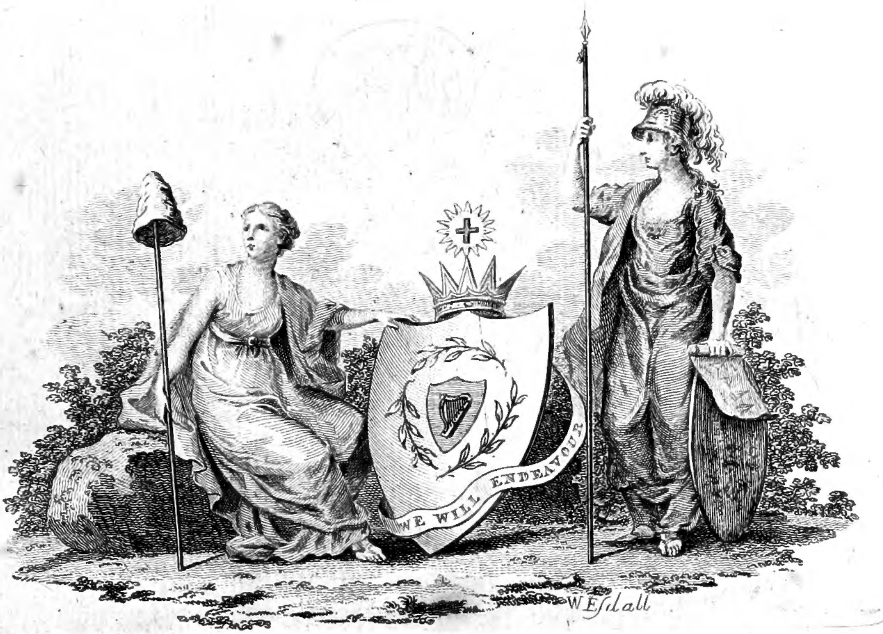
THE

TRANSACTIONS

OF THE

ROYAL IRISH ACADEMY.

VOL. V.



DUBLIN:

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FOR THE ACADEMY.

THE ACADEMY deſire it to be underſtood that, as a body, they are not anſwerable for any opinion, repreſentation of facts, or train of reaſoning, which may appear in the following papers. The authors of the ſeveral eſſays are alone reſponſible for their contents.

E R R A T A.

SCIENCE.

Page 4, Line 20, for *empyric*, read empiric.
Page 11, Line 8, after 279 insert feet.
Page 47, In Table, February, Rain, for 2.8240 read 1.576.
March, Rain, for 2.3644 read 1.655.
Total, Rain, for 30.700 read 28.793.
Page 131, Line 7, }
Page 154, Line 11, } for *Hassenfraz*, read Hassenfratz.
Page 155, Line 7, }
Page 131, Note, for *chymigues*, read chymiques.
Page 134, Line 18, for *magnesia—it*, read magnesia. It, &c.
Page 136, Line 12, for *akin. To*, read akin to, &c.
Page 204, Line 7, }
Page 208, Line 12, } for *besides*, read beside.
Page 217, Line 12, for *hid*, read hidden.
Page 217, Line 17, for *Elanchi*, read *Elenchi*.
Page 323, Line 14, for *ærial*, read aërial.

POLITE LITERATURE.

Page 31, Line 23, for *pathethic*, read pathetic.
Page 64, Line 3, for *charcter*, read character.

ANTIQUITIES.

Page 5, for πρεσβυτερους, read πρεσβυτεροις.
Page 21, Line 1, for 237, read 302.
Page 30, Line 9, for *omnipotent*, read omnipotence.
Page 35, Line 21, for *graduation*, read gradation.
Page 53, Line 13, for *who*, read which.

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S C I E N C E.

SCIENCE.

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A COMPARATIVE VIEW *of* METEREOLOGICAL OBSERVATIONS *made in* IRELAND *ſince the Year* 1788; *with ſome Hints towards forming Prognoſtics of the Weather.* *By* RICHARD KIRWAN, *Eſq; F. R. S. and M. R I. A.*

IN the firſt and rudeſt ſtage of ſociety the purſuits of mankind were neceſſarily confined to the means of ſupplying their primary wants. Where, by the progreſs of agriculture, a fixed mode of procuring this ſupply had been introduced, civilization gradually took place, and thence forward the general attention has been directed to procuring the comforts of human life; from this attention the various arts ſubſervient to their production have ariſen. Read Feb. 2, 1793.

THE arts themſelves being nothing elſe than an application of the laws of nature to *particular* caſes, it was ſoon found that a knowledge of theſe laws, independent of and abſtracted from any particular uſe, would render their application in various *other* cafes much more certain and eafy than it could be under the guidance of blind and fortuitous experiment. Hence a connexion of fome kind or other, whether real or imaginary, betwixt known facts, began to be traced, and thence the fciences originated. With refpect to *Metereology*, the connexion of the different feafons of the year with each other, and with the general ftate of vegetables and animals, prefenting little or no variation, was difcovered from the earlieft times; but the numerous modifications of each feafon, whether of heat or cold, moifture or drynefs, though known to have fome connexion with the preceding weather, yet not being connected with that fingly, but with the recent and actual ftate of the atmofphere in the moft diftant countries, the order in which they prefent themfelves and fucceed each other has hitherto in a great meafure eluded all refearch.

The defire, however, of gaining the flighteft view even of the fhorteft period of this fucceffion has been evermore fo urgent, that fome mode or other of divining it has always been adopted *. Thefe modes are either empyric, fcientific or mixed. Of the empyric methods fome are general, but vague, and uncertain for the moft part; others topical and more certain, but varying with the place of obfervation. The fcientific method, as yet in its infancy, is grounded on a long feries of obfervations accurately taken, of all the changes in the atmofphere,

* See Pliny's Nat. Hift. Lib. 18. Cap. 35.

atmofphere, from whence fome general laws may at length be deduced.

THIS method has been attempted by moft of the learned focieties in Europe, though hitherto, for want of a more general and permanent eftablifhment, with inconfiderable fuccefs.

THE Royal Irifh Academy has not been wanting in its duty to the public and to fcience in this refpect; it has already provided at its own coft, and difperfed through the kingdom, fome of the moft ufeful and beft conftructed inftruments; yet its wifhes have not hitherto been completely anfwered, few obfervations having been communicated. Of thefe, and of my own made in Dublin, I fhall now give a fummary view.

1789.

DUBLIN lies in latitude 53° 21′. Long. 6° 5′ weft of London.

THE winter months which preceded the commencement of this year were remarkably dry, no rain having fallen between the 3d and 18th of November. The barometer all the time ftood above 30 inches; yet the weather could not be called cold until the 12th, when it grew clearer, and the thermometer at night was generally under 36°, the wind being moftly at E. The 18th it grew milder but ftill dry, there being fcarce any rain until December the 13th, the wind being E. S. E. On the night of the 13th it blew a ftorm at E. S. E. with fnow;

after

after which the cold increafed. On the night of the 15th the thermometer ftood at 24°, and on that of the 17th at 22°; but on the 23d the wind came about to W. and the weather grew milder until the 26th, when it again blew a ftorm at night, I believe from the E. the thermometer at night being 33°. The 27th and 28th it froze by day and thawed at night, and thus continued until January the 9th, when at night there was a heavy fall of fnow with hail, thermometer 24°. The 11th much fnow, barometer 29.66, thermometer 26°, and at night 15°, and fo it was on the nights of the 12th and 13th. This *maximum* of cold was indicated by Six's thermometer. On the 13th there was a heavy fall of fnow, and the barometer fell to 28.7, wind S. thermometer 35°. On the 15th a thaw, at night a ftorm at *Weft*, barometer next day 29.7. And on the night of the 19th an aurora borealis, which was fucceeded on the night of the 21ft by a ftorm at S. or S. W.

HENCEFORWARD the feafon was mild, but fomewhat windy in fome *wefterly point*, until *March the 5th*; when it began to blow from the *Eaft* accompanied with fnow, and continued fo with fome inclination to the *North* or *South* until the 14th.

ON the night of the 14th there were frequent fqualls at S. E. after which the wind held a *foutherly* direction or *South Weft* until the 22d, and then veered to the S. E. with rain.

ON the night of the 27th a great and beautiful aurora with the vertex in the zenith; fucceeded on the 29th by a high wind at N. Thermometer at twelve o'clock 41°.

HENCE-

HENCEFORWARD the weather was moſtly dry until *April the 8th*, but afterwards wet and cold to May the 7th. On the 23d of April there was a ſtorm at W.

MAY 7th, fine. Thermometer the whole day at 64°, but after that the weather was various but moſtly wet, and at the end exceeding wet, until June the 10th; the wind being moſtly W. or S. W. and in June W. and N.-W. On the 6th of June a ſtorm at night.

FROM June the 10th unto the 16th, fine and hot. Thermometer in the ſhade being 73°. Wind E. and S. E. On the night of the 16th rain, wind S. W.; and this wind predominated with uncommonly wet weather until July the 19th.

FROM July the 1ſt to the 13th it rained every day in ſhowers reſembling thunder ſhowers; even ſome diſtant thunder was heard. Yet the barometer held at 29.5 or 29.6 all along until July the 13th, when it roſe to 29.89.

JULY 19th and 20th moſtly dry, but the 22d it rained all day; on the 23d a heavy ſhower as if there was thunder, though none was heard. From this to Auguſt the 3d it rained every ſecond day; the barometer about 30, thermometer from 64° to 70°.

AUGUST the 3d my obſervations in Dublin were interrupted by a journey to the counties of Cavan, Leitrim, Sligo, Mayo and

and Galway. On the 7th, about fixty-four miles N. W. of Dublin, I met a violent ftorm with rain and fome hail. From the 8th to the 12th there was fcarce any rain, but variable weather thence to the 16th, and dry from that to the 21ft. On the 16th an aurora borealis. After the 21ft every fecond day was wet until *September the 23d*, from which day until the 27th it rained only at night; wind conftantly W. or S. W. On the 26th an aurora borealis, and a ftorm accompanied with thunder, which lafted alfo the next day.

October the 4th, I refumed my obfervations in Dublin. The weather was generally wet until the 22d, when the wind changed to the Eaft for a few days, after which it again turned rainy until *November the 16th*. On the 14th a remarkable aurora borealis, after which fucceeded a calm, each day alternately wet and dry until the 26th, when the wind ftood at *North*, accompanied with a froft which lafted until the 30th. On the 27th at night the thermometer was at 27°, wind high at *North Weft*.

In December the weather was moftly fine until the 17th, when there was a violent ftorm at *Weft*, fucceeded by a few days froft. The 21ft another ftorm at *Weft*, as alfo on the 27th and 30th, but the weather mild.

At BELFAST.

Belfast lies in latitude 54° 38'.

In

In *January* the weather agreed nearly with that in Dublin; here alſo the ſnow began on the night of the 9th. Barometer 29.755. On the 12th the thermometer was at 10°.5 at ten o'clock in the morning, and probably much lower at night. On the 15th at night there was a ſtorm at W. or S. W. The barometer, which was on that day at 29.15, was the next morning at 29.8, ſomewhat higher than in Dublin.

In *February* the weather was ſeverer than in Dublin.

In *March* the eaſterly wind did not begin here until the 6th, and grew ſqually on the 13th and 14th, as in Dublin.

In the beginning of *April* the weather was not ſo dry as in Dublin; the remainder of the month moſtly wet.

May more rainy than in Dublin. On the 7th it rained in the morning, and the thermometer was at 54° at ten o'clock in the morning.

In *June* alſo the weather was colder; from the 10th to the 15th moſtly fine, but the thermometer roſe no higher than 68°.

Observations on the remaining months have not as yet been received.

ATHLONE.

This town lies fifty miles due Weſt of Dublin, and nearly in the center of the kingdom.

The obſervations taken here begin in May 1789, and the rainy days are for the moſt part omitted.

On the 7th of May the height of the thermometer here was 56°, in Dublin 64°.

The 12th thunder was heard here; none in Dublin or Belfaſt, but much rain.

June 1ſt, thunder both here and at Belfaſt; none noticed in Dublin.

June 4th, a violent ſtorm both here and at Belfaſt; not violent in Dublin.

The mean height of the barometer at Athlone during the month of May was 29.645, and that of the thermometer obſerved at eight o'clock in the morning and two in the afternoon 53°.7. At Belfaſt the mean height of the barometer was 29.951, and that of the thermometer obſerved only at ten o'clock in the morning 55°.

The mean height of the barometer at Athlone in June was 29.765, and of the thermometer 57.19.

At Belfaſt that of the barometer was 29.965, and of the thermometer 59.12.

The mean of the morning obſervations at Athlone in the month of May was as to the barometer 29.630, and as to the thermometer 48°.9, and at ten o'clock we may ſuppoſe it 50°.

By calculating from theſe data Athlone lies 279 higher than Belfaſt.

But in the month of June the mean of the barometrical obſervations in the mornings gives 29.72 inches, and the mean of the thermometrical obſervations at eight in the morning 52.6, which at ten o'clock we may ſuppoſe 53.6.

Calculating from hence we have the height of Athlone over Belfaſt = 210 feet; perhaps the truth may be between both, and then the height of Athlone over the ſea ſhould be 244 feet. But obſervations of this ſort are generally taken too careleſsly to impreſs much confidence.

At GALWAY.

This antient, once opulent and flouriſhing town lies about 12′ ſouth of Dublin on the weſtern coaſt of the kingdom. Its capacious bay, which opens to the weſt, occaſions a conſiderable indraught, while the Atlantic vapours are condenſed into rain on the neighbouring mountains.

THE weather was nearly the ſame here as in Dublin in the beginning of January, but there was no fall of ſnow until the 13th. Barometer 28.75. 16th barometer 29.62. Wind N. W.

March the 5th, wind Eaſt as in Dublin, but no ſnow.

12th and 15th, ſtorms at N. W. Squalls at S. W. on the 20th, and on the 29th a ſtorm at N. W. Thermometer at twelve o'clock 40.7. After which no rain until *April the 10th*, wind being either N. W. or S. E. and from the 10th to the 17th ſcarce any rain; but on the night of the 17th it rained heavily. On the night of the 22d a ſtorm at S.

May the 7th, gloomy. Thermometer 50°.4; the remainder of the month moſtly wet, yet not ſo wet as in Dublin.

June the 6th, a ſtorm at W. S. W. From the 10th to the 16th fair, but on the noon of the 16th heavy rain and thunder. Thermometer 60°.4. 22d, thunder with rain and hail. Wind W. Afterwards wet and windy until *July the 17th*. Barometer moſtly at 29.6 and 29.7. Wind ſhifting from S. E. to N. W. alternately. 19th and 20th, fair. 21ſt, diſtant thunder, but fair. 22d, fair. 24th, wet. The ſucceeding days fair, but on the 31ſt it rained.

Auguſt

August the 3d, ſlight rain, after which it was moſtly fair until the 24th, and then moſtly wet to the end. And here the obſervations cloſe for 1789.

At LONDON.

I FIND by the metereological obſervations made at the houſe of the Royal Society that the barometer there alſo was 30 inches or above during the greateſt part of November 1788, with ſcarce any rain. Nor had they any rain during the whole month of *December*, nor any ſtorm until the 15th, and the wind with them was N. E. The ſnow did not reach them until the 17th. Their greateſt cold at eight o'clock in the morning was 18°, which happened twice, namely on the 18th and 30th. The ſtorm of the 26th was alſo felt there, and laſted the next day.

IN 1789 the ſnow began in London *January the 6th*, with a ſtorm at E. S. E.; the greateſt was January the 5th, ſix days earlier than with us. The thermometer ſtood at eight o'clock in the morning at 17°.5, and at two o'clock at 24°; whereas with us it roſe on that day to 28°. On the 13th the barometer ſtood at 29.1, and with us at 28.7. On the 15th it roſe to 29.23, but with us to 29.7, which height it did not attain in London until the following day. It grew ſtormy at the ſame time as with us, and from the ſame points. The N. and E. winds and dry weather began in London ſix or ſeven days ſooner than with us, namely *February the 28th*, and continued

to

to *March the 13th*, when some snow fell as with us. The latter end of the month was colder than in Dublin.

The beginning of *April* was attended with more rain than in Dublin, but from the 4th to the 16th it was drier; the remainder of the month was nearly equally wet in both places, but more stormy in London.

In *May* the fair weather began the 5th, and lasted until the 15th, and the whole month much drier than with us, and warmer. On the 17th, 18th and 19th they had storms at S. W.

June the 7th, a storm at N. W. On the 11th the easterly winds began, which lasted with dry weather until the 19th, from which to the end of the month there were heavy and frequent rains. The 21st and 22d were stormy at S. and S. W.

In *July* there were more fair days than with us; the barometer much higher; but on the 13th, the day on which it rose with us to 29.89, it fell in London to 29.54, under an E. S. E. wind, but which was immediately succeeded by a S. W. wind.

August was also drier than with us, and *easterly* winds more frequent.

September was more stormy here than in the *West* of Ireland, and from the 5th to the 11th drier; from the 20th to the 28th

scarce

ſcarce any rain, after which the weather was ſtormy to the 4th of October.

THIS month was moſtly wet until the 21ſt, and from that day to the 31ſt dry.

IN *November* rains were frequent until the 19th. The 26th the wind was E. and there ſeems to have been ſome froſt the ſucceeding nights.

December was moſtly dry until the 26th, though the wind was generally W. S. or S. W. It grew ſtormy on the 15th, and ſo continued to the 18th, and afterwards continued ſo with ſhort intervals to the end. Theſe ſtorms were moſtly from the W. or S. W.

JUNE and October were the moſt rainy months in London; each produced above three cubic inches, and the whole year 21.976. I have ſeen no account of the quantity fallen in Dublin, but I form no doubt but it was much greater.

THE laſt month of 1788 and the firſt of 1789 were remarkably cold, as well on the Continent of Europe as in Great Britain and Ireland; and it has been obſerved that the cold was proportionably greater in the ſouthern than in the northern parts of Europe, which would induce one to think that the eaſterly winds which produced this cold proceeded from Tartary and the ſouthern parts of Siberia between the 55th and 40th degrees of latitude.

latitude. This would appear at times S. E. and again N. E. as it was nearer to either of the limits.

Of the Year 1790, *Dublin.*

January the 1ſt was a fine ſlight froſt, after which mild open weather ſucceeded until the 11th. On the 11th at night a ſtorm at S. W.

19th, froſt; wind N. but on the 21ſt changed to S. W. yet the weather continued fine and fair until the 25th. Thermometer at night 40°, and by day 55°, when there was a violent ſtorm at S. W. after which the weather was moſtly wet until February the 3d.

February the 3d, the barometer now roſe to 30.66, the air remarkably dry, though a ſouth wind prevailed. Thermometer moſtly 55°, barometer 30.55. It did not rain until the 10th, and then but little. On the night of the 11th a ſtorm at S. and another on the 16th and 17th, yet the air ſtill ſo warm that at two o'clock in the afternoon the thermometer was conſtantly at 54° or 55°. On the 18th it grew colder; thermometer at noon 40°. The 20th, wind N. E. cold. At noon only 44°, every day fair and ſunſhine, and even warm; heat at two o'clock 55°, and ſo continued until the 25th. On the 25th at night a ſtorm at S. after which there were a few rainy days; thermometer at a medium 50° until March the 3d.

March the 4th, the easterly wind began, and continued with fine dry but cold weather until the 9th; thermometer at night 33°, and at noon 51°. 9th, high wind at West, and some rain; but scarce any except at night from that day until the 24th. 13th, wind North. 17th, E. S. E. Thermometer in this interval 32° at night, and at noon 52°; barometer 30.6 and 30.8. 23d, cloudy, but dry. 24th, rain; wind S. S. W.; barometer 29.9. Hence until *April the 8th* no rain; wind E. and S. E. On the 2d I left Dublin. On the 8th a storm at N. E. No rain until the 21st, on which day I returned to Dublin.

April the 22d, heavy rain; wind South, but not warm; thermometer at noon 46°. The rains continued with little intermission until *May the 9th*; but from that day to the 15th dry, clear and sunshine; wind E. or N. E.; thermometer at noon 63° or 64°. From hence to the 29th wind shifted frequently; weather cloudy, wet and cold. 19th and 20th stormy, with rain and hail; yet the barometer stood higher than could be expected, mostly between 29.8 and 30.3; thermometer at noon at a medium 54°. From the 29th of May to *June the 9th* mostly dark but dry, with odd showers; thermometer at a medium 58°, barometer 30. On the 9th heavy rain with hail. Hence to the 13th wind N. E.; barometer 30.2, thermometer at noon 66°, at night 50°. 14th, clear, hot; thermometer 70° at noon, at night 55°; barometer 30.3. It continued moderately fine, with little rain, until the 18th, and then it blew a violent gale at N. W. and the 19th at S. W. with rain. 20th and 21st, calm; thermometer 75° at two o'clock in the afternoon.

From the 24th to *July the 15th* it rained every day; thermometer by day no higher than 58°; wind moftly S. or W.; and from that to the 19th not a day paffed without a fhower. July 19th a ftorm at S. From June 26th to July 20th the mean heat was 65°, it being feldom below 60° but at night, and feldom above 70°.

From July 24th to the 28th fine and dry; the 27th I left Dublin.

Auguft the 1ft, heavy rain in Galway. 3d, 4th and 5th, dark and cloudy, but no rain; but from the 6th fcarce a day paffed without rain until the 24th. The 22d at night a ftorm at S. W.; but from the 24th to the 30th moftly dry, but cloudy; after which only flight fhowers until *September the 4th*, which was fine; wind N. E. Thence to the 13th ftormy; high winds and fome rain to the 18th, on the night of which there was a violent ftorm at S. W.; thence forward variable to the 27th; and from that day to October 11th moftly dry, and fo continued (the 11th and 12th excepted) until the 20th, when I refumed my obfervations in Dublin. On the 20th a ftorm at W. or W. S. W.; wet until the 23d, then fair to *November the 3d*, when there was fome rain.

November the 4th, fine; wind N. W. and thus continued, with fome froft, after the 13th (when the wind changed to the E.) to the 18th; barometer in this interval from 30 to 30.6. From the 18th to the 22d moftly wet, but afterwards dry until the end of the month.

December

December the 1st, heavy rain and high wind at S. W. ſucceeded by variable weather, but moſtly dry, until the 10th, on the night of which there was a ſtorm at W. or S. W. The 11th and 12th, dry; wind N. W. 13th, wet; wind S. W. 14th, dry; at night a violent ſtorm at W. 15th, a ſlight ſhower; thermometer 48°, barometer 28.9. The 16th, rain, and at night it blew hard at N. W.; ſome rain and ſnow. 17th, fine, as alſo the following days until the 23d, but ſtorms at S. W.; thermometer moſtly at 46°, barometer 29.3. 23d, wind W.; heavy rain at night. 24th and 25th, calm, no rain by day; thermometer 46°. 27th, fine; froze hard at night; thermometer 28°; and the ſame weather on the 28th. 29th, rain. 30th and 31ſt, fine; froſt at night; thermometer by day not below 42°.

Of the Signs of wet and dry Weather.

If metereological obſervations were taken at proper diſtances all over the globe, and with tolerable accuracy, they probably would in a few years diſcloſe that connexion which all the phænomena of the atmoſphere have with each other, and the particular ſpecies of weather which would take place in any given region might be foreſeen either to a certainty or at leaſt to a high degree of probability, but until this happens the only uſe of metereological tables, as far as regards the art of forming prognoſtics, is to exhibit a view of the ſort of weather that moſt uſually precedes *wet*, *dry*, *hot* or *cold* ſeaſons (theſe being the modifications moſt intereſting as well to agriculture as to medicine) and tracing their recurrency by the laws of probability.

bility. With us, however, theſe four ſpecies of weather may be reduced to two, as winters and ſprings if dry are moſt commonly cold, or warm if moiſt; and on the contrary, dry ſummers and autumns are uſually hot, and moiſt ſummers cold. The uſual mean heat of ſummer in thoſe parts of the kingdom that lie between latitude 52° 30′ and 53° 30′ is 58 degrees, as I believe, and of winter is 44°.

ON peruſing a multitude of obſervations taken in England from 1677 to 1789 at different intervals, I find,

1ſt, That when there has been no ſtorm before or after the vernal equinox, the enſuing ſummer is generally *dry*, at leaſt five times in ſix.

2d, That when a ſtorm happens from any eaſterly point either on the 19th, 20th or 21ſt of March, the ſucceeding ſummer is generally *dry*, four times in five.

3d, That when a ſtorm ariſes on the 25th, 26th or 27th of March, and not before, in any point, the ſucceeding ſummer is generally *dry*, four times in five.

4th, If there be a ſtorm at S. W. or W. S. W. on the 19th, 20th, 21ſt or 22d, the ſucceeding ſummer is generally *wet*, five times in ſix.

AGAIN,

Again, I observe that it generally rains leſs in *March* than in *November* in the proportion at a medium of ſeven to twelve.

It generally rains leſs in *April* than in *October* in the proportion of one to two nearly at a medium; I believe it to be otherwiſe in Ireland.

It generally rains leſs in *May* than in *September;* the chances that it does ſo are at leaſt as four to three; but when it rains plentifully in *May* (as 1.8 inches or more) it generally rains but little in *September*; and when it rains one inch or leſs in May, it rains plentifully in September; this applies not only to England and Ireland, but alſo I believe to all the weſtern parts of Europe.

If we had tables of the quantities of rain that fall in each month for eighty or one hundred years, we might calculate the mean proportion of each, whether taken ſingly or in groups, and thence deduce the probable quantities of rain in the ſucceeding months; the table would every year grow more perfect, and in time approach very near the truth. But I have met no account of the quantities of rain that annually or monthly fall in Ireland, nor any account of the weather except that taken by the induſtrious Doctor Rutty with a view to medicine; his obſervations extend to forty-one years, but his eſtimations are merely vague and popular. However, I ſhall exhibit

exhibit a view of them, and to ſhew how more accurate obſervations might be managed, deduce ſome conſequences from them.

Wet, *dry* and *variable* are denoted by the letters W. D. and V *.

THE *ſpring* begins with him in April, the *ſummer* with June, and the *autumn* with September.

* The preciſe ſignification of theſe words might eaſily be had, by meaſuring the proportion of rain in each ſeaſon; but unfortunately this has not been done. However I ſhall endeavour to throw ſome light on this matter in another paper.

Table

Table of the State of Spring, Summer and Autumn in Dublin from 1725 to 1765 inclusively.

Year.	Spring.	Summer.	Autumn.
1725	D.	W.	V.
1726	D.	V.	V.
1727	V.	V.	W.
1728	D.	W.	V.
1729	D.	D.	W.
1730	V.	W.	V.
1731	D.	D.	V.
1732	V.	D.	W.
1733	D.	D.	W.
1734	V.	W.	V.
1735	V.	WW.	W.
1736	V.	DD.	D.
1737	D.	D.	D.
1738	V.	W.	W.
1739	W.	W.	V.
1740	D.	D.	W.
1741	DD.	D.	V.
1742	D.	D.	V.
1743	V.	D.	V.
1744	D.	V.	W.
1745	D.	W.	V.
1746	D.	W.	V.
1747	D.	D.	D.
1748	V.	D.	D.
1749	W.	V.	D.
1750	D.	W.	V.
1751	W.	W.	V.
1752	D.	WW.	D.
1753	W.	W.	D.
1754	V.	W.	D.
1755	W.	W.	W.
1756	V.	W.	V.
1757	W.	W.	D.
1758	D.	W.	D.
1759	D.	DD.	D.
1760	D.	V.	W.
1761	D.	DD.	V.
1762	D.	D.	W.
1763	V.	W.	V.
1764	D.	W.	V.
1765	V.	DD.	V.

HENCE we fee that in forty-one years there were,

6 wet *Springs*,	22 dry,	and 13	variable.
20 wet *Summers*,	16 dry,	and 5	variable.
11 wet *Autumns*,	11 dry,	and 19	variable.

A *dry Spring* has been followed by	A dry Summer	11 times.
	A wet	8
	A variable	3
A *wet Spring* has been followed by	A dry Summer	0
	A wet	5
	A variable	1
A *variable Spring* has been followed by	A dry Summer	5
	A wet	7
	A variable	1
A *dry Summer* has been followed by	A dry Autumn	5
	A wet	5
	A variable	6
A *wet Summer* has been followed by	A dry Autumn	5
	A wet	3
	A variable	12
A *variable Summer* has been followed by	A dry Autumn	1
	A wet	3
	A variable	1

HENCE

* HENCE in the beginning of any year,

I. The probability of a *dry Spring* is - $\frac{22}{41}$
of a wet - - $\frac{6}{41}$
of a variable - - $\frac{13}{41}$

II. The probability of a *dry Summer* is - $\frac{16}{41}$
of a wet - - $\frac{20}{41}$
of a variable - - $\frac{5}{41}$

III. The probability of a *dry Autumn* is - $\frac{11}{41}$
of a wet - - $\frac{11}{41}$
of a variable - - $\frac{19}{41}$

IV.
After a dry Spring the probability of
- A dry Summer is - $\frac{11}{22}$
- A wet - - $\frac{8}{22}$
- A variable - $\frac{3}{22}$

V.
After a wet Spring the probability of
- A dry Summer is - o
- A wet - - $\frac{5}{6}$
- A variable - $\frac{1}{6}$

VI.
After a variable Spring the probability of
- A dry Summer is - $\frac{5}{13}$
- A wet - - $\frac{7}{13}$
- A variable - $\frac{1}{13}$

* These rules relate chiefly to the climate of Ireland.

VII.

After a dry Summer the probability of	A dry Autumn is	$\frac{5}{16}$
	A wet - -	$\frac{5}{16}$
	A variable -	$\frac{6}{16}$

VIII.

After a wet Summer the probability of	A dry Autumn is	$\frac{5}{20}$
	A wet - -	$\frac{3}{20}$
	A variable -	$\frac{12}{20}$

IX.

After a variable Summer the probability of	A dry Autumn is -	$\frac{1}{5}$
	A wet - -	$\frac{3}{5}$
	A variable -	$\frac{1}{5}$

But the probability of the autumnal weather will be attained much more perfectly by taking in the consideration of the preceding Spring also; in order to which we may observe that,

A *dry Spring and dry Summer* were followed by a	Dry Autumn	3 times.
	Wet -	4
	Variable -	4

A *dry Spring and wet Summer* were followed by a	Dry Autumn	2
	Wet - -	0
	Variable -	6

A *wet Spring and dry Summer* were followed by a	Dry Autumn	0
	Wet - -	0
	Variable -	0

A *wet*

A *wet Spring and wet Summer* were followed by a	Dry Autumn	2 times.
	Wet - -	1
	Variable -	2
A *wet Spring and variable Summer* were followed by a	Dry Autumn	1
	Wet - -	0
	Variable -	0
A *dry Spring and variable Summer* were followed by a	Dry Autumn	0
	Wet - -	2
	Variable -	1
A *variable Spring and dry Summer* were followed by a	Dry Autumn	2
	Wet - -	0
	Variable -	2
A *variable Spring and wet Summer* were followed by a	Dry Autumn	1
	Wet - -	1
	Variable -	5
A *variable Spring and variable Summer* were followed by a	Dry Autumn	0
	Wet - -	1
	Variable -	0

X. Hence after a *dry Spring and dry Summer* the probability of a	Dry Autumn is -	$\frac{3}{11}$
	Wet - -	$\frac{4}{11}$
	Variable - -	$\frac{4}{11}$

XI. After a *dry Spring and wet Summer* the probability of a	Dry Autumn -	$\frac{2}{8}$
	Wet - -	$\frac{0}{41}$
	Variable - -	$\frac{6}{8}$

XII. After a *dry Spring and variable Summer* the probability of a	Dry Autumn -	$\frac{0}{0}$
	Wet - -	$\frac{2}{3}$
	Variable - -	$\frac{1}{3}$

XIII. After a *wet Spring and dry Summer* the probability of a	Dry Autumn -	$\frac{0}{41}$
	Wet - -	$\frac{0}{41}$
	Variable - -	$\frac{0}{41}$

XIV. After a *wet Spring and wet Summer* the probability of a	Dry Autumn -	$\frac{2}{5}$
	Wet - -	$\frac{1}{5}$
	Variable - -	$\frac{2}{5}$

XV. After a *wet Spring and variable Summer* the probability of a	Dry Autumn -	$\frac{1}{41}$
	Wet - -	$\frac{0}{41}$
	Variable - -	$\frac{0}{41}$

XVI. After a *variable Spring and a dry Summer* the probability of a	Dry Autumn -	$\frac{2}{4}$
	Wet - -	$\frac{0}{41}$
	Variable - -	$\frac{2}{4}$

XVII. After

XVII. After *a variable Spring and a wet Summer* the probability of a	Dry Autumn is -	$\frac{1}{7}$
	Wet - -	$\frac{1}{7}$
	Variable - -	$\frac{5}{7}$

XVIII. After *a variable Spring and a variable Summer* the probability of a	Dry Autumn -	$\frac{0}{41}$
	Wet - -	$\frac{1}{41}$
	Variable -	$\frac{0}{40}$

REFLEXIONS *on* METEREOLOGICAL TABLES.

By RICHARD KIRWAN, *Efq; F.R.S. and M.R.I.A.*

IN my former paper on this fubject I endeavoured to fhew one important purpofe to which the obfervations of a number of years already made by Doctor Rutty might be applied, but found myfelf not a little embarraffed by the undefined ufe of the terms *wet* and *dry*, fo frequently applied to the periods obferved. To remove this embarraffment I meafured the quantity of rain, and obferved its duration during fome of the periods that are decidedly deemed *wet*, both here and in England, and have thereby been enabled to fix in fome meafure the fenfe of thefe terms; for I have obferved that calling the *day* that fpace of time during which men are ufually occupied in the open air, viz. from fix o'clock in the morning to fix in the evening, we account a day *wet* if it rains above half that period, that is feven hours, and if the quantity of rain that

Read July 23, 1793.

that falls during that time is nearly one pound troy (or more) on the ſpace of one ſquare foot; which quantity would riſe in a hollow cube of that dimenſion to the height of 0.157639. It is by this laſt method that the quantity of rain is uſually indicated in metereological tables, without any regard however to its duration.

If the quantity of rain that falls in ſeven hours be only about half a pound, it is called only a *mizzle*. If it conſiderably exceeds one pound, and laſts eight, nine or ten hours, the day is called exceeding wet; on ſuch days I have known it to rain five or ſix pounds. On the contrary, if it rains one pound in two hours, particularly in the morning, and all the reſt of the day is fine, we ſhall ſcarce call it wet, but at moſt a *variable* day.

Hence the wetneſs or dryneſs of any other period is generally in a compound ratio of its quantity and duration. A *week* is counted wet when it contains four wet days or more, a *month* when it contains three wet weeks, and a *ſeaſon* or period of three months when it contains two wet months. With reſpect to a *year*, theſe denominations are applied ſomewhat differently; here regard is chiefly had only to the operations of agriculture, which chiefly take place in Spring, Summer and Harveſt ſeaſons. If the two laſt ſeaſons be ſo wet as to impede the huſbandman in any conſiderable degree the year will univerſally be denominated wet, though the quantity of rain be on the whole much ſmaller than in other years.

The quantity of rain falling in one day, fufficient to have that day denominated wet, I have faid to be about one pound troy. It is evident, however, that this cannot be fettled with fuch mathematical precifion that even $\frac{3}{4}$ of that quantity may not be fufficient. And hence a *month* may be denominated *wet* during feventeen or eighteen days, of which only 12 lb. of rain or 1.891668 inches have fallen, which is about $\frac{3}{4}$ of a lb. per day, particularly in the fummer and autumnal feafons; variable if ten, and dry if only eight.

In denominating the Spring feafon *wet* or *dry*, I now proceed to fhew that Doctor Rutty, in ufing the words *wet* and *dry*, has applied them conformably to the above determinations. In order to which nothing more will be neceffary than to prove that he has applied them to the fame modifications of the weather and the fame meafures of each as Mr. Barker of Lyndon, whom I may ftile a cotemporary reporter for a great part of the time, and a more affiduous and meritorious England has not yet produced.

Mr. Barker then in the Philofophical Tranfactions for the year 1770 has given an account of the monthly quantity of rain that fell at Lyndon in Rutland from the year 1737 to the year 1770 inclufively. He obferves that in common fpeaking thofe are called *wet* years in which the Summer, the growing feafon, was wet and cold, and thofe dry ones in which the Summer was dry and burning. He then tells us that the years 1737, 40, 41, 50, 60, 62 and 65 were complained of as dry, and the years 1738, 39, 51, 52, 56, 63 and 66 were wet. Now on inſpecting the table which I have extracted from Doctor Rutty's obſervations, it will be found that the Summers of each of thoſe are marked *dry*, except the year 1750, which was certainly different from that of Lyndon, and 1760, which he denotes variable. As to the *wet* years they both perfectly agree. A ſtandard conformable to their ideas may be deduced from my own obſervations of that frequency and quantity of rain which muſt conſtitute a wet or dry period; its conformity with Mr. Barker's determinations may be ſeen in the following tables:

TABLE THE FIRST.

Of the Quantity of Rain in dry Summers.

	1737.	1741.	1750.	1760.	1762.	1765.	1771.
June - -	.720	1.366	2.069	2.470	.764	.788	1.588
July - -	.306	.873	1.510	.895	1.119	.582	1.043
Auguſt -	6.300	1.633	.640	1.644	3.615	2.805	2.131
	7.326	4.219	4.219	5.009	5.498	4.175	4.762

On

On this table we may remark, firſt, that when the quantity of rain during the Summer months does not amount to 5.044 inches, or 32 lb. troy, which is at the rate of 10,33 lb. per month, or 1,628 inches, the Summer is counted *dry*. To this, however, it may be objected, firſt, that the Summer of 1737 was accounted *dry*, though the quantity of rain amounted to above ſeven inches, but it muſt be obſerved that above ſix of them fell in Auguſt, and the two preceding months were moſt remarkably dry; ſecondly, it may be ſaid that the Summer of 1762 was deemed *dry* though there fell 5.498 inches; but in this caſe alſo June and July were remarkably dry; beſides much of the rain might have fallen in a few days, and not have been ſufficiently diſperſed and divided through each month, the frequency of rain not being noticed in the tables. All this being duly conſidered, it muſt be allowed that theſe reſults are ſtrictly conformable to my determinations. I have omitted the *year* 1740 becauſe that year could not poſſibly be counted *dry* at Lyndon from the ſmall quantity of rain that fell in Summer, it having rained more in each of the Summer months that year than in 1739, whoſe Summer was accounted *wet*; but it evidently acquired that denomination from the exceeding ſcarcity of rain in all other months.

TABLE THE SECOND.

Of the Quantity of Rain in wet Summers.

	1739.	1751.	1752.	1756.	1763.	1766.	1767.
June -	1.537	1.847	3.084	2.973	2.426	2.279	2.163
July - -	1.965	4.989	3.678	3.197	5.657	2.363	3.682
Auguſt -	2.350	1.580	1.334	4.257	2.929	.409	1.527
	5.852	8.416	8.096	10.427	11.012	5.051	7.372

THESE reſults ſhew us that a Summer is reckoned *wet* when it rains 1,8916 inches, or 12 lb. a month or more for any two months. But in general in wet Summers there falls about five inches, that is, above 36 lb. in three months.

HENCE the *variable* Summers are thoſe in which there falls between 24 lb. and 28 lb. in two months, or between 30 lb. or 36 lb. in three months. Perhaps alſo thoſe Summers are called *variable* in which larger quantities of rain fall than thoſe here mentioned, if they fall at diſtant intervals, as from twelve to fourteen or ſixteen days. But the former interpretation being more conformable to the table, ſeems to me moſt probable.

IN Spring the two laſt months, April and May, are chiefly regarded; the terms *wet*, *dry* and *variable* are applied to them to the ſame extent as to the Summer months.

IN

In Autumn, on the contrary, the ſtate of the two firſt months is moſt important. If 11 lb. or 12 lb. of rain be diſtributed through ſixteen or eighteen days of each of theſe months it will be accounted *wet;* but if only 8 lb. or 10 lb. it will be deemed *variable*, and if ſtill leſs *dry*.

TABLE THE THIRD.

Of the Correſpondence of the Weights and Meaſures of Rain.

	Inches.		Inches.		Inches.
$\frac{1}{4}$ lb. troy =	.039409	22 lb. troy =	3.468058	46 lb. troy =	7.2512
$\frac{1}{2}$ - -	.078819	23 -	3.625697	47 -	7.4088
$\frac{3}{4}$ - -	.118228	24 - -	3.783336	48 - -	7.5666
1 - -	.157639	25 -	3.940975	49 -	7.7242
2 - -	.315278	26 - -	4.098614	50 - -	7.8818
3 - -	.472917	27 -	4.256255	51 -	8.0394
4 - -	.630556	28 - -	4.413892	52 - -	8.1972
5 - -	.788195	29 -	4.571531	53 -	8.3548
6 - -	.945834	30 - -	4.7291	54 - -	8.5124
7 - -	1.103473	31	4.8868	55 -	8.6700
8 - -	1.261112	32 - -	5.0444	56 - -	8.8276
9 - -	1.418751	33 -	5.2020	57 -	8.9852
10 -	1.57639	34 - -	5.3596	58 - -	9.1430
11 - -	1.734029	35 -	5.5172	59 -	9.3006
12 -	1.8911668	36 - -	5.6750	60 - -	9.4582
13 - -	2.049307	37 -	5.8326		
14 -	2.206946	38 - -	5.9902	70 -	11.0347
15 - -	2.364585	39 -	6.1478	80 - -	12.6051
16 -	2.522224	40 - -	6.3054	90 -	14.1875
17 - -	2.679863	41 -	6.4635	100 - -	15.7639
18 -	2.837502	42 - -	6.6208		
19 - -	2.995141	43 -	6.7784	$\frac{1}{3}$ of a lb. =	.0524944
20 -	3.15078	44 - -	6.9360	$\frac{2}{3}$ -	.10498824
21 - -	3.310419	45 -	7.0936	1 oz. - -	.01313

SPRING

SPRING contains fixty-one days.

	It rains	Inches
If wet	36 days or more	3.783 or more.
variable	30 - -	3.150 or one month dry and one wet.
dry	24 or lefs - -	2.522 or lefs.

SUMMER contains ninety-two days.

wet	54 or more - -	5.67 or more, or two wet months.
variable	45 - -	4.729
dry	36 or lefs - -	3.783 or two dry months.

AUTUMN contains fixty-one days.

wet	36 or more - -	3.783
variable	30 - -	3.150
dry	24 or lefs - -	2.522 or lefs.

STATE *of the* WEATHER *in* DUBLIN *from the 1st of June* 1791 *to the 1st of January* 1793. *By* RICHARD KIRWAN, *Esq; F. R. S. and M. R. I. A.*

THESE obſervations were made at my houſe in Cavendiſh-row; the barometer within doors ſuſpended in a room where no fire is kept, about forty feet above high-water mark, and inſpected daily about two o'clock.

THE thermometer, one of Six's conſtruction, which marks the maximum and minimum of temperature in the twenty-four hours. It is ſuſpended without doors in a northern expoſition, about five feet and a half above ground.

THE rain gage receives the rain on a ſurface of one ſquare foot; it is elevated about thirty feet above the ſurface of the earth, and at the diſtance of at leaſt one hundred feet from any building.

building. The rain is collected and weighed two or three times a month. I began to use it on the 1st of July 1791.

In my absence these instruments were daily observed and their indications noted by the Rev. Mr. Mc. Mahon, whose skill and diligence in matters of this nature are well known.

June 1791.

BAROMETER.

The *greatest* height of the barometer in this month happened on the 7th, and amounted to 30.4. Wind E. by S. Very warm and fair.

It *stood lowest* on the 30th, being 29.55. Wind S. Windy with showers, but warm.

Its mean height during the month was 30.06.

THERMOMETER.

Greatest heat 74°.5, wind E. by S. on the 7th.

The *least* 42°, wind W. N. W. cloudy and rainy on the 16th.

Mean of the month 58°.76.

In

In London, greatest height of the barometer 30.22 on the 7th. *Least* 29.39 on the 16th. Mean of the month 29.93.

Greatest heat 80° on the 7th. *Least* 47° on the 13th and 14th. *Mean* of the month 61°.3.

But it is to be noted that in the house of the Royal Society nocturnal observations are not taken, so that the greatest cold does not appear.

July 1791.

BAROMETER.

Greatest height 30.32 on the 15th. Fair. Wind E. N. E.
Least 29.45 on the 4th. Rainy. High wind W.
Mean 29.929.

THERMOMETER.

Greatest heat 71°.5 on the 2d. Wind S. by W.
Least 49° on the 18th. Rainy. Wind S. W.
Mean 61°.13.

RAIN.

It rained twenty-two days, more or less; the quantity that month amounted to 2.469 inches; hence it was a *wet* month.

In London the greateſt height of the barometer was 30.24 on the 15th; the *leaſt* 29.24 on the 11th. Mean of the month 29.89.

Greatest heat 78°.5 on the 18th; leaſt 52° on the 7th; mean 62°.6.

There were twelve rainy days, and the mean quantity was 2.194 inches; but the obſerver remarks that the quantity of rain noted was through ſome defect in the inſtrument or its poſition this year remarkably deficient.

August 1791.

BAROMETER.

Greateſt height 30.68 on the 19th. Wind W. by S. Fair.
Leaſt 29.64 on the 31ſt. Wind E. N. E. Heavy rain, and high wind.
Mean of the month 30.172.

THERMOMETER.

Greateſt heat 75°.5 on the 6th. Wind W. by S. Fair.
Leaſt 48° on the 19th. Wind S. W. Fair.
Mean of the month 62°.82.

RAIN.

RAIN.

It rained ſeventeen days, more or leſs; the quantity was 2.876 inches; hence it was a wet month.

In London the *greateſt* height of the barometer was 30.52 on the 20th; the *leaſt* 29.65 on the 28th. The *mean* 30.06.

The *greateſt heat* 78°.5 on the 6th; *leaſt* 50° on the 30th. *Mean* 64°.9.

The rain uncertain.

September 1791.

BAROMETER.

Greateſt height 30.51 on the 14th and 15th.
Leaſt 29.6 on the 3d.
Mean 30.239.

THERMOMETER.

Greateſt heat 72°.5 on the 16th.
Leaſt 46°.5 on the 19th.
Mean 59°.35.

RAIN.

There were but eight rainy days in this month, and there fell only 1.2611 inches, ſo that it may be accounted *variable.*

In London the *greatest height of the barometer* was 30.33 on the 16th; the *least* 29.52 on the 4th. *Mean* of the month 30.09.

Greatest heat 77° on the 11th; *least* 43° on the 20th. *Mean* 59.5.

OCTOBER 1791.

BAROMETER.

Greatest height 30.55 on the 28th.
Least 28.64 on the 20th.
Mean 29.76.

THERMOMETER.

Greatest heat 66°.5 on the 3d.
Least 38° on the 23d.
Mean 51°.

RAIN.

THERE were fifteen rainy days; the quantity amounted to 2.522 inches, hence this month may be deemed *wet*.

In London, *greatest barometrical height* 30.46 on the 28th and 29th; *least* 28.89 on the 21st. *Mean* 29.69.

Greatest heat 62°.5 on the 5th; *least* 34° on the 24th. *Mean* 48°.9.

RAIN 2,+ inches.

NOVEMBER.

NOVEMBER.

BAROMETER.

Greatest height 30.34 on the 26th.
Least 28.96 on the 18th.
Mean 29.74.

THERMOMETER.

Greatest height 57°.5 on the 30th.
Least 31° on the 17th and 18th.
Mean 43°.

RAIN.

IT rained twenty-two days in more or less quantity; the amount was 2.1088 inches, consequently the month may be called *wet*.

In London the *greatest barometrical height* was 30.28 on the 27th; the *least* 28.76 on the 19th; the *mean* 29.68.

Greatest heat 52°.5 on the 24th; *least* 25° on the 7th. *Mean* 43.6.

RAIN, uncertain, 2.5+.

December.

BAROMETER.

Greateſt height 30.36 on the 20th.
Leaſt 29.13 on the 13th.
Mean 29.723.

THERMOMETER.

Greateſt heat 48°.5 on the 27th.
Leaſt 25°. on the 16th.
Mean 36°.34.

RAIN.

It rained thirteen days and ſnowed five during this month; both amounted to 1.891 inches.

In *London* the *greateſt height* of the barometer was 30.38 on the 17th; the *leaſt* 28.9 on the 4th; the *mean* 29.64.

The *greateſt heat* 48° on the 23d; the *leaſt* 21° on the 12th; the *mean* 36°.7.

Rain 1.12+.

Thus far I have given a comparative view of the ſtate of the atmoſphere in Dublin and London, from which many important conſequences may be deduced relatively to the progreſs and

and regreſs of the accumulations of the atmoſphere, as well as heat and cold, but which I muſt leave to thoſe who profeſſedly purſue ſuch inquiries.

A Synoptical View of the State of the Weather in 1792 in Dublin.

	BAROMETER.			THERMOMETER.			RAIN.	
	Higheſt.	Loweſt	Mean.	Higheſt.	Loweſt.	Mean.	Days.	Inches.
January - -	30.57	28.76	29.721	53	19.5	39.92	21	2.679
February - -	30.65	29.42	30.019	58.5	25.5	43.78	19	2.8240
March - -	30.47	29.18	29.707	60	26	44.09	25	2.3644
April - -	30.43	29.24	29.909	64	40.5	51.125	19	2.5616
May - -	30.57	29.12	30.061	67	29.5	52.193	17	1.8128
June - - -	30.55	29.59	30.093	74	43	56.975	16	0.8669
July - -	30.3	29.68	30.020	75.75	51	61.056	17	2.6141
Auguſt - -	30.43	29.4	30.043	77	52	62.584	15	5.8588
September -	30.69	29.26	29.915	67	42	54.788	25	3.0213
October - -	30.68	29.13	29.880	59	35	49.18	23	2.7980
November -	30.53	29.2	30.053	64	33	48	14	0.3940
December -	30.46	29.14	29.986	55	33	42.403	17	2.9163
Mean of the Year -			29.950	55.66		50.509	Total. 228	Total. 30.700

OBSER-

OBSERVATIONS.

I SHALL firſt compare the wetneſs of the ſeaſons with the rules of probability above given.

1ſt, In the *Spring* months there fell 4,374 inches of rain, that is above two per month on an average, therefore this ſeaſon was *wet*. Moſt rain fell in the firſt month.

2dly, The Spring being *wet*, the probability of a *wet* Summer was $\frac{5}{6}$ by the fifth rule; accordingly, except in June, it rained above two inches in each month, and upon an average above three. However it rained but forty-eight days inſtead of fifty-four.

3dly, The Summer being wet, the probability of wet, dry and variable Autumns were as 3, 5, and 12, by the eighth rule; however it turned out *wet*, which was the leaſt probable event. It rained forty-eight days, and there fell above 5.8 inches.

AGAIN, after a *wet Spring* and *wet Summer* the probabilities of *wet*, *dry* and *variable* Autumns were 1, 2 and 2 reſpectively by the eleventh rule; by which it appears that the wetneſs of this Autumn was perfectly *extraordinary*, and not to be expected.

LASTLY, there were ſtorms on the 19th and 20th of March from the South, therefore the probability of a *wet Summer* was 5 to 1 according to the fourth obſervation.

THE

The moſt important changes that take place in the atmoſphere ſeem to me to be thoſe that happen five or ſix days before, or during, or five or ſix days after the vernal equinox, that is from the 16th to the 28th of March. In Dublin the natural height of the barometer is 30 inches, but in the above-mentioned period its mean height was 29,838, that is 0.162 parts of an inch too low; and the mean height of the whole month was 29.707, that is 0.293 parts of an inch too low, or below the ſtandard height. Yet the wind was chiefly S. or S. W. which ſeems to denote an accumulation in that quarter; for otherwiſe why ſhould it blow from a warmer to a colder region?

January.

The coldeſt days in January were the 11th, 12th, 13th and 14th; wind N. and N. E. except the 14th, when it was S. S. E.; yet the barometer was rather low, being between 29.28 and 29.42; on the 15th it was 28.76. This deſerves attention.

February.

The coldeſt days in February were the 17th, 18th and 19th; wind E. and N.; barometer in the mean time from 30.65 to 30.06.

At Montmorenci, Pere La Cotte obſerved a much greater cold between the 16th and 24th; on the 8th a ſtorm at W.

With us it was squally on the night of the 7th at W.; barometer 29.53.

MARCH.

PERE La Cotte remarked that the mean height of the ☿ in this month was below its standard, which at Montmorency is 27 inches 10 lines and ½, or 29.705 English.

JULY, AUGUST and SEPTEMBER.

THESE months were uncommonly wet in France, as with us.

I HAVE often remarked that the more it rains in May the less it rains in September. If it rains two inches in May there falls less than one in September. This I observed in England. But it is about four to three in any year that the fall of rain in September will be greater than in May.

DR. Hales has shewn that on plains, and in the climate of London, twenty-two inches of rain are fully sufficient for all the purposes of vegetation, and forty-two in a hilly or mountainous country. 1 HALES, 56.

EXAMINATION *of the* SUPPOSED IGNEOUS ORIGIN *of* STONY SUBSTANCES. *By* RICHARD KIRWAN, *Esq; F. R. S. and M. R. I. A.*

THE origin of ftony fubftances, feemingly an object of mere curiofity, is neverthelefs connected with defiderata of the greateft utility to mankind; to fay nothing of the arts of fabricating glafs, artificial gems, mortar, cement, bricks, pouzzolane and earthen-ware, which have evidently fome reference thereto, nature prefents us with various ftones, as flates, mica, &c. with whofe artificial compofition, though highly important, we are at prefent unacquainted, and muft remain fo until the mode of their production is fatisfactorily afcertained; then, and then only, experiments tending to form them by art may be planned and attempted. Hence the propriety of examining the foundation of the different opinions of mineralogifts and geologifts on this head; if we can exclude any of them, we are fo much the nearer

Read Feb. 3, 1793.

nearer to the proper end of our enquiries. Fortunately in this case, relatively to the general mode, there neither are nor can be more than two opinions; if one can be proved falſe, the other muſt of neceſſity be true. All are agreed that ſtony ſubſtances were originally ſoft, and even liquid; but ſome think this fluidity was occaſioned by their having been diſſolved, or at leaſt diffuſed in water, and afterwards cryſtallized, precipitated, or otherwiſe ſeparated from it. This is, at this day, the doctrine moſt generally adopted by mineralogiſts; volcanic ſtones, by far the leaſt numerous of the ſtony tribe, forming but a trifling exception to this general fact.

LATELY, however, a very different theory has been offered to the public in the firſt volume of the Tranſactions of Edinburgh, and ably ſupported by Doctor Hutton, a gentleman advantageouſly known to the philoſophic world through an important meterological diſcovery, the General Law of the Formation of Rain. His opinion ſeemingly reſembles, but in fact differs eſſentially, from thoſe entertained by Leibnitz, Telliamed, Moro and Buffon. They held ſtones to have been originally ſolid, and afterwards liquified by heat; but Doctor Hutton endeavours to prove that previous to the preſent ſtate of our globe they were utterly deprived of ſolidity, and have ſince acquired it by fuſion, and ſubſequent congelation on cooling. This ſyſtem, the moſt ingenious certainly that has as yet been deviſed, is however but little known or at leaſt noticed on the continent; the few that have mentioned it do not ſeem to have well comprehended it; in thoſe points of view in which I have

I have confidered it, I muft fay it appears to me improbable; upon difcuffion, however, it is poffible my objections may be removed; thofe that have occurred to me I fhall now briefly ftate to the Academy in the cleareft and fhorteft manner I am able.

After fome profound reflections on the wifdom difplayed in the conftitution of the globe we inhabit, our author tells us, "It is neceffary to diftinguifh three different bodies which "compofe the whole of it; a folid body of earth, an aqueous "body of fea, and an elaftic fluid of air. There is a central "body in the globe which is commonly fuppofed to be folid "and inert, but which he will afterwards prove not to be fo. "There is alfo an irregular body of land raifed above the level "of the ocean, which is doubtlefs the fmalleft portion of the "globe. There is alfo an atmofphere of air, neceffary for the "fuftenance of fire, animal life, vegetation," &c. *Pages* 211 *and* 212.

After mentioning the general powers that actuate the whole machine, he confines his views to that part which we inhabit. "That we may confider the *natural confequences* of thofe opera-"tions, which, being within our view, we are better qualified "to examine. In purfuit of this object we employ our fkill "*in refearch*, and not in forming *vain conjectures*, and, *if data* "*are to be found*, on which fcience may reafon, we fhould "not long remain ignorant of the natural hiftory of the globe, "a fubject on which opinion only and not evidence has

"hitherto

" hitherto decided; for in this ſubject there is naturally leſs " defect of evidence, although philoſophers, led by prejudice or " miſguided by falſe theory, have neglected to employ that " light." This paragraph ſeems to me ſomewhat obſcure; the exiſtence of thoſe data on which ſcience is to reaſon being only hypothetically laid down in the firſt part, but in the concluſion the exiſtence of ſuch data ſeems poſitively affirmed, ſince philoſophers are reproached with neglecting to employ the light reſulting from them; this is however but of little conſequence. " But to proceed, in purſuing further our general preparatory " ideas, a ſolid body of land could not anſwer the purpoſe of " an habitable world, for a *ſoil* is neceſſary to the growth of " plants, and this conſiſts of materials collected from the " deſtruction of ſolid land. Therefore the ſurface of the land " inhabited by man is made by nature to *decay*, in diſſolving " from that hard and compact ſtate in which it is found below " the ſoil, and this ſoil is neceſſarily waſhed away by the con" tinual circulation of the water running from the ſummits of " the mountains towards the general receptacle of that fluid." *Page* 214. Here we muſt arreſt the courſe of our ingenious philoſopher. He aſſerts that the terreſtrial part of the globe was originally a ſolid compact maſs, from the diſſolution of which the leſs compact and looſer earths, as chalk, clay, magneſia and moulds have ariſen. This preliminary propoſition cannot be allowed; for the earthy and ſtony part may be of equal antiquity, or the terreſtrial part may have been originally formed in a highly comminuted ſtate, and have afterwards partially coaleſced into ſtony maſſes, and the remainder may have continued in its original ſtate, or nearly ſo, its particles having acquired only that degree of conſiſtence which we obſerve in clays and earths. That the ſoil, however, receives an increaſe from ſome ſpecies of ſtones that moulder by expoſure to the air cannot be denied, but there is no proof that *all ſoil* has ariſen from decompoſition. He next tells us that this ſoil is neceſſarily waſhed away by the continual circulation of water running from mountains to the ſea. Here are two ſuppoſitions, neither of which is grounded on facts. Soil is not conſtantly carried away by the water, even from mountains, as Mr. De Luc has clearly ſhewn in his nineteenth and twentieth letters to the Queen*; and if it were, it would be depoſited on the plains, for there are plains as well as mountains on the dry parts of our globe. All water does not flow into the ſea; much of it is carried off by evaporation. Moſt of the earth ſwept off by rivers is depoſited at their mouths; of that which is carried into the ſea, much, if not all, is rejected on the ſhore. Neither has the ſea that deſtructive action on the ſhores univerſally that Buffon and others have ſuppoſed. This is evident by inſpecting the baſaltic pillars on the coaſt of Antrim; the angles of ſuch of theſe as are and have been expoſed to the waves, perhaps for ſome thouſand years, are juſt as ſharp as thoſe of ſuch pillars as are placed far beyond their reach.

HENCE

* Theſe letters and ſeveral other papers of this excellent philoſopher in Rozier's Journal contain much uſeful information on geological ſubjects. But unhappily it muſt be purchaſed by a great expence of *time*.

Hence the conclufions of our author relative to the imperfect conftitution of the globe fall to the ground; and the pains he takes to learn "by what means a decayed world may be "renovated," are fuperfluous. "But further (continues our "author) the folid parts of the globe are in general compofed "of fand, gravel, argillaceous and calcareous ftrata, or of "various compofitions of thefe with other fubftances." This certainly cannot be affumed as a fact, but rather the contrary; it holds true only of the furface, the bafis of the greater part of Scotland is evidently a granitic rock, to fay nothing of the Continents both of the Old and New World, according to the teftimony of all mineralogifts

In the fucceeding paragraphs, pages 119 and 120, there is fome ambiguity, which it is proper to explain. In all regions of the globe immenfe maffes are found, which though at prefent in the moft folid ftate, appear to have been formed by the collection of the calcareous *exuviæ* of marine animals. "That "all maffes of marble or limeftone are compofed of the calca-"reous matter of *marine bodies* may be concluded from the "following facts:—Firft, few beds or marble or limeftone occur "in which may not be found fome of thofe objects which "indicate the marine origin of the mafs. We fhall thus find "the greater part of the calcareous maffes upon the globe to "have originated from *marine calcareous bodies*. That thefe beds "had their origin at the bottom of the fea, and that they have "the calcareous fubftance which they contain, from the fame "fource as marble or limeftones." If by *marine origin* the author

author means that moſt or all calcareous mountains were formed in the ſea, this will not be diſputed; but if his meaning be, that all calcareous matter conſiſts of marine exuviæ, this cannot be allowed, as huge maſſes of marble exiſt which diſcover not the leaſt trace of marine exuvia, and calcareous ſubſtance is found in many granites and the component parts of granite, which was never ſuſpected to be of teſtaceous origin. The exiſtence of ſuch maſſes is not diſputed by our author. "There are "(ſays he) in all regions of the earth, huge maſſes of calcareous "matter, in a cryſtalline or ſparry ſtate, in which, perhaps, no "veſtige can be found of any organized body, nor any indication "that ſuch calcareous matter had belonged to any animals, but "as in other maſſes this ſparry ſtructure or cryſtalline ſtate is "evidently aſſumed by the marine calcareous ſubſtances in oper-"ations which are neceſſary to the conſolidation of the ſtrata, it "does not appear that the ſparry maſſes in which no figured body "is formed have been originally different from other maſſes "which leave ample evidence of their marine origin." That is to ſay, ſince ſparry maſſes are found among calcareous ſtrata of teſtaceous origin, other ſparry maſſes may alſo have the ſame origin. This reaſoning does not appear to me at all concluſive, any more than if an inhabitant of the interior parts of a continent, unacquainted with any calcareous ſtones but thoſe of a ſparry ſtructure, ſhould conclude that all this matter originally proceeded from the bones of land animals, becauſe they alſo are of a calcareous nature. It is much more probable that ſea animals themſelves derive their calcareous matter from a pre-exiſting ſubſtance of the ſame nature contained in their food, as we have no proof of the actual

 productibility

productibility of any fimple earth. Our author's conclufion, however, is, "That all the ftrata of the earth, not only thofe confifting of calcareous maffes, but others fuperincumbent on thefe, "have had their origin at the bottom of the fea, by the collection "of fand, gravel, fhells, coralline and cruftaceous bodies, and of "earths and clays varioufly mixed, feparated and accumulated," page 221. Various geological obfervations contradict this conclufion. There are many ftratified mountains of argillaceous flate, gneifs, ferpentine, jafper, and even marble, in which either fand, gravel, fhells, coralline, or cruftaceous bodies are never or fcarce ever found *.

The general amount of our author's reafoning however is, "That nine-tenths perhaps, or ninety nine hundredths of this "earth, fo far as we fee, have been formed by natural opera-"tions of the globe, in collecting loofe materials and depofiting "them at the bottom of the fea; confolidating thofe collections "in various degrees, and either elevating thofe confolidated "maffes above the level on which they are formed, or lowering "the level of the fea." How ill fupported by facts this conclufion is we have already fhewn; and our author himfelf will prefently difcover, for he adds, "There is a part of the "folid earth, which we may at prefent neglect, not as being "perfuaded that this part may not alfo be found to come under "the general rule of formation with the reft, but as confidering "it as of no confequence in forming a general theory which "fhall

* 1 Gerh. Gefch. p. 72. 85. 2 Gerh. 413.

" ſhall comprehend almoſt the whole." This excluded part conſiſts of mountains and maſſes of granite. And yet moſt geologiſts look on this excluded ſubſtance as forming by far the greater part of the globe, all other parts being commonly found to reſt upon it *.

Having thus found the greater part, if not the whole of the ſolid land, to have been originally compoſed at the bottom of the ſea, our author proceeds to examine, how ſuch continents as we now have could be erected above its level; he ſhews that no motion of the ſea could produce that effect; or if it could, yet ſuch a continent could not produce maſſes of ſolid marble and other minerals in a ſtate very different from that in which they were originally collected. " Conſequently, beſides an operation " by which the earth at the bottom of the ſea ſhould be con- " verted into elevated land, a *conſolidating* power is required, " by which the looſe materials ſhould be formed into maſſes of " the moſt perfect ſolidity; and, if this were underſtood, " we might poſſibly become acquainted with the power that " elevated our continents above the level of the waters." Of this conſolidating power he treats in the ſecond part of his eſſay.

Beginning his ſecond part, he reaſons thus, p. 225. " There are juſt two ways by which porous and ſpongy bodies " may be conſolidated into maſſes of a natural ſhape and

* Hoffm. in Berg. Kalend. 197. Voight. 7. Gerhard, Bergman, Pallas, &c.

" regular ſtructure; the one congealation from a fluid ſtate by " means of *cold*; the other *accretion*, and this includes a ſepa- " ratory operation; to produce ſolidity either way, fluidity muſt " be induced, either by heat or by a ſolvent." This reaſoning tacitly ſuppoſes a fact which we have already ſeen to be either falſe or precarious. The particles which now form the ſolid parts of the globe need not be ſuppoſed to have originally been either ſpungy or porous, the interior parts at the depth of a few miles might have been originally, as at preſent, a ſolid maſs. The more ſuperficial maſſes might have been partly diffuſed and partly diſſolved in the primogenial fluid. The particles, for inſtance, of which argillaceous ſlates were formed, might have been originally barely diffuſed, as they ſeem to have been formed by mere ſubſidence. " The ſtrata formed at the bottom " of the ſea are to be conſidered as having been conſolidated " either by aqueous ſolution and cryſtallization, or by the " effect of heat and fuſion; if by the firſt of theſe two ways " the ſolid ſtrata have attained their preſent ſtate, there will be " a certain uniformity obſervable in the effects; and general " laws by which this operation muſt have been conducted." Here ſubſidence and precipitation, as part of the general means of the formation of ſtones in the moiſt way, ſhould not have been omitted.

As to the uniformity to be expected in the effects of cryſtallization, the learned author is certainly too well acquainted with the ſubject not to know that this uniformity is not to be expected but when all the circumſtances are perfectly ſimilar.

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He muſt know that ſuperſaturation, a ſlight contamination with certain heterogeneous ſubſtances, a variation in the temperature, a variable degree of agitation, a difference in the quantity of the menſtruum, or in the time of its diſparition, beſides many other unknown circumſtances, daily produce different effects in the cryſtallization of ſalts, the bodies in which this operation has been moſt attended to.

" BUT water (continues he) being the general medium in which " bodies collected at the bottom of the ſea are always con- " tained, if thoſe maſſes of collected matter are to be conſolidated " by ſolution, it muſt be by the diſſolution of thoſe bodies " in that water as a menſtruum, and by the concretion or " cryſtallization of that diſſolved matter, that the ſpaces firſt " occupied by water in thoſe maſſes are afterwards to be filled " with a hard and ſolid ſubſtance; but without ſome other power " by which the water contained in thoſe cavities and endleſs " labyrinths of the ſtrata ſhould be ſeparated in proportion " as it has performed its taſk, it is inconceivable how thoſe " maſſes, however changed from the ſtate of their firſt ſub- " ſidence, ſhould be abſolutely conſolidated, without a particle " of fluid water in their compoſition." Abſtracting from his own gratuitous hypotheſis, it is very eaſy to ſatisfy our author on this head; the concreting and conſolidating power in moſt caſes ariſes from the mutual attraction of the component particles of ſtones to each other; if theſe particles leave any interſtices, theſe are filled with water which no way obſtructs their ſolidity when the points of contact are numerous; hence the decrepitation

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of many ſpecies of ſtones when heated. Many ſorts of ſtones are ſoft while in their ſtrata, as ſandſtones, limeſtones, &c. but loſe their water and acquire hardneſs by expoſure to the air. But perhaps the moſt effectual means of convincing our author that a conſolidating power *may* take place in water, is to remind him that in many caſes it does *actually* take place. Thus mortar made by pouzzolona or terras is well known to harden under water; nay Mr. Smeaton has obſerved it to throw out under water a ſtalactite, which alſo hardens in that ſituation. *Eddy ſtone*, § 181. The calcareous depoſits formed in certain waters, and which attain a ſtony hardneſs, are a further proof that immerſion in water does not always obſtruct the formation of ſolid maſſes. "But (adds our author) we "find ſtrata conſolidated by various ſubſtances which water "cannot diſſolve; thus we have water conſolidated by calcareous "ſpar, a thing perfectly diſtinguiſhable from the ſtalactitical "concretion of calcareous earth, in conſequence of aqueous "ſolution; we have ſtrata made ſolid by the formation of "fluor, a ſubſtance not ſoluble, as far as we know, by water; "we have ſtrata conſolidated by ſiliceous earth in a ſtate wholly "different from that in which it was obſerved, on certain "occaſions, to have been depoſited by water; we have ſtrata "conſolidated by ſulphureous and bituminous ſubſtances which "refuſe a watery ſolution; laſtly, we have ſome conſolidated "by almoſt all the various metallic ſubſtances. If it is by "means of water that all theſe interſtices have been filled "with thoſe materials, water muſt be, like fire, an univerſal

"ſolvent,

" ſolvent, and we muſt change our opinion of its chymical " character."

HERE the difficulties to the ſuppoſition of an aqueous ſolution are placed in the ſtrongeſt light; yet it muſt be owned that they partly ariſe from the author's own gratuitous ſuppoſition, that ſtrata exiſted at the bottom of the ſea previous to their conſolidation; a circumſtance which will not be allowed by the patrons of the aqueous origin of ſtony ſubſtances, as we have already ſeen.

SECONDLY, That water, in certain circumſtances, and with the addition of certain ſubſtances, may be admitted as a univerſal ſolvent, ſhould not be denied, merely on account of our ignorance of thoſe circumſtances and auxiliary ſubſtances. Before the diſcovery of the ſparry acid, it was not known that water, by the aid of that acid, could diſſolve ſiliceous bodies, a power which, by this help, it is now known to poſſeſs; there may be various other menſtrua in nature of which we are as yet ignorant; it is well known that certain proportions of the ſimple earths act upon each other as menſtrua in the dry way, why not alſo in the moiſt way, if equally divided? and what hinders us from ſuppoſing that they were originally created in that ſtate of diviſion that would render them capable of acting on each other? why ſhould we ſuppoſe this habitable earth to ariſe from the ruins of another anterior to it, contrary to reaſon and the tenor of the moſaic hiſtory? What do we gain by that ſuppoſition? Muſt not the origin of of that anterior world, if composed of materials similar to those of this, be equally accounted for? and must we suppose that anterior world destitute of calcareous earth because it was not formed at the bottom of the sea? If it were destitute of that earth, it could not contain plants or animals similar to ours, as ours essentially require that earth: or must we allow that anterior solid land to have been itself also formed of the ruins of another still prior to it, and thus admit a process *in infinitum*; an abyss from which human reason recoils? Into this gulph our author however boldly plunges; towards the end of his Essay he tells us, this earth is derived partly from one immediately anterior, and partly from another anterior to that again. In a word, to make use of his own expression, "We find no vestige of a beginning." Then this system of successive worlds must have been eternal; now succession without a beginning is generally allowed to involve a contradiction, therefore the system that forces us to adopt that conclusion must necessarily be false. Our author was led to it by his, and our common ignorance, of the means by which stones of the siliceous class were consolidated or dissolved in liquid menstruums, but the rules of exact reasoning require that, before we deny the general possibility of producing an effect by any given cause, we should be acquainted with all the possible methods of applying that cause; if any of them be unknown, our conclusion must be defective; more especially if we have strong reasons to suspect that some modes or circumstances in the application, that cause do exist with

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whofe detail we are unacquainted. Now this happens to be the cafe with refpect to the folution of earths or ftones of the filiceous kind. Mr. Bergman had already obferved that filiceous earth, fufficiently divided, was foluble in all acids *. Mr. Klaproth, the worthy fucceffor of the immortal Scheele, found it foluble in mere water in the fame ftate of divifion †. The great geologift, Mr. Dolomieu, feems to have difcovered, by the help of the chymical abilities of Mr. Pelletier, the very circumftances on which its folubility depends. Mr. Morveau has alfo difcovered another, and a very different method of effecting this folution ‡. Mr. Laffone found the furface of grit, which had been broken a year before, invefted with filiceous cruft nearly as hard as agate, which therefore muft have been newly formed. Hence ftrata might be confolidated without fufion §. It were eafy, but needlefs, to accumulate more teftimonies of this fact, as other proofs of fuch production will occur in the fequel. Dolomieu obferved the growth of fhorls on the Pyrenees ||.

Our author next proceeds to ftate, " That if it is by means " of heat and fufion that the loofe and porous ftructure of the " ftrata fhall be fuppofed to have been confolidated, then " every difficulty which had occurred on the power and agency

* 5 Bergm. 128, in Note.
† 3 Berl. Beobacht. 160.
‡ Swed. Abhandl. 1790.
§ Mem. Paris, 1774. 13 in 8vo.
|| Surles Ifles Pouces. 249.

" of water is at once removed; the loofe and difcontinuous body " of a ftratum may be clofed by means of *foftnefs and com-* " *preffion*, the porous ftructure of the materials may be con- " folidated in a fimilar manner by the *fufion* of their fub- " ftance, and foreign matter may be introduced into the open " ftructure of ftrata, in form of *fteam or exhalation*; confequently " heat is an agent competent for the confolidation of ftrata, " which water alone is not. The examination of nature gives " countenance to this fuppofition; confequently, however difficult " it may appear to have this application of heat, we cannot " from natural appearances fuppofe any other caufe." *Pages* 229, 230, and 237. He adds " For the explanation of thofe natural " appearances which are fo general, no further conditions are " required, than the fuppofition of a fufficient intenfity of fire or " heat, and a fufficient degree of compreffion upon thofe bodies, " which are to be fubjects to that violent heat, without " calcination or change. So far as this fuppofition is not gratuitous, " the appearances of nature will be thus explained."

HERE we have the whole theory of our author; in oppofition to which I make bold to fay, 1ft, That the fuppofition of a degree of heat under any given compreffion, fufficient for the fufion of ftony fubftances in general, without calcination or change, is not only gratuitous, but contrary to all that we at prefent know of the agency of heat. Secondly, That all the appearances of nature depofe in favour of an aqueous folution or diffufion, and a cryftallization, concretion or fubfidence therefrom, and againft an igneous folution or fufion.

AND

And first, it is *gratuitous*, not only because it is unnecessary, as we have already shewn, but also because it is inconsistent with our author's own theory. According to him these strata which were consolidated by heat were composed of materials gradually worn from a preceding continent, casually and successively deposited in the sea; where then will he find, and how will he suppose, to have been formed those enormous masses of sulphur, coal, or bitumen necessary to produce that immense heat necessary for the fusion of those vast mountains of stone now existing? All the coal, sulphur, and bitumen, now known, does not form the $\frac{1}{100.000}$ part of the materials deposited within one quarter of a mile under the surface of the earth; if therefore they were, as his hypothesis demands, carried off and mixed with the other materials, and not formed in vast and separate collections, they could never occasion, by their combustion, a heat capable of producing the smallest effect, much less those gigantic effects which he requires. Again, it *is contrary to all we know of the action* of heat; by this we are informed that heat may be produced among *hard* bodies by attrition, and in *inflammable* bodies by combustion. To produce heat by attrition it is necessary that the bodies rubbed together be so hard as that their particles should not easily be abraded, and also that they be perfectly dry; if, therefore, the strata formed in the bed of the ocean were loose, porous, and spongy, previous to the production of heat, and also intimately penetrated with water, as our author repeatedly asserts, it is evident, from all we at present know, that no degree of attrition which they might endure could produce the smallest degree of heat. Even if it could produce ſome heat, nay an intenſe heat, yet a heat productive of fluidity could never be the conſequence of attrition, for the inſtant the bodies ſubjected to it would acquire the firſt degree of emolleſcence, the calorific power of attrition muſt neceſſarily ceaſe.

But granting to our author (what we have ſhewn to be inconſiſtