the "ball and ſtem *."

FROM this experiment Dr CROUNE drew the concluſion, that water, when ſubjected to cold, actually began to expand before it began to freeze. On announcing it, however, to the Royal Society, on the 6th of February 1683, Dr HOOKE immediately expreſſed ſtrong doubts, and aſcribed the aſcent of the water in the neck of the veſſel to the ſhrinking of the glaſs occaſioned by the cold.

To obviate this objection, and to preclude, as far as was poſſible, the influence of the change of capacity in the apparatus from an alteration of its temperature, a bolt-head was immerſed in a mixture of ſalt and ſnow, and into it, when cooled, was poured, to a certain height, water previouſly brought to near the freezing

* BIRCH's *Hiſtory of the Royal Society*, vol. IV. p. 263.

freezing point. The water began inftantly to rife as before, and when it had afcended about one-fourth of an inch in the ftem, the veffel was taken out, the whole water remaining fluid.

These experiments, fupported by others of a fimilar nature, communicated by Dr Slare to the Society on the 20th of the fame month, appear to have fatisfied its members, in general, of this fact, that water, when on the point of congealing, and while ftill fluid, is actually fomewhat dilated previous to the remarkable expanfion which accompanies its converfion into ice.

Dr Hooke, however, continued unfhaken, and retained the doubts he had expreffed.

Remarkable as the fact, as now ftated, muft have appeared, it feems not to have excited particular attention, nor to have folicited more minute examination ; and indeed though philofophers did not lofe fight of it, yet for near a century no one inveftigated it more carefully. Mairan, in his treatife on ice in 1749, and Du Crest in his differtation on thermometers in 1757, appear to be well aware of this property of water ; but it is to M. De Luc that we owe the knowledge of the leading and more interefting circumftances, (*vide Recherches*, &c. 1772).

Having devoted his attention to the examination and improvement of the thermometer, he was naturally led to the inveftigation, while engaged in afcertaining the phenomena of the expanfion and contraction of different fluids by heat and cold.

He employed in his experiments thermometer glaffes ; and the included water, at or near the term of liquefaction, defcended in the ftem, and appeared to him to fuffer a diminution of bulk by every increafe of temperature, till it arrived at the 41ft degree. From this point its volume increafed with its temperature, and it afcended in the tube. This fluid, when heated and allowed to cool, feemed to him to contract in the ordinary way, till its temperature funk to the 41°, but to expand and increafe in volume, as the temperature fell to the freezing point.

The

THE denſity of water, he thence inferred, is at its maximum at 41°, and decreaſes with equal certainty whether the temperature is elevated or depreſſed.

M. DE LUC ſays, indeed, that very nearly the ſame alteration in volume is occaſioned in water of temperature 41°, by a variation of any given number of degrees of temperature, whether they be of increaſe or of diminution; and conſequently that the denſity of water at temperature 50°, and at temperature 32°, is the ſame.

THIS philoſopher did not conceive that the conſtitution of water, in relation to caloric, undergoes a change at the temperature of 41°, ſuch that ſhort of this degree caloric ſhould occaſion contraction, and beyond it expanſion. He imagined that heat in all temperatures tends to produce two but quite oppoſite effects on this fluid, the one expanſion, the other contraction.

IN low temperatures, the contractive effects ſurpaſs the expanſive, and contraction is the conſequence: In temperatures beyond 41°, the expanſive predominate, and the viſible expanſion is the exceſs of the expanſive operation over the contractive.

IN 1788, Sir CHARLES BLAGDEN added the curious obſervations, that water, which by ſlow and undiſturbed refrigeration permits its temperature to fall many degrees below its freezing point, perſeveres in expanding gradually as the temperature declines; and that water having ſome muriate of ſoda or ſea-ſalt diſſolved in it, begins to expand about the ſame number of degrees above its own term of congelation that the expanſion of pure water precedes its freezing, that is, between eight and nine degrees. More lately, (*Philoſophical Tranſactions*, 1801), he, or rather Mr GILPIN by his direction endeavoured to aſcertain, by the balance and weighing bottle, the amount of this change of denſity cauſed by a few degrees of temperature.

EVERY one muſt be familiar with the uſe which Count RUMFORD has made of this peculiarity in the conſtitution of water,

 in

in explaining many curious appearances that prefented themfelves in his experiments upon the conducting power of fluids, and in accounting for certain remarkable natural occurrences. The Count, with his ufual ingenuity, has endeavoured to point out the important purpofes which this peculiarity ferves in the economy of nature, and to affign the final caufe of fo remarkable an exception from a general law.

In recording the obfervations and opinions that have been publifhed concerning this point, I might now, in order, notice thofe of Mr Dalton of Manchefter, related in the fifth volume of the *Manchefter Memoirs*, which tended to confirm and enlarge our knowledge of it. But as Mr Dalton himfelf has called in queftion the accuracy of the conclufions which had been drawn from his experiments, and from thofe of preceding obfervers, I fhall only remark, that they are of the fame nature, and nearly to the fame purport, as thofe of M. de Luc.

It was in confequence of a communication with which Mr Dalton favoured me, three months ago, that my attention was directed to this fubject. He informed me, that after a long train of experiments he was led to believe that he, and his predeceffors in the fame field of inveftigation, had fallen into a miftake with regard to the contraction of water by heat, and its expanfion by cold, in confequence of overlooking or underrating the effect which the change in the capacity of the thermometer-fhaped apparatus employed, muft occafion on the apparent volume of the fluid. He ftated, in general terms, that on fubjecting water to different degrees of temperature, in inftruments made of different materials, he found the point of greateft denfity was indicated at a different temperature in each.

In an apparatus, having a ball of earthen-ware, it was at the 34th degree; of glafs at the 42d; of brafs at the 46th; and of lead at the 50th. And as water could not follow a different law, according to the nature of the fubftance of the inftrument, he conceived

conceived that the appearance of anomaly in this fluid originated entirely in the containing veffel, which muft caufe the fluid in the ftem to fall or rife according as its expanfions are greater or lefs than thofe of the included liquor.

A DETAIL of thefe important experiments has, ere now, been tranfmitted for publication in the *Journals of the Royal Inftitution of London.*

I HAVE already noticed, that Dr HOOKE endeavoured to explain in the fame manner the original experiment of Dr CROUNE. This explanation apparently gathers much force from thefe experiments of Mr DALTON.

It is proper, however, to ftate, that M. DE LUC was perfectly aware of the alteration in the dimenfions of his glafs apparatus, but deemed the change too trifling to have any material influence.

Sir CHARLES BLAGDEN paid greater attention to the circumftance, and by calculation attempted to appreciate what allowance ought to be made for the change of capacity in the amount of the apparent changes of volume.

WHEN it is confidered, that the whole amount of the apparent change is but very fmall, and that the expanfibility of the glafs is with difficulty afcertained, and is variable by reafon of the fluctuating proportions of its heterogeneous conftituents, it muft be acknowledged, that precifion in fuch a calculation cannot poffibly be attained, and can fcarcely be approached. On this account, all the experiments already noticed are open to the explanation of Dr HOOKE, and in fome meafure liable to the objection which he had urged. I confefs, that the experiments of Mr DALTON, in perfect concurrence with that explanation, created confiderable doubts refpecting the exiftence of the peculiarity of water ; againft the probability of which circumftance, all analogical reafoning, and every argument *à priori*, ftrongly militate.

Unwilling to remain in uncertainty, and confidering it as a point of much curiofity and intereft, I have endeavoured to inveftigate the fubject by experiments conducted in a totally different manner, equally calculated to exhibit the fingular truth, but free from the objections to which the others are liable. In them, it was my object to provide, that neither the changes of the actual volume of the water, nor the alterations in the dimenfions of the inftrument, fhould have any influence whatever.

I have already taken occafion to ftate, that the purpofe of this paper is to prove, by experiments on the principle now mentioned, that in the conftitution of water there really exifts the fingularity often noticed.

I shall firft ftate the plan of the experiments, and then detail the particulars of the moft remarkable of them.

When any body is dilated, whether by heat or cold, it neceffarily becomes lefs denfe, or fpecifically lighter; and the oppofite effects refult from contraction. This is the circumftance, as every one knows, which caufes various movements among the particles of fluids, when any inequality of temperature prevails in the mafs; hence thefe particles are little acquainted with a ftate of reft.

If a partial application or fubtraction of heat produce an inequality of denfity in a mafs of fluid, the lighter parts rife to the furface, or the denfer fall to the bottom.

It readily occurred, that I might avail myfelf of thefe movements, and upon ftatical principles determine the queftion in difpute.

I had only to examine attentively water, as it was heated or cooled in a jar, and to obferve, by means of thermometers, what fituation the warmer, and what the cooler parts of this fluid affected.

If I fhould find that ice-cold water, in acquiring temperature, fhowed, in its whole progrefs, the warmer parts near the top, it would

would indicate that water follows the ufual law, and is expanded like other bodies by heat.

Or if I fhould obferve that warm water, in cooling to the freezing point, had the coldeft portion uniformly at the bottom, the fame conclufion would follow; while a different inference, and the exiftence of the fuppofed anomaly, would be deducible fhould the event prove different. The only circumftance, I can figure to myfelf as tending in any meafure to render this mode of examining the point doubtful, is, that water near its congealing point may have fo little change of denfity occafioned by a fmall variation of temperature, that its particles may be prevented by their inertia, or by the tenacity of the circumfluent mafs, from affuming that fituation which their fpecific gravity would allot to them.

It will appear, however, very clear, from the circumftances of the experiments which I fhall immediately detail, that no obftacle to the fuccefs and precifion of the experiments proceeded from this fource.

It is not neceffary for me to relate all the experiments I have made. I fhall reftrict myfelf to the detail of fix, which prefent varieties in the modes of procedure, and which afford the moft ftriking refults.

Experiment I.

I filled a cylindrical jar of glafs $8\frac{1}{2}$ inches deep, and $4\frac{1}{2}$ in diameter, with water of temperature 32°, and placed it on a table, interpofing a confiderable thicknefs of matter poffeffed of little power of conducting heat. I fufpended two thermometers in the fluid, nearly in the axis of the jar, one with its ball about half an inch from the bottom, the other at the fame diftance

below

below the furface. The jar was freely expofed to the air of the room, the temperature of which was from 60° to 62°.

THE experiment commenced at noon:

	Top Thermom.		Bottom do.
	32°	-	32°
In 10 minutes, -	33+	-	34+
— 30 ——— -	35.5	-	37
— 50 ——— -	37	-	38+
— an hour, -	38	-	38+
— ——— and 10 minutes,	42	-	38.25
— ——— — 30 ———	44	-	40
— ——— — 50 ———	46+	-	41+
— 2 hours and 10 minutes,	48	-	42.5
— ——— — 30 ———	50	-	44
— ——— — 50 ———	50.5	-	45
— 4 hours, -	54	-	49

CONFIDING in the indications of the thermometers, from this experiment we learn, that when heat flows on all fides from the ambient air into a column of ice-cold water, the warmer portions of the fluid actually defcend, and take poffeffion of the bottom of the veffel.

THIS downward courfe proclaims an increafed denfity, and teftifies that the cold water is contracted by heat. As foon, however, as the fluid at the bottom exhibits a temperature of 38°, this courfe is retarded and foon ftopped, and with the rife of temperature beyond 40° is totally changed: for when the mafs attains this degree, the experiment equally fhows, that the warmer fluid afcends and occupies the fummit, by its route announcing its diminifhed denfity, and proving that water is now expanded by heat.

EXPE-

Experiment II.

I filled the ſame jar with water of temperature 53° ; and that I might obſerve the phenomena of cooling, I placed it in the axis of a much larger cylindrical veſſel, nearly full of water, of temperature 41°, and, by an earthen-ware ſupport, raiſed it about three inches from the bottom, taking care that the water ſhould be on the ſame level in both veſſels. As ſoon as I had adjuſted the two thermometers, as in the former experiment, I obſerved that the top of the fluid was ſtill at 53°, but the bottom had fallen to 49°.

	Top.		Bottom.
In 9 minutes,	52°	-	45
— 15 ———	52	-	44

Now, to accelerate the cooling, I withdrew by a ſyphon the water from the large cylinder, and ſupplied its place by ice-cold water, mixed with fragments of ice, which by repeated cautious agitation was kept uniformly at the temperature of 32°.

In 23 minutes,	48°	-	42+
— 38 ———	44	-	40
— 43 ———	42	-	40
— 46 ———	40	-	40
— 52 ———	36	-	40
— 58 ———	35—	-	39
— 65 ———	34	-	37
— 75 ———	34	-	36
— 103 ———	34	-	34

This

This experiment is the counterpart of the foregoing, and from the teſtimony of the ſame inſtruments, it appears, that when a cylinder of water of 53° is cooled by circumfluent iced fluid, the colder part of the water takes poſſeſſion of the bottom of the veſſel, ſo as to eſtabliſh a difference of temperature from the ſurface, amounting ſometimes to 8°. And that as ſoon as the fluid at the bottom arrives at the 40th degree, the temperature of the fluid in that ſituation is ſtationary till the ſurface reaches the ſame point.

During the ſubſequent refrigeration, the progreſs of the cooling undergoes a total change. The thermometers tell that the colder fluid riſes to the ſurface; ſo that the top gets the ſtart of the bottom ſoon by 4°, and attains the loweſt temperature of 34° very long before the other falls to the ſame degree.

These circumſtances, I think, lead to the concluſion, that by the loſs of caloric, water at 53° is contracted and rendered ſpecifically heavier, and that this continues to happen till the water come to the temperature of 40°, at which period an oppoſite effect is produced; for now the water, as it cools, becomes ſpecifically lighter, or is expanded.

In this, as well as the former experiment, the complete change in the ſituation, which the warmer and colder parts of the fluid affected, in the progreſs both of the heating and cooling, while every external circumſtance of the proceſs continued unaltered, is particularly worthy of remark.

Experiment III.

I took a glaſs jar, 17.8 inches deep, and 4.5 in diameter, internal meaſure, having a neck and tubulature very near the bottom. I provided alſo a cylindrical baſon of tinned iron, 4.8 inches

inches deep, and 10 inches in diameter, with a circular hole in the middle of the bottom, large enough to receive the top of the jar. By means of a collar and cement I ſecured this baſon, ſo that it encircled the upper part of the jar.

The object of the contrivance was to have the means of applying a cooling medium to the ſuperior portion of a cylinder of water, and it anſwered the purpoſe completely. I introduced the ball of a thermometer through the tubulature, till the extremity of it nearly reached the axis at three-fourths of an inch above the riſing of the bottom, and having fixed it in this ſituation, I rendered the aperture water-tight, by a perforated cork and lute.

This very tall jar was placed on a table, with the interpoſition of ſome folds of thick paper, in a room without a fire, of the temperature 42°.

I filled it with water of 50°, and poured into the baſon, which embraced the top, a mixture of powdered ice and ſalt.

From time to time I explored the temperature near the ſurface, by inſerting the bulb of a thermometer to the depth of half an inch nearly in the axis.

	Bottom.	Top.	Air.
One o'clock,	50°	50°	42°
In 11 minutes,	46.+	—	
— 15 ———	45	48	
— 21 ———	44—	46—	
— 31 ———	42	44	
— 41 ———	41	42	
— 51 ———	40.+	34	At this time a thin film of ice began to form in contact with the glaſs.
— 1 hour 6 min.	40	34	
——— 20 ———	39·5		

 In

	Bottom.	Top.
In 1 hour 44 min,	39.5	
In $4\frac{1}{2}$ hours,	39.5	A cruſt of ice of ſome thickneſs now lined the glaſs, and air had fallen to 40°.
— $5\frac{1}{2}$ hours,	39	
— 11 hours, *i. e.* at midnight,	39	Cruſt of ice complete.
— 19 hours, *i. e.* next morning,	39	Air 40°.
— 26 hours,	40.	Air 40°. So much ice had melted that the cake was detached from the ſide of the veſſel, and floated.
— 32 ——	40	
— 41 ——	40	Air 41°. Ice not all melted.
— 50 ——	41	Air 42°. Ice not entirely gone.

This long protracted experiment preſents ſome ſtriking facts, and its general import, with regard to the ſubject of inveſtigation, agrees with the preceding. In it we ſee, that when the frigorific mixture abſtracted caloric from the upper extremity of a cylinder of water, nearly 18 inches long, and at 50°, the reduction of temperature appeared ſooner, and advanced quicker, at its lower extremity than in the axis at the top, not two and a half inches diſtant from the cooling power. No one can entertain a doubt that this is owing to a current of cooled and condenſed fluid deſcending, and a correſponding one of a warmer temperature aſcending. Now, if water obſerved the ſame law that other bodies do, and had no peculiarity of conſtitution, the ſame progreſs of cooling ſhould continue. This, however, the experiment teaches us, is not the caſe: as ſoon as the fluid at the bottom exhibits a temperature of 40°, it ceaſes. The colder fluid remains at top, and quickly loſing temperature, ere long begins to freeze. The continuance of the colder fluid at the ſurface ſurely denotes, that it is not more denſe than the ſubjacent warmer

warmer water. The legitimate inference from this is, that water of temperature 40° is not contracted by being cooled to 32°.

DID water obferve the ufual law, and lofe volume along with temperature, this experiment, by its long duration, afforded ample time for the manifeftation of it.

FOR not lefs than two days did ice-cold water maintain poffeffion of the top, and for the fame period the temperature at the bottom never fell below 39°. No current, therefore, of cold and condenfed fluid moved from the furface, to affect the inferior thermometer, or to atteft the contraction of water by cold.

THIS experiment, however, I muft remark, does not warrant the conclufion, that the water is actually expanded, though it in no degree oppofes it. It proves no more, than that the contraction ceafes at 40° ; and that water of 32° is not more denfe than of 39° or 40°. Nay, fome may perchance allege, that it does not prove fo much ; conceiving, that if at 40° the contraction, without ceafing altogether, becomes very inconfiderable, the difference of denfity occafioned by the fubfequent reduction of temperature may be fo very trifling, as not to enable the cold particles to take that fituation which their gravity affigns to them, in oppofition to the inertia and tenacity of the fubjacent mafs ; and therefore that the colder, though heavier fluid, may be conftrained to remain above. That this allegation fhould have no weight attached to it, the circumftances of the fucceeding experiment will clearly fhow, as I fhall foon notice.

BEFORE quitting the confideration of the prefent experiment, it may be worth while to remark, that it may feem rather furprifing, that the bottom of the fluid was not apparently affected in its temperature by the ice which fo long occupied its furface. It might be expected, though no cold currents defcended from above, that the caloric fhould be conducted from below, and that the temperature ſhould by that have been reduced *. I ſuppoſe that the caloric did paſs from the lower ſtrata upwards, but

* THIS experiment may perhaps be thought to give countenance to the opinion of the very ingenious Count RUMFORD, that fluids cannot conduct heat, and that no interchange of heat can take place between the particles of bodies in a fluid ſtate, ſeeing that for two days the fluid at the bottom of the veſſel never fell below 39°, though the ſurface was at 32°.

FROM the circumſtances detailed in his ſeventh eſſay, the Count concluded, that heat cannot deſcend in a fluid. From the preſent, it might with equal juſtice be inferred, that heat cannot aſcend.

HAD I not the fulleſt conviction that this celebrated philoſopher has puſhed his ideas too far, I might be diſpoſed to conſider this experiment as according well with the hypotheſis.

SOON after the intereſting ſpeculations of the Count appeared, I began to inveſtigate the ſubject; and, by a pretty long train of experiments, which I have annually taken an opportunity of detailing in my lectures, ſatisfied myſelf that he aſſigned to fluidity a character that does not belong to it. Though ſince the date of theſe experiments, the public has become poſſeſſed of ſeveral ſeries, well deviſed, and, in my opinion, of themſelves concluſive, it may yet be worth while to ſtate the tenor and reſult of them, by which the value of their teſtimony in favour of the conducting power of liquids may be eſtimated.

THE experiments were of two deſcriptions.

THE one ſet, of the ſame nature nearly with thoſe of Count RUMFORD, was deſigned to examine, Whether heat, when applied to the ſurface, can deſcend in a fluid; and the other to diſcover, Whether, on the mixture of different portions of fluid at different temperatures, an interchange of caloric takes place between the particles;—Water, oil and mercury, having been the ſubjects of the Count's experiments, were employed for the firſt ſet.

To explore the conducting power of water and oil, the apparatus which I uſed conſiſted of two veſſels of tinned iron, both cylindrical, and the one ſomewhat larger than the other. The larger had a diameter of eleven inches, and into it were poured the ſubjects of the trial, to different depths on different occaſions. The ſmaller was ten and a half inches in diameter. By three hooks it was ſuſpended within the larger pan, in ſuch a manner, that the bottom of it exactly reached and came in contact with the ſurface of the fluid. This ſmaller veſſel became the ſource of the heat, by being filled with boiling hot water. The water was changed frequently, care being taken to avoid, by the uſe of a ſyphon, all agitation and diſturbance.

IN

but extremely flowly, by reafon that fluids, as Count RUMFORD taught us, are exceffively bad conductors of heat, and fo very flowly,

IN experiments of this nature, the difficulty is to prevent the conveyance of caloric by the fides of the veffel. I attempted, and, I think, I fucceeded, in overcoming this difficulty, by encircling the larger veffel, at a height exactly correfponding with that of the furface of the fluid within, with a gutter or channel about half an inch in depth; and by caufing a ftream of cold water to flow conftantly through a fyphon into this gutter, while from the oppofite fide it ran off by a fmall fpout.

THE water was feveral degrees colder than the fubject of the experiment; and keeping cool the portion of the veffel with which it was in contact, it intercepted the heat that would otherwife have travelled by this route to the bottom.

FOR mercury I had recourfe to veffels of glafs.

IN all the experiments a thermometer bore teftimony that the caloric defcended from the furface to the bottom of the fluid, and demonftrated, at leaft to my conviction, that fluids can conduct heat.

THE progrefs of the heat, however, was very flow, and attefted the important fact, for which we ought to be thankful to the Count,—That fluids are very bad conductors.

THE fecond fet of experiments was calculated to examine, in a very different manner, the pofition, That all interchange and communication of heat between the particles of fluids is impoffible.

WHEN a hot and a cold fluid are mixed together and well agitated, very foon an uniform temperature is produced. This equality muft proceed either from a communication of heat from the warmer to the colder fluid, agreeably to the common opinion, or from a perfect intermixture of hot and cold particles, according to the notion of Count RUMFORD. To which caufe it ought to be attributed, I conceived I might difcover, by afcertaining whether, after fuch an intermixture, any feparation of the hot and cold portions took place. If the equilibrium of temperature be owing to intermixture without interchange of caloric, the hotter particles, as foon as the agitation ceafes, ought, by reafon of their greater rarity, to accumulate, to a certain degree, at the furface, and there exhibit a temperature above the common one.

I FIRST tried water, and mixing this fluid boiling-hot, with an equal quantity nearly ice-cold, in a ftoppered glafs jar, I fhook them well for a fhort time.

I THEN noticed the refulting temperature, and raifing the ball of the thermometer towards the furface, I had an opportunity of obferving, that it never rofe the ſlowly, that the caloric entered from the atmoſphere with ſufficient quickneſs to prevent any depreſſion of temperature below the 39th degree.

THIS experiment, I may conclude with remarking, is very well calculated to exhibit the error of the popular opinion, that "heat has a tendency to aſcend."

EXPERIMENT

the ſmalleſt portion of a degree above the common temperature which had been eſtabliſhed.

I NEXT made a ſimilar experiment with alcohol, ſelecting it on account of its remarkable dilatability. I ſhook well, for half a minute, a mixture of equal parts of alcohol at temperature 40° and at temperature 170°. The reſulting temperature of the maſs was 104°.

NOW, if this was a mixture of particles at 40° and at 170°, as the difference of ſpecific gravity between the fluid at theſe temperatures is very conſiderable, ſome ſeparation of the warmer and lighter particles from the others, ought, I conceive, to have taken place. The temperature of the top, however, never indicated the arrival of warmer particles. It never aſcended above the point of equilibrium.

FROM theſe experiments I concluded, that the uniformity of temperature was eſtabliſhed by an actual communication and interchange of heat between the particles.

IT may not, however, be improper to ſtate, that Count RUMFORD, with whom ſeveral years ago I had the pleaſure of converſing upon this ſubject, alleged, that the intermixture might be ſo complete as to prevent any ſeparation whatever.

IF it be a property eſſential to fluidity, that heat cannot paſs from one particle to another, the particles of different fluids ought to be equally incapable of imparting caloric mutually to each other. Unfortunately, however, for the ſpeculation, the caloric is ſo communicated. Though, *à priori*, I entertained no doubts reſpecting the reſult of the experiment, I poured a quantity of olive oil which had been heated by immerſion in a veſſel of boiling water for half an hour, upon an equal volume of water of 38°, and agitated the mixture, by ſhaking for a quarter of a minute. The common temperature produced was 78°, and the heat had gone from the oil into the water; for when the fluids ſeparated, and had arranged themſelves according to their ſpecific gravity, both of them had the ſame temperature of 78°.

THE experiments of the two deſcriptions now recorded, left on my mind little doubt that the Count had overſtrained his concluſions.

EXPERIMENT IV.

I TOOK the ſame tall jar, and ſtopping the tubulature with a cork, I filled it with water of temperature 40°, and placed it in a pan. After ſuſpending two thermometers, as in experiment firſt and ſecond, I poured a mixture of ice and ſalt into the pan, to the depth of 4.2 inches, the air of the room being 40°, as in the laſt experiment.

	Bottom.	Top.	Air.
Eleven o'clock,	40°	40°	40°
In 10 minutes,	38+	38+	
— 20 ———	38—	38—	
— 30 ———	37—	37—	
— 40 ———	36	36	
— 60 ———	35·5	35·5	
— 80 ———	35	35	
— 100 ———	34·5	35	
— 120 ———	34—	34	
— 8 hours,	34—	34	

A CRUST of ice began to form on the inſide of the glaſs when the water in the axis of the bottom and of the top was at 36°. In the courſe of the experiment, it became at leaſt an inch thick.

WE learn from this experiment, that cold applied to the lower part of a cylinder of water, nearly 18 inches long, and having the temperature of 40°, is actually as ſpeedily perceived at the ſummit as in the axis of that part, on the external ſurface of which it immediately acts. As fluids conduct heat ſo very tardily, this can only ariſe from currents of cooled water aſcend-

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ing from the bottom, and theſe cold currents cannot move upwards, were not the water of them ſpecifically lighter than that of the incumbent warmer fluid.

The water, therefore, which at the bottom is cooled by the contiguous frigorific mixture, muſt be expanded by the loſs of caloric.

This experiment ſecures full force to the laſt, as it obviates the objection already noticed, and alſo precludes another. I have already ſtated, that it may perhaps be alleged, that the fluid at the top, in experiment third, though cooled to 32° did not deſcend, becauſe below 40° the contraction is ſo trifling, that it does not occaſion a difference of ſpecific gravity ſufficiently great to cauſe the particles to deſcend, when oppoſed by the inertia and tenacity of the fluid through which they have to fall; or it may be conceived, that the deſcent is ſo tardy, that time is given to the ambient air or ſubjacent fluid to furniſh heat enough to raiſe the temperature of the deſcending ſtream, and by that arreſt it in its downward courſe.

But from the particulars above recorded, it is manifeſt, that the change of denſity between the temperature of 32° and 40° is quite ſufficient to put into motion the particles, and to enable them to overcome the obſtacle ariſing from inertia and tenacity, and to withſtand the arreſting effects of atmoſpheric heat.

Though theſe experiments, and ſome others of a ſimilar nature, carried conviction to my mind, and perfectly ſatisfied me reſpecting the reality of the anomaly of water, I determined to vary ſomewhat the mode of making the experiment, ſo as to obtain ſtill more ſtriking reſults.

For the fifth experiment, I uſed an apparatus which conſiſted of a ſtill taller jar. It was 21 inches high, and 4 in diameter. I adjuſted at the middle of its height a perforated baſon of tinned iron, 2 inches in depth, and 10 in diameter. As this baſon embraced

embraced the middle of the jar, I could, by filling it with hot water, or a frigorific mixture, apply heat or cold to the middle portion of the fluid in the jar, and thence, by the thermometer, learn what courſe the heated or cooled fluid ſhould take.

Experiment V.

I filled the jar with water at 32°. I placed it upon ſeveral folds of thick carpet, previouſly cooled to the ſame degree. The air of the room going from 33° to 35°, I introduced two thermometers, as in experiments firſt and ſecond. I then poured water of temperature 68° into the baſon, and by means of a ſpout ariſing from the ſide of it, and a ſyphon connected with a reſervoir of water at the temperature now mentioned, I renewed the contents of the baſon frequently, but without cauſing any agitation.

	Bottom.	Top.	Air.
At commencement,	32°	32°	33—35
In 10 minutes,	35	32	
— 15 ———	36—	32	
— 20 ———	36+	32	
— 25 ———	37	33	
— 30 ———	38	33	*** From this time I charged the baſon with water of temperature 88°, and renewed it frequently.
— 38 ———	38+	33	
— 45 ———	39—	33	
— 50 ———	39+	44	
— 55 ———	39+	45	
— 60 ———	39+	48	

Nothing can be more deciſive with regard to the queſtion in diſpute, than the particulars of this experiment. Heat is applied to the middle of a column of ice-cold water. The heated por-

tion

tion has an equal ſhare of the column of cold fluid above it and beneath it. There is nothing to determine its courſe in one direction or another, excepting its actual change of denſity.

THE thermometer evinces that the warm current ſets downwards, and carries the increaſed temperature to the bottom. There, this inſtrument indicates the ſucceſſive riſe of ſeveral degrees, before the ſurface indicates the ſmalleſt acquiſition of heat.

THE inference is plain, that the cold water is contracted by the heat.

THE change of the effect of heat is equally well illuſtrated by this experiment.

No ſooner did the inferior portion attain the temperature of 39°, than the heated fluid altered its courſe, and, by aſcending, carried the increaſe of temperature very rapidly to the ſurface, ſo that it ſoon ſurpaſſed the bottom, and continued to riſe, while the other remained ſtationary.

EXPERIMENT VI.

I FILLED the jar uſed in the laſt experiment with water of temperature 39½°, the air and the ſupport being at 39°. Diſpoſing the thermometers in the uſual manner, I introduced a mixture of ſnow and ſalt into the baſon.

	Bottom.	Top.	Air.
At commencement,	39.5	39.5	39°
In 10 minutes,	39+	38+	
— 25 ——	39+	36.5	*** At this time ice
— 35 ——	39	36—	began to be
— 55 ——	39	35	formed on the
— an hour and 10 min.	39—	34+	ſide of the veſ-
— —— — 35 ——	39—	34—	ſel.
— 2 hours,	39—	33+	

THIS

This experiment ſpeaks in as decided language as the preceding. It ſhows that when a portion, in the middle of a column of water at temperature 39.5 is cooled, the colder fluid riſes, and does not deſcend through the warmer maſs, and preſents the unequivocal demonſtration, that water of temperature $39\frac{1}{2}°$ is actually expanded by loſing heat.

The different experiments which I have in detail recorded, agree perfectly with each other in the evidence they give relative to the ſubject of inquiry. The general import of them is, that water which is ice-cold, or a few degrees warmer, when heated, becomes ſpecifically heavier,—that water of 40° when heated becomes ſpecifically lighter,—that water above 40°, by the loſs of heat, or by cold, is rendered ſpecifically heavier; and that water below 40° is, by the ſame cauſe, rendered ſpecifically lighter.

Such being the general import, the concluſion is irreſiſtible, that heat, in low temperatures, cauſes water to contract, and at ſuperior temperatures to expand. The opinion, therefore, is founded in truth, that water poſſeſſes a peculiarity of conſtitution in relation to the effects of caloric, and that it is, within a ſhort range of temperature, an exception to the general law of "expanſion by heat."

So far as I can judge from theſe experiments, I am diſpoſed to believe that the point at which the change in the conſtitution of this fluid in relation to heat takes place, lies between the $39\frac{1}{2}°$ and the 40th degree.

I am not at preſent aware of any objection to the method I have followed in eſtabliſhing this ſingular anomaly, and in removing any doubts which may have ariſen from the unavoidable influence which the inſtrument muſt have in the mode of conducting the inveſtigation that had previouſly been adopted.

The plan of operation above deſcribed, however, only aſcertains the fact; it gives no data for aſcertaining the amount of the anomalous effect of heat.

I have already stated, that M. de Luc alleged, that from the temperature of 41°, the expansion occasioned by cold was very nearly equal to that produced by the same number of degrees of heat; and consequently that water possesses the same density at any given number of degrees of temperature above and below 41°. The first experiments of Mr Dalton appeared to confirm this opinion, and to enlarge the range to which it applied, by extending it to temperatures as far below 32°, as water allows itself to be cooled before it begins to freeze. From one circumstance that constantly occurred, I am inclined to think, that the amount of the dilatation by cold is inferior to that caused by heat.

During the heating or cooling of water below 40°, the difference of temperature between the top and bottom of the fluid was less than what occurred during the cooling or heating of the fluid through the same number of degrees above it; and I conceive that, when other circumstances, but particularly the rate of the change, are alike, the difference of temperature between the upper and lower parts of the fluid, as it depends upon, may prove a measure of, the difference of density.

Alcohol, when heated or cooled, presents, by reason of its greater expansibility, a greater difference of temperature in these situations than water; and upon the same principle I infer, that water from 40° is more expanded by an equal number of degrees of elevation than of depression.

As the concurrence of the testimony of the experiments above related with the general opinion, will probably remove every doubt respecting the matter of fact, it remains a very difficult problem for those who are fond of philosophical investigation, to explain how heat shall occasion in the same fluid, without producing any alteration of mechanical form or chemical condition, at one time contraction and at another expansion, and to reconcile the contractive effect to the conceived notions of the mechanism of the operations of this energetic agent.

When

When heat caufes expanfion, it is imagined to act by inducing a repulfion among the particles of bodies, which, oppofing and overpowering the cohefive attraction, caufes the particles to recede.

In what manner, then, the addition of heat can occafion, or allow, the particles of water to approach each other, and how the fubtraction of it can make them retire to a greater diftance, I confefs I can in no meafure comprehend.

An explanation, abundantly plaufible at firft view, very readily fuggefts itfelf to every one who is aware of the great and forcible expanfion which happens to this fluid at the moment of its congelation. It is ftated by Sir Charles Blagden, in the paper already quoted.

The remarkable dilatation which water experiences at the inftant of being converted into ice, is very generally afcribed, and I prefume very properly, to a new arrangement which the particles affume, determined probably by their polarity; by which one fide of the particle A is attractive of one fide of B, while it is repulfive of another.

Now, if this polarity operates with fo much energy as to impart almoft irrefiftible expanfive force at temperature 32°, it is reafonable to fuppofe that it may begin to exert its influence, though in a far inferior degree, at temperatures fomewhat more elevated. The expanfion, therefore, that takes place, during the fall of temperature from 40°, may be imputed to the particles beginning or affecting to affume that new arrangement which their polarity affigns them, in which arrangement thefe particles occupy more fpace than before.

Again, when heat caufes water of 32° to contract, upon the fame principle, it may be conceived to operate, by counteracting the fmall portion of the difpofition to polarity that furvives the liquefaction.

I

I AM afraid that we cannot reſt ſatisfied with this explanation. We muſt not be deceived by the plauſibility of it.

THE ſtate of perfect fluidity depends upon the circumſtance, that the particles of any body admit of ready motion upon each other, and that the change of relative ſituation meets with little or no ſenſible reſiſtance.

WATER certainly poſſeſſes fluidity in a great degree, and its particles muſt of courſe encounter but little reſiſtance, as they glide the one upon the other. But if theſe particles ſhall begin to exert any degree of polarity, by which certain faces become more diſpoſed to attach to each other than certain others, this tendency would neceſſarily oppoſe that indifference with regard to poſition, which is eſſential to fluidity, and of courſe muſt impair the fluidity, and induce ſome degree of tenacity or viſcidity.

TO appearance, however, water at 32° has its fluidity as perfect as at temperatures conſiderably elevated. Unwilling to truſt to appearance, where experiment might decide, I have attempted in various ways to aſcertain whether the water ſuffers any ſenſible diminution in this reſpect while it is expanded by cold. The following method I deem the moſt correct.

FOR the purpoſe, I employed a gravimeter, the one contrived by Mr NICHOLSON for diſcovering the weight and ſpecific graty of ſolids.

THIS is a convenient inſtrument, but, unfortunately, it is by no means ſo ticklifh as a balance. Duly loaded, ſo as to be equiponderant with the water in which it is plunged, Mr NICHOLSON ſays, it is ſenſible to the 20th part of a grain. The one I have, though its ſtem be ſlender, is ſcarcely ſenſible to leſs than two or three twentieths of a grain.

THE want of ſenſibility in the gravimeter ariſes, in a great meaſure, though not entirely, from a certain degree of tenacity ſubſiſting among the particles of the fluid; and any thing that tends

tends to increafe this tenacity, muft, in the fame proportion, augment this want of fenfibility.

To afcertain whether any fenfible change in the tenacity or fluidity accompanies the expanfion of water by cold, which the theory requires, I examined the mobility of the inftrument when immerfed in water at different temperatures. I firft plunged it into this fluid, heated to between 60° and 70°. Under due loading, which funk it to the mark on the ftem, it was not fenfible to a weight lefs than two or three twentieths of a grain.

I THEN tried it in ice-cold water, and found that its fenfibility was in no perceptible degree impaired. The coldnefs of the water, it muft be remembered, caufes fome degree of contraction of the gravimeter. This contraction cannot fail to render the inftrument in fome fmall meafure more fenfible, and, fo far as it goes, to counteract the fluggifhnefs produced by any increafed tenacity in the fluid.

BUT as the body of the inftrument is made of glafs, the amount of the contraction muft be very fmall, and the change of fenfibility arifing from it fo very trifling, as certainly by no means to obfcure fuch an effect as an increafe of tenacity would occafion. I therefore with fome confidence conclude, that the fluidity of the water is not fenfibly diminifhed, and confequently that the polarity has not begun to exert any fenfible influence; it can fcarcely, therefore, be accounted the caufe of the dilatation.

END OF PART SECOND.

HISTORY

OF

THE SOCIETY.

IT has been thought more confiftent with the form which the Tranfactions of the Society have affumed in this volume, to arrange the abftracts and accounts of papers in the order of the fubjects, than in the order of time. Under each head, the papers are placed according to the dates of their communication.

METEOROLOGY.

A COMPARISON of fome Obfervations on the Diurnal Variations of the Barometer, made in PEYROUSE's Voyage round the world, with thofe made at Calcutta by Dr BALFOUR, was read by Profeffor PLAYFAIR. 1799. Jan. 7. Diurnal variations of the barometer.

THE

THE first of the obſervations here referred to were made by M. LAMANON, an ingenious naturaliſt who accompanied PEYROUSE, and who has given an account of them, (ſee 4th volume of the *Voyage*, 8vo edit.), in a letter to M. DE CONDORCET, dated, St Catherine, 5th November 1785. Dr BALFOUR's Obſervations are in the *Aſiatic Reſearches* for 1744, and a ſhort account of them is alſo inſerted in the 4th volume of the *Tranſactions, R. S. Edin.* Hiſt. p. 23.

M. LAMANON's obſervations were made in conſequence of inſtructions from the Academy of Sciences, directing him to keep an exact account of the heights of the barometer in the vicinity of the equator at different hours of the day, with a view to diſcover, if poſſible, the quantity of the variation of that inſtrument, due to the action of the ſun and moon, that quantity being there probably at its *maximum*, while the variations ariſing from other cauſes are at their *minimum*.

M. LAMANON was provided with one of NAIRNE's marine barometers, which, he ſays, was ſo little affected by the motion of the ſhip, that it might be depended on to the $\frac{1}{100}$ of an inch. In this barometer, he tells us, that from about the 11th degree of north latitude, he began to perceive a certain regular motion, ſo that the mercury ſtood higheſt about the middle of the day, from which time it deſcended till the evening, and roſe again during the night. As they approached the equator, this became more diſtinctly perceptible; and on the 28th of September, the ſhip being then in 1° 17′ north latitude, a ſeries of obſervations was begun, and continued for every hour till the 1ſt of October, at 6 A. M. The following abſtract ſhews the reſult of the obſervations on the 28th and 29th.

28th Sept.	From 4 to 10 A. M.	Barometer roſe 1 *l.* $\frac{9}{10}$
	From 10 A. M. to 4 P. M.	fell 1 $\frac{2}{10}$
	From 4 to 10 P. M.	roſe 0 $\frac{9}{10}$

29th

29th Sept.	From 10 (28th) to 4 A. M.	fell	1 *l.* $\frac{3}{10}$
	From 4 to 10 A. M.	rose	1 $\frac{5}{10}$
	From 10 A. M. to 4 P. M.	fell	1 $\frac{3}{10}$
	From 4 to 10 P. M.	rose	1

The obſervations on the 30th were to the ſame effect; and hence it is concluded that at the equator the flux and reflux of the atmoſphere produces in the barometer a variation of about 1 line $\frac{2}{10}$ Engliſh, correſponding, as M. LAMANON remarks, to a height in the atmoſphere of nearly 100 feet. According to BERNOUILLI, the action of the ſun and moon ſhould produce a tide of about 7 feet, and according to Mr DE LA PLACE, a tide not nearly ſo great.

IT ſhould be obſerved, that when theſe obſervations were made, the moon was in her laſt quarter, and the ſun a few degrees to the ſouth of the equator. The latitude on the 28th was 50′ north, and 11′ north on the 29th; in the night between that and the 30th, the ſhip croſſed the line; and on the 30th at noon, the latitude was 42′ ſouth: the longitude all this while between 17° 31′ and 18° 33′ weſt of Paris, by the time-keeper; ſo that the coaſt of Africa, which was the neareſt land, was diſtant about 8° of a great circle, and the American continent about 19°.

THE agreement between theſe, and Dr BALFOUR's obſervations at Calcutta is very remarkable. Dr BALFOUR found that during the whole lunation, in which he obſerved the barometer from half-hour to half-hour, the mercury conſtantly fell from 10 at night to 6 in the morning; from 6 to 10 in the morning it roſe; from 10 in the morning to 6 at night it fell again; and laſtly, roſe from 6 to 10 at night. The *maximum* height is therefore at 10 at night and 10 in the morning, and the *minimum* at 6 at night and 6 in the morning. The only difference is, that in Mr LAMANON's obſervations, the *minimum* is ſtated to have happened about 4, inſtead of 6. This, however, will not

not ſeem a very material difference, when it is remembered, that the inſtant when any quantity attains either its greateſt or its leaſt ſtate is not eaſily aſcertained with preciſion. From the obſervations as detailed by M. LAMANON, the time of the *minimum* ſeems to anſwer fully as well to 5 as to 4; ſo that the difference of the reſults is in every view inconſiderable, and their coincidence, on the whole, not a little ſingular. The variations in Dr BALFOUR's barometer between the neareſt *maximum* and *minimum*, is ſometimes about $\frac{1}{10}$th of an inch, though in general conſiderably leſs.

IN the abſtract of Dr BALFOUR's obſervations referred to above, it is remarked, that it ſeems not improbable that theſe variations of the barometer are connected with the reciprocations of the ſea and land winds during the day and night. But whatever may have been formerly the probability of this ſuppoſition, it is entirely deſtroyed by the obſervations of the French navigators. Theſe obſervations were made too far out at ſea to leave room for ſuppoſing that the land winds had any influence on the phenomena to which they refer. It is at the ſame time doubtful, whether thoſe phenomena can be aſcribed to the atmoſpherical tides produced by the ſun and moon, as the *ebbing* and *flowing* of the mercury in the barometer appears to have no dependence on the poſition of thoſe luminaries relatively to one another, but happens, it would ſeem, conſtantly at the ſame hour, in all aſpects of the moon and all ſeaſons of the year. The ſubject is well deſerving of a fuller inveſtigation. We ſhould probably before now have had farther information reſpecting it, if happily the able navigator above named, and his brave aſſociates, had been deſtined to reviſit their native ſhores. The cruel fate of an expedition ſo well planned, and ſo well appointed for the purpoſes of ſcience, will never ceaſe to be matter of ſincere regret.

AN

An Account of an Aurora Borealis, obſerved in day-light at Aberfoyle in Perthſhire, on the 10th February 1799, by Patrick Graham, D. D. miniſter of Aberfoyle, was communicated by the Reverend Dr Finlayson. 1799. Nov. 4. An aurora borealis obſerved by day-light.

"On the 10th of February 1799, about half-an-hour paſt 3 o'clock P. M., the ſun being then a full hour above the horizon, and ſhining with an obſcure luſtre through a leaden-coloured atmoſphere, I obſerved," ſays Dr Graham, "the rare phenomenon of an aurora borealis by day-light. The weather, for ſeveral days before, had been intenſely cold; and during the two preceding days, much ſnow had fallen. On this day a thaw had come on, and the temperature of the air was mild. The general aſpect of the ſky was ſerene. Some dark clouds hung on the horizon between S. W. and W. I was intenſely obſerving a large halo about the ſun, of about 20 degrees in ſemidiameter: It exhibited the priſmatic colours, though obſcurely, except in one quarter, where it coincided with the ſkirt of a dark cloud on the horizon, almoſt directly weſt. In that portion of the halo, the colours of the iris were very diſtinctly exhibited.

Whilst I was attending to this appearance, the whole viſible hemiſphere of the heavens became covered with a light paliſh vapour, as I at firſt imagined it to be. It was diſpoſed in longitudinal ſtreaks, extending from the weſt, by the zenith, and all along the ſky towards the eaſt. On examining this appearance more narrowly, I found it to be a true aurora borealis, with all the characters which diſtinguiſh that meteor when ſeen by night, excepting that it was now entirely pale and colourleſs. The ſtream of electric matter iſſued very perceptibly from the cloud in the weſt, on the ſkirts of which the halo exhibited the priſmatic colours; thence diffuſing themſelves, the rays converged towards the zenith, and diverged again towards every quarter of the horizon; and the corruſcations were equally inſtantaneous, and as diſtinctly perceptible as they are by night.

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This appearance continued for more than 20 minutes, when it gradually vaniſhed, giving place to thin ſcattered vapours, which, towards ſun-ſet, began to overſpread the ſky. Through the enſuing night, I could not diſcern the ſmalleſt trace of theſe meteors in the ſky.

This appearance, I find, is not altogether new. In the *Annual Regiſter* for 1789, there is an account, extracted from the *Philoſophical Tranſactions*, of an aurora borealis ſeen by day-light in Ireland, by Dr Henry Usher, who, conſidering himſelf as the firſt obſerver of this phenomenon, requeſts that any perſon to whom a ſimilar appearance may occur would communicate his obſervations. There is indeed reaſon to believe, that the phenomenon occurs not unfrequently; but unleſs it is attended to very accurately, it will in general eſcape obſervation; nor ſhould I have at this time remarked it, had I not been engaged in obſerving the ſolar halo. Whenever the ſky, being for the moſt part cloudleſs, is ſuffuſed with thin pale vapours, eſpecially if diſpoſed in longitudinal ſtreaks, obſervers ſhould look out for this phenomenon."

1800. Jan. 6. Phenomenon of two interſecting rainbows.

An account of two interſecting rainbows, ſeen at Dunglaſs in Eaſt Lothian in July laſt, was communicated by Profeſſor Playfair.

"At Dunglaſs, where I happened to be in the beginning of July laſt, our attention was called one evening, a little before ſunſet, to a very large and beautiful rainbow, formed on a cloud which hung over the ſea, and from which a ſhower was falling at a conſiderable diſtance to the S. E. The ſun was about 2° high, ſo that the arch was not much leſs than a ſemicircle, with its higheſt point elevated about 40°. At the point where the northern extremity of this arch touched the horizon, another arch ſeemed alſo to ſpring from the ſea, diverging from the former at an angle of 3° or 4°, on the ſide toward the ſun.

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This arch did not exceed 7° or 8° in length; it was of the ſame breadth with the principal bow; it had the colours in the ſame order, and nearly of the ſame brightneſs; or, if any difference was diſcernible, it was, that the tranſition from one colour to another was not made with ſo much delicacy in the laſt-mentioned rainbow as in the former.

We recollected that a phenomenon ſimilar to this is deſcribed in the *Philoſophical Tranſactions*, as having been ſeen at Spithead, and that it is aſcribed by the gentleman who obſerved it to the reflection of the ſun's rays from the ſurface of the ſea, ſo as to fall on the cloud where the rainbow was formed. This hypotheſis ſeemed to agree exactly with the phenomenon now before us.

The accidental rainbow, for ſo it may be called, was ſeen only at the extremity where the principal arch roſe from the ſea, and where, of conſequence, the ſun's rays, reflected from the ſurface of the water, at that moment very ſmooth, might fall on the drops of rain. The other parts of the cloud could not receive rays ſo reflected, as the land intervened, and there, accordingly, no veſtige of the accidental rainbow was obſerved.

The accidental rainbow lay, as was already ſaid, on the ſide toward the ſun, and this is agreeable to the hypotheſis; for the rays that after reflection from the ſurface of the water fell on the drops of rain, muſt have come as from a point as much depreſſed below the horizon, as the ſun was at that inſtant elevated above it. The axis of the accidental rainbow muſt therefore have made with the axis of the principal, an angle equal to twice the ſun's elevation, and its centre muſt have been elevated by that ſame quantity above the centre of the other, ſo that if it had been complete, it would have been wholly between the principal rainbow and the ſun.

The only circumſtance in which the appearances did not perfectly correſpond with this hypotheſis, was, that the two rainbows did not interſect one another in the horizon, but rather a little above it. This, however, ought to have no great weight, as the reflected image of the ſun cannot have preſented to the cloud a diſk ſo regular and well defined as the ſun itſelf and the accidental rainbow muſt have ſomewhat participated of this indiſtinctneſs.

When phenomena of this kind occur, it would afford a ſure means of trying the juſtneſs of the explanation, if the inclination of the two bows were obſerved, and alſo the ſun's altitude at the ſame time. Theſe two things are neceſſarily connected; for if we call I the angle of their interſection, E the elevation of the ſun, and S the angle ſubtended at the eye by the ſemidiameter of the rainbow, if complete, an angle which is conſtantly the ſame, and nearly equal to 42°, it is eaſy to infer from ſpherical trigonometry, that $\text{ſin} \frac{1}{2} I = \frac{\text{ſin } E}{\text{ſin } S}$.

Computing from this formula, the inclination of the two bows in the preſent inſtance comes out nearly 5°; ſomewhat greater than I was inclined to eſtimate it by the eye.

Phenomena of this kind can but rarely occur, as the neceſſary conditions will not often come together. The principal rainbow muſt be over the ſea; the ſea itſelf muſt extend ſomewhat on the ſide toward the ſun; it muſt be ſmooth and tranquil, and the ſun ſo low that the light reflected from the water may be conſiderable. Were it ever to happen that the accidental bow was completely formed, the effect could not fail to be very ſtriking.

CHEMISTRY.

CHEMISTRY.

Sir George Mackenzie, Bart. read a paper containing an account of experiments which he had made on the Combuſtion of the Diamond. 1800. Feb. 3. On the combuſtion of the diamond.

What follows is a ſhort abſtract of this paper; the paper itſelf having been publiſhed in *Nicholſon's Journal* for July 1800.

The firſt object of theſe experiments was to aſcertain the temperature at which the combuſtion of the diamond takes place. For this purpoſe a diamond, together with a pyrometer of Wedgewood, was placed on a piece of baked clay, and puſhed gradually into the muffle: when both were perfectly red throughout, the pyrometer was withdrawn, and indicated 13°. The diamond had acquired the dim milky appearance which is known to indicate an incipient combuſtion. It was then replaced in the muffle, together with the pyrometer; and the heat, being ſlowly increaſed till a glow, indicating that the diamond was completely on fire, appeared, was continued, as equal as poſſible, till the diamond was totally conſumed. The pyrometer, when meaſured, indicated 14°. In another experiment, the heat required to produce the glow was 15°, and at this temperature the diamond was wholly conſumed.

Similar experiments were repeated, with different diamonds, and with nearly the ſame reſult, which ſhews that the heat required for their combuſtion is much leſs than it has hitherto been ſuppoſed.

Guyton's experiment of converting iron into ſteel by means of the diamond, was repeated in the following manner: Into a hollow cylinder of ſoft iron, cloſed at one end, ſome ſmall diamonds were put, and a ſtopper of the ſame iron being afterwards applied, the two pieces were rivetted together at the top.

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The cylinder being then placed in a Hessian crucible, and surrounded with a mixture of dry sand and clay, was subjected to the fire of a smith's forge for an hour. When taken out, the pyrometer indicated 151°; the upper part of the iron appeared to have been melted, and several bright metallic globules were seen adhering to the compacted mass of sand and clay next the iron. The lower part of the cylinder retained its shape, but was blistered on the surface, except at the bottom, which remained smooth. When the ends were polished and touched with diluted nitric acid, they exhibited the spot characteristic of steel, that on the end which had been fused being considerably darker than the other. The part which had remained smooth after being cut off, was heated red hot, and plunged into cold water, when it became so hard, that it received no impression from a file. Several cavities were found within the cylinder, but the diamonds had totally disappeared.

The whole cylinder of soft iron was thus converted into steel; one end of it having been melted, was in the state of cast steel; the other end, having remained solid, was of the kind of steel produced by cementation.

A gentleman of the Clyde Ironworks having about that time been led, by some experiments, to doubt of the conclusiveness of those made by Guyton, and to suspect that the diamond had contributed nothing to the conversion of the iron into steel, Sir George Mackenzie thought it necessary to direct some experiments to the solution of this question.

In one of these a piece of soft iron, in all respects similar to that used in the last experiment, was exposed to the same degree of heat as in that experiment, and with all the circumstances alike, except that the diamonds were wanting. The result was, that the iron was not altered in its shape or qualities. It had been exposed to the heat an hour, and the pyrometer marked 152°.

152°. Hence it was natural to conclude, that the converſion into ſteel in the former experiment, is ſolely to be attributed to the action of the diamond. It had been ſuggeſted by the gentleman above alluded to, that when ſoft iron is expoſed to a high temperature, carbon diſſolved in caloric penetrates the crucibles, and converts the iron into ſteel. To bring this hypotheſis to the teſt of experience, it was thought proper to expoſe the ſoft iron to a ſtrong heat in an apparatus which muſt exclude all carbonaceous matter, except ſuch as was ſo diſſolved. The ſubſtance which, by being interpoſed between the iron and the crucibles, ſeemed to be moſt likely to anſwer this purpoſe, was the white felſpar or adularia; Dr KENNEDY, in an analyſis of this ſubſtance, having found that it began to be vitrified in a heat of 90°, ſo that there was reaſon to think, that before the heat was intenſe enough to melt the iron, the iron would be ſurrounded by a maſs free from carbonaceous matter, and impervious alſo to carbon coming from without.

INTO a ſmall crucible, made of the porcelain clay of Cornwall, was put ſome felſpar in fine powder, and upon this a ſmall cylinder of ſoft iron, after which the crucible was filled with the felſpar, which therefore ſurrounded the iron on all ſides. The crucible was then placed in an air furnace, in which the heat was gradually raiſed for an hour, and continued for another hour at its higheſt pitch, after which the crucible was withdrawn. The heat marked by the pyrometer was 152°. The iron retained its ſhape and all its properties unchanged. The felſpar was reduced to a glaſs, tranſparent and colourleſs, except that it had received a greeniſh tinge where it was in contact with the iron.

SEVERAL other experiments were made with the ſame view as this laſt, but with the circumſtances varied as much as poſſible, and in all the reſult was the ſame; that is, the iron, without the preſence of the diamond, was never converted into ſteel; ſo that

that there appears no reafon to fuppofe that foft iron can be converted into fteel by carbon penetrating from the fuel through the crucibles.

Sir George Mackenzie concludes his account of thefe very interefting experiments with the defcription of one which appears to be entirely new, and tends ftill farther to prove the identity of carbon and diamond.

Having prepared fome pure oxide of iron from a folution of the fulphate, by precipitation with cauftic ammonia, he mixed a fmall quantity of it with one-fourth of its weight of diamond powder, prepared in the following manner.

The diamond, being reduced to powder in a fteel mortar, was boiled in muriatic acid, to diffolve the iron which might have been abraded from it. After proper edulcoration, it was heated in a muffle, to burn off the carbon of the fteel, which remained after treatment with the acid, and which rendered the powder of a grey colour. He obferved the coaly matter take fire at the edge of the heap of powder next the ftrongeft heat, and gradually fpread itfelf, till at laft the whole appeared as if burning. The glow through the powder ceafed foon after, and on removing it, he found it perfectly clean and white. From the diminution of the original weight of the diamond, he found that a part of it had alfo been confumed.

The mixture of oxide and diamond powder thus prepared was put into a Cornifh-clay crucible, and expofed to a pretty ftrong heat for half an hour, after which the oxide was found to be reduced into a metallic button of caft-iron *.

Another portion of the oxide of iron, ufed in this experiment, was not reduced, when placed in the fame circumftances, without the diamond.

MINERALOGY.

* In the courfe of this experiment it was afcertained, that the fufing point of iron is between 153° and 158° of Wedgwood's pyrometer.

MINERALOGY.

THE Reverend Dr WILLIAM RICHARDSON, late F. T. C. D., having ſent to Dr HOPE a collection of ſpecimens from the northern coaſt of Antrim, with a catalogue, and obſervations, the ſpecimens were exhibited, and the obſervations were read in the Royal Society, March 1803.

1803. March 7. Remarks on the baſaltes of the Coaſt of Antrim.

SILICEOUS BASALT.

DR RICHARDSON diſcovered the foſſil to which he gives this name in the peninſula of Portruſh, four or five years ago. It abounds alſo in the Skerry iſlands, a reef of rocky iſlots extending from the northern point of Portruſh-head for about a mile eaſtward. A ſmall part of every one of thoſe iſlots is formed of this ſtone, while the remainder conſiſts of coarſe baſalt, ſimilar in all reſpects to that on the eaſt ſide of the above-mentioned peninſula. It is met with in one or two other places.

THIS ſtone is arranged in ſtrata, from ten to twenty inches thick, all ſteadily parallel to one another, and every ſtratum, as far as can be obſerved, preſerving an uniform thickneſs through its whole extent. When theſe ſtrata are quarried into, they appear to be conſtructed of large priſms, generally pentagonal, which when broken divide into ſmaller priſms. This internal priſmatic conſtruction frequently gives an irregular or ſhivery appearance to the fracture, which however is often conchoidal, and the grain as uniform as in the Giant's Cauſeway baſaltes.

THE beds of this foſſil are remarkable for containing marine exuviæ in great abundance, particularly impreſſions of *cornua ammonis*. The flat ſhells and impreſſions contained in theſe ſtones are ſteadily parallel to each other, and perpendicular to the axis of the priſms. It muſt be obſerved, that the priſmatic conſtruction is never interrupted by the ſhells diſperſed through

 it;

it; the planes which ſeparate the priſms paſſing equally through the ſhells and the ſtone itſelf.

The grain of this ſtone paſſes by inſenſible ſhades from a high degree of fineneſs, until it become undiſtinguiſhable from that of the common columnar baſaltes.

The name of Siliceous Baſalt, which Dr Richardson employs, was firſt given to this foſſil by Mr Pictet of Geneva, when he viſited Portruſh, in a tour through Ireland two years ago. He conſidered it as a variety of baſalt, containing a greater proportion of *ſilica* than uſual.

The ſtrata of ſiliceous baſalt, both at Portruſh and the Skerry iſlands, generally alternate with ſtrata of equal thickneſs of a coarſe-grained baſalt of a grey colour. The materials of the ſtrata grow into each other, ſo as to form one ſolid maſs, from which it is eaſy to quarry pieces in the confine of the two ſtrata, with a part of each adhering; but the coarſe baſalt, as it approaches very near to the fine, always abates ſomewhat of its coarſeneſs; yet the line of demarcation is left completely diſtinct.

The peninſula of Portruſh lies about ſix miles to the weſt of the Giant's Cauſeway, and on its eaſtern ſurface alone preſents theſe ſtrata.

In the ſpace of about 700 yards, it exhibits in miniature thoſe changes and interruptions of the ſtrata, which occur on the large ſcale along the northern baſaltic coaſt of Ireland. At the place where it emerges from the ſtrand, there firſt occurs a maſs compoſed of ſtrata of the coarſe and ſiliceous baſalt, placed over each other alternately; this is ſucceeded by an accumulation of regular ſtrata of the coarſe baſalt alone. A ſecond alternation, and a ſecond accumulation of the coarſe-grained ſtrata, come in order, and extend to the well called Tubber Wherry. Here commences an accumulation of many ſtrata of the ſiliceous baſalt alone, which ſtretches along the ſhore for about 100 yards, and then changes into a third alternation, which continues neus to the little boat-harbour, called *Port-in-too*, near which the ſiliceous baſalt diſappears. Over this ſtretch, notwithſtanding the frequent change in the arrangement of the ſtrata, the thickneſs of each ſtratum, of both ſpecies, remains pretty nearly the ſame, and the poſition of them all ſteadily ſo, viz. with a conſiderable dip to E. N. E.

The weſt ſide of the peninſula, though only about 400 yards diſtant, conſiſts entirely of coarſe baſalt. It ſhows a bolder face, and is formed of rude maſſive pillars from 60 to 80 feet long.

"I am aware," ſays Dr Richardson, "that ſeveral mineralogiſts deny the ſhell-bearing ſtone to be baſalt, while others contend ſtrenuouſly that it is. I will not venture to decide on the queſtion, but muſt remark, that I have never met with it but contiguous to baſalt, and ſo ſolidly united to this laſt, that the continuity of the whole maſs was uninterrupted. The grain of the ſtone graduates, as has been already remarked, into that of the common baſaltes; and the arrangement of it, and that of the baſalt, with which it is ſo much mixed at Portruſh and the Skerry iſlands, is exactly the ſame; the ſtrata of each ſcarcely differing in thickneſs, and not at all in inclination. The ſtrata of both kinds break into priſms, and the ſurfaces, where acceſſible, exhibit the appearance of cauſeways, differing only in this, that in the ſiliceous baſalt, the pentagon is the prevalent figure, and in the coarſe baſalt, the quadrangle. The fuſibility of both ſtones is alſo nearly the ſame; the ſhells in the ſiliceous baſalt are calcined in the fire, and many more are then diſcovered which had before eſcaped the eye *."

 WHINSTONE

* Dr Richardson obſerves, that ſome mineralogiſts deny that this foſſil is baſalt. Several of the members preſent when this paper was read, ſome of whom had examined the ſtone in its native place, were of that number. It was remarked, that though certain portions of the ſtrata of this foſſil bore much reſemblance to ſome ſpecies of baſalt, by far the greater part of the maſs bore no reſemblance whatever to any.

It was alſo ſtated, that the ſubſtance of the coarſe-grained, undiſputed baſalt, which lies between the ſtrata of this ſtone, does not contain any veſtiges of marine animals;

WHINSTONE DIKES ON THE COAST OF ANTRIM.

Dr Richardson defcribes fome particulars in the conftruction of the whinftone dikes on the coaft of Antrim, which appear fingular, and deferving of attention. Thefe dikes, he fays, are uniformly formed of large maffive prifms laid horizontally, which are always divifible into fmaller prifms that are likewife horizontal. To prevent confufion, he calls the firft of thefe *component prifms*, and the fecond, or fmaller ones into which the others break, *conftituent prifms*.

The component prifms are fometimes of enormous fize, and in the fame dike are nearly equal; the conftituent prifms are fmall, (the fides about an inch long), and neatly formed.

The dike which traverfes the Giant's Caufeway, differs from thofe on other parts of the coaft, by having no component prifms. It refembles a plain wall, of which the parts fhiver under the hammer into very neat conftituent prifms. In the dike at Seaport the fame thing is obferved; the prifmatic ftructure does not penetrate two inches from its edge; the whole interior feems an amorphous mafs.

The fpecimens of this latter dike, fent to Dr Hope, exhibit its continuity with the adjacent bafaltic rock which it traverfes, and alfo the continuity of the fine bafalt of its edge with the *granular* ftone which compofes the middle of the dike.

The dike of *Port-coan* is a very folid mafs, compofed of ftones apparently round, and imbedded in a bafaltic pafte, or indurated mortar. The round ftones are formed of concentric fpheres,

animals: That veins often iffue from the beds of this real bafalt, and pervade the fuppofed filiceous fpecies; fome of them connecting together the feparate beds of the real bafalt; others dying away in flender ramifications, as they rife through the interpofed ftratum. In no inftance is this reverfed: The veins never proceed from what is called the Siliceous Bafalt. It was farther obferved, that both the fracture and external furface of this ftone exhibit a ftratified ftructure, in many inftances, which never happens in the true bafaltes.

ſpheres, like the coats of an onion; they exceed a foot in diameter, and, together with the mortar by which they are united, they form a very compact and highly indurated rock.

Besides theſe large dikes, Dr Richardson remarks, that veins from half an inch to an inch and a half thick, often cut the baſaltic ſtrata on that coaſt in all directions. The materials of theſe veins are never the ſame with the contiguous baſalt, but are generally finer. At Portruſh is a large vein, and near it a ſmaller vein, not an inch thick, which, proceeding from below, terminates in the ſolid rock before it reaches the ſurface.

MISCELLANEOUS OBSERVATIONS.

Some of the ſpecimens in Dr Richardson's catalogue are from a quarry in a maſs of baſalt at Ballylugan, two miles ſouth of Portruſh. This baſalt contains ſmall cavities in its interior; many of them full of freſh water, which guſhes out when the ſtone is broken by the hammer, as if it had been in a ſtate of compreſſion. The ſtone is ſo hard, and flies ſo in pieces, that Dr Richardson has not been able to collect any of the water for the purpoſe of analyſis.

The face of the quarry in which this variety of the baſalt is found is about 15 feet high, and is cut into a ſtratum, the thickneſs of which is not yet aſcertained. The rock is entirely columnar, the pillars ſomewhat ſmaller than thoſe of the Giant's Cauſeway, leſs perfect, not articulated, ſometimes bent, and variouſly inclined. The ſides and the interior of the pillars are full of cavities. In conſequence of the obſervations of Dr Hamilton and Mr Whitehurst reſpecting the porous texture of the air or bladder holes of the baſaltes of the Cauſeway and its vicinity, Dr Richardson has examined a great variety; but in no inſtance, except this of Ballylugan, has he found

cavities,

cavities in the interior of the basaltic rocks on this coast, though they are frequent on the surface exposed to the air.

The last variety of whinstone enumerated by Dr Richardson is the Ochrous, which makes, as he says, a conspicuous figure in the stupendous precipices along the coast of Antrim. It is disposed in extensive strata of every thickness, from an inch to twenty-four feet, and varies in colour, from a bright minium to a dull ferruginous brown.

Three remarks are made by Dr Richardson, that are undoubtedly of importance, and show that this stone is merely basalt in a certain state of decomposition.

1. The ochrous strata are extensive; they remain always parallel to the basalt strata which they separate; they unite to the basalt without interrupting its solidity; the change from the one to the other is sudden, and the lines of demarkation are distinct. The ochrous stone is never found but contiguous to other basalt.

2. The substances imbedded in the ochrous rock, and in basalts, are exactly the same; calcareous spar, zeolite, chalcedony, &c.

3. Among the varieties which this rock presents, there may be found every intermediate stage between sound basalt and perfect ochre. The change is often partial, beginning with veins and slender ramifications.

ALGEBRA.

1800. Jan. 10. Rule for reducing a square root to a continued fraction.

Rule for reducing to a Continued Fraction the Square Root of any given Integer Number, not a Square. By James Ivory, Esq. Communicated 10th January 1801.

1. Let N be the given integer number, and take n the root of the square next less than N; and, for the sake of uniformity, put $P^\circ = 1$, $R^\circ = 0$, $\mu = n$.

2. Take

2. TAKE $P' = N - n^2$ for a divisor, and $2n - R^\circ = 2n$ for a dividend. Let the quotient be μ', and remainder R'.

3. TAKE $P'' = P^\circ - (R^\circ - R')\mu'$, for a divisor, and $2n - R'$ for a dividend. Let the quotient be μ'' and the remainder R''.

4. TAKE $P''' = P' - (R' - R'') \times \mu''$ for a divisor, and $2n - R''$ for a dividend. Let the quotient be μ''', and the remainder R'''.

5. THESE operations may be continued without end; the divisor P^ρ being found from the formula $P^\rho = P^{\rho-2} - (R^{\rho-2} - R^{\rho-1}) \times \mu^{\rho-1}$: the corresponding dividend being $2n - R^{\rho-1}$; and the quotient of the division being denoted by μ^ρ, and the remainder by R^ρ. But it will only be necessary to continue these operations till we arrive at a value $P^\rho = P^\circ = 1$, which will always necessarily be the case. After this, the series of numbers, $\mu^{\rho+1}$, $\mu^{\rho+2}$, $\mu^{\rho+3}$, will necessarily be the same as the numbers μ', μ'', μ''', &c.; μ^ρ, continually repeated in their order.

THE rule may be shortly expressed in algebraic language, thus:

$P^\circ = 1$;	$\mu = n \times 1 + R^\circ = n$;
$P' = N - n^2$;	$2n - R^\circ = 2n = P' \times \mu' + R'$;
$P'' = 1 - \mu'(R^\circ - R') = 1 + \mu' R'$;	$2n - R' = P'' \times \mu'' + R''$;
$P''' = P' - \mu''(R' - R'')$;	$2n - R'' = P''' \times \mu''' + R'''$;
$P^{iv} = P'' - \mu'''(R'' - R''')$;	$2n - R''' = P^{iv} \times \mu^{iv} + R^{iv}$;

and so on.

HAVING thus found the numbers, μ, μ', μ'', μ^ρ, we shall have the continued fraction sought,

$$\sqrt{N} = \mu + \cfrac{1}{\mu' + \cfrac{1}{\mu'' + \cfrac{1}{\mu''' + \text{\&c.}\ \cfrac{1}{\mu^\rho}}}}$$

And the fraction may be continued indefinitely, by repeating the denominators μ', μ'', ... μ^ρ, continually in their order.

EXAMPLE

EXAMPLE I. To reduce the square root of 13 to a continued fraction.

THE operation will be as under:

$N = 13,\ n = 3,\ 2n = 6.$

$$P^{\circ} = 1,\ \mu = 3,\ R^{\circ} = 0.$$

$$P' = 13 - 9 = 4; \quad \frac{6-R^{\circ}}{4} = \frac{6}{4} = 1 + \frac{2}{4};\ P' = 4,\ \mu' = 1,\ R' = 2.$$

$$P'' = 1 + \mu' R' = 1 + 2 = 3; \quad \frac{6-R'}{3} = \frac{4}{3} = 1 + \frac{1}{3};\ P'' = 3,\ \mu'' = 1,\ R'' = 1.$$

$$P''' = 4 - 1 \times (2-1) = 3; \quad \frac{6-R''}{3} = \frac{5}{3} = 1 + \frac{2}{3};\ P''' = 3,\ \mu''' = 1,\ R''' = 2.$$

$$P^{iv} = 3 - 1 \times (1-2) = 4; \quad \frac{6-R'''}{4} = \frac{4}{4} = 1 + \frac{0}{4};\ P^{iv} = 4,\ \mu^{iv} = 1,\ R^{iv} = 0.$$

$$P^{v} = 3 - 1 \times (2-0) = 1; \quad \frac{6-R^{iv}}{1} = \frac{6}{1} = 6 + \frac{0}{1};\ P^{v} = 1,\ \mu^{v} = 6,\ R^{v} = 0.$$

Here I stop, because, $P^{v} = P^{\circ} = 1$; and I conclude that the fraction sought is formed by the numbers, $\mu', \mu'', \mu''', \mu^{iv}, \mu^{v}$; that is, by the numbers 1, 1, 1, 1, 6, continually repeated in their order. Thus,

$$\sqrt{13} = 3 + \cfrac{1}{1 + \cfrac{1}{1 + \cfrac{1}{1 + \cfrac{1}{1 + \cfrac{1}{6 + \cfrac{1}{1}\ \&c.}}}}}$$

EXAMPLE II. To reduce $\sqrt{61}$ to a continued fraction:

$N = 61,\ n = 7,\ 2n = 14.$

$$P^{\circ} = 1,\ \mu = 7,\ R^{\circ} = 0.$$

$$P' = 61 - 49 = 12; \quad \frac{14-R^{\circ}}{12} = \frac{14}{12} = 1 + \frac{2}{12};\ P' = 12,\ \mu' = 1,\ R' = 2.$$

$$P'' = 1 + 1 \times 2 = 3; \quad \frac{14-2}{3} = \frac{12}{3} = 4 + \frac{0}{3};\ P'' = 3,\ \mu'' = 4,\ R'' = 0.$$

$$P''' = 12 - 4 \times (2-0) = 4; \quad \frac{14-0}{4} = \frac{14}{4} = 3 + \frac{2}{4};\ P''' = 4,\ \mu''' = 3,\ R''' = 2.$$

$$P^{iv} = 3 - 3 \times (0-2) = 9; \quad \frac{14-2}{9} = \frac{12}{9} = 1 + \frac{3}{9};\ P^{iv} = 9,\ \mu^{iv} = 1,\ R^{iv} = 3.$$

P^{v}

$P^{v} = 4 - 1 \times (2 - 3) = 5$; $\frac{14-3}{5} = \frac{11}{5} = 2 + \frac{1}{5}$; $P^{v} = 5, \mu^{v} = 2, R^{v} = 1$.

$P^{vi} = 9 - 2 \times (3 - 1) = 5$; $\frac{14-1}{5} = \frac{13}{5} = 2 + \frac{3}{5}$; $P^{vi} = 5, \mu^{vi} = 2, R^{vi} = 3$.

$P^{vii} = 5 - 2 \times (1 - 3) = 9$; $\frac{14-3}{9} = \frac{11}{9} = 1 + \frac{2}{9}$; $P^{vii} = 9, \mu^{vii} = 1, R^{vii} = 2$.

$P^{viii} = 5 - 1 \times (3 - 2) = 4$; $\frac{14-2}{4} = \frac{12}{4} = 3 + \frac{0}{4}$; $P^{viii} = 4, \mu^{viii} = 3, R^{viii} = 0$.

$P^{ix} = 9 - 3 \times (2 - 0) = 3$; $\frac{14-0}{3} = \frac{14}{3} = 4 + \frac{2}{3}$; $P^{ix} = 3, \mu^{ix} = 4, R^{ix} = 2$.

$P^{x} = 4 - 4 \times (0 - 2) = 12$; $\frac{14-2}{12} = \frac{12}{12} = 1 + \frac{0}{12}$; $P^{x} = 12, \mu^{x}, = 1, R^{x} = 0$.

$P^{xi} = 3 - 1 \times (2 - 0) = 1$; $\frac{14-0}{1} = \frac{14}{1} = 14 + \frac{0}{1}$; $P^{xi} = 1, \mu^{xi} = 14, R^{xi} = 0$.

HERE I ſtop, becauſe $P^{xi} = P^{\circ} = 1$. And the ſeries of numbers ſought is, 1, 4, 3, 1, 2, 4, 1, 3, 4, 1, 14.

ON turning to page 378 of the Engliſh edition of EULER's *Algebra*, it will be found that the table there given conſiſts of the two ſeries of numbers, P°, P', P'', &c. and μ, μ', μ'', μ''', &c.

THIS rule is the more worthy of notice, that it proceeds by certain definite arithmetical operations: whereas the method of M. DE LA GRANGE determines the numbers, μ, μ', μ'', &c. by appreciating the value of certain expreſſions to the neareſt unit, or by a proceſs that is in ſome meaſure tentative, and therefore not ſtrictly analytical.

SURGERY.

Mr RUSSEL read an account of a ſingular variety of Hernia which occurred to him while he was delivering clinical lectures in conjunction with Dr BROWN and Mr THOMSON. Mr THOMSON diſſected the parts with great care and accuracy, and diſcovered certain peculiarities, which makes the knowledge of this

1803. Mar. 7. A ſingular variety of hernia.

variety a real addition to the pathology of the diſeaſe. It is a modification of Inguinal Hernia. But as the circumſtances of difference are conſtant and eſſential, it may fairly be regarded as a diſtinct ſpecies. In the common caſes of inguinal hernia, the viſcera which are to form the protruſion enter the upper and internal orifice of the abdominal ring, along with the ſpermatic cord, accompany the cord through the whole length of the paſſage, and come out along with it at the inferior and external orifice of the ring. Thus the hernia is formed by the dilatation of a natural paſſage. But in this new and hitherto undeſcribed variety, the viſcera burſt through the common parietes of the abdomen, exactly oppoſite to the lower and external orifice of the ring, where they come into contact with the ſpermatic cord, and deſcend along with it directly into the ſcrotum. This hernia, therefore, reſembles a ventral hernia in its commencement, by beginning to protrude where there is no natural opening; and it reſembles an inguinal hernia, by paſſing through the lower and external orifice of the abdominal ring, where the protruſion of the common inguinal hernia is completed. Thus it is a hernia of a mixed nature, forming an intermediate ſpecies between a ſimple and pure ventral hernia, and the common and perfect inguinal hernia.

ANOTHER eſſential circumſtance reſpects the poſition of the hernia with regard to the courſe of the epigaſtric artery. In ordinary caſes of inguinal hernia, the epigaſtric artery runs on the inſide of the hernia, but in this variety it conſtantly runs on the outſide. This leads to ſome important conſiderations in practice, though Mr RUSSEL did not enter fully into the applications of the peculiarities, either to practical points, or to the explanation of various curious circumſtances in the hiſtory of hernial ſwellings.

ANTIQUITIES.

ANTIQUITIES.

THE following letter from the Abbé MANN was read 18th June 1799. 1799. June 18. Concerning the Chartreuſe of Perth.

To the Preſident and Members of the Royal Society of Edinburgh.

THE following few notes, concerning the *Chartreuſe* of Perth, of no merit but as they regard an exiguous part of the Scotiſh hiſtory, were made in the Auſtrian Netherlands, from original manuſcripts now ſwallowed up by the revolution of thoſe countries, and for ever loſt. That theſe ſhort extracts may not likewiſe fall into oblivion in a foreign country, they are offered, with great reſpect, to the learned Royal Society of Scotland, by their moſt obedient humble ſervant,

T. A. MANN, F. R. & A. S. of London.

Prague in Bohemia, May 1. 1798.

EXTRACTA MANUSCRIPTORUM *de Carthuſiâ* Vallis Virtutum, *in Suburbano Perthano.*

"ANNO 1430, JACOBUS I. Scotiæ Rex, fundavit Domum Virtutum in ſuburbano Perthano. OSWALD DE CORDE primus Prior *."

THE Scots hiſtorians ſay that King JAMES I. of Scotland founded with great coſt and magnificence the Carthuſian monaſtery near Perth, and endowed it with large revenues; and alſo, that he was buried in the church thereof †.

 "OSWALDUS

* Ex Collectaneis Manuſcriptis PETRI DE WALE, vol. i. p. 9. penes Carth. Bruxellenſem.

† So BUCHANNAN. STEWART's Short Hiſtorical and Genealogical Account, p. 64. 65. Scottiſh Compend. p. 18.

"Oswaldus de Corda, primus Prior in Scotia, fuit imprimis Carthusiæ majoris Vicarius; subindè Domûs Virtutum in Scotia, Prior primus effectus. Fuit acer ingenio, doctrinâ præclarus, vitæ sanctimoniâ eximius. Inter alia multa, condidit pro correctura, librorumque Ordinis Carthusiani correctoribus, opus imprimis utile, multisque acceptum, et junioribus monachis pernecessarium; utpote quod in lecturis suis eos probè dirigere queat. Postquam autem diutinos in suscepto regimine suscepisset labores, tandem coram positis fratribus ac orantibus, diem clausit extremum, anno Christi tricesimo quarto post mille quadringentos."

"Obiit D. Oswaldus primus Prior Domûs Vallis Virtutum in Scotia, professus domûs Hortus Christi, deinde vicarius Majoris Carthusiæ *."

Ordinatio Capituli Generalis, anni m,cccc,ii.

"Domus Vallis Virtutum in Scotia, ad instantiam Vicarii et Conventûs, eximitur à provincia Picardiæ remotioris, et provinciâ Gebennensi incorporatur, et sub speciali cura Reverendi Patris (Generalis) ponitur †."

From this time (1442) the Chartreuse of the *Vale of Virtues* near Perth, was annumerated in the province of the *Grande Chartreuse*, or of Geneva, as it was anciently called, after having been annexed for twelve years to the province of the Low Countries.

1442. "Mortuo Adamo Priore, præficitur in priorem domûs Vallis Virtutum, à capitulo generali, Dom. Laurentius Henton, professus dictæ domûs, non obstante quòd prædictus in ordine Cistercienfi fuit professus, super quo cum illo dispensatur ‡."

1489.

* Ex Chartis Capituli Generalis Ordinis Carthus., anni 1435.

† Ex Charta Capituli Generalis, anni 1442.

‡ Ibid. Ex Chartis Capituli Gen. Ord. Cart. 1489.

1489. "OBIIT D. RICH. GAERGEN, monachus professus domûs Axholme, Prior Domûs Vallis Virtutum in Scotia."

1502. "D. ROB. BELLINTON, professus Domûs Vallis Virtutum in Scotia, et singularis benefactor ejus, obiit 21. Septembris. Aliàs fuit abbas Sanctæ Crucis, ordinis SANCTI AUGUSTINI."

1518. "OBIIT ROB. DANSON, professus Domûs Mount-grace; aliàs procurator Vallis Virtutum in Scotia.

1525. "OBIIT ROLAND BROWN, diaconus professus Domûs Scotiæ."

ABOUT this time, ADAM, natural son of King JAMES V. of Scotland, by ELIZABETH, daughter of JOHN, Earl of Lennox, was Prior of the Charter-house at Perth *.

IN the year 1559, after the preaching of JOHN KNOX, says SPEED †, the Charter-house at Perth, which was the fairest abbey in the realm of Scotland, was destroyed.

1561. "Ultimus Prior *Domûs Virtutum* fuit D. ADAM FORMAN, qui cùm anno 1561, esset procurator Domûs Belnæ, missus fuit cum Patribus GUIL. RUPRE, procuratore domûs Aurai, et ADAMO DESPES, priore Domûs Rosæ, à Capitulo Generali ordinis, ad visitandum desolatam et destructam Domum Vallis Virtutum in Scotia; Priorque factus aliquandiu latitans, domûs negotia prout poterat administrabat, usque ad annum 1562 ‡."

1567. "OBIIT D. JOANNES SPEY, procurator Domûs Scotiæ ‖."

1590. "OBIIT D. GUIL. CLAPEN, ultimus professus Domûs Vallis Virtutum in Scotia, hospes in domo Vallis profundi ‖."

1593. "OBIIT Reverendissimus in Christo Pater D. GUILL. CHERSOLME, Dumblanensis §, professus majoris Carthusiæ (propè Gratianopolim),

* Stewart's Historical and Genealogical Account, p. 90.

† Speed's History of England, p. 1137.

‡ Ex Collectaniis Petri de Wale, MSS. penes Carthusiam Bruxellensem, vol. ii. p. 185.

‖ Ex Chartis Capituli Gen. Ord. Carthusiensis.

§ Ex Collectaneis ejusd. de Wale, vol. iii. p. 24.

Gratianopolim), Prior Carthusiæ Romæ, et procurator generalis ordinis; aliàs Prior Sancti Spiritûs Lugduni, ante ordinis ingressum *Baro.* Sub Scotiæ defectionem, Romam se conferens, atque episcopali dignitate se abdicari, impetrato à Summo Pontifice (qui eum ad altiorem gradum promovere studebat) consensu, ad Majorem Carthusiam contendit, in qua monachus professus ad annos aliquot quàm humillimè vixit, conferens subinde ordines sacros suis confratribus. Missus quoque fuit Legatus ad JACOBUM, Scotiæ regem, quem ex sacro fonte olim susceperat, scribitur rem prudenter fecisse. Fuit aliquandò Episcopus Vassionensis.

N. B. The above particulars are all that I have found concerning the Carthusian Order in Scotland. There appears nowhere the least trace or mention of any other Chartreuse having ever existed in that kingdom, besides the above *Vale of Virtues* near Perth.

TO the above information by the Abbé MANN, a communication from the Reverend JAMES SCOTT, one of the ministers of Perth, and a learned Scotish antiquary, enables us to add, that the *Chartreuse* of Perth was a very considerable edifice, copied from the Grande Chartreuse in Dauphiny, though on a smaller scale, and situated at the west end of the town, on the spot where the King's Hospital now stands. It therefore occupied a beautiful situation, and one not unworthy of a colony from the fairest seat of monastic retirement. No vestiges of the building now remain.

THE letter of the Prior of the Grande Chartreuse is still extant, addressed to JAMES I. of Scotland, authorising the erection of this religious establishment, and directing that the prior of the Carthusian Monastery of the *Mount of Grace* at Ingleby in Yorkshire,

ſhire, ſhould ſend two of his monks to ſuperintend the conſtruction of the building. This letter is dated in 1426. The demolition of the religious houſes at Perth began in 1559. ADAM FORMAN, the laſt prior, and ſome other of the monks who did not adopt the reformed religion, retired to Errol, not far diſtant, where they poſſeſſed a conſiderable property. This was probably after the date of 1562, mentioned above.

ACCOUNT of the term SKULL or SKOLL as uſed in old Writings, being an Article in the *Etymological Dictionary of the Scotiſh Language*, propoſed to be publiſhed by the Reverend JOHN JAMIESON, D. D. Read April 1803. 1803. April. Explanation of the old word *ſkull* or *ſkoll.*

THE work, of which this communication is to make a part, is intended to illuſtrate the affinity of the language ſpoken in the low country of Scotland, with the Saxon, the Iſlandic, and other dialects of the Gothic. The importance of this undertaking cannot be queſtioned. Etymology is often the only light by which we can trace the migrations of the early inhabitants of countries, and, in the preſent inſtance, may afford the explanation of a leading fact in the hiſtory of this iſland, viz. the eſtabliſhment of the language in queſtion over all the low part of Scotland, from a period of very remote antiquity, while the *Gaelic* was the language of the mountainous part. The following is an extract from Dr JAMIESON's paper.

SKUL,

" SKUL, SKULL, SKOLL. 1. A goblet or large bowl, for containing liquor of any kind.

The Troiane women ſtude with hare doun ſchaik
About the bere, weping with mony allake:
And on we keſt of warme milk mony a *ſkul*,
And of the blude of ſacrifice coupis ful:
The ſaule we bery in ſepulture on this wyſe,
The lattir haleſing ſyne loud ſchoutit thrys.

DOUGL. *Virg*. 69. *l*. 20.

As *coupis* correſponds to *pateras* in the original, *ſkul* is uſed for *cymbia*, which DOUGLAS elſewhere renders in this manner;

Tua ſilver coppis ſchapin *like ane bote.*

Ibid. 136. *l*. 35.

We are not, however, hence to conclude, that the word *ſkull* neceſſarily denoted a veſſel of this form; for he elſewhere uſes it, conjoined with *flagon*, in rendering *crateras*:

For ioy thay pingil than for till renew
Thare bankettis with al obſeruance dew;
And for thir tithingis, in *flakoun* and in *ſkull*,
Thay ſkynk the wyne, and wauchtis cowpis full.

Ibid. 210. *l*. 5.

2. THE term has been metonymically uſed to denote the ſalutation of one who is preſent, or the reſpect paid to an abſent perſon, by expreſſing a wiſh for his health; while he who does ſo at the ſame time partakes of the drink that is uſed by the company, in token of his cordiality. This is what is now called " drinking one's health." In this ſenſe it occurs in the account of Gowrie's conſpiracy, publiſhed by royal authority. " The kinge

king called for drinke, and in a merry and homely manner fayde to the Earle, that although the Earle had feen the fafhion of entertaynements in other countries, yet hee would teach him the Scottifh fafhion, feeing he was a Scottifh man; and therefore, fince he had forgotten to drinke to his Majeftie, or fit with his guefts and entertayne them, his Majeftie would drinke to him his owne welcome, defiring hime to take it forth and drink to the reft of the company, and in his Majefties name to make them welcome."—

"When they had near-hand dined, the Earl of GOWRIE came from his Majefties chamber, to drink his *fcoll* to my Lord Duke and the reft of the company, which he did. And immediately after the *fcoll* had paffed about, this deponent raife from the table, to have waited upon his Majefty, conform to his former direction," &c. p. 196. 227. Perth edit. 1774. In CROMARTY's edition there is the following note, "*Scoll*, the word ufed then for drinking a health." The paffage itfelf is alfo differently expreffed in this edition. "The Earl of Gowrie came from his Majefty's chamber *to the hall, and called for wine, and faid that he was directed from his Majefty's chamber* to drink his *fcoll* to my Lord Duke," &c. Hiftorical Account, p. 40.

As it is faid, that "GOWRIE came from his Majefties chamber to drink his fcoll to my Lord Duke," it has been fuppofed that the king defired them to drink his health in his abfence. But in what way foever the paffage be read, it does not appear that this is the meaning. The relative *his*, might be underftood in reference to GOWRIE himfelf; as intimating that the king defired the Earl to go and welcome the company to his houfe, by drinking to them. But although it be viewed as referring to JAMES, as it is immediately connected with thefe words, "came from his Majefties chamber," it will not follow, that it was the king's defire that his own health fhould be drunk. From what he had previoufly faid to GOWRIE with refpect to his omiffion, it is evi-

 dent

dent that this is not the ſenſe of the language. He, in a jocular way, reprehended the Earl for not drinking to him; "deſiring him to take it forth, (that is, the drink formerly mentioned) and drink *to* the reſt of the company." Therefore, even admitting that the expreſſion *his ſcoll* means the *king's* ſcoll, we cannot with propriety ſuppoſe that any thing more is meant, than that GOWRIE went to the antichamber, to convey to the noblemen and gentlemen who were there, his Majeſty's ſalutation; or, as expreſſed in the narrative, "to drink to the reſt of the company, and in his Majeſty's name" to give them that welcome, which he had neglected to give them in his own.

THUS it appears, that the term, primarily denoting a veſſel for containing liquor, was, in conſequence of the cuſtoms connected with drinking, at length uſed to ſignify the mutual expreſſions of regard employed by thoſe engaged in compotation, or their united wiſhes for the health and proſperity of one individual, diſtinguiſhed in rank, or peculiarly endeared to them all, whether he were preſent or abſent.

I HAVE met with one paſſage in which that expreſſion, the *king's ſkole*, is diſtinctly uſed in the ſenſe which has been improperly attached to the phraſe already conſidered. After the Bridge of Berwick had been rebuilt in the year 1621, "Sir WILLIAM BEYER, mayor of the town, ſtayed the taking away of the centries, and putting in the keyſtone, till *the king's ſkole* were drunk at that part of the bridge." CALDERWOOD'S *Hiſt.* p. 787. But the expreſſion, although equivalent to what is now called drinking the *king's health*, ſeems ſtrictly to ſignify, drinking the *king's cup*, or a cup in honour of the king.

For we are not to ſuppoſe, that the word *ſkoll* has any primary or proper relation to health or proſperity. This would be totally repugnant to analogy; as will appear from a compariſon of our term with its cognates in the other Northern languages.

ISL.

Isl. *Skal, ſkaal, ſkylldi,* Aleman. *ſkala,* Germ. *ſchale,* SuioGoth. and Dan. *ſkaal,* all ſignify a cup, a bowl, or drinking veſſel. From the Gothic nations, this word ſeems to have paſſed to the Celtic. For, in the Corniſh, *ſkala* has the ſame meaning, being rendered by Lhuyd *patera.* Ruddiman, in his Gloſſary to Douglas's *Virgil,* mentions the verb to *ſkole* or *ſkolt,* as uſed Scot. Bor. in the ſenſe of *pocula exinanire.* This verb has undoubtedly been formed from the noun. In the North of Scotland, alſo, *ſkiel* ſtill denotes a tub; thus a waſhing-tub is called a *waſhing-ſkiel.* The tubs uſed by brewers for cooling their wort, are in like manner called *ſkiels.* It affords a ſtrong preſumption that this is originally the ſame word with *ſkoll, ſkull,* immediately under conſideration, that the goblet employed by the inhabitants of the north for preparing their *ale* for immediate uſe, is called *kalt-ſkaal.* This ſeems to intimate, that our uſe of the term, with reſpect to the operation of brewing, contains an alluſion to its more ancient appropriation. "*Kaltſkaal,* eodem tropo illis quo Sueonibus eſt *patera,* in qua *frigidus* cereviſiæ potus in æſtate, et calidus in hieme fieri ſolet." Loccenii *Antiq. Sue-Goth.* p. 96.

It may be added, that *ſkiel* is ſtill uſed in Orkney as the name of a flaggon, or wooden drinking veſſel with a handle.

Skull is a term of general uſe in Scotland for a baſket of a ſemi-circular form. It was uſed in this ſenſe ſo early as the time of Dunbar:

> "Fiſh wyves cry Fy, and caſt down *ſkulls* and *ſkeills.*"
>
> *Evergreen,* ii. p. 59. ſt. 23.

It is probable that *ſkiel* was uſed by him as if it had been ſynonymous, becauſe of the alliteration. Or, from the reſemblance with reſpect to form, it may actually have been uſed in the ſame ſenſe in his time. Eng. *Skillet,* a ſmall kettle or boiler, might appear at firſt view to have ſome affinity. But it ſeems immediately formed from Fr. *eſculette*, a porringer; and this again from Ital. *ſcudella*, uſed in the ſame ſenſe. This is derived from Lat. *ſcutula*, which was a kind of concave veſſel, a ſaucer. The learned IHRE, in his *Gloſſarium SuioGothicum*, views theſe French, Italian, and Latin words, as allied to Gothic *ſkaal*. But it is ſurpriſing, that he ſhould conſider *ſkaal* itſelf as formed, *per craſin*, from Lat. *ſcutula*. The quotations he has himſelf made for illuſtrating this word, certainly ſupplied him with a far more natural etymon. But before proceeding to this, it may be remarked as a ſingular analogy, that, according to ATHENÆUS, liv. iv. Gr. σκαλλιον is a ſmall cup, and σκαλις is equivalent to σκαφιον, which ſignifies a drinking-veſſel.

IT is highly probable, that a cup or bowl received this name from the barbarous cuſtom which prevailed among ſeveral ancient nations, of drinking out of the ſkulls of their enemies. WARNEFRID, in his work *De Geſtis Longobard.*, ſays, ' ALBIN ſlew CUNIMUND; and having carried away his head, converted it into a drinking-veſſel; which kind of cup is with us called *ſchala*, but in the Latin language it has the name of *patera*," lib. i. cap. 27. The ſame thing is aſſerted of the Boii by LIVY, lib. xxiii. c. 24.; of the Scythians by HERODOTUS, lib. ix.; of their deſcendants the Scordiſci, by RUFUS FESTUS in *Breviario;* of the Gauls by DIODORUS SICULUS, lib. v.; of the Celts by SILIUS ITALICUS, lib. xiv.

> At Celtæ vacui capitis circumdare gaudent,
> Oſſa, nefas! auro, et menſis ea pocula ſervant.
>
> *Vide* KEYSLER. *Antiq. Septentr. p.* 363.

HENCE RAGNAR LODBROG, in his death-ſong, conſoles himſelf with this reflection; " I ſhall ſoon drink beer from the hollow cups made of *ſkulls*." St. 25. WORMII *Literat. Dan. p.* 203.

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The ſame word in SuioG. ſignifies both a ſkull (*cranium*), and a drinking-veſſel. This obſervation is equally applicable to Germ. *ſchale.* But Ihre is ſo unfavourable to this derivation, principally, as it would appear, from its exhibiting our Gothic anceſtors as ſo extremely barbarous, that he conſiders the human ſkull as receiving the name of *ſkaal* from its reſemblance to the *patera* or bowl. This is ſurely to invert the natural order. Although the Northern nations were greatly addicted to inebriety, yet we can ſcarcely ſuppoſe that they found it neceſſary to borrow a name for their ſkulls from their drinking-veſſels. The ſkull itſelf ſeems to have received this deſignation from its reſemblance to a *ſhell*, in Ang. Sax. *ſceala*, *ſcala*, Belg. *ſchaele*, Germ. *ſchele*, Iſl. *ſkael*, SuioG. and Dan. *ſkaal.* Allied to this is Moeſo-Goth. *ſkaljos*, the tiling of a roof.

Ihre objects to this etymon, not only on the ground of the inhumanity of the cuſtom ſuppoſed to be alluded to; but eſpecially, he ſays, becauſe he does not find that the word *ſkaal* is uſed by ancient writers, as denoting a memorial potation, or the act of drinking in honour of ſome diſtinguiſhed perſonage; adding, that *minne* and *full* are the terms uſed by old Iſlandic authors. Even ſuppoſing this to be true, it will not diſprove the antiquity of the word. Nothing more could reaſonably be inferred, than that *ſkaal*, in more early ages, had retained its original and proper ſignification, as it is uſed in the other ſenſe only by a ſtrong metonymy. It was natural to prefer *minne;* for, as literally ſignifying *memory*, it ſimply and directly ſuggeſted the reaſon of this particular mode of drinking. Nor need we be ſurpriſed, although they even preferred the other term *full;* not only as the figure is leſs ſtrong, to ſpeak of drinking the *fill* of a cup, than of drinking the cup itſelf; but alſo becauſe it referred to the eſtabliſhed cuſtom with reſpect to this draught, that the cup muſt be *full* and be completely evacuated. This is only to ſuppoſe the Iſlandic word to have been for ſome

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time ſtationary in its meaning, in the ſame manner with our *ſkull* or *ſkoll*. For there is not the ſlighteſt evidence that, in the age of GAWIN DOUGLAS, it was uſed in that figurative ſenſe which it bore a century afterwards.

BUT it is aſtoniſhing, that this learned writer, after he has quoted WARNEFRID, ſhould lay any ſtreſs on this circumſtance, "He does not find that the word *ſkaal* is uſed by ancient writers." And can he deny this character to WARNEFRID, who flouriſhed about the year 774? Does not he ſay that this kind of cup, made of a human ſkull, is by the Goths called *ſchale?* Can any Scandinavian writer be produced, who uſes *minne* and *full*, to the excluſion of *ſkaal*, in an earlier age? There is no evidence that either of theſe terms was written for ſome ages after. WARNEFRID was not only a writer of great reputation, but himſelf a *Goth;* and his poſitive teſtimony is ſurely far preferable to the negative evidence deduced from poſterior writers. Although it could be proved, as it cannot, that the term was not uſed, in that early period, in the particular ſenſe referred to, it would by no means follow, that it was unknown in its ſimple ſignification, as denoting a drinking-veſſel. As the *Longobardi* were a Gothic nation, it is extremely improbable, that a term which had ſo ſingular an origin would be unknown to other nations belonging to the ſame race; although, without any reaſon that we can ſee, it might be more uſed by one nation than by another.

NOT only is the meaning of this term, as it occurs in other Northern languages, preſerved in ours; but the figurative ſenſe is alſo the ſame. Thus LOCCENIUS tells us, "Illud nomen in his ſeptentrionalibus locis adhuc ita remanet, ut *dricka ſkala*, *i. e.* bibere pateram, metonymicè dicatur, quando bibitur alicujus honori et memoriæ, quòd ex hoc vaſculo quondam frequentius fieri ſuetum, notio vocis indicat." *Antiq. Sueo-Goth.* p. 96. "In compotations," ſays IHRE, "the name of *ſkaal* is given to the

the memory of the abſent, or the ſalutation of thoſe who are preſent, which goes round in the time of drinking;" or more fully, "*dricka ens ſkaal.*" As Dan. *ſkaal* ſignifies a bowl or drinking-veſſel; *at dricka ens ſkaal* is to drink one's health. Voc. *ſkaal.* In Isidorus we find the phraſe, "Calices et *ſcaliæ*, poculorum genera." *Origin.* lib. xx. c. 5.

In the ſame manner did the ancient Goths expreſs their regard to their ſovereigns. They drank the *king's ſkoll.* Hence Warnefrid relates, that when Grimaold, King of the Lombards, had determined to kill Bertaridus, after he was overpowered with wine, the miniſters of the palace being ordered to bring to him liquors, with diſhes of various kinds, aſked of him, in the king's name, to drink a full bowl in honour of him. But he, ſuſpecting the ſnare, ſecretly procured that it ſhould be filled with water. Immediately, promiſing that he would drink it off in honour of the king, he made a libation, by pouring out a little of the water. *De Geſtis Longobard.* lib. v. Theſe *ſkolls* in honour of the king, as we learn from Loccenius, they uſed alſo to drink ſtanding. *Ubi ſupra.*

Biographical

BIOGRAPHICAL ACCOUNT *of the late Dr JAMES HUTTON*, F. R. S. EDIN.

[*Read by Mr* PLAYFAIR, *10th January* 1803.]

DR JAMES HUTTON was the ſon of Mr WILLIAM HUTTON, merchant in Edinburgh, and was born in that city on the 3d of June 1726. His father, a man highly reſpected for his good ſenſe and integrity, and who for ſome years held the office of City Treaſurer, died while JAMES was very young. The care of her ſon's education devolved of courſe on Mrs HUTTON, who appears to have been well qualified for diſcharging this double portion of parental duty. She reſolved to beſtow on him a liberal education, and ſent him firſt to the High School of Edinburgh, and afterwards to the Univerſity, where he entered as a ſtudent of humanity in November 1740.

OF the maſters under whom he ſtudied there, MACLAURIN was by far the moſt eminent, and Dr HUTTON, though he had cultivated the mathematical ſciences leſs than any other, never mentioned the lectures of that celebrated Profeſſor but in terms of high admiration.

HE uſed alſo to acknowledge his obligations to Profeſſor STEVENSON's Prelections on Logic; not ſo much, however, for having made him a logician as a chemiſt. The fact that gold is diſſolved in *aqua regia*, and that two acids which can each of

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them ſingly diſſolve any of the baſer metals, muſt unite their ſtrength before they can attack the moſt precious, was mentioned by the Profeſſor as an illuſtration of ſome general doctrine. The inſtinct of genius, if I may call it ſo, enabled Mr HUTTON, young as he then was, to feel, probably, rather than to underſtand, the importance of this phenomenon; and as if, by the original conſtitution of his mind, a kind of elective attraction had drawn him towards chemiſtry, he became from that moment attached to it by a force that could never afterwards be overcome. He made an immediate ſearch for books that might give him ſome farther inſtruction concerning the fact which he had juſt heard of; but the only one he could procure, for a long time, was HARRIS's *Lexicon Technicum*, the predeceſſor of thoſe voluminous compilations which have ſince contributed ſo much more to extend the ſurface, than to increaſe the ſolidity of ſcience. It was from the imperfect ſketch contained in that dictionary, that he derived his firſt knowledge of chemiſtry, his love for which never forſook him afterwards, and was in truth the propenſity which decided the whole courſe and complexion of his future life.

THOUGH his taſte and capacity for inſtruction were ſufficiently conſpicuous during his courſe of academical ſtudy, his friends wiſhed him rather to purſue buſineſs than ſcience. This was a meaſure by no means congenial to his mind, yet he acquieſced in it without difficulty.

ACCORDINGLY, in 1743 he was placed as an apprentice with Mr GEORGE CHALMERS, writer to the Signet; and ſubjection to the *routine* of a laborious employment, was now about to check the ardour and repreſs the originality of a mind formed for different purſuits. But happily the force of genius cannot always be controlled by the plans of a narrow and ſhort-ſighted prudence. The young man's propenſity to ſtudy continued, and he was often found amuſing himſelf and his fellow apprentices with

with chemical experiments, when he ſhould have been copying papers, or ſtudying the forms of legal proceedings; ſo that Mr CHALMERS ſoon perceived that the buſineſs of a writer was not that in which he was deſtined to ſucceed. With much good ſenſe and kindneſs, therefore, he adviſed him to think of ſome employment better ſuited to his turn of mind, and releaſed him from the obligations which he had come under as his apprentice. In this he did an eſſential ſervice to ſcience, and to the young man himſelf. A man of talents may follow any profeſſion with advantage; a man of genius will hardly ſucceed but in that which nature has pointed out.

THE ſtudy of medicine, as being the moſt nearly allied to chemiſtry, was that to which young HUTTON now reſolved to dedicate his time. He began that ſtudy under Dr GEORGE YOUNG, the father of the late Dr THOMAS YOUNG, and at the ſame time attended the lectures in the Univerſity. This courſe of medical inſtruction he followed from 1744 to 1747.

THOUGH a regular ſchool of medicine had now been eſtabliſhed in the Univerſity of Edinburgh for ſeveral years, the ſyſtem of medical education was neither in reality, nor in the opinion of the world, ſo complete as it has ſince become. Some part of a phyſician's ſtudies was ſtill to be proſecuted on the Continent; and accordingly, in the end of 1747, Mr HUTTON repaired to Paris, where he purſued with great ardour the ſtudies of chemiſtry and anatomy. After remaining in that metropolis nearly two years, he returned by the way of the Low Countries, and took the degree of Doctor of Medicine at Leyden in September 1749. His theſis is entitled, *De Sanguine et Circulatione in Microcoſmo.*

ON his return to London about the end of that year, he began to think ſeriouſly of ſettling in the world. His native city, to which his views of courſe were firſt turned, afforded no very flattering proſpect for his eſtabliſhment as a phyſician. The

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bufinefs there was in the hands of a few eminent practitioners who had been long eftablifhed; fo that no opening was left for a young man whofe merit was yet unknown, who had no powerful connections to affift him on his firft outfet, and very little of that patient and circumfpect activity by which a man pufhes himfelf forward in the world.

THESE confiderations feem to have made a very deep impreffion on his mind, and he wrote on the fubject of his future profpects with confiderable anxiety to his friends in Edinburgh.

ONE of thefe friends was Mr JAMES DAVIE, a young man nearly of his own age, with whom he had early contracted a very intimate friendfhip, that endured through the whole of his life, without interruption, to the mutual benefit of both. The turn which both of them had for chemical experiments formed their firft connection, and cemented it afterwards. They had begun together to make experiments on the nature and production of fal ammoniac. Thefe experiments had led to fome valuable difcoveries, and had been farther purfued by Mr DAVIE during Dr HUTTON's abfence. The refult afforded a reafonable expectation of eftablifhing a profitable manufacture of the falt juft named from coal-foot.

THE project of this eftablifhment was communicated by Mr DAVIE to his friend, who was ftill in London, and it appears to have leffened his anxiety about fettling as a phyfician, and probably was one of the main caufes of his laying afide all thoughts of that profeffion. Perhaps, too, on a nearer view, he did not find that the practice of medicine would afford him that leifure for purfuing chemical and other fcientific objects, which he fancied it would do when he faw things at a greater diftance. Whatever was the caufe, it is certain that foon after his return to Edinburgh in fummer 1750, he abandoned entirely his views of the practice of medicine, and refolved to apply himfelf to agriculture.

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The motives which determined him in the choice of the latter, cannot now be traced with certainty. He inherited from his father a ſmall property in Berwickſhire, and this might ſuggeſt to him the buſineſs of huſbandry. But we ought rather, I think, to look for the motives that influenced him, in the ſimplicity of his character, and the moderation of his views, than in external circumſtances. To one who, in the maturity of underſtanding, has leiſure to look round on the various employments which exerciſe the ſkill and induſtry of man, if his mind is independent and unambitious, and if he has no ſacrifice to make to vanity or avarice, the profeſſion of a farmer may ſeem fairly entitled to a preference above all others. This was exactly the caſe of Dr Hutton, and he appears to have been confirmed in his choice by the acquaintance which he made about that time with Sir John Hall of Dunglaſs, a gentleman of the ſame county, a man of ingenuity and taſte for ſcience, and alſo much converſant with the management of country affairs.

As he was never diſpoſed to do any thing by halves, he determined to ſtudy rural economy in the ſchool which was then reckoned the beſt, and in the manner which is undoubtedly the moſt effectual. He went into Norfolk, and fixed his reſidence for ſome time in that country, living in the houſe of a farmer, who ſerved both for his landlord and his inſtructor. This he did in 1752; and many years afterwards I have often heard him mention, with great reſpect, the name of John Dybold, at whoſe houſe he had lived with much comfort, and whoſe practical leſſons in huſbandry he highly valued. He appears, indeed, to have enjoyed this ſituation very much: the ſimple and plain character of the ſociety with which he mingled, ſuited well with his own, and the peaſants of Norfolk would find nothing in the ſtranger to ſet them at a diſtance from him, or to make them treat him with reſerve. It was always true of Dr Hut-

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TON, that to an ordinary man he appeared to be an ordinary man, poſſeſſing a little more ſpirit and livelineſs, perhaps, than it is uſual to meet with. Theſe circumſtances made his reſidence in Norfolk greatly to his mind, and there was accordingly no period of his life to which he more frequently alluded, in converſation with his friends; often deſcribing, with ſingular vivacity, the rural ſports and little adventures, which, in the intervals of labour, formed the amuſement of their ſociety.

WHILE his head-quarters were thus eſtabliſhed in Norfolk, he made many journeys on foot into different parts of England; and though the main object of theſe was to obtain information in agriculture, yet it was in the courſe of them that to amuſe himſelf on the road, he firſt began to ſtudy mineralogy or geology. In a letter to Sir JOHN HALL, he ſays that he was become very fond of ſtudying the ſurface of the earth, and was looking with anxious curioſity into every pit, or ditch, or bed of a river that fell in his way; "and that if he did not always avoid the fate of THALES, his misfortune was certainly not owing to the ſame cauſe." This letter is from Yarmouth; it has no date, but it is plain from circumſtances, that it muſt have been written in 1753.

WHAT he learned in Norfolk made him deſirous of viſiting Flanders, the country in Europe where good huſbandry is of the oldeſt date. He accordingly ſet out on a tour in that country, early in ſpring 1754, and travelling from Rotterdam through Holland, Brabant, Flanders, and Picardy, he returned to England about the middle of ſummer. He appears to have been highly delighted with the garden culture which he found to prevail in Holland and Flanders, but not ſo as to undervalue what he had learnt in England. He ſays in a letter to Sir JOHN HALL, written ſoon after his arrival in London, "Had I doubted of it before I ſet out, I ſhould have returned fully convinced that they are good huſbandmen in Norfolk."

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Though his principal object in this excursion was to acquire information in the practice of husbandry, he appears to have bestowed a good deal of attention on the mineralogy of the countries through which he passed, and has taken notice in his *Theory of the Earth* of several of the observations which he made at that time.

About the end of the summer he returned to Scotland, and hesitated a while in the choice of a situation where he might best carry into effect his plans of agricultural improvement. At last he fixed on his own farm in Berwickshire, and accordingly set about bringing it into order with great vigour and effect. A ploughman whom he brought from Norfolk set the first example of good tillage which had been seen in that district, and Dr Hutton has the credit of being one of those who introduced the new husbandry into a country where it has since made more rapid advances than in any other part of Great Britain.

From this time till about the year 1768, he resided for the most part on his farm, visiting Edinburgh, however, occasionally. The tranquillity of rural life affords few materials for biographical description; and an excursion to the North of Scotland, which he made in 1764, is one of the few incidents which mark an interval of fourteen years, passed mostly in the retirement of the country. He made this tour in company with Commissioner afterwards Sir George Clerk, a gentleman distinguished for his abilities and worth, with whom Dr Hutton had the happiness to live in habits of the most intimate friendship. They set out by the way of Crieff, Dalwhinnie, Fort Augustus, and Inverness; from thence they proceeded through East-Ross into Caithness, and returned along the coast by Aberdeen to Edinburgh. In this journey Dr Hutton's chief object was mineralogy, or rather geology, which he was now studying with great attention.

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For ſeveral years before this period, Dr Hutton was concerned in the ſal-ammoniac work, which had been actually eſtabliſhed on the foundation of the experiments already mentioned, but remained in Mr Davie's name, only, till 1765: at that time a copartnerſhip was regularly entered into, and the work carried on afterwards in the name of both.

He now found that his farm was brought into the regular order which good huſbandry requires, and that as the management of it became more eaſy, it grew leſs intereſting. An occaſion offering of letting it to advantage, he availed himſelf of it. About the year 1768 he left Berwickſhire entirely, and became reſident in Edinburgh, giving his undivided attention from that time to ſcientific purſuits.

Among other advantages which reſulted to him from this change of reſidence, we muſt reckon that of being able to enjoy, with leſs interruption, the ſociety of his literary friends, among whom were Dr Black, Mr Russel, profeſſor of Natural Philoſophy, Profeſſor Adam Ferguson, Sir George Clerk, already mentioned, his brother Mr Clerk of Elden, Dr James Lind, now of Windſor, and ſeveral others. Employed in maturing his views, and ſtudying nature with unwearied application, he now paſſed his time moſt uſefully and agreeably to himſelf, but in ſilence and obſcurity with reſpect to the world. He was, perhaps, in the moſt enviable ſituation in which a man of ſcience can be placed. He was in the midſt of a literary ſociety of men of the firſt abilities, to all of whom he was peculiarly acceptable, as bringing along with him a vaſt fund of information and originality, combined with that gayety and animation which ſo rarely accompany the profounder attainments of ſcience. Free from the interruption of profeſſional avocations, he enjoyed the entire command of his own time, and had ſufficient energy of mind to afford himſelf continual occupation.

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A GOOD deal of his leisure was now employed in the prosecution of chemical experiments. In one of these experiments, which he has no where mentioned himself, but which I have heard of from Dr BLACK, he discovered that mineral alkali is contained in zeolite. On boiling the gelatinous substance obtained from combining that fossil with muriatic acid, he found that, after evaporation, sea-salt was formed. Dr BLACK did not recollect exactly the date of this experiment, but from circumstances judged that it was earlier than 1772: It is, if I mistake not, the first instance of an alkali being discovered in a stony body. The experiments of M. KLAPROTH and Dr KENNEDY have confirmed this conclusion, and led to others of the same kind.

IN 1774 he made a tour through part of England and Wales, of which, I find no memorandum whatever among his papers. I know, however, that at this time he visited the salt-mines in Cheshire, and made the curious observation of the concentric circles marked on the roof of these mines, to which he has referred in his *Theory of the Earth*, as affording a proof that the salt rock was not formed from mere aqueous deposition. His friend Mr WATT of Birmingham accompanied him in his visit to the mines.

IT was after returning to Birmingham from Cheshire, that he set out on the tour into Wales. One of the objects of this tour, as I learnt from himself, was to discover the origin of the hard gravel of granulated quartz, which is found in such vast abundance in the soil about Birmingham, and indeed over a great tract of the central part of England. This gravel is so unlike that which belongs to a country of secondary formation, that it very much excited his curiosity; and his present journey was undertaken with a view to find out whether among the primitive mountains of Wales, there were any that might be supposed to have furnished the materials of it. In Wales, however,

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he ſaw none that could, with any probability, be ſuppoſed to have done ſo ; and he was equally unſuccefsful in all the other parts he viſited, till returning, at a ſmall diſtance from Birmingham, the place from whence he had ſet out, he found a rock of the very kind which he had been in ſearch of. It belongs to a body of ſtrata apparently primary, which break out between Broomſgrove and Birmingham, and have all the characters of the indurated gravel in queſtion. If, however, they have furniſhed the materials of that gravel, it ſeems probable that it has been through the medium of the red ſand-ſtone, which abounds in thoſe countries *.

In 1777 Dr Hutton's firſt publication was given to the world, viz. a ſmall pamphlet, intituled, *Conſiderations on the Nature, Quality, and Diſtinctions of Coal and Culm.* This little work, an octavo pamphlet of 37 pages, was occaſioned by a queſtion that had ariſen, Whether the ſmall coal of Scotland is the ſame with the culm of England ? and, Whether of courſe, like the latter, it is entitled, when carried coaſtwiſe, to an exemption from the duty on coal ? Some of the ſmall coal from the Frith of Forth, which had been carried to the northern counties for the purpoſe of burning lime, had been conſidered by the revenue officers as liable to the ſame duty with other coal, while the proprietors contended that it ought only to pay the lighter duty levied on culm. This was warmly diſputed ; and after occupying for ſome time the attention of the Board of Cuſtoms in Scotland, was at laſt brought before the Privy Council.

Dr Hutton's pamphlet was intended to ſupply the information neceſſary for forming a judgment on this queſtion. It is very ingenious and ſatisfactory, though perhaps, conſidering the purpoſe for which it was written, it is on too ſcientific a plan, and

* Illuſtrations of the Huttonian Theory, p. 375.

and conducted too ftrictly according to the rules of philofophical analyfis. It proves that culm is the fmall, or refufe, of the infufible, or ftone coal, fuch as that of Scotland for the moft part is; that the fmall of the fufible coal, by caking or uniting together, becomes equally ferviceable with the large coal; whereas the fmall of the infufible, by running down like loofe fand, cannot be made to burn in the ordinary way, and is ufeful but for few purpofes, fo that it has been properly exempted from the ufual duty on coal. A criterion is alfo pointed out for determining when fmall coal is to be regarded as culm, and when it may be confidered as coal;—if, when a handful of it is thrown into a red-hot fhovel, the pieces burn without melting down or running together, it decidedly belongs to the former *.

In the conclufion, an exemption from duty was obtained for the fmall coal of Scotland, when carried coaftwife, and this regulation was owing in a great degree to the fatisfactory information contained in Dr Hutton's pamphlet. It was a ftep, alfo, toward the entire abolition of thofe injudicious duties which had been fo long levied on coal, when carried by fea beyond a certain diftance from its native place. This abolition happened feveral years after the period we are fpeaking of, much to the benefit of the country, and to the credit of the adminiftration under which it took place.

As Dr Hutton always took a warm intereft in whatever concerned the advancement of the arts, particularly in his native country, he entered with great zeal into the project of an internal navigation between the Friths of Forth and Clyde. The comparative merit of the different plans, according to which that work was to be executed, gave rife to a good deal of difcuffion, and even of controverfy. In thefe debates Dr Hutton

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* A few copies of the *Confiderations on Culm* are ftill to be found in the fhop of C. Elliot, Edinburgh.

took a ſhare, and wrote ſeveral pieces, in which the grave and the ludicrous were both occaſionally employed. None of theſe pieces have been publiſhed; but the plan that was in the end adopted was that in favour of which they were written. It is unneceſſary, however, to enter into the merits of a queſtion which has long ceaſed to intereſt the public.

FROM the time of fixing his reſidence in Edinburgh, Dr HUTTON had been a member of the Philoſophical Society, known to the world by the three volumes of phyſical and literary eſſays ſo much and ſo juſtly eſteemed *. In that ſociety he read ſeveral papers; but it was during the time that elapſed between the publication of the laſt of the volumes juſt mentioned, and the incorporation of the Philoſophical into the Royal Society of Edinburgh; which laſt was eſtabliſhed by a royal charter in 1783. None of theſe papers have been publiſhed, except one in the ſecond volume of the *Tranſactions of the Royal Society*, "*On certain Natural Appearances of the Ground on the Hill of Arthur's Seat.*"

THE inſtitution of the Royal Society of Edinburgh had the good effect of calling forth from Dr HUTTON the firſt ſketch of a theory of the earth, the formation of which had been the great object of his life. From the date formerly mentioned, when he was yet a very young man, and making excurſions on foot through the different counties of England, till that which we are now arrived at, a period of about thirty years, he had never ceaſed to ſtudy the natural hiſtory of the globe, with a view of aſcertaining

* THE Philoſophical Society was inſtituted about the year 1739. The firſt volume of *Eſſays* was publiſhed in 1754; the ſecond in 1756; the third in 1771. From the year 1777 to 1782, the meetings of the Society were pretty regular, much owing to the zeal of Lord KAMES. Mr MACLAURIN may be regarded as the founder of this Society.

ing the changes that have taken place on its furface, and of difcovering the caufes by which they have been produced.

He had become a fkilful mineralogift, and had examined the great facts of geology with his own eyes, and with the moft careful and fcrupulous obfervation. In the courfe of thefe ftudies he had brought together a confiderable collection of minerals peculiarly calculated to illuftrate the changes which foffil bodies have undergone. He had alfo carefully perufed almoft every book of travels from which any thing was to be learned concerning the natural hiftory of the earth; and, in confequence both of reading and obfervation, was eminently fkilled in phyfical geography.

If to all this it be added, that Dr Hutton was a good chemift, and poffeffed abilities excellently adapted to philofophical refearch, it will be acknowledged, that few men have entered with better preparation on the arduous tafk of inveftigating the true theory of the earth. Several years before the time I am now fpeaking of, he had completed the great outline of his fyftem, but had communicated it to very few; I believe to none but his friends Dr Black and Mr Clerk of Elden. Though fortified in his opinion by their agreement with him, (and it was the agreement of men eminently qualified to judge), yet he was in no hafte to publifh his theory; for he was one of thofe who are much more delighted with the contemplation of truth, than with the praife of having difcovered it. It might therefore have been a long time before he had given any thing on this fubject to the public, had not his zeal for fupporting a recent inftitution which he thought of importance to the progrefs of fcience in his own country induced him to come forward, and to communicate to the Royal Society a concife account of his theory of the earth.

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As I have treated of this theory in a feparate Effay, particularly deftined to the illuftration of it, I fhall here content myfelf with a very general outline.

I. The object of Dr Hutton was not, like that of moft other theorifts, to explain the firft origin of things. He was too well fkilled in the rules of found philofophy for fuch an attempt; and he accordingly confined his fpeculations to thofe changes which terreftrial bodies have undergone fince the eftablifhment of the prefent order, in as far as diftinct marks of fuch changes are now to be difcovered.

With this view, the firft general fact which he has remarked is, that by far the greater part of the bodies which compofe the exterior cruft of our globe, bear the marks of being formed out of the materials of mineral or organized bodies, of more ancient date. The fpoils or the wreck of an older world are every where vifible in the prefent, and, though not found in every piece of rock, they are diffufed fo generally as to leave no doubt that the ftrata which now compofe our continents are all formed out of ftrata more ancient than themfelves.

II. The prefent rocks, with the exceptions of fuch as are not ftratified, having all exifted in the form of loofe materials collected at the bottom of the fea, muft have been confolidated and converted into ftone by virtue of fome very powerful and general agent. The confolidating caufe which he points out is fubterraneous heat, and he has removed the objections to this hypothefis by the introduction of a principle new and peculiar to himfelf. This principle is the compreffion which muft have prevailed in that region where the confolidation of mineral fubftances was accomplifhed. Under the weight of a fuperincumbent ocean, heat, however intenfe, might be unable to volatilize any part of thofe fubftances which, at the furface, and under the lighter preffure of our atmofphere, it can entirely co fume. The fame preffure, by forcing thofe fubftances to remain united,

ted, which at the ſurface are eaſily ſeparated, might occaſion the fuſion of ſome bodies which in our fires are only calcined. Hence the objections that are ſo ſtrong and unanſwerable, when oppoſed to the theory of volcanic fire, as uſually laid down, have no force at all againſt Dr HUTTON's theory; and hence we are to conſider this theory as hardly leſs diſtinguiſhed from the hypotheſis of the Vulcaniſts, in the uſual ſenſe of that appellation, than it is from that of the Neptuniſts, or the diſciples of WERNER.

III. THE third general fact on which this theory is founded, is, that the ſtratified rocks, inſtead of being either horizontal, or nearly ſo, as they no doubt were originally, are now found poſſeſſing all degrees of elevation, and ſome of them even perpendicular to the horizon; to which we muſt add, that thoſe ſtrata which were once at the bottom of the ſea are now raiſed up, many of them, ſeveral thouſand feet above its ſurface. From this, as well as from the inflexions, the breaking and ſeparation of the ſtrata, it is inferred, that they have been raiſed up by the action of ſome expanſive force placed under them. This force, which has burſt in pieces the ſolid pavement on which the ocean reſts, and has raiſed up rocks from the bottom of the ſea, into mountains 15,000 feet above its ſurface, exceeds any which we ſee actually exerted, but ſeems to come nearer to the cauſe of the volcano or the earthquake than to any other, of which the effects are directly obſerved. The immenſe diſturbance, therefore, of the ſtrata, is in this theory aſcribed to heat acting with an expanſive power, and elevating thoſe rocks which it had before conſolidated.

IV. AMONG the marks of diſturbance in which the mineral kingdom abounds, thoſe great breaches among rocks, which are filled with materials different from the rock on either ſide, are among the moſt conſpicuous. Theſe are the veins, and comprehend, not only the metallic veins, but alſo thoſe of whin-

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ſtone, of porphyry, and of granite, all of them ſubſtances more or leſs cryſtallized, and none of them containing the remains of organized bodies. Theſe are of poſterior formation to the ſtrata which they interſect, and in general alſo they carry with them the marks of the violence with which they have come into their place, and of the diſturbance which they have produced on the rocks already formed. The materials of all theſe veins Dr Hutton concludes to have been melted by ſubterraneous heat, and, while in fuſion, injected among the fiſſures and openings of rocks already formed, but thus diſturbed, and moved from their original place.

This concluſion he extends to all the maſſes of whinſtone, porphyry, and granite, which are interpoſed among ſtrata, or raiſed up in pyramids, as they often appear to be, through the midſt of them. Thus, in the fuſion and injection of the unſtratified rocks, we have the third and laſt of the great operations which ſubterraneous heat has performed on mineral ſubſtances.

V. From this Dr Hutton proceeds to conſider the changes to which mineral bodies are ſubject when raiſed into the atmoſphere. Here he finds, without any exception, that they are all going to decay; that from the ſhore of the ſea to the top of the mountain, from the ſofteſt clay to the hardeſt quartz, all are waſting and undergoing a ſeparation of their parts. The bodies thus reſolved into their elements, whether chemical or mechanical, are carried down by the rivers to the ſea, and are there depoſited. Nothing is exempted from this general law: among the higheſt mountains and the hardeſt rocks, its effects are moſt clearly diſcerned; and it is on the objects which appear the moſt durable and fixed, that the characters of revolution are moſt deeply imprinted.

On comparing the firſt and the laſt of the propoſitions juſt enumerated, it is impoſſible not to perceive that they are two

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ſteps of the ſame progreſſion, and that mineral ſubſtances are alternately diſſolved and renewed. Theſe viciſſitudes may have been often repeated; and there are not wanting remains among mineral bodies, that lead us back to continents from which the preſent are the third in ſucceſſion. Here, then, we have a ſeries of great natural revolutions in the condition of the earth's ſurface, of which, as the author of this theory has remarked, we neither ſee the beginning nor the end; and this circumſtance accords well with what is known concerning other parts of the economy of the world. In the continuation of the different ſpecies of animals and vegetables that inhabit the earth, we diſcern neither a beginning nor an end; and in the planetary motions, where geometry has carried the eye ſo far both into the future and the paſt, we diſcover no mark either of the commencement or termination of the preſent order. It is unreaſonable, indeed, to ſuppoſe that ſuch marks ſhould any where exiſt. The Author of nature has not given laws to the univerſe, which, like the inſtitutions of men, carry in themſelves the elements of their own deſtruction; he has not permitted in his works any ſymptom of infancy or of old age, or any ſign by which we may eſtimate either their future or their paſt duration. He may put an end, as he no doubt gave a beginning, to the preſent ſyſtem, at ſome determinate period of time; but we may reſt aſſured, that this great cataſtrophe will not be brought about by the laws now exiſting, and that it is not indicated by any thing which we perceive.

It would be deſirable to trace the progreſs of an author's mind in the formation of a ſyſtem where ſo many new and enlarged views of nature occur, and where ſo much originality is diſplayed. On this ſubject, however, Dr Hutton's papers do not afford ſo much information as might be wiſhed for, though ſomething may be learnt from a few ſketches of an Eſſay on the *Natural Hiſtory of the Earth*, evidently written at a very early period, and intended, it would ſeem, for parts of an extenſive

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work, of which, as often happens with the firſt attempts to generalize, the plan was never executed, and may never have been accurately digeſted.

From theſe ſketches it appears that the firſt of the propoſitions juſt enumerated, viz. that a vaſt proportion of the preſent rocks is compoſed of materials afforded by the deſtruction of bodies, animal, vegetable, and mineral, of more ancient formation; was the firſt concluſion that he drew from his obſervations.

The ſecond ſeems to have been, that all the preſent rocks are without exception going to decay, and their materials deſcending into the ocean. Theſe two propoſitions, which are the extreme points, as it were, of his ſyſtem, appear, as to the order in which they became known, to have preceded all the reſt. They were neither of them, even at that time, entirely new propoſitions, though, in the conduct of the inveſtigation, and in the uſe made of them, a great deal of originality was diſplayed. The compariſon of them naturally ſuggeſted to a mind not fettered by prejudice, nor ſwayed by authority, that they are two ſteps of the ſame progreſſion; and that, as the preſent continents are compoſed from the waſte of more ancient land, ſo, from the deſtruction of them, future continents may be deſtined to ariſe. Dr Hutton accordingly, in the notes to which I allude, inſiſts much on the perfect agreement of the ſtructure of the beds of grit or ſandſtone, with that of the banks of unconſolidated ſand now formed on our ſhores, and ſhews that theſe bodies differ from one another in nothing but their compactneſs and induration.

In generalizing theſe appearances, he proceeded a ſtep farther, conſidering this ſucceſſion of continents as not confined to one or two examples, but as indefinitely extended, and the conſequence of laws perpetually acting. Thus he arrived at the new and ſublime concluſion, which repreſents nature as having provided for a conſtant ſucceſſion of land on the ſurface of the earth, according

according to a plan having no natural termination, but calculated to endure as long as thofe beneficent purpofes, for which the whole is deftined, fhall continue to exift.

THIS conclufion, however, was but a fuggeftion, till the mechanifm was inquired into by which this grand renovation may be brought about, or by which loofe materials can be converted into ftone, and elevated into land. This led to an inveftigation of the mineralizing principle, or the caufe of the confolidation of mineral bodies: And Dr HUTTON appears accordingly, with great impartiality, and with no phyfical hypothefis whatever in his mind, to have begun with inquiring into the nature of the fluidity which fo many mineral fubftances feem to have poffeffed previous to the acquifition of their prefent form. After a long and minute examination, he came to the conclufion, That the fluidity of thefe fubftances has been what he terms SIMPLE, that is to fay, not fuch as is produced by combination with a folvent. The two general facts from which this conclufion follows, are, firft, that no folvent is capable of holding in folution all mineral fubftances, nor even all fuch varieties of them as are often united in the fame fpecimen; and, fecondly, that in the bodies compofed of fragments of other bodies, the confolidation is fo complete that no room is left for a folvent to have ever occupied. The fubftance, therefore, which was the caufe of the fluidity of mineral bodies, and prepared them for confolidation, muft have been one that could act on them all, which occupied no fpace within them, and could find its way through them, whatever was the degree of their compactnefs and induration. Heat is the only fubftance which has thefe properties; and is the only one, therefore, which, without manifeft contradiction, can be affigned as the caufe of mineral confolidation.

MANY difficulties, however, were ftill to be removed before this hypothefis was rendered completely fatisfactory; but in what order Dr HUTTON proceeded to remove them, the notes above mentioned do not enable me to ſtate. We may nevertheleſs conjecture, with conſiderable probability, what the ſtep was which immediately followed.

It muſt have occurred to him, as an objection to the conſolidation of minerals by ſubterraneous heat, that many ſubſtances are found in the bowels of the earth in a ſtate altogether unlike that into which they are brought by the action of our fires at the ſurface. Coal, for inſtance, by expoſure to fire, has its parts diſſipated; the aſhes which remain behind are a ſubſtance quite different from the coal itſelf; and hence it would ſeem that this foſſil can never before have been ſubjected to the action of fire. But is it certain, (we may ſuppoſe Dr Hutton to have ſaid to himſelf), if the heat had been applied to the coal in the interior of the earth, at the bottom of the ſea, for example, that the ſame diſſipation of the parts would have taken place? Would not the greater compreſſion that muſt prevail in that region have prevented the diſſipation, at leaſt till a more intenſe heat was applied? And if the diſſipation was prevented, might not the maſs, after cooling, be very different from any thing that can be obtained by burning at the ſurface of the earth? It is plain that there is no reaſon whatever for anſwering theſe queſtions in the negative. And, on the contrary, if the analogy of nature is conſulted, if the fact of water requiring more heat to make it boil when it is more compreſſed, or the experiments with Papin's digeſter, be conſidered, it will appear that the anſwer muſt be in the affirmative. Nay, it could not but ſeem reaſonable to proceed a ſtep farther, and, as the mixture of ſubſtances is known in ſo many inſtances to promote their fuſibility, to ſuppoſe that when the volatile parts of bodies were reſtrained, the whole maſs might be reduced into fuſion by heat, though, when theſe ſame parts were driven off, the reſiduum might be altogether infuſible. Thus coal, when the charcoal and bitumen are forced to remain in union, may very well be a fuſible ſubſtance,

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though, when the latter is permitted to efcape, the former becomes one of the moft refractory of all bodies.

In this way, and probably from this very inftance, the effects of compreffion may have fuggefted themfelves to Dr Hutton. He would foon perceive that the fame principle could be very generally applied, and that it afforded the folution of a difficulty concerning limeftone, fimilar to that which has been juft ftated with refpect to coal. Limeftone is not found in the bowels of the earth having the caufticity which it acquires by the action of fire, and hence one might conclude that it had never been expofed to the action of that element. But the experiments of Dr Black, before his friend was engaged in this geological inveftigation *, had proved that the caufticity of lime depends on the expulfion of the aëriform fluid, fince diftinguifhed by the name of carbonic gas, which compofes no lefs than two-fifths of the whole. This great difcovery, which has extended its influence fo widely over the fcience of chemiftry, alfo led to important confequences in geology; and Dr Hutton inferred from it, that ftrong compreffion might prevent the caufticity of lime, by confining the carbonic gas even when great heat was applied, and that, as has been fuppofed of coal, the whole may have been melted in the interior of the earth, fo as on cooling to acquire that cryftallized or fparry ftructure which the carbonate of lime fo frequently poffeffes †.

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* Dr Black's paper on magnefia, which contained this difcovery, was communicated to the Philofophical Society of Edinburgh in June 1755, and was publifhed in the fecond volume of their Effays, in the year following. Dr Hutton had at this time only begun his geological refearches. It was not, I imagine, till after the year 1760 that they came to take the form of a theory.

† In the view here prefented of the principle of compreffion, as employed in the Huttonian Theory, it is confidered as a hypothefis, conformable to analogy, affumed for the purpofe of explaining certain phenomena in the natural hiftory of

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It is unneceſſary to carry our conjectures concerning the train of Dr Hutton's diſcoveries to a greater length; the development of the principles now enumerated, and the compariſon of the reſults with the facts obſerved in the natural hiſtory of minerals, led to thoſe diſcoveries, by a road that will be eaſily traced by thoſe who ſtudy his theory with attention.

It might have been expected, when a work of ſo much originality as this Theory of the Earth, was given to the world, a theory which profeſſed to be the reſult of ſuch an ample and accurate

the earth. It reſts, therefore, as to its evidence, partly on its conformity to analogy, and partly on the explanation which it affords of the phenomena alluded to. In ſuppoſing that it derives probability from the laſt-mentioned ſource, we are far from aſſuming any thing unprecedented in ſound philoſophy. A principle is often admitted in phyſics, merely becauſe it explains a great number of appearances; and the theory of Gravitation itſelf reſts on no other foundation.

The degree of this evidence will perhaps be differently appreciated, according to a man's habits of thinking, or the claſs of ſtudies in which he has been chiefly engaged. To Dr Hutton himſelf it appeared very ſtrong; for he conſidered the fact of the liquefaction of mineral ſubſtances by heat as ſo completely eſtabliſhed, that it affords a full proof of the fuſibility of thoſe ſubſtances having been increaſed by the compreſſion which they endured in the bowels of the earth. In his view of the matter, no other proof ſeemed neceſſary, and he did not appear to think that the direct teſtimony of experiment, could it have been obtained, would have added much to the credibility of this part of his ſyſtem.

For my part, I will acknowledge that the matter appears to me in a light ſomewhat different, and that though the arguments juſt mentioned are ſufficient to produce a very ſtrong conviction, it is a conviction that would be ſtrengthened by an agreement with the reſults even of ſuch experiments as it is within our reach to make. It ſeems to me, that it is with this principle in geology, much as it is with the parallax of the earth's orbit in aſtronomy; the diſcovery of which, though not neceſſary to prove the truth of the Copernican System, would be a moſt pleaſing and beautiful addition to the evidence by which it is ſupported. So, in the Huttonian geology, though the effects aſcribed to compreſſion, are fairly deducible from the phenomena of the mineral kingdom itſelf, compared with certain analogies which ſcience has eſtabliſhed, yet the teſtimony of direct experiment would make the evidence complete, and would leave nothing that incredulity itſelf could poſſibly deſiderate.

accurate induction, and which opened up so many views, interesting not to mineralogy alone, but to philosophy in general, that it would have produced a sudden and visible effect, and that men of science would have been every where eager to decide concerning its real value. Yet the truth is, that it drew their attention very slowly, so that several years elapsed before any one shewed himself publicly concerned about it, either as an enemy or a friend.

SEVERAL causes probably contributed to produce this indifference. The world was tired out with unsuccessful attempts to form geological theories, by men often but ill informed of the phenomena which they proposed to explain, and who proceeded also on the supposition that they could give an account of the *origin* of things, or the first establishment of that system which is now the order of nature. Men who guided their inquiries by a principle so inconsistent with the limits of the human faculties, could never bring their speculations to a satisfactory conclusion, and the world readily enough perceived their failure, without taking the trouble to inquire into the cause of it.

TRUTH, however, forces me to add, that other reasons certainly contributed not a little to prevent Dr HUTTON's theory from making a due impression on the world. It was proposed too briefly, and with too little detail of facts, for a system which involved so much that was new, and opposite to the opinions generally received. The descriptions which it contains of the phenomena of geology, suppose in the reader too great a knowledge of the things described. The reasoning is sometimes embarrassed by the care taken to render it strictly logical; and the transitions, from the author's peculiar notions of arrangement, are often unexpected and abrupt. These defects run more or less through all Dr HUTTON's writings, and produce a degree of obscurity astonishing to those who knew him, and who heard

him

him every day converſe with no leſs clearneſs and preciſion, than animation and force. From whatever cauſes the want of perſpicuity in his writings proceeded, perplexity of thought was not among the number; and the confuſion of his ideas can neither be urged as an apology for himſelf, nor as a conſolation to his readers.

ANOTHER paper from his pen, a *Theory of Rain*, appeared alſo in the firſt volume of the *Edinburgh Tranſactions*. He had long ſtudied meteorology with great attention; and this communication contains one of the few ſpeculations in that branch of knowledge entitled to the name of *theory*.

DR HUTTON begins with ſuppoſing that the quantity of humidity, which air is capable of diſſolving, increaſes with its temperature. Now, this increaſe muſt either be in the ſame ratio with the increaſe of heat, in a leſs ratio, or in a greater: in other words, for equal increments of heat, the increments of humidity muſt either conſtitute a ſeries of which all the terms are equal to one another, or a ſeries in which the terms continually decreaſe, or one in which they continually increaſe *. If either of the two firſt laws was that which took place in nature, a mixture of two portions of air, though each contained as much humidity as it was capable of diſſolving, would never produce a condenſation of that humidity. According to the firſt

* To ſpeak ſtrictly, the law which connects the increments of humidity in the air with the increments of temperature, is not confined to any one of the three ſuppoſitions here made, but may involve them all. The humidity diſſolved may be proportional to ſome *function* of the heat, that varies in ſome places faſter, and in others ſlower, than in the ſimple ratio of the heat itſelf. Nevertheleſs, for that extent to which obſervation reaches, the reaſoning of DR HUTTON is quite ſufficient to prove that it varies faſter; or, in other words, that if a curve be ſuppoſed, of which the abſciſſæ repreſent the temperature, and the ordinates the humidity, this curve, though it may in the courſe of its indefinite extent be in ſome places concave and in others convex toward the axis, is wholly convex in all that part with which our obſervations are concerned.

firſt law, the temperature, the humidity, and the power of containing humidity, in the mixture, being all arithmetical means between the ſame quantities, as they exiſted previouſly to the mixture, the temperature produced would be exactly that which was required by the humidity to preſerve it in its inviſible form. If the ſecond law took place, the moiſture actually contained in the mixture would be leſs than the temperature was capable of ſupporting; ſo that inſtead of a condenſation of humidity, the air would become drier than before.

If, on the other hand, the third law be that which takes place, after the mixture of two portions of air of different temperatures, the humidity will be greater than the temperature is able to maintain, and therefore a condenſation of it will follow. Now, the experience of every day proves, that the mixture of two portions of humid air of unequal temperatures, does indeed produce a condenſation of moiſture, and therefore we are authoriſed to conclude that the laſt-mentioned law is that which actually prevails *.

It is obvious that this principle affords an explanation of the formation of clouds in the atmoſphere, and that currents of air,

 or

* It has been ſuppoſed that the chemical ſolution of humidity in air is neceſſarily implied in this theory of rain. The truth is, that the air is here conſidered only as the vehicle of the vapour, and that the tranſparent ſtate of the latter is ſuppoſed to depend on the temperature, or the quantity of heat; but whether that heat act on the vapour ſolely and directly, or indirectly, by increaſing the power of the air to retain it in ſolution, is, with reſpect to this theory, altogether immaterial.

Dr Hutton has indeed uſed the common language concerning the ſolution of humidity in air; but the ſuppoſition of ſuch ſolution is not eſſential to his theory. He ſeemed, indeed, to entertain doubts about the reality of that operation, founded on the circumſtance of evaporation taking place *in vacuo*. Experiments made by M. Dalton ſince the death of Dr Hutton, ſhew that there is great reaſon for ſuppoſing that the air has no chemical action whatever on the aqueous vapour contained in it. *Mancheſter Memoirs*, vol. v. p. 538.

or winds, of different temperatures, when they meet, muſt produce ſuch mixtures as have been deſcribed, and give riſe conſequently to the condenſation of aqueous vapour. When the ſupply of the humid air, entering into the mixture, is continued, the quantity of cloud formed will continually increaſe, and the ſmall globules of condenſed moiſture, uniting into drops, muſt deſcend in rain.

But though we are thus in poſſeſſion of a principle by which rain may be certainly produced, yet whether it be the only one by which rain is produced may require ſome farther inveſtigation. Dr Hutton accordingly, in order to determine this point, has entered into a very ample detail concerning the rain under different climates, and in different regions of the earth. The reſult is, that the quantity of rain is, as nearly as can be eſtimated, every where proportional to the humidity contained in the air, and the cauſes which promote the mixture of different portions of air, in the upper regions of the atmoſphere. Between the tropics, for inſtance, the dry ſeaſon is that in which the uniform current of the trade-wind meets with no obſtruction in its circuit round the globe; and the rainy ſeaſon happens when the ſun approaches to the zenith, and when the ſteadineſs of the trade-wind either yields to irregular variations, or to the ſtated changes of the monſoons.

Thus too, (to mention another extreme caſe), in certain countries diſtant from the ſea, having little inequality of ſurface, and expoſed to great heat, no rain whatever falls, and the ſands of the deſert are condemned to perpetual ſterility. Even there, however, where a mountainous tract occurs, the mixture of different portions of air produces a depoſition of humidity; perennial ſprings are found; and the fertile vales of Fezzan or Palmyra are exempted from the deſolation of the ſurrounding wilderneſs.

This ingenious theory attracted immediate attention, and was valued for affording a diſtinct notion of the manner in which

which cold acts in caufing a precipitation of humidity. It met, however, from M. DE LUC with a very vigorous and determined oppofition; Dr HUTTON defended it with fome warmth, and the controverfy was carried on with more fharpnefs, on both fides, than a theory in meteorology might have been expected to call forth. For this Dr HUTTON had leaft apology, if greateft indulgence, on the fcore of temper, is due to the combatant who has the worft argument. The merits of the queftion cannot be confidered here: It is fufficient to remark, that they came ultimately to reft on a fingle point, Whether the refrigeration of air is carried on by the mixture of the cold and the hot air, or by the paffage of the heat itfelf, without fuch mixture, from one portion of air to another. If the former holds, Dr HUTTON's theory is eftablifhed; if the latter be true, M. DE LUC's objections may at leaft merit examination.

Now, it is certain, that if not the only, yet almoft the only, communication of heat through fluids, is produced by the mixture of one part of the fluid with another. The ftatical principle by which heat is thus propagated, was firft, I believe, accurately explained by Dr BLACK, and fince his time has been farther illuftrated by the experiments of Count RUMFORD. Thefe laft have led their ingenious author to conclude that heat has no tendency to pafs through fluids, otherwife than by the mixture of the parts of different temperature. The accuracy of this conclufion, in its full extent, may reafonably be queftioned; but this much of it is undoubtedly true, that when the particles of a body are at liberty to move freely among themfelves, the direct communication of heat, compared with the ftatical, is evanefcent, and may be regarded as a mere infinitefimal. M. DE LUC's objections are therefore of no weight.

THE *Theory of Rain* was republifhed by Dr HUTTON in his *Phyfical Differtations* feveral years afterwards, together with his anfwers to M. DE LUC, and feveral other meteorological tracts, which contain many excellent examples of generalization, in a branch of natural hiſtory where it is more eaſy to accumulate facts, and more difficult to aſcertain principles, than in any other *.

AFTER

* IT may be proper to mention here ſome uſeful obſervations in meteorology which Dr HUTTON made, but of which he has given no account in any of his publications.

HE was, I believe, the firſt who thought of aſcertaining the medium temperature of any climate by the temperature of the ſprings. With this view he made a great number of obſervations in different parts of Great Britain, and found, by a ſingular enough coincidence between two arbitrary meaſures, quite independent of one another, that the temperature of ſprings, along the eaſt coaſt of this iſland, varies nearly at the rate of a degree of FAHRENHEIT's thermometer for a degree of latitude. This rate of change, though it cannot be general over the whole earth, is probably not far from the truth for all the northern part of the temperate zone.

FOR eſtimating the effect which height above the level of the ſea has in diminiſhing the temperature, he alſo made a ſeries of obſervations at a very early period. By theſe obſervations he found that the difference between the ſtate of the thermometer in two places of a given difference of level, and not very diſtant, in a horizontal direction, is a conſtant quantity, or one which remains at all ſeaſons nearly the ſame, and is about 1° for 230 feet of perpendicular height.

I MUST, however, obſerve, that on verifying theſe obſervations, I have found the rate of the decreaſe of temperature a little ſlower than this, and very nearly a degree for 250 feet. This ſeems to hold for a conſiderable height above the earth's ſurface, and will be found to come pretty near the truth, to the height of five or ſix thouſand feet. It is not however probable that the diminution of the temperature is exactly proportional to the increaſe of elevation; and it would ſeem that at heights greater than the preceding, the deviation becomes ſenſible; the differences of heat varying in a leſs ratio than the differences of elevation.

IN explaining this diminution of temperature as we aſcend in the atmoſphere, Dr HUTTON was much more fortunate than any other of the philoſophers who have conſidered the ſame ſubject. It is well known that the condenſation of air converts part of the latent into ſenſible heat, and that the rarefaction of air converts part of the ſenſible into latent heat. This is evident from the experiment of the air-gun, and from many others. If, therefore, we ſuppoſe a given quantity of air to be ſuddenly tranſported from the ſurface to any height above it, the air will expand on account of the diminution of preſſure, and a part of its heat becoming latent, After the period of the two publications juſt mentioned, Dr Hutton made ſeveral excurſions into different parts of Scotland, with a view of comparing certain reſults of his theory of the earth with actual obſervation. His account of granite, viz. that it is a ſubſtance which, having been reduced into fuſion by ſubterraneous heat, has been forcibly injected among the ſtrata already conſolidated, was ſo different from that of other mineralogiſts, that it ſeemed particularly to require farther examination. He concluded, that if this account was juſt, ſome confirmation of it muſt appear at thoſe places where the granite and the ſtrata are in contact, or where the former emerges from beneath the latter. In ſuch ſituations, one might expect veins of the ſtone which had been in fuſion to penetrate into the ſtone which had been ſolid; and ſome imperfect deſcriptions of granitic veins gave reaſon to imagine that this phenomenon was actually to be obſerved. Dr Hutton was anxious that an *inſtantia crucis* might ſubject his theory to the ſevereſt teſt.

One

latent, it will become colder than before. Thus alſo, when a quantity of heat aſcends by any means whatever, from one ſtratum of air to a ſuperior ſtratum, a part of it becomes latent, ſo that an equilibrium of heat can never be eſtabliſhed among the ſtrata; but thoſe which are leſs, muſt always remain colder than thoſe that are more, compreſſed. This was Dr Hutton's explanation, and it contains no hypothetical principle whatſoever.

To one who conſiders meteorology with attention, the want of an accurate hygrometer can never fail to be a ſubject of regret. The way of ſupplying this deficiency which Dr Hutton practiſed was by moiſtening the ball of a thermometer, and obſerving the degree of cold produced by the evaporation of the moiſture. The degree of cold, *cæteris paribus*, will be proportional to the dryneſs of the air, and affords, of courſe, a meaſure of that dryneſs. The ſame contrivance, but without any communication whatſoever, occurred afterwards to Mr Leslie, and being purſued through a ſeries of very accurate and curious experiments, has produced an inſtrument which promiſes to anſwer all the purpoſes of photometry, as well as hygrometry, and ſo to make a very important addition to our phyſical apparatus.

One of the places where he knew that a junction of the kind he wifhed to examine muft be found, was the line where the great body of granite which runs from Aberdeen weftward, forming the central chain of the Grampians, comes in contact with the fchiftus which compofes the inferior ridges of the fame mountains toward the fouth. The neareft and moft acceffible point of this line feemed likely to be fituated not far to the eaftward of Blair in Athol, and could hardly fail to be vifible in the beds of fome of the moft northern ftreams which run into the Tay. Dr Hutton having mentioned thefe circumftances to the Duke of Athol, was invited by that nobleman to accompany him in the fhooting feafon into Glentilt, which he did accordingly, together with his friend Mr Clerk of Elden, in fummer 1785.

The Tilt is, according to the feafons, a fmall river, or an impetuous torrent, which runs through a glen of the fame name, nearly fouth-weft, and deeply interfects the fouthern ridges of the Grampian Mountains. The rock through which its bed is cut is in general a hard micaceous fchiftus; and the glen prefents a fcene of great boldnefs and afperity, often embellifhed, however, with the accompaniments of a fofter landfcape.

When they had reached the Foreft Lodge, about feven miles up the valley, Dr Hutton already found himfelf in the midft of the objects which he wifhed to examine. In the bed of the river, many veins of red granite, (no lefs, indeed, than fix large veins in the courfe of a mile), were feen traverfing the black micaceous fchiftus, and producing, by the contraft of colour, an effect that might be ftriking even to an unfkilful obferver. The fight of objects which verified at once fo many important conclufions in his fyftem, filled him with delight; and as his feelings, on fuch occafions, were always ftrongly expreffed, the guides who accompanied him were convinced that it muft be nothing

lefs

lefs than the difcovery of a vein of filver or gold, that could call forth fuch ftrong marks of joy and exultation.

Dr HUTTON has defcribed the appearances at this fpot in the third volume of the Edinburgh *Tranfactions*, p. 79. and fome excellent drawings of them were made by Mr CLERK, whofe pencil is not lefs valuable in the fciences than in the arts. On the whole, it is certain, that of all the junctions of granite and fchiftus which are yet known, this at Glentilt fpeaks the moft unambiguous language, and moft clearly demonftrates the violence with which the granitic veins were injected among the fchiftus *.

In the year following, Dr HUTTON and Mr CLERK alfo vifited Galloway, in fearch of granitic veins, which they found at two different places, where the granite and fchiftus come in contact. One of thefe junctions was afterwards very carefully examined by Sir JAMES HALL and Mr DOUGLAS, now Lord SELKIRK, who made the entire circuit of a tract of granite country, which reaches from the banks of Loch Ken, where the junction is beft feen, weftward to the valley of Palnure, occupying a fpace of about 11 miles by 7. See *Edinburgh Tranfactions*, vol. III. *Hiftory*, p. 8.

IN fummer 1787, Dr HUTTON vifited the ifland of Arran in the mouth of the Clyde, one of thofe fpots in which nature has collected, within a very fmall compafs, all the phenomena moft interefting to a geologift. A range of granite mountains, placed

* I MUST take this opportunity of correcting a miftake which I have made in defcribing the junction in Glentilt, (*Illuftrations of the Huttonian Theory*, p. 310.) where I have faid, that the great body of granite from which thefe veins proceed, is not immediately vifible. This, however, is not the fact, for the mountains on the north fide of the glen are a mafs of granite to which the veins can be directly traced. This I have been affured of by Mr CLERK. Dr HUTTON has not defcribed it diftinctly; and not having feen the union of the veins with the granite on the north fide, when I vifited the fame fpot, I concluded too haftily, that it had not yet been difcovered.

ced in the northern part of the iſland, have their ſides covered with primitive ſchiſtus of various kinds, to which, on the ſea-ſhore, ſucceed ſecondary ſtrata of grit, limeſtone, and even coal. Here, therefore, Dr Hutton had another opportunity of examining the junction of the granite and ſchiſtus, and found abundance of the veins of the former penetrating into the latter. In three different places he met with this phenomenon; in the torrents that deſcend from the ſouth ſide of Goatfield; in Glenroſa, on the weſt, and in the little river Sannax, on the north-eaſt, of that mountain. From the firſt of theſe he brought a ſpecimen of ſome hundred weight, conſiſting of a block of ſchiſtus, which includes a large vein of granite.

At the northern extremity of the iſland he had likewiſe a view of the ſecondary ſtrata lying upon the primary, with their planes at right angles to one another. In the great quantity, alſo, of pudding-ſtone, containing rounded quartzy gravel, united by an arenaceous cement; in the multitude of whinſtone dikes, which abound in this iſland; and in the veins of pitch-ſtone, a foſſil which he had not before met with in its native place; he found other intereſting ſubjects of obſervation; ſo that he returned from this tour highly gratified, and uſed often to ſay that he had no where found his expectations ſo much exceeded, as in the grand and inſtructive appearances with which nature has adorned this little iſland.

Mr John Clerk, the ſon of his friend Mr Clerk of Elden, accompanied him in this excurſion, and made ſeveral drawings, which, together with a deſcription of the iſland drawn up afterwards by Dr Hutton, ſtill remain in manuſcript.

The leaſt complete of the obſervations at Arran was that of the junction of the primitive with the ſecondary ſtrata, which is but indiſtinctly ſeen in that iſland, and only at one place. Indeed, the contact of theſe two kinds of rock, though it forms a line circumſcribing the baſes of all primitive countries,

countries, is ſo covered by the ſoil, as to be viſible in very few places. In the autumn of this ſame year, however, Dr HUTTON had an opportunity of obſerving another inſtance of it in the bank of the river Jedd, about a mile above the town of Jedburgh. The ſchiſtus there is micaceous, in vertical plates, running from eaſt to weſt, though ſomewhat undulated. Over theſe is extended a body of red ſandſtone, in beds nearly horizontal, having interpoſed between it and the vertical ſtrata a breccia full of fragments of theſe laſt. Dr HUTTON has given an account of this ſpot in the firſt volume of his *Theory of the Earth*, p. 432., accompanied with a copper-plate, from a drawing by Mr CLERK.

IN 1788 he made ſome other valuable obſervations of the ſame kind. The ridge of the Lammer-muir Hills, in the ſouth of Scotland, conſiſts of primary micaceous ſchiſtus, and extends from St Abb's-head weſtward, till it join the metalliferous mountains about the ſources of the Clyde. The ſea-coaſt affords a tranſverſe ſection of this alpine tract at its eaſtern extremity, and exhibits the change from the primary to the ſecondary ſtrata, both on the ſouth and on the north. Dr HUTTON wiſhed particularly to examine the latter of theſe, and on this occaſion Sir JAMES HALL and I had the pleaſure to accompany him. We ſailed in a boat from Dunglaſs, on a day when the fineneſs of the weather permitted us to keep cloſe to the foot of the rocks which line the ſhore in that quarter, directing our courſe ſouthwards, in ſearch of the termination of the ſecondary ſtrata. We made for a high rocky point or head-land, the SICCAR, near which, from our obſervations on ſhore, we knew that the object we were in ſearch of was likely to be diſcovered. On landing at this point, we found that we actually trode on the primeval rock, which forms alternately the baſe and the ſummit of the preſent land. It is here a micaceous ſchiſtus, in beds nearly vertical, highly indurated, and ſtretching from S. E. to N. W. The ſurface of this rock runs with a moderate aſcent from the level of low-water, at which we landed, nearly to that of high-water, where the ſchiſtus has a thin covering of red horizontal ſandſtone laid over it; and this ſandſtone, at the diſtance of a few yards farther back, riſes into a very high perpendicular cliff. Here, therefore, the immediate contact of the two rocks is not only viſible, but is curiouſly diſſected and laid open by the action of the waves. The rugged tops of the ſchiſtus are ſeen penetrating into the horizontal beds of ſandſtone, and the loweſt of theſe laſt form a breccia containing fragments of ſchiſtus, ſome round and others angular, united by an arenaceous cement.

Dr Hutton was highly pleaſed with appearances that ſet in ſo clear a light the different formations of the parts which compoſe the exterior cruſt of the earth, and where all the circumſtances were combined that could render the obſervation ſatisfactory and preciſe. On us who ſaw theſe phenomena for the firſt time, the impreſſion made will not eaſily be forgotten. The palpable evidence preſented to us, of one of the moſt extraordinary and important facts in the natural hiſtory of the earth, gave a reality and ſubſtance to thoſe theoretical ſpeculations, which, however probable, had never till now been directly authenticated by the teſtimony of the ſenſes. We often ſaid to ourſelves, What clearer evidence could we have had of the different formation of theſe rocks, and of the long interval which ſeparated their formation, had we actually ſeen them emerging from the boſom of the deep? We felt ourſelves neceſſarily carried back to the time when the ſchiſtus on which we ſtood was yet at the bottom of the ſea, and when the ſandſtone before us was only beginning to be depoſited, in the ſhape of ſand or mud, from the waters of a ſuperincumbent ocean. An epocha ſtill more remote preſented itſelf, when even the moſt ancient of theſe rocks, inſtead of ſtanding upright in vertical

beds,

beds, lay in horizontal planes at the bottom of the ſea, and was not yet diſturbed by that immeaſurable force which has burſt aſunder the ſolid pavement of the globe. Revolutions ſtill more remote appeared in the diſtance of this extraordinary perſpective *. The mind ſeemed to grow giddy by looking ſo far into the abyſs of time; and while we liſtened with earneſtneſs and admiration to the philoſopher who was now unfolding to us the order and ſeries of theſe wonderful events, we became ſenſible how much farther reaſon may ſometimes go than imagination can venture to follow. As for the reſt, we were truly fortunate in the courſe we had purſued in this excurſion; a great number of other curious and important facts preſented themſelves, and we returned, having collected, in one day, more ample materials for future ſpeculation, than have ſometimes reſulted from years of diligent and laborious reſearch.

In the latter part of this ſame ſummer, (1788), Dr Hutton accompanied the Duke of Athol to the Iſle of Man, with a view of making a mineral ſurvey of that iſland. What he ſaw there, however, was not much calculated to illuſtrate any of the great facts in geology. He found the main body of the iſland to conſiſt of primitive ſchiſtus, much inclined, and more interſected with quartzy veins than the correſponding ſchiſtus in the ſouth of Scotland. In two places on the oppoſite ſides of the iſland, this ſchiſtus was covered by ſecondary ſtrata, but the junction was no where viſible. Some granite veins were obſerved in the ſchiſtus, and many looſe blocks of that ſtone were met with in the ſoil, or on the ſurface, but no maſs of it was to be ſeen in its native place. The direction of the primitive ſtrata correſponded very well with that in Galloway, running near-

* For a fuller deduction of the concluſions here referred to, ſee *Theory of the Earth*, Vol. I. p. 458.; alſo *Illuſtrations of the Huttonian Theory*, p. 213.

ly from eaſt to weſt. This is all the general information which was obtained from an excurſion, which, in other reſpects, was very agreeable. Dr HUTTON performed it in company with his friend Mr CLERK, and they again experienced the politeneſs and hoſpitality of the ſame nobleman who had formerly entertained them, on an expedition which deſerves ſo well to be remembered in the annals of geology.

THOUGH from the account now given, it appears that Dr HUTTON's mind had been long turned with great earneſtneſs to the ſtudy of the theory of the earth, he had by no means confined his attention to that ſubject, but had directed it to the formation of a general ſyſtem, both of phyſics and metaphyſics *. He tells us himſelf, that he was led to the ſtudy of general phyſics, from thoſe views of the properties of body which had occurred to him in the proſecution of his chemical and mineralogical inquiries. In thoſe ſpeculations, therefore, that extended ſo far into the regions of abſtract ſcience, he began from chemiſtry ; and it was from thence that he took his departure in his circum-navigation both of the material and intellectual world.

THE chemiſt, indeed, is flattered more than any one elſe with the hopes of diſcovering in what the eſſence of matter conſiſts ; and Nature, while ſhe keeps the aſtronomer and the mechanician at a great diſtance, ſeems to admit him to more familiar converſe, and to a more intimate acquaintance with her ſecrets. The vaſt power which he has acquired over matter, the aſtoniſhing

* AT what time theſe laſt ſpeculations began to ſhare his attention with the former, I have not been able to diſcover, though I have reaſon to believe that before I became acquaint.d with him, which was about 1781, he had completed a manuſcript treatiſe on each of them, the ſame nearly that he afterwards gave to the world. His ſpeculations on general phyſics were of a date much earlier than this.

THE Phyſical Syſtem, referred to here, forms the third part of a work, entitled, *Diſſertations on different Subjects in Natural Philoſophy*, in one vol. 4to, 1792.

ing transformations which he effects, his fuccefs in analyzing almoft all bodies, and in reproducing fo many, feem to promife that he fhall one day difcover the effence of a fubftance which he has fo thoroughly fubdued; that he fhall be able to bind PROTEUS in his cave, and finally extort from him the fecret of his birth; in a word, that he fhall find out what matter is, of what elements it is compofed, and what are the properties effential to its exiftence.

IN entering upon this new inquiry, Dr HUTTON was forcibly ftruck with the very juft reflection, That we do by no means explain the nature of body, when we defcribe it as made up of fmall particles; becaufe if we allow to thefe particles any magnitude whatfoever, we do no more than affirm that great bodies are made up of fmall ones. The elements of body muft, therefore, be admitted to be fomething unextended. To thefe unextended elements, Dr HUTTON gave the name of MATTER, and carefully diftinguifhed between that term and the term BODY, which he applied only to thofe combinations of matter that are neceffarily conceived to poffefs impenetrability, extenfion and inertia.

THE moft accurate examination of the properties of body confirms the truth of the opinion, that it is compofed of unextended elements. Bodies may be compreffed into fmaller dimenfions; many by the application of mechanical force, and all by the diminution of their heat: nor is there any limit to this compreffion, or any point beyond which the farther reduction of volume becomes impoffible. This holds of fubftances the moft compact, as well as the moft volatile and elaftic, and clearly evinces that the elements of body are not in contact with one another, and that in reality we perceive nothing in body but the exiftence of certain powers or forces, acting with various intenfities, and in various directions. Thus the fuppofed impenetrability, and of courfe the extenfion of body, is nothing elfe than the effort of a refifting

ing or repulsive power; its cohesion, weight, &c. the efforts of attractive power; and so with respect to all its other properties.

But if this be granted, and if it be true that in the material world every phenomenon can be explained by the existence of Power, the supposition of extended particles as a *substratum* or residence for such power, is a mere hypothesis, without any countenance from the matter of fact. For if these solid particles are never in contact with one another, what part can they have in the production of natural appearances, or in what sense can they be called the residence of a force which never acts at the point where they are present? Such particles, therefore, ought to be entirely discarded from any theory that proposes to explain the phenomena of the material world.

Thus, it appears, that power is the essence of matter, and that none of our perceptions warrant us in considering even body as involving any thing more than force, subjected to various laws and modifications.

Matter, taken in this sense, is to be considered as indefinitely extended, and without inertia. Its presence through all space is proved by the universality of gravitation; and its want of inertia, by the want of resistance to the planetary motions. Thus, in our inquiry concerning physical causes, we are relieved from one great difficulty, that of supposing matter to act where it is not. The force of gravitation, according to this system, is not the action of two distant bodies upon one another, but it is the action of certain powers, diffused through all space, which may be transmitted to any distance. There seems to me, however, to remain a difficulty hardly less than that from which we appear to be relieved, *viz.* to assign a reason why the intensity with which such powers act on any body, should depend on the position and magnitude of all the bodies in the universe, and should bear to these continually the same relation. But, however this be, the ingenuity of Dr Hutton's reasonings cannot be questioned, ed, nor, I think, the juftnefs of many of his conclufions. His explanations of cohefion, heat, fluidity, deferve particular attention. In one thing, however, he feems to have fallen into an error, which runs through much of his reafoning, concerning the principles of gravitation and inertia. He affirms, that "without "gravity, a body endowed with all the other material qualities "would have no inertia; that it would not diminifh the velo"city of the moving body by which it fhould be actuated, nor "would it move a heavy body whatever were its velocity *." Now, this propofition, though from its nature it cannot be brought to the immediate teft of experience, is certainly inconfiftent with the principles of mechanics; at the fame time, it is true, that we would not, in the cafe here fuppofed, have the fame means of meafuring the motion loft, or gained by collifion, which we have in the actual ftate of bodies. This is perhaps what mifled Dr Hutton; and though his remarks on the meafures of motion and force are very acute, and many of them very juft, the mathematical reader will regret the want of that mode of reafoning, which has raifed mechanics to fo high a rank among the fciences.

It is impoffible not to remark the affinity of this theory with that of the celebrated Boscovich, in which, as in this, all the phenomena of the material world are explained, by the fuppofition of forces varioufly modified, and without the affiftance of folid or extended particles. Thefe forces are fuppofed to be arranged round mathematical points, which are moveable, and act on one another by means of the forces furrounding them. A moft ingenious application of this principle is made to all the ufual refearches of the mechanical philofophy, and, it muft be confeffed, that few theories have more beauty and fimplicity to recommend them, or do better affift the imagination in the explanation

* *Differtations*, &c. p. 312. § 31.

planation of natural appearances. But it involves, in the whole of it, this great difficulty, that mathematical points are not only capable of motion, but capable of being endowed, or, at leaſt, diſtinguiſhed, by phyſical qualities. Dr HUTTON, in his theory, has avoided this difficulty, by giving no other than a negative definition of the MATTER which he ſuppoſes the elementary principle of body. On this account, though to the imagination his theory may want the charms which the other poſſeſſes, yet it has the advantage of going juſt to the extent to which our perceptions or our obſervations authoriſe us to proceed, and of being accurately circumſcribed by the limits pointed out by the laws of philoſophical induction *.

THE exiſtence of matter neither heavy nor inert, which he had taken ſo much pains to eſtabliſh, was applied by him to explain

* THOUGH BOSCOVICH's Theory was publiſhed long before Dr HUTTON's, ſo early, indeed, as the year 1758, there is no reaſon to think that the latter was in any degree ſuggeſted by the former. BOSCOVICH's theory was hardly known in this country till about the year 1770, and the firſt ſketches of Dr HUTTON's theory are of a much older date. Beſides, the method of reaſoning purſued by the authors is quite different; and their concluſions, though alike in ſome things, directly contrary in others, as in what regards gravity, inertia, &c. The Monads of LEIBNITZ might more reaſonably be ſuppoſed to have pointed out to Dr HUTTON the neceſſity of ſuppoſing the elements of body to be unextended, if the originality of his own conceptions, and the little regard he paid to authority in matters of theory, did not relieve us from the neceſſity of looking to others for the ſources of his opinions.

THE principal defect of his theory ſeems to me to conſiſt in this, that it does not ſtate with preciſion the difference between the conſtitution of thoſe *powers* which ſimply form matter, and thoſe that form the more complex ſubſtance, body. In other words, it does not explain what muſt be added to matter to make it body. The anſwer ſeems to me to be, that the addition of a repelling power, in all directions, is ſufficient for that purpoſe. Such a repulſion, if ſtrong enough, would produce both impenetrability and inertia. The matter, again, that poſſeſſed only an attractive power, like *gravity*, or a repulſive power only in a certain direction, like *light*, would not be inert nor impenetrable. In this inference, however, from his ſyſtem, I am not ſure if I ſhould meet with the author's approbation.

plain the phenomena of light, heat, and electricity. He consider-ed all these three as modifications of the solar substance, and thought that many of the appearances they exhibit, are only to be explained on the supposition that they consist of an expan-sive force, of which inertness is not predicable; in particular, that light is a power propagated from the sun in all directions, like gravity, with this difference, that it is repulsive, while gra-vity is attractive, and requires time for its transmission, which the latter does not, at least in any sensible quantity *.

THE prosecution of this subject has led him to consider the nature of PHLOGISTON, a substance once so famous in chemistry, but of which the name has almost as entirely disappeared from the vo-cabulary of that science, as the word Vortex from the language of physical astronomy. The new and important experiments made on the calcination of metals, and on the composition of water, are, as is well known, the foundations of the antiphlogi-stic theory. Nobody was more pleased than Dr HUTTON with these experiments, nor held in higher estimation the character and abilities of the chemists and philosophers by whom they were conducted. He was nevertheless of opinion, that the con-clusions drawn from them are not altogether unexceptionable, nor deduced with a sufficient attention to every circumstance. This remark he thought peculiarly applicable to what regards the composition of water, to the phenomena of which experi-ment, the dissertation we are now speaking of is chiefly directed. The two aëriform fluids, it is there observed, which compose water, in order to unite, must not simply be brought together, for in that state they might remain for ever unchanged, but they must be set on fire, and made to burn, and from this burning there are evidently two substances which make their escape,

namely,

* See Dissertations V. and VI. on Matter and Motion, in the work above quot-ed. The Chemical Dissertation on Phlogiston is in the same volume, p. 171.

namely, Light and Heat. Though, therefore, the weight of the water generated, and of the gaſes combined, may be admitted to be equal, yet it muſt be acknowledged, that two ſubſtances are loſt, which the chemiſt cannot confine in his cloſeſt veſſel, nor weigh in his fineſt balance, and it is going much farther than we are authoriſed to do, either by experiment or analogy, to conclude that theſe ſubſtances have had no effect. As heat and light, in Dr HUTTON's ſyſtem, are compoſed of that matter which does not gravitate, the exact coincidence which M. LAVOISIER obſerved between the weight of the water produced and that of the two elaſtic fluids, united in the compoſition of it, was no argument, in his eyes, againſt the eſcape of a very eſſential part of the ingredients.

PURSUING the ſame reaſoning, he ſhews how little ground there is to ſuppoſe that the heat and light evolved in this experiment proceed from the vital air; and he concludes, that the real explanation of the proceſs is, that by burning, the matter of light and heat, or the phlogiſton of the hydrogenous gas, is ſet at liberty, and is thus enabled to unite with the vital air.

IN the ſame manner, on examining what relates to the burning of inflammable bodies, he finds the oxygenous gas unequal to the effect of furniſhing by its latent heat, or caloric, the whole of the ſenſible heat that is produced. He concludes, therefore, that the hypotheſis of the exiſtence of phlogiſton in thoſe bodies that are termed inflammable, is neceſſary to account for the phenomena of burning; phenomena, as he juſtly remarks, which are among the moſt curious and important of any that are exhibited by the material world. On the whole, it cannot be doubted, that great ingenuity and much ſound argument are diſplayed throughout the whole of this diſſertation, and that whatever be ultimately decided with regard to the principle for which the author ſo ſtrenuouſly contends, he has made it evident, that the concluſions conclufions of the antiphlogiftic theory have been drawn with too much precipitancy, and carried farther than is warranted by the ftrict rules of inductive philofophy.

THE fubject of Fire, Light and Heat was refumed by Dr HUTTON feveral years after this period, and formed the fubject of a feries of papers which he read in the Royal Society of Edinburgh, and afterwards publifhed feparately. He there explains more fully his notion of the fubftances juft mentioned, which he confiders as different modifications of the folar matter, alike deftitute of inertnefs and of gravity.

A MORE voluminous work from Dr HUTTON's pen, made its appearance foon after the Phyfical Differtations, *viz. An Inveftigation of the Principles of Knowledge, and of the Progrefs of Reafon from Senfe to Science and Philofophy*, in three volumes quarto.

HE informs us himfelf of the train of thought by which he was led to the metaphyfical fpeculations contained in thefe volumes. He had fatisfied himfelf, by his phyfical inveftigations, that body is not what it is conceived by us to be, a thing neceffarily poffeffing volume, figure and impenetrability, but merely an affemblage of powers, that by their action produce in us the ideas of thefe external qualities. His curiofity, therefore, was naturally excited to inquire farther into the manner in which we form our conceptions of body, or into the nature of the intercourfe which the mind holds with thofe things that exift without it. In purfuing this inquiry, he foon became convinced, that magnitude, figure and impenetrability, are no otherwife perceived by the mind than colour, tafte and fmell; that is, that what are called the primary qualities of body, are precifely on the fame footing with the fecondary, and are both conceptions of the mind, which can have no refemblance to the external caufe by which thofe conceptions are produced. The world, therefore, as conceived by us, is the creation of the mind itfelf, but of the mind acted on from without, and receiving information from ſome external power. But though, according to this reaſoning, there be no reſemblance between the world without us, and the notions that we form of it, though magnitude and figure, though ſpace, time and motion, have no exiſtence but in the mind; yet our perceptions being conſiſtent, and regulated by conſtant and uniform laws, are as much realities to us, as if they were the exact copies of things really exiſting; they equally intereſt our happineſs, and muſt equally determine our conduct. They form a ſyſtem, not dependent on the mind alone, but dependent on the action which certain external cauſes have upon it. The whole doctrine, therefore, of moral obligation, remains the ſame in this ſyſtem, and in that which maintains the perfect reſemblance of our ideas to the cauſes by which they are produced.

MANY philoſophers have regarded our ideas as very imperfect repreſentations of external things; but Dr HUTTON conſiders their perfect diſſimilitude as completely proved. PLATO has likened the mind to an eye, ſo ſituated, as to ſee nothing but the faint images of objects projected on the bottom of a dark cave, while the objects themſelves are entirely concealed; but he thinks, that by help of philoſophy, the mental eye may be directed toward the mouth of the cave, and may perceive the objects in their true figure and dimenſions. But, with Dr HUTTON, the figures ſeen at the bottom of the cave have no reſemblance to the originals without; nor can man, by any contrivance, hold communication with thoſe originals, nor ever know any thing about them, except that they are not what they ſeem to be, and have no property in common with the figures which denote their exiſtence. In a word, external things are no more like the perceptions they give riſe to, than wine is ſimilar to intoxication, or opium to the delirium which it produces.

IT has been already remarked, that this ſyſtem, however peculiar in other reſpects, involves in it the ſame principles of morals

rals with thofe more generally received; and the fame may be faid as to the exiftence of God, and the immortality of the foul. The view which it prefents of the latter doctrine, deferves particularly to be remarked. Death is not regarded here as the diffolution of a connexion between mind, and that fyftem of material organs, by means of which it communicated with the external world, but merely as an effect of the mind's ceafing to perceive a particular order or clafs of things; it is therefore only the termination of a certain mode of thought; and the extinction, not of any mental power, but of a train of conceptions, which, in confequence of external impulfe, had exifted in the mind. Thus, as nothing effential to intellectual power perifhes, we are to confider death only as a paffage from one condition of thought to another; and hence this fyftem appeared, to the author of it, to afford a ftronger argument than any other, for the exiftence of the mind after death.

Indeed, Dr Hutton has taken great pains to deduce from his fyftem, in a regular manner, the leading doctrines of morality and natural religion, having dedicated the third volume of his book almoft wholly to that object. It is worthy of remark, that while he is thus employed, his ftyle affumes a better tone, and a much greater degree of perfpicuity, than it ufually poffeffes. Many inftances might be pointed out, where the warmth of his benevolent and moral feelings burfts through the clouds that fo often veil from us the cleareft ideas of his underftanding. One, in particular, deferves notice, in which he treats of the importance of the female character to fociety, in a ftate of high civilization *. A felicity of expreffion, and a flow of natural eloquence, infpired by fo interefting a fubject, make us regret that his pen did not more frequently do juftice to his thoughts.

The metaphyfical theory, of which the outline, (though very imperfectly), has now been traced, cannot fail to recall the opinions

* *Inveftigation of the Principles of Knowledge*, Vol. III. p. 588. &c.

nions maintained by Dr BERKELEY concerning the exiſtence of matter. The two ſyſtems do indeed agree in one material point, but differ eſſentially in the reſt. They agree in maintaining, that the conceptions of the mind are not copied from things of the ſame kind exiſting without it; but they differ in this, that Dr BERKELEY imagined that there is nothing at all external, and that it is by the direct agency of the Deity that ſenſation and perception are produced in the mind. Dr HUTTON holds, on the other hand, that there is an external exiſtence, from which the mind receives its information, and by the action of which, impreſſions are made on it; but impreſſions that do not at all reſemble the powers by which they are cauſed.

THE reaſonings alſo by which the two theories are ſupported, are very diſſimilar, though perhaps they ſo far agree, that if Dr BERKELEY had been better acquainted with phyſics, and had made it more a rule to exclude all hypotheſis, he would have arrived preciſely at the ſame concluſion with Dr HUTTON. Indeed, I cannot help being of opinion, that every one will do ſo, who, in inveſtigating the origin of our perceptions, determines to reaſon without aſſuming any hypotheſis, and without taking for granted any of thoſe maxims which the mind is diſpoſed to receive, either, as ſome philoſophers ſay, from habit, or, as others maintain, from an inſtinctive determination, (ſuch as has been termed *common ſenſe*) that admits of no analyſis. Though this may not be the kind of reaſoning beſt ſuited to the ſubject, yet it is ſo analogous to what ſucceeds in other caſes, that it is good to have an example of it, and, on that account, were it for nothing elſe, the theory we are now ſpeaking of certainly merits more attention than it has yet met with *. The great ſize of the book, and the obſcurity

* I HAVE hardly found this work of Dr HUTTON's quoted by any writer of eminence, except by Dr PAR, in his *Spital Sermon*, a tract no leſs remarkable for learning and acuteneſs, than for the liberality and candour of the ſentiments which it contains.

obſcurity which may juſtly be objected to many parts of it, have probably prevented it from being received as it deſerves, even among thoſe who are converſant with abſtract ſpeculation. An abridgment of it, judiciouſly executed, ſo as to ſtate the argument in a manner both perſpicuous and conciſe, would, I am perſuaded, make a valuable addition to metaphyſical ſcience.

THE publication of this work was Dr HUTTON's occupation on his recovery from a ſevere illneſs, with which he was ſeized in ſummer 1793. Before this time he had enjoyed a long continuance of good health, and great activity both of body and mind. The diſorder that now attacked him, (a retention of urine), was one of thoſe that moſt immediately threaten life, and he was preſerved only by ſubmitting to a dangerous, and painful operation. He was thus reduced to a ſtate of great weakneſs, and was confined to his room for many months. By degrees, however, the goodneſs of his conſtitution, aided, no doubt, by the vigour and elaſticity of his mind, reſtored him to a conſiderable meaſure of health, and rendered his recovery much more complete than could have been expected. One of his amuſements, when he had regained ſome tolerable degree of ſtrength, was in ſuperintending the publication, and correcting the proof-ſheets of the work juſt mentioned.

DURING his convaleſcence, his activity was farther called into exertion, by an attack on his *Theory of the Earth*, made by Mr KIRWAN, in the *Memoirs of the Iriſh Academy* *, and rendered formidable,

* THIS was not the firſt attack which had been made on his theory, for M. DE LUC, in a ſeries of letters, inſerted in the *Monthly Review* for 1790 and 1791, had combated ſeveral of the leading opinions contained in it. To theſe Dr HUTTON made no other reply, than is to be met with occaſionally in the enlarged edition of his *Theory*, publiſhed four years afterwards. If I do not miſtake, however, he intended a more particular anſwer, and actually ſent one to the editors of

dable, not by the ſtrength of the arguments it employed, but by the name of the author, the heavy charges which it brought forward, and the groſs miſconceptions in which it abounded *.

Before this period, though Dr Hutton had been often urged by his friends to publiſh his entire work on the *Theory of the Earth*, he had continually put off the publication, and there ſeemed to be ſome danger that it would not take place in his own life time. The very day, however, after Mr Kirwan's paper was put into his hands, he began the reviſal of his manuſcript, and reſolved immediately to ſend it to the preſs. The reaſon he gave was, that Mr Kirwan had in ſo many inſtances completely miſtaken, both the facts, and the reaſonings in his *Theory*, that he ſaw the neceſſity of laying before the world a more ample explanation of them. The work was accordingly publiſhed, in two volumes octavo, in 1795; and contained, beſides what was formerly given in the *Edinburgh Tranſactions*, the proofs and reaſonings much more in detail, and a much fuller application of the principles to the explanation of appearances. The two volumes, however, then publiſhed, do not complete the theory: a third, neceſſary for that purpoſe, remained behind, and is ſtill in manuſcript.

After

of the ſame *Review*, who refuſed to inſert it. This, indeed, I do not ſtate with perfect confidence, as I ſpeak only from recollection, and would not, on that authority, bring a poſitive charge of partiality againſt men who exerciſe a profeſſion in which impartiality is the firſt requiſite. Suppoſing, however, the ſtatement here given to be correct, an excuſe is ſtill left for the Reviewers; they may ſay, that in communicating original papers, as they do not act in their judicial capacity, they are not bound to diſpenſe juſtice with their uſual blindneſs and ſeverity, but may be permitted to relax a little from the exerciſe of a virtue that is ſo often left to be its own reward.

* For a defence of Dr Hutton againſt the charges here alluded to, I muſt take the liberty of referring to the *Illuſtrations of the Huttonian Theory*, p. 119. and 125.

After the publication of the work juſt mentioned, he began to prepare another for the preſs, on a ſubject which had early occupied his thoughts, and had been at no time of his life entirely neglected. This ſubject was huſbandry, on which he had written a great deal, the fruit both of his reading and experience; and he now propoſed to reduce the whole into a ſyſtematic form, under the title of *Elements of Agriculture*. This work, which he nearly completed, remains in manuſcript. It is written with conſiderable perſpicuity; and though I can judge but very imperfectly of its merits, I can venture to ſay, that it contains a great deal of ſolid and practical knowledge, without any of the vague and unphiloſophic theory ſo common in books on the ſame ſubject. In particular, I muſt obſerve, that where it treats of climate, and the influence of heat, in accelerating the maturity of plants, it furniſhes ſeveral views that appear to be perfectly new, and that are certainly highly intereſting.

The period, however, was now not far diſtant, which was to terminate the exertions of a mind of ſuch ſingular activity, and of ſuch ardour in the purſuit of knowledge. Not long after the time we are ſpeaking of, Dr Hutton was again attacked by the ſame diſorder from which he had already made ſo remarkable a recovery. He was again ſaved from the danger that immediately threatened him, but his conſtitution had materially ſuffered, and nothing could reſtore him to his former ſtrength. He recovered, indeed, ſo far as to amuſe himſelf with ſtudy, and with the converſation of his friends, and even to go on with the work on agriculture, which was nearly completed. He was, however, confined entirely to the houſe; and in the courſe of the winter 1796-7, he became gradually weaker, was extremely emaciated, and ſuffered much pain, but ſtill retained the full activity and acuteneſs of his mind. He conſtantly employed himſelf in reading and writing, and was particularly pleaſed with the third and fourth volumes of Saussure's *Voyages* *aux Alpes*, which reached him in the courfe of that winter, and became the laft ftudy of one eminent geologift, as they were the laft work of another. On Saturday the 26th of March he fuffered a good deal of pain; but, neverthelefs, employed himfelf in writing, and particularly in noting down his remarks on fome attempts which were then making towards a new mineralogical nomenclature. In the evening he was feized with a fhivering, and his uneafinefs continuing to increafe, he fent for his friend Mr Russel, who attended him as his furgeon. Before he could poffibly arrive, all medical affiftance was in vain: Dr Hutton had juft ftrength left to ftretch out his hand to him, and immediately expired.

Dr Hutton poffeffed, in an eminent degree, the talents, the acquirements, and the temper, which entitle a man to the name of a philofopher. The direction of his ftudies, though in fome refpects irregular and uncommon, had been highly favourable to the developement of his natural powers, efpecially of that quick penetration, and that originality of thought, which ftrongly marked his intellectual character. From his firft outfet in fcience, he had purfued the track of experiment and obfervation, and it was not till after being long exercifed in this fchool, that he entered on the field of general and abftract fpeculation. He combined accordingly, through his whole life, the powers of an accurate obferver, and of a fagacious theorift, and was as cautious and patient in the former character, as he was bold and rapid in the latter.

Long and continued practice had increafed his powers of obfervation to a high degree of perfection; fo that, in difcriminating mineral fubftances, and in feizing the affinities or differences

ferences among geological appearances, he had an acutenefs hardly to be excelled. The eulogy fo happily conveyed in the Italian phrafe, of *offervatore oculatiffimo*, might moft juftly be applied to him; for, with an accurate eye for perceiving the characters of natural objects, he had in equal perfection the power of interpreting their fignification, and of decyphering thofe ancient hieroglyphics which record the revolutions of the globe. There may have been other mineralogifts, who could defcribe as well the fracture, the figure, the fmell, or the colour of a fpecimen; but there have been few who equalled him in reading the characters, which tell not only what a foffil *is*, but what it *has been*, and declare the feries of changes through which it has paffed. His expertnefs in this art, the finenefs of his obfervations, and the ingenuity of his reafonings, were truly admirable. It would, I am perfuaded, be difficult to find in any of the fciences a better illuftration of the profound maxims eftablifhed by BACON, in his *Prærogativæ Inftantiarum*, than were often afforded by Dr HUTTON's mineralogical difquifitions, when he exhibited his fpecimens, and difcourfed on them with his friends. No one could better apply the luminous inftances to elucidate the obfcure, the decifive to interpret the doubtful, or the fimple to unravel the complex. None was more fkilful in marking the gradations of nature, as fhe paffes from one extreme to another; more diligent in obferving the *continuity* of her proceedings, or more fagacious in tracing her footfteps, even where they were moft lightly impreffed.

WITH him, therefore, mineralogy was not a mere ftudy of names and external characters, (though he was fingularly well verfed in that ftudy alfo), but it was a fublime and important branch of phyfical fcience, which had for its object to unfold the connexion between the paft, the prefent, and the future conditions of the globe. Accordingly, his *collection of foffils* was formed for explaining the principles of geology, and for illuftrating the changes which mineral fubftances have gone through, in the paffage which, according to all theories, they have made, from a foft or fluid, to a hard and folid ftate, and from immerfion under the ocean, to elevation above its furface. The feries of thefe changes, and the relative antiquity of the different fteps by which they have been effected, were the objects which he had in view to explain; and his cabinet, though well adapted to this end, with regard to other purpofes was very imperfect. They who expect to find, in a collection, fpecimens of all the fpecies, and all the varieties, into which a fyftem of artificial arrangement may have divided the foffil kingdom, will perhaps turn faftidioufly from one that is not remarkable either for the number or brilliancy of the objects contained in it. They, on the other hand, will think it highly interefting, who wifh to reafon concerning the natural hiftory of minerals, and who are not lefs eager to become acquainted with the laws that govern, than with the individuals that compofe, the foffil kingdom.

The lofs fuftained by the death of Dr Hutton, was aggravated, to thofe who knew him, by the confideration of how much of his knowledge had perifhed with himfelf, and, notwithftanding all that he had written, how much of the light collected by a long life of experience and obfervation, was now completely extinguifhed. It is indeed melancholy to reflect, that with all who make proficiency in the fciences founded on nice and delicate obfervation, fomething of this fort muft unavoidably happen. The experienced eye, the power of perceiving the minute differences, and fine analogies, which difcriminate or unite the objects of fcience; and the readinefs of comparing new phenomena with others already treafured up in the mind; thefe are accomplifhments which no rules can teach, and no precepts can put us in poffeffion of. This is a portion of knowledge which every man muft acquire for himfelf, and which nobody can leave as an inheritance to his fucceffor. It feems, indeed, as if nature had

had in this inſtance admitted an exception to the rule, by which ſhe has ordained the perpetual accumulation of knowledge among civilized men, and had deſtined a conſiderable portion of ſcience continually to grow up and periſh with the individual.

A CIRCUMSTANCE which greatly diſtinguiſhed the intellectual character of the philoſopher of whom we now ſpeak, was an uncommon activity and ardour of mind, upheld by the greateſt admiration of whatever in ſcience was new, beautiful, or ſublime. The acquiſitions of fortune, and the enjoyments which moſt directly addreſs the ſenſes, do not call up more lively expreſſions of joy in other men, than hearing of a new invention, or being made acquainted with a new truth, would, at any time, do in Dr HUTTON. This ſenſibility to intellectual pleaſure, was not confined to a few objects, nor to the ſciences which he particularly cultivated: he would rejoice over WATT's improvements on the ſteam-engine, or COOK's diſcoveries in the South Sea, with all the warmth of a man who was to ſhare in the honour or the profit about to accrue from them. The fire of his expreſſion, on ſuch occaſions, and the animation of his countenance and manner, are not to be deſcribed; they were always ſeen with great delight by thoſe who could enter into his ſentiments, and often with great aſtoniſhment by thoſe who could not.

WITH this exquiſite reliſh for whatever is beautiful and ſublime in ſcience, we may eaſily conceive what pleaſure he derived from his own geological ſpeculations. The novelty and grandeur of the objects offered by them to the imagination, the ſimple and uniform order given to the whole natural hiſtory of the earth, and, above all, the views opened of the wiſdom that governs nature, are things to which hardly any man could be inſenſible; but to him they were matter, not of tranſient delight, but

but of ſolid and permanent happineſs. Few ſyſtems, indeed, were better calculated than his, to entertain their author with ſuch noble and magnificent proſpects; and no author was ever more diſpoſed to conſider the enjoyment of them, as the full and adequate reward of his labours.

THE great range which he had taken in ſcience, has ſufficiently appeared, from the account already given of his works *. There were indeed hardly any ſciences, except the mathematical, to which he had not turned his attention, and his neglect of theſe probably aroſe from this, that, at the time when his acquaintance with them ſhould have commenced, his love of knowledge had already fixed itſelf on other objects. The aptitude of his mind for geometrical reaſoning, was, however, proved on many occaſions. His theory of rain reſts on mathematical principles, and the concluſions deduced from them are perfectly accurate, though by no means obvious. I may add, that he had an uncommon facility in comprehending the nature of mechanical contrivances; and, for one who was not a practical engineer, could form, beforehand, a very ſound judgment concerning their effects.

NOTWITHSTANDING a taſte for ſuch various information, and a mind of ſuch conſtant activity, he read but few ſpeculative books, directing his attention chiefly to ſuch as furniſhed the materials of ſpeculation. Of voyages, travels, and books relating to the

* He had ſtudied with great care ſeveral ſubjects of which no mention is made above. One of theſe was the Formation, or, as we may rather call it, the Natural Hiſtory of Language. A portion of his metaphyſical work is dedicated to the Theory of Language, vol. I. p. 574, &c.; and vol. II. p. 624, &c. He read ſeveral very ingenious papers on the Written Language, in the Royal Society of Edinburgh, ſee *Tranſactions of the Royal Society of Edinburgh*, vol. II. *Hiſt.* p. 5. &c. The Chineſe language, as an extreme caſe in the invention of writing, had greatly occupied his thoughts, and is the ſubject of ſeveral of his manuſcripts.

the natural hiftory of the earth, he had an extenfive knowledge: he had ftudied them with that critical difcuffion which fuch books require above all others; carefully collecting from them the facts that appeared accurate, and correcting the narratives that were imperfect, either by a comparifon with one another, or by applying to them the ftandard of probability which his own obfervation and judgment had furnifhed him with. On the other hand, he beftowed but little attention on books of opinion and theory; and while he trufted to the efforts of his mind for digefting the facts he had obtained from reading or experience, into a fyftem of his own, he was not very anxious, at leaft till that was accomplifhed, to be informed of the views which other philofophers had taken of the fame fubject. He was but little difpofed to concede any thing to mere authority; and to his indifference about the opinions of former theorifts, it is probable that his own fpeculations owed fome part, both of their excellencies, and their defects.

As he was indefatigable in ftudy, and was in the habit of ufing his pen continually as an inftrument of thought, he wrote a great deal, and has left behind him an incredible quantity of manufcript, though imperfect, and never intended for the prefs. Indeed his manner of life, at leaft after he left off the occupations of hufbandry, gave him fuch a command of his time, as is enjoyed by very few. Though he ufed to rife late, he began immediately to ftudy, and generally continued bufy till dinner. He dined early, almoft always at home, and paffed very little time at table; for he ate fparingly, and drank no wine. After dinner he refumed his ftudies, or, if the weather was fine, walked for two or three hours, when he could not be faid to give up ftudy, though he might, perhaps, change the object of it. The evening he always fpent in the fociety of his friends. No profeffional, and rarely any domeftic arrangements interrupted this uniform courfe of life, fo that his time was wholly divided between the purfuits

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of ſcience and the converſation of his friends, unleſs when he travelled from home on ſome excurſion, from which he never failed to return furniſhed with new materials for geological inveſtigation.

To his friends his converſation was ineſtimable; as great talents, the moſt perfect candour, and the utmoſt ſimplicity of character and manners, all united to ſtamp a value upon it. He had, indeed, that genuine ſimplicity, originating in the abſence of all ſelfiſhneſs and vanity, by which a man loſes ſight of himſelf altogether, and neither conceals what is, nor affects what is not. This ſimplicity pervaded his whole conduct; while his manner, which was peculiar, but highly pleaſing, diſplayed a degree of vivacity, hardly ever to be found among men of profound and abſtract ſpeculation. His great liveline ſs, added to this aptneſs to loſe ſight of himſelf, would ſometimes lead him into little excentricities, that formed an amuſing contraſt with the graver habits of a philoſophic life.

THOUGH extreme ſimplicity of manner does not unfrequently impart a degree of feebleneſs to the expreſſion of thought, the contrary was true of Dr HUTTON. His converſation was extremely animated and forcible, and, whether ſerious or gay, full of ingenious and original obſervation. Great information, and an excellent memory, ſupplied an inexhauſtible fund of illuſtration, always happily introduced, and in which, when the ſubject admitted of it, the witty and the ludicrous never failed to occupy a conſiderable place.—But it is impoſſible by words to convey any idea of the effect of his converſation, and of the impreſſion made by ſo much philoſophy, gaiety and humour, accompanied by a manner at once ſo animated and ſo ſimple. Things are made known only by compariſon, and that which is *unique* admits of no deſcription.

THE whole exterior of Dr HUTTON was calculated to heighten the effect which his converſation produced. His figure was

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ſlender, but indicated activity; while a thin countenance, a high forehead, and a noſe ſomewhat aquiline, beſpoke extraordinary acuteneſs and vigour of mind. His eye was penetrating and keen, but full of gentleneſs and benignity; and even his dreſs, plain, and all of one colour, was in perfect harmony with the reſt of the picture, and ſeemed to give a fuller *relief* to its characteriſtic features *.

THE friendſhip that ſubſiſted between him and Dr BLACK has been already mentioned, and was indeed a diſtinguiſhing circumſtance in the life and character of both. There was in theſe two excellent men that ſimilarity of diſpoſition which muſt be the foundation of all friendſhip, and, at the ſame time, that degree of diverſity, which ſeems neceſſary to give to friends the higheſt reliſh for the ſociety of one another.

THEY both cultivated nearly the ſame branches of phyſics, and entertained concerning them nearly the ſame opinions. They were both formed with a taſte for what is beautiful and great in ſcience; with minds inventive, and fertile in new combinations. Both poſſeſſed manners of the moſt genuine ſimplicity, and in every action diſcovered the ſincerity and candour of their diſpoſitions; yet they were in many things extremely diſſimilar. Ardour, and even enthuſiaſm, in the purſuit of ſcience, great rapidity of thought, and much animation, diſtinguiſhed Dr HUTTON on all occaſions. Great caution in his reaſonings, and a coolneſs of head that even approached to indifference, were characteriſtic of Dr BLACK. On attending to their converſation, and the way in which they treated any queſtion of ſcience or philoſophy, one would ſay that Dr BLACK dreaded nothing ſo much as error, and that Dr HUTTON dread-

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* A PORTRAIT of Dr HUTTON, by RAEBURN, painted for the late JOHN DAVIDSON, Eſq; of Stewartfield, one of his old and intimate friends, conveys a good idea of a phyſiognomy and character of face to which it was difficult to do complete juſtice.

ed nothing ſo much as ignorance; that the one was always afraid of going beyond the truth, and the other of not reaching it. The curioſity of the latter was by much the moſt eaſily awakened, and its impulſe moſt powerful and imperious. With the former, it was a deſire which he could ſuſpend and lay aſleep for a time; with the other, it was an appetite that might be ſatisfied for a moment, but was ſure to be quickly renewed. Even the ſimplicity of manner which was poſſeſſed by both theſe philoſophers, was by no means preciſely the ſame. That of Dr BLACK was correct, reſpecting at all times the prejudices and faſhions of the world; that of Dr HUTTON was more careleſs, and was often found in direct colliſion with both.

FROM theſe diverſities, their ſociety was infinitely pleaſing, both to themſelves and thoſe about them. Each had ſomething to give which the other was in want of. Dr BLACK derived great amuſement from the vivacity of his friend, the ſallies of his wit, the glow and original turn of his expreſſion; and that calmneſs and ſerenity of mind which, even in a man of genius, may border on languor or monotony, received a pleaſing impulſe by ſympathy with more powerful emotions.

ON the other hand, the coolneſs of Dr BLACK, the judicioufneſs and ſolidity of his reflections, ſerved to temper the zeal, and reſtrain the impetuoſity of Dr HUTTON. In every material point of philoſophy they perfectly agreed. The theory of the earth had been a ſubject of diſcuſſion with them for many years, and Dr BLACK ſubſcribed entirely to the ſyſtem of his friend. In ſcience, nothing certainly is due to authority, except a careful examination of the opinions which it ſupports. It is not meant to claim any more than this in favour of the Huttonian Geology; but they who reject that ſyſtem, without examination, would do well to conſider, that it had the entire and unqualified approbation of one of the cooleſt, and

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foundeſt reaſoners of which the preſent age furniſhes any example.

Mr Clerk of Elden was another friend, with whom, in the formation of his theory, Dr Hutton maintained a conſtant communication. Mr Clerk, perhaps from the extenſive property which his family had in the coal-mines near Edinburgh, was early intereſted in the purſuits of mineralogy. His inquiries, however, were never confined to the objects which mere ſituation might point out, and, through his whole life, have been much more directed by the irreſiſtible impulſe of genius, than by the action of external circumſtances. Though not bred to the ſea, he is well known to have ſtudied the principles of naval war with unexampled ſucceſs; and though not exerciſing the profeſſion of arms, he has viewed every country through which he has paſſed with the eye of a ſoldier as well as a geologiſt. The intereſt he took in ſtudying the ſurface no leſs than the interior of the earth; his extenſive information in moſt branches of natural hiſtory; a mind of great reſource, and great readineſs of invention; made him, to Dr Hutton, an invaluable friend and coadjutor. It cannot be doubted, that, in many parts, the ſyſtem of the latter has had great obligations to the ingenuity of the former, though the unreſerved intercourſe of friendſhip, and the adjuſtments produced by mutual ſuggeſtion, might render thoſe parts undiſtinguiſhable even by the authors themſelves. Mr Clerk's pencil was ever at the command of his friend, and has certainly rendered him moſt eſſential ſervice.

But it was not to philoſophers and men of ſcience only that Dr Hutton's converſation was agreeable. He was little known, indeed, in general company, and had no great reliſh for the enjoyment which it affords; yet he was fond of domeſtic ſociety, and took great delight in a few private circles, where ſeveral excellent and accompliſhed individuals of both ſexes thought themſelves happy to be reckoned in the num-

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ber of his friends. In one or other of theſe, he was accuſtomed almoſt every evening to ſeek relaxation from the ſtudies of the day, and found always the moſt cordial welcome. A brighter tint of gaiety and chearfulneſs ſpread itſelf over every countenance when the Doctor entered the room; and the philoſopher who had juſt deſcended from the ſublimeſt ſpeculations of metaphyſics, or riſen from the deepeſt reſearches of geology, ſeated himſelf at the tea-table, as much diſengaged from thought, as chearful and gay, as the youngeſt of the company. Theſe parties were delightful, and, by all who have had the happineſs to be preſent at them, will never ceaſe to be remembered with pleaſure.

He uſed alſo regularly to unbend himſelf with a few friends, in the little ſociety alluded to in Profeſſor Stewart's *Life of* Mr Smith, and uſually known by the name of the *Oyſter Club*. This club met weekly; the original members of it were Mr Smith, Dr Black, and Dr Hutton, and round them was ſoon formed a knot of thoſe who knew how to value the familiar and ſocial converſe of theſe illuſtrious men. As all the three poſſeſſed great talents, enlarged views, and extenſive information, without any of the ſtatelineſs and formality which men of letters think it ſometimes neceſſary to affect; as they were all three eaſily amuſed; were equally prepared to ſpeak and to liſten; and as the ſincerity of their friendſhip had never been darkened by the leaſt ſhade of envy; it would be hard to find an example, where every thing favourable to good ſociety was more perfectly united, and every thing adverſe more entirely excluded. The converſation was always free, often ſcientific, but never didactic or diſputatious; and as this club was much the reſort of the ſtrangers who viſited Edinburgh, from any object connected with art or with ſcience, it derived from thence an extraordinary degree of variety and intereſt. It

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is matter of real regret, that it has been unable to ſurvive its founders.

THE ſimplicity of manner that has been already remarked as ſo ſtrikingly exemplified in Dr HUTTON, was but a part of an extreme diſintereſtedneſs which manifeſted itſelf in every thing he did. He was upright, candid, and ſincere; ſtrongly attached to his friends; ready to ſacrifice any thing to aſſiſt them; humane and charitable. He ſet no great value on money, or, perhaps, to ſpeak properly, he ſet on it no more than its true value; yet, owing to the moderation of his manner of life, and the ability with which his friend Mr DAVIE conducted their joint concerns, he acquired conſiderable wealth.

HE was never married, but lived with his ſiſters, three excellent women, who managed his domeſtic affairs; and of whom, only one, Miſs ISABELLA HUTTON, remained to lament his death. By her his collection of foſſils, about which he left no particular inſtructions, was preſented to Dr BLACK; who thought that he could not better conſult the advantage of the public, or the credit of his friend, than by giving it to the Royal Society of Edinburgh, under the condition that it ſhould be completely arranged, and kept for ever ſeparate, for the purpoſe of illuſtrating the HUTTONIAN THEORY OF THE EARTH.

II. MINUTES *of the* LIFE *and* CHARACTER *of* JOSEPH BLACK, M. D. *Addreſſed to the Royal Society of Edinburgh.*

[*Read Aug.* 3. 1801.]

THE merits of ſtudious men are to be eſtimated by the aids they have given to the advancement of ſcience, or the literary monuments they have left with poſterity; but if the public be gratified by their labours in theſe reſpects, readers are generally willing alſo to be told, who and whence they were.

JOSEPH BLACK, the perſon to whom theſe minutes relate, ſucceſſively Profeſſor in the Univerſities of Glaſgow and of Edinburgh, Member of this Society, and of other royal and public inſtitutions in Europe; having made important diſcoveries, and having laid the foundations of many others, towards erecting a fabric of ſcience, which has ſince been raiſed to a conſiderable height; and having been himſelf diſtinguiſhed for modeſty and felicity of manners, as well as correctneſs of underſtanding, and ingenuity of reſearch,—will, it is hoped, be thought worthy of notice on theſe accounts. He was born on the banks of the Garrone, in France, in the year 1728. His parents were Iriſh and Scots. His father, JOHN BLACK, a native of Belfaſt in Ireland, was ſettled in the wine-trade at Bourdeaux. His mother was a daughter of ROBERT GORDON, of the family of Halhead in Aberdeenſhire, who was likewiſe ſettled in the ſame trade, and at the ſame place, and in conſequence of his ſucceſs, was enabled to purchaſe, with additions, the eſtate of his elder brother,

brother, greatly encumbered by the debts of thoſe who had poſſeſſed it for ſome generations preceding. The mother of JOSEPH BLACK, and the mother of JAMES RUSSEL, late Profeſſor of Natural Philoſophy in the Univerſity of Edinburgh, were ſiſters; and the mother of ADAM FERGUSON, Profeſſor of Moral Philoſophy, was their aunt; a circumſtance which was the origin, though not the cement, of a friendſhip ſubſiſting between them through life.

WHILE Mr BLACK, the father, lived at Bourdeaux, the great MONTESQUIEU, being Preſident of the Parliament or Court of Juſtice in that province, honoured Mr BLACK with a friendſhip and intimacy, of which his deſcendants, to this hour, are juſtly proud. They preſerve letters, or ſcraps of correſpondence, that paſſed between the Preſident MONTESQUIEU and their anceſtor, as they would titles of honour deſcending in their race. On a paper wrapped round a bundle of ſuch letters, the following note is found in the handwriting of JOSEPH BLACK. "My father was honoured with Preſident MONTESQUIEU's friendſhip, "on account of his good character and virtues. He had no "ambition to be very rich; but was chearful and contented, "benevolent and liberal-minded. He was induſtrious and prudent in buſineſs, of the ſtricteſt probity and honour, very "temperate and regular in his manner of life. He and my "mother, who was equally domeſtic, educated thirteen of their "children, eight ſons and five daughters, who all grew up to "men and women, and were ſettled in different places. My "mother taught her children to read Engliſh, there being no "ſchool for that purpoſe at Bourdeaux."

So much may ſuffice for an account of JOSEPH BLACK's parentage. No words, added to thoſe uſed by his ſon, to delineate the father's character, can improve it; and nothing more is wanting, to account for the regard with which he was honoured by the Preſident MONTESQUIEU. This illuſtrious perſonage, together

together with a great ſimplicity of heart in himſelf, had a glowing ſenſe of modeſt merit in others, and a partiality alſo for the manners and inſtitutions of the Britiſh nations, which he thought ſingularly happy, and in reſpect of which, he willingly liſtened to any important details. On being made acquainted with BLACK's intention of leaving Bourdeaux, he wrote to him a letter, in which, with other expreſſions of kindneſs, are the following : " I cannot be reconciled to the thoughts of your leaving " Bourdeaux. I loſe the moſt agreeable pleaſure I had, that of " ſeeing you often, and forgetting myſelf with you *."

A FEW years before Mr BLACK retired from buſineſs, his ſon JOSEPH, in the year 1740, being about twelve years of age, was ſent home in order to have the education of a Britiſh ſubject; and having, for ſome years, gone to ſchool at Belfaſt, or its neighbourhood, was ſent to continue his education at Glaſgow College, where he paſſed through the ordinary courſe of preparation for any of the learned profeſſions to which he might devote himſelf; and upon his father's requiring him to make a choice, he took to medicine, as agreeable to the habit of phyſical ſtudies which he had already acquired.

IN entering on this purſuit, he became connected with the celebrated WILLIAM CULLEN, M. D. then Profeſſor of Medicine at Glaſgow, and was diſtinguiſhed through life by this ingenious man with an uncommon degree of friendſhip and confidence, which he did not fail to acknowledge and to return. In continuing the ſame ſtudies afterwards at Edinburgh, he, as well as ADAM FERGUSON, lived with their relation JAMES RUSSEL, whoſe ſingular correctneſs, and preciſion of thought, in various branches of ſcience, could not fail to be of uſe to all who approached him.

* " JE ne me fais point à l'idée de vous voir quitter Bourdeaux. Je perds le " plaiſir le plus agréable que j'euſſe, qui étoit de vous voir ſouvent, et de m'oublier avec vous."

BEFORE JOSEPH BLACK offered himſelf a candidate for a degree in medicine, he had already made his diſcovery of Fixed Air; that is to ſay, of an elaſtic fluid, which being fixed in calcareous and alkaline ſubſtances, is diſpelled from them in their calcination by fire, or efferveſcence with acids, leaving a reſiduum, which, in the abſence of this air with which it had been combined, becomes cauſtic; a quality which it retains until the air of which it was deprived is again reſtored. Pure chalk, or pure calcareous matter of any form, in this experiment, loſes about two-fifths of its weight, or, more accurately, forty-one parts in the hundred. Here, accordingly, is a ready way of aſſaying limeſtone or marl, to aſcertain its purity or its value, to the huſbandman in particular, who would employ it to improve his ſoil.

THE air obtained in this experiment has its peculiar qualities. Beſides that of being fixable in ſtone, it is heavier than common air, its ſpecific gravity being nearly double. Flame is extinguiſhed, and animals are ſuffocated in it equally as in water. By this diſcovery, notwithſtanding the ingenious and copious obſervations of Dr HALES and others, on the ſubject of what were called Factitious Airs, it was reſerved for BLACK to give a ſpur and a new direction to the reſearches of ſcience.

FROM this beginning, great progreſs has been made in diſtinguiſhing varieties of elaſtic fluid, and in aſcertaining the compoſition of the atmoſphere itſelf. Chemiſtry was become a favourite ſtudy in France, as it had long been in Germany, and the report of a new ſpecies of air was every where received with avidity. The experiments of BLACK were repeated, and the inferences confirmed by ingenious men both at home and abroad; though by ſome in Germany the reſult was at firſt conteſted, and a controverſy aroſe, in which it does not appear that BLACK took any part, nor does any doubt now remain of the fact, as ſtated by him, or of the inferences he drew from it *.

AN

* Vid. Opuſcles Phyſiques et Chimiques par M. LAVOISIER.

An elaſtic fluid, indeed, of the ſame qualities, has been ſince obtained from various ſources; from vinous fermentation, from the earth itſelf, laid open in grottos or fiſſures, from mineral ſprings, from the breathing of animals, and from the combuſtion of charcoal. This air is found to have the properties of an acid; and the French chemiſts, being led by their experiments to conſider it as produced by the union of the principle of charcoal with vital air, have laid aſide the appellation of Fixed Air, and have ſubſtituted that of Carbonic Acid Gas in its ſtead.

If the reſearches to which the obſervation of fixed air gave occaſion, had terminated here, the acceſſion to ſcience, though important, had been comparatively ſmall. The compoſition of the atmoſphere, the diſtinction of vital and mephitic air, the effects of reſpiration, and the conjectural theories of combuſtion and animal heat, might have remained unknown.

Here, however, no mean progreſs has been made, and names too numerous to be recited in ſuch minutes as theſe, will, on account of their part in theſe ſtudies, go down, with well-merited luſtre, to future ages. Nations may hereafter contend for the honour of ſuch diſcoveries as were made by Lavoisier, Priestley and others *, not merely of ſingle or inſulated facts, but of a magnificent order exiſting in nature, for the continuance of vegetation and animal life. Even France and England, though

 rich

* To Lavoisier we owe the diſcovery, that atmoſpheric air is not homogeneous, but compoſed of nearly three parts of azot, which is not of any effect in reſpiration, otherwiſe than as it dilutes the remaining fourth part, or the vital air, which, without being ſo diluted, would be too intenſe either for the purpoſe of animal reſpiration or of common fire.

To Priestley we owe the diſcovery, that the well-known corruption or waſte of vital air, in the burning of fuel, or the reſpiration of animals, is repaired in the vegetation of plants.

If in ſcience there might be any choice of truths, I would willingly hope that the decompoſition of water may be found a miſtake.

rich in the treaſures of ſcience, and each having much to boaſt of its own, may, in the future, as in times paſt, be jealous of what is gained by its neighbour; it is pleaſant, however, to obſerve, that the individuals chiefly concerned were untainted with ſuch illiberal paſſions. LAVOISIER, in ſending to BLACK a copy of his experiments on reſpiration, tells him: "It is but juſt you "ſhould be one of the firſt to receive information of the pro-"greſs made in a career which you yourſelf had opened, "and in which (he ſays) all of us here conſider ourſelves as "your diſciples *." To this BLACK replied, with a juſt admiration of what the French chemiſts were doing, and, as might be expected from his modeſty, without reference to any merit of his own.

NEARLY about the ſame time that BLACK made the diſcovery of fixed air, his mind was already at work on the phenomena of latent heat. Among his papers are found ſome note-books, in which he had inſerted obſervations and queries, medical, chemical, and miſcellaneous; accompanied with particular references or alluſions to times and circumſtances, which ſerve to fix dates within certain limits at leaſt. In one book, the notes appear to have been written while he was a ſtudent at Edinburgh, or candidate for his degree, in the year 1756; at which time he made or publiſhed his diſcovery of fixed air. In this book are contained conjectures and queries relating to the cold produced in the liquefaction of ſalt and ſnow, and in general in the ſolution of ſalts in water. Seeming to have the doctrine of latent heat in his view, "Is it not," he ſays, in one place, "owing to this "that all bodies, in becoming fluid, have occaſion for more "heat than in their ſolid ſtate?" In another place he ſays, "Does

* "IL eſt bien juſte, Monſieur, que vous ſoyez un des premiers informés des "progrès qui ſe font dans une cariere que vous avez ouverte, et dans laquelle nous "nous regardons tous comme vos diſciples."

"Does not the heat produced in the flacking of quicklime, arife "from the fudden fixing a part of this fluid (water) into a folid "ftate? Is not this the cafe in the fetting of gypfum?" In the fame way he accounts for the intenfe cold produced by ice and nitrous acid, applies the principle again to all faline folutions, and afcribes the flow granulation of falt to the re-appearance of heat. "Is not ice," he fays, "cryftallized water? and does it not always feel cold, becaufe it melts on our handling it? This is "fimilar to the folution of falt in water." Thefe notes, from circumftances intermixed, appear to have been written as early as 1756. In a following note-book, which, from circumftances alfo, does not appear to be of a later date than the year 1757, there are feveral queries, of the fame nature with thofe above mentioned, particularly with refpect to the curious obfervation of FAHRENHEIT, that water, if not difturbed, will cool below 32 degrees without freezing, but the moment it is difturbed, it raifes the thermometer to 32 degrees, and freezes. "Is not this," he fays, "the heat that is unneceffary to ice?" In the fame note-book conjectures are carried ftill farther, and applied to the production of vapour. A way is mentioned to eftimate the quantity of heat which is employed and difappears in the formation of vapour, and cannot afterwards be difcovered by the thermometer. "Place a phial," he fays, "with water, clofe-"corked in a ftove; open it fuddenly, and fee how much of it "is converted into vapour, while the water comes down to the "boiling point." Obfervations to the fame effect are continued through fix fmall note-books; in different places of which, it is obferved, that animal heat originates in the lungs, and that heat always accompanies the production of fixed air.

FROM thefe notes, it appears, that the experiments in which BLACK evinced his doctrine of latent heat to the fatisfaction of his pupils, were not made at random, or without previous apprehenfion of the fact, as it appeared in the ordinary courfe of nature

ture

ture or chemical phenomena. " Heat," he obferved, " is in na-
" ture the principle of fluidity and evaporation, though, in pro-
" ducing thefe effects, it is latent in refpect to the thermometer,
" or any fenfation of ours ; and as matter, otherwife quiefcent,
" becomes voluble and volatile in liquid and in vapour, heat may
" be confidered in nature as the great principle of chemical
" movement and of life. If it pafs through vacuity as well as
" through body, as it certainly does in its communication from
" the fun to the planets, we muft confider it not as an accident
" in bodies, but as a feparate and fpecific exiftence, not lefs fo
" than light or electric matter * ; and though agreeing with
" thefe in fome of its effects, in its nature poffibly different from
" either." But fuch was BLACK's caution not to outrun the courfe of actual evidence, that he declined any difcuffion of the queftion, relating to the abfolute nature of this magnificent power in the fyftem of nature.

MY reading in chemiftry does not enable me to fay, how far the doctrine of latent heat is, in thefe precife terms at leaft, admitted as a principle in the received theories of combuftion and animal heat ; but, to my limited apprehenfion, it appears to be the only folid foundation of any theory that proceeds upon the fuppofed decompofition of *ignifying* or vital air, manifefting a light and heat previoufly latent in fuch air. We can have no direct proof of latent heat in the atmofphere, or permanently elaftic fluids, and it is from analogy only that we affume it to exift in fuch fluids. The maxim of NEWTON, indeed, may be applied here, that what is uniformly obferved in any department of nature, as far as our experience reaches, may be fafely deemed general within fuch department ; and the heat which we find dif-
appear

* IT is no doubt a mighty increment in fcience, to have found fuch powerful fubftances operating, as the writer of thefe minutes apprehends, without gravitation, inertia, or impenetrability, the great bafes and columns of the mechanical philofophy.

appear in liquefaction and evaporation, we may safely assume as existing also, though in a latent state, in every fluid of a similar form. Even Black admitted, that in the atmosphere, under all the manifest changes of its temperature, there may be, and probably is, a very great measure of latent heat.

Such were the scientific discoveries of Black, when residing as a student at Glasgow or at Edinburgh, at least before his nomination to a professorship at either place. When his friend Dr Cullen was removed from Glasgow to Edinburgh, Black was appointed to succeed as Professor of Medicine at the former place; and following his example as lecturer on Chemistry also, was, from this time forward, more employed in detailing particulars already known, for the information of his pupils, than in pursuing any series of investigation for himself.

In entering upon this task, it became matter of course to define the science, and even in this our Professor gave a specimen of his character, and the modesty of his pretensions, in a matter formerly enveloped in mystery, and the affectation of magical power.

The science of nature, indeed, is interesting in all its branches, whether they rise into the heavens, or sink into the bowels of the earth. We are placed in a busy scene, and have our safety and accommodations at stake, in the midst of operations and changes that greatly affect them. The principles that operate in our system are the objects of our science, and when known, become the instruments of our art, towards procuring what it has pleased Providence to make the conditions of our preservation and well-being. In respect to these principles, science becomes an accession of skill to every manufacturer, and to every labourer of the soil, as it is to the contemplative an opening into the interior springs, which are employed in the production of so much beauty and order in the system of nature around us; and this is the rank of importance, which, without the pretensions of

alchemy,

alchemy, or the myfteries of fupernatural power, the chemift might affume to his ftudies. But, in thefe, BLACK ftill faw no more than refearch and inquiry, far fhort of complete fcience or comprehenfive fyftem, and he characterized the ftudy on which he was to enter, by the ufe of its principal inftruments, Mixture and Heat. If difcoveries have fince raifed it to a higher rank among the branches of natural fcience, he himfelf has contributed his fhare in the progrefs it has made. He had publifhed his difcovery of fixed air, in a pamphlet, about the time that he obtained his degree in medicine; but that of latent heat was never otherwife publifhed than as a part of his academical courfe. His pupils, indeed, being numerous, and from different parts of the new, as well as the old world, made his doctrine fufficiently known *.

AFTER a few years employed by CULLEN in the firft profeffion to which he was appointed at Edinburgh, he was removed to a different branch of the medical fchool; and BLACK, as before, being called to fucceed him, was, on the 17th of April 1766, admitted Profeffor of Chemiftry at Edinburgh. His talent for communication being noway inferior to that which he poffeffed for obfervation and inference, his manner of acquitting himfelf in his new fituation gave additional luftre to the fchools of fcience, and rendered him one of the principal ornaments of the Univerfity, in which he continued, for about thirty years, to teach the different branches of chemiftry, with a reputation always increafing. His ftyle was characterized by the moft elegant fimplicity. His addrefs in performing experiments was remarkable, and

* THE writer of this article has little more than heard of chemiftry as a branch of general fcience, and fondly embraced any doctrine as it feemed to connect with the fyftem of nature; but farther, his own ftudies have been fo different, that he would not, if he could, charge his mind with any of its practical details; and he pleads the reader's indulgence, if, under this defect in treating of BLACK, he haftens to fubjoin a defcription of the man to fo imperfect an account of his fcience.

and in the impreſſion he made, ſubjects perplexed or intricate became perſpicuous and clear of ſuperfluous or queſtionable matter. To the laſt, and under ſymptoms of declining health, his mind gave proofs of ſtrength undiminiſhed. In ſpeaking, his voice, though low, had an articulation which made him be diſtinctly heard through every corner of a ſpacious hall, crowded with ſome hundreds of his pupils: and the ſimplicity of his expreſſion, if not eloquence, had, to thoſe who liſtened for information, ſomething more engaging and powerful than any ornament of ſpeech could produce.

AVERSE to hypotheſis or vain conjecture, his ſcience was a juſt comprehenſion of facts, and might be adopted by any artiſt who conſulted him, with as much ſafety as he relied on any practice he himſelf had experienced. His time and attention being devoted to the communications which his pupils had a right to expect from him, very much limited his practice as a phyſician. But where he was called or could attend, his manner was ſingularly acceptable. Without flattery, or uncommon pretenſions to ſkill, he won the confidence of his patients, and, with unaffected concern for their benefit, was often ſucceſsful in mitigating their ſufferings, if not in removing their complaints. He was, in ſhort, a phyſician of great repute, in a place where the character of a phyſician implies no common degree of liberality, propriety, and dignity of manners, as well as learning and ſkill. Never being anxious to bring himſelf forward into public view, little may remain with poſterity to diſtinguiſh him as an author, unleſs his executors ſhould think proper to publiſh the notes from which he uſed to prelect, and which, notwithſtanding the ſubſequent progreſs of ſcience, may ſtill be of uſe to the ſtudent, as a moſt ſolid foundation on which to proceed in his ſtudies.

AMONG the few things publiſhed by himſelf in his own time, we may reckon his Theſis or inaugural eſſay, *De Acido a Cibis*

 orto,

orto, et de Magneſia Alba:—Experiments on Magneſia, Quicklime, and other Alkaline Subſtances, printed between the years 1754 and 1756:—*Obſervations on the Effect boiling has upon Water, in making it freeze more readily*, inſerted in the ſixty-fifth volume of the Tranſactions of the Royal Society of London, for the year 1774: —*Analyſis of the Waters of ſome Hot Springs in Iceland*, made at the requeſt of his friend Mr STANLEY, who brought the ſpecimen for his inſpection; inſerted in the third volume of the Tranſactions of the Royal Society of Edinburgh:—*Two Letters*, one publiſhed in the year 1784, by Profeſſor CRELL, in the tenth volume of his Collections; the other addreſſed to Monſieur LAVOISIER, and publiſhed in the *Annales de Chimie*.

WITH theſe notes of our Author's profeſſional diſtinction, readers will not expect any tale of adventures or remarkable events, which the lives of ſtudious perſons ſeldom afford; and we muſt conclude with a few lines of character, ſuch as thoſe who are placed in like circumſtances may underſtand as ſpoken of themſelves, and for their advantage.

HIS aſpect was comely, his manner unaffected and plain, and as he never had any thing about him for oſtentation, he was at all times preciſely what the occaſion required, and nothing more. Much as he was engaged in the details of his public ſtation, and chemical exhibitions, his chamber never was lumbered with books, ſpecimens of natural hiſtory, or the apparatus of experiments; nor did any one ever ſee him hurried at one time to recover any matter which had been improperly miſſed at a former. Every thing being done in its proper time and place, he ever ſeemed to have leiſure in ſtore, and he was ready to receive his friend or acquaintance, and take his part with complacence, in any converſation that occurred. No one ever, with more eaſe to himſelf, refrained from profeſſional diſcuſſions of any ſort, or, in mixed company, more willingly left the ſubject of converſation

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to be chofen by others. Many years member of a fociety * compofed of men of the higheft rank, of Judges, lawyers and military men, as well as citizens and profeffed men of letters; he kept his place with propriety, and in this group never betrayed any awkward peculiarity of his education or calling. His more intimate affociates and friends, befides thofe of his own profeffion Cullen and Monro, were James Watt, now of Birmingham, an engineer of the firft order, whether ancient or modern, and who has happily united, in the grounds of his art, chemical as well as mechanical fcience; Mr Geddes of the Glafs-works at Leith, an eminent manufacturer, who well knew the value of his friend's fuggeftions. Such intimates as thefe could fcarcely allow Black to relax from the toils of his profeffional ftudies. But he had others, for whom his profeffion had no charm, and whofe attachment to him, as to one another, arofe from the experience of ingenuity and candour, rather than the identity of ftudies, or agreement of opinions. Such were David Hume, Efq; Dr Adam Smith, John Home, Alexander Carlyle; and John Clerk of Elden, of whom the laft, without having ever been at fea, inftructed the navy itfelf in naval tactics. At the head of either lift, however, in refpect to Black's habits of intimacy, ought, perhaps, to have been placed James Hutton, who made up in phyfical fpeculation all that was wanting in any of the others. It may be difficult to fay, whether the characters of Black and Hutton, fo often mentioned together, were moft to be remarked for refemblance or contraft. Both profound in

P 2 phyfical

* This fociety formed itfelf, about the year 1770, upon a principle of zeal for the Militia, and a conviction that there could be no lafting fecurity for the freedom and independence of thefe iflands, but in the valour and patriotifm of an armed people. It became known, by fome whimfical accident, by the name of the *Poker Club.*

phyſical ſcience, and rigid adherents to fact, in excluſion of hypotheſis or vain conjecture. Both of conſummate humanity and candour. BLACK was ſerious, but not moroſe; HUTTON playful, but not petulant. The one never cracked a joke, the other never uttered a ſarcaſm. BLACK was always on ſolid ground, and of him it might be ſaid, *Nil molitur ineptè.* HUTTON, whether for pleaſantry or ſerious reflection, could be in the air, ſpeculate beyond the laws of nature, and treat the common notion of body, with its magnitude and figure, as a miſtake. In theſe ſpeculations BLACK never took any part, farther than to be diverted with any play of fancy, or refinement of thought, which, he well knew, in the caſe of his friend HUTTON, did not preclude the uſe of correct and ſober reaſon when the ſubject required it, or was within its cogniſance. The reſearches of HUTTON were unremitted, and his reference to the order and arrangement of this, which he called a Living World, was judicious and happy *. Unreal as corporeal ſubjects were in his apprehenſion, he eſtabliſhed a lucrative manufacture, on principles of chemiſtry, and was for many years of his life keenly employed in the practice of agriculture. To this he was led by becoming a proprietor of land on the death of his father, when he haſtened to Norfolk, where he had formerly lived with a farmer, to obſerve the huſbandry of that country. There he purchaſed a plough, hired a ploughman, and brought both on the poſt-chaiſe with him to Berwickſhire. The neighbours were diverted with this aſſortment of company and baggage, and no leſs with the attempt which followed, to plough with a pair of horſes without

* HE, too, had carried his ſtudies ſo far as to obtain a degree in medicine; but an attempt to conſult or fee him would have been met with a laugh, or ſome ludicrous fancy, to turn off the ſubject.

out a driver. This joke, however, has become ferious, and is now the general practice from one end of Scotland to the other *.

In returning to BLACK from this enumeration of his intimate friends, ferving, perhaps, to difclofe his character no lefs than any other circumftance relating to him, we but haften to the conclufion of a life no lefs diftinguifhed by correctnefs and propriety of conduct, than by ingenious reafoning, and fcientific refearch. Fully entitled to the appellation of *Frugi* †,—that feemingly cant expreffion, of which the old Romans were fo fond,

* HAVING faid fo much of HUTTON in this occafional notice, fo far fhort of his merits, it may not be improper to prepare thofe who may confult him as an author, to meet with a difappointment for which his friends could never rightly account. Though uncommonly luminous and pleafant in converfation, he was obfcure, unintelligible, and dry in writing, to an equal degree. His favourite fpecimens of natural hiftory, he ufed to fay, were GOD'S Books, and he treated the books of men comparatively with neglect. This may, in fome meafure, account for his want of ftyle or his indifference to language. In company, he fpoke to be underftood by fuch as were prefent, and when obfcure, was called upon to explain himfelf. But alone, he was not aware that others could be at a lofs for a meaning fo clear to himfelf. From this circumftance, (notwithftanding many volumes written in the laft years of his life, more numerous, perhaps, than all he ever read that were written by others, except the voyages and travels, from which he was perpetually collecting facts to complete his view of the terreftrial fyftem), his very ingenious conceptions, to be received as they ought, muft come from fome other pen than his own.

† ON this fubject we may confult the following paffage of CICERO, *Tufc. Quæft.* lib. iii. c. 300. "Sed quia, nec qui, propter metum, præfidium reliquit, quod eft "Ignaviæ; nec qui, propter avaritiam, clam depofitum non reddidit, quod eft In-"juftitiæ; nec qui, propter temeritatem, malè rem geffit, quod eft Stultitiæ, *Fru-"gi* appellari folet. Eas tres virtutes, Fortitudinem, Juftitiam, Prudentiam, "frugalitas eft complexa; etfi hoc quidem commune eft virtutum: omnes enim "inter fe connexæ et jugatæ funt. Reliqua igitur et quarta virtus, ut fit ipfa "Frugalitas. Ejus enim videtur effe proprium, motus animi appetentis regere et "fedare; femper adverfantem libidini, moderatam in omni re fervare conftan-"tiam, cui contrarium vitium Nequitia dicitur."

fond, that they ufed it to denote the foundation, or the fummary of all the virtues, he carried into his private affairs the fame order and good conduct which he employed in his profeffional duties; and he reaped, through life, the benefit of his attention to this particular, in the eafe of his circumftances, and in the power it gave him, on occafion, to affift a friend, or contribute to the attainment of any public convenience. From thofe, indeed, who can miftake remiffnefs or improvident wafte for generofity, BLACK may have incurred the imputation of penury. But the proofs of this charge, if ever it were brought, in his cafe were not to be diftinguifhed from the effects of found reafon and good fenfe. No one ever ftruck the proper medium more exactly than he did. His expences were regulated, but nowife fordid, or unbecoming his ftation. His houfe was fpacious; and his table, at which he never imroperly declined any company, was elegant and plentiful, rather above than below his condition. His contributions for public purpofes were liberal, and his purfe was always open to affift a friend. Much of his practice as a phyfician arofe from his previous connection with the patient as a friend; and he was as affiduous where he would not accept, or where he could not expect a fee, as in the moft lucrative part of his profeffion.

HIS own conftitution never was robuft; and every cold he caught, or any approach to repletion, affected his breaft fo much as to occafion a fpitting of blood. This he guarded againft, by reftricting himfelf to a moderate or abftemious diet. As his infirmities increafed with age, he met them with a proportional attention and care, regulating his food and exercife by the meafure of his ftrength; and thus preventing the accefs of difeafe from abroad, he enjoyed a health which was feeble, but uninterrupted, and a mind undifturbed in the calm and chearful ufe of his faculties. A life fo prolonged had the advantage of prefent eafe, and the profpect, when the juft period fhould arrive, of a calm diffolution.

tion. This accordingly followed, on the 26th of November 1799, and in the feventy-firft year of his age, without any convulfion, fhock, agitation or ftupor, to announce or retard the approaches of death. Being at table, with his ufual fare, fome bread, a few prunes, and a meafured quantity of milk diluted with water; and having the cup in his hand when the laft ftroke of his pulfe was to be given, he appeared to have fet it down on his knees, which were joined together, and in this action expired, without fpilling a drop,—as if an experiment had been purpofely made, to evince the facility with which he departed. So ended a life which had paffed in the moft correct application of reafon and good fenfe to all the objects of purfuit which Providence had prefcribed in his lot. His effects, when looked into, fhewed how much he had profited by the order and juft arrangement he had ever maintained in his affairs; amounting almoft to double of what any one thought his income or his frugality could have enabled him to acquire. The whole was difpofed of by will, without fpecifying any fum, in a neat and fatisfactory manner. Being divided into ten thoufand fhares, it was parcelled to a numerous lift of relations, in fhares, in numbers, or fractions of fhares, according to the degree in which they were proper objects of his care or folicitude.

This account of Joseph Black, however inadequate to the merit of its fubject, is infcribed to the Prefident and Members of the Royal Society of Edinburgh,

By their faithful Affociate,

And humble fervant,

Hallyards, in Tweeddale,
23*d* *April* 1801.

ADAM FERGUSON.

APPENDIX.

LIST of Members *or* Fellows *of the* Royal Society *of* Edinburgh, *continued from the Fourth Volume.* [Hiſtory of the Society, Appendix.]

The following Members were elected at the General Meeting, Jan. 28. 1799. Members choſen, Jan. 28. 1799.

RESIDENT.

The Reverend *Thomas Macknight*, one of the Miniſters of South Leith. *L.*

William Robertſon, Eſq; one of the Keepers of the Records of Scotland. *L.*

NON-RESIDENT.

The Right Honourable Lord *Webb Seymour*. *P.*

Gavin Hamilton, Eſq; Calcutta. *L.*

Henry Stuart, Eſq; of Alanton. *L.*

June 24. 1799. Members choſen, June 24. 1799.

RESIDENT.

Sir *George Mackenzie* of Coul, Baronet. *P.*

Mr *Robert Jamieſon*, junior, of Leith. *P.*

NON-RESIDENT.

Matthew Baillie, M. D. London. *P.*

John Lind, M. D. Phyſician to the Royal Hoſpital at Haſlar. *P.*

Mr *William Blizard*, F. R. S. London, Surgeon to the London Hoſpital. *P.*

Mr *Thomas Blizard*, Surgeon to the London Hoſpital. *P.*

Jan.

Members chosen, Jan. 27. 1800.

Jan. 27. 1800.

RESIDENT.

William Arbuthnott, Esq; *L.*

Members chosen, June 23. 1800.

June 23. 1800.

RESIDENT.

Mr *Alexander Christison*, one of the Masters of the High-School, Edinburgh. *L.*
Gilbert Innes, Esq; of Stow. *L.*
Walter Scott, Esq; Advocate. *L.*
The Reverend *Andrew Bell*, D. D. *P.*

NON-RESIDENT.

James Fellows, M. B. Physician to the Army. *P.*

Members chosen, Jan. 26. 1801.

Jan. 26. 1801.

RESIDENT.

Alexander John Alexander, Esq. *L.*

NON-RESIDENT.

Benjamin, Count *Rumford*. *P.*

Members chosen, Jan. 25. 1802.

Jan. 25. 1802.

RESIDENT.

William Porter, Esq; of the Russian Factory. *L.*
Lieutenant-Colonel *David Robertson*, Deputy Adjutant-General, Ceylon. *P.*

NON-RESIDENT.

Francis Gregor, Esq; M. P. of Cornwall. *P.*
Sir *William Ouseley*. *L.*

Jan.

Jan. 31. 1803.

Members chofen, Jan. 31. 1803.

RESIDENT.

The Right Honourable Lord *Minto*. *L.*
Reverend Dr *Jamieſon*, Edinburgh. *L.*
Thomas Telford, Eſq; Civil Engineer. *P.*

June 27. 1803.

Members chofen, June 27. 1803.

RESIDENT.

Mr *James Bryce*, Surgeon, Edinburgh. *P.*
Reverend Dr *Brown*, Profeſſor of Rhetoric, Edinburgh. *L.*

Jan. 30. 1804.

Members chofen, Jan. 30. 1804.

NON-RESIDENT.

Mr *William Wallace*, Profeſſor of Mathematics in the Royal Military College, Marlow. *P.*
Dr *James Robertſon*, Phyſician in Barbadoes. *P.*
The Reverend *Jonathan Boucher*, A. M. F. A. S. London, Vicar of Epſom, in Surry. *L.*

June 25. 1804.

Members chofen, June 25. 1804.

RESIDENT.

Lieutenant-General *Richard Vyſe*. *P.*
William Forbes, Eſq; *P.*
Alexander Irving, Eſq; Advocate, Profeſſor of Civil Law, Edinburgh. *P.*

NON-RESIDENT.

John Rutherford, M. D. *P.*

Lift of Donations.

DONATIONS made to the ROYAL SOCIETY OF EDINBURGH, *fince* 1797.

From Dr *Jofeph Black*, Profeffor of Chemiftry in the Univerfity of Edinburgh, F. R. S. Edin.,

The Collection of Foffils of the late Dr *James Hutton*, F. R. S. Edin. *May* 1798. See Hiftory of the Society, p. 39.

From Mr *J. Rennie*, Civil Engineer, F. R. S. Edin.

Reports of the late Mr *John Smeaton*, F. R. S. made in the courfe of his employment as an engineer. 1797.

From the Academy of Geneva.

Voyages dans les Alpes, par H. Benedict de Sauffure, tom. iii. et iv. 1796.

Effai fur l'Hygrometrie, par le même. 1783.

Defenfe de l'Hygrometre à Cheveu, par le même. 1788.

Effais Philofophiques de feu Adam Smith, *&c. Traduits par* M. Prevoft, *Prof. de Philofophie à Geneve, de l'Acad. de Berl. et de Societ. Royale d'Edimbourg.* 1797.

From Mr *Baron Maferes.*

Scriptores Logarithmici. 4 vol. 4to. 1801.

From

From Sir *W. Ouseley.*

Persian Lyrics from the *Diwan-i-Hafiz.* By *J. Haddon Hindley.* 1800.

From the Author.

Description and Use of the Sliding Rule, by *Andrew Mackay,* LL. D. F. R. S. Edin. 8vo. 1799.

From the Author.

Essay on the Comparative Advantages of Vertical and Horizontal Wind Mills, by *R. Beatson,* Esq; F. R. S. Edin. 8vo. 1798.

From the Author.

Des Signes et de l'Art de penser, considerées dans leur Rapports Mutuels. J. M. Degerando. 4 vols. 8vo. 1800.

From the Right Honourable Sir *J. Banks.*

Catalogus Bibliothecæ Historico-Naturalis Josephi Banks. *Auctore* Jona Dryander, *Reg. Soc. Bibliothecario,* tom v. 1800.

From the Editor.

Lectures on Natural and Experimental Philosophy, by the late *George Adams,* Mathematical Instrument-maker to his Majesty. 2d Edition, by Mr *Jones,* Mathematical Instrument-maker. 5 vols. 8vo. 1797.

From the Linnean Society, London.

Transactions of the Linnean Society, vol. i, ii, &c. vol. v. 1800. Also Charter of the Linnean Society, 4to.

From

From the Asiatic Society.

Asiatic Researches, or Transactions of the Society instituted in Bengal for inquiring into the History, Arts, Sciences, Literature, &c. of Asia. 7 vols. 4to. 1801.

From the Author.

Illustrations of the Huttonian Theory. 8vo. 1802.

From Dr Blane, London.

Inquiry into the Causes and Remedies of the late and present Scarcity of Provisions, in a Letter to the Right Honourable Earl Spencer. 8vo. 1800.

From the Author.

Dissertatio Inauguralis de Necrosi ac Callo. Auctore Alex. Hermanno Macdonald, *Batavo.* 8vo. 1799.

From the Author.

The Morbid Anatomy of some of the most important parts of the Human Body, by *Matthew Baillie*, M. D. F. R. S. London, 2d edition. 8vo. 1797.

From the Author.

Series of Engravings, intended to illustrate the Morbid Anatomy of the Human Body, 10 Fasciculi, large 4to, from 1799 to 1802. By *Matthew Baillie.*

From the Royal Society of London.

The Transactions of the Society, from 1797 to 1803 inclusive.

From

From the Author.

Essays, Literary, Political, and Economical, in 2 vols. by *J. Gardiner*, M.D. Fellow of the Royal College of Physicians, and of the Royal Society of Edinburgh. 2 vols. 8vo. 1803.

From the Council of the Interior of the Batavian Republic.

Flora Batava, 8 *Livraisons*, *J. C. Sepp et Fills*. 1803.

From the American Society.

Transactions of the American Philosophical Society at Philadelphia. 4 vols. 4to. 1799.

From the Imperial Academy of Petersburgh.

Nova Acta Acad. Scientiarum Imperialis Petropolitanæ, tom. ix, x, xi, xii, xiii. 1802.

From Baron *Maseres*.

Tracts on the Resolution of adfected Equations by various methods of Approximation. Published by *Francis Maseres*, Esq; F. R. S. London Baron of Exchequer. 8vo. 1800.

From the Translator, *P. B coumin*, F. R. S. Edin.

De l'Etat Politique de l'Europe avant et après la Revolution. Traduit de l'Allemand de F. GENZ. 1803.

From the Author.

Memoir on a Subterraneous Graphometer, invented by *John Komarzewski*, formerly Lieutenant-General in the service of the King and Republic of Poland. 1803.

From

From Major *Rennel.*

African Researches, or Proceedings of the Association for promoting the Discovery of the Interior Parts of Africa, vols. i. & ii. 1802.

From the National Institute.

Mémoires de l'Académie des Sciences, 1789 & 1790.

END OE THE FIFTH VOLUME.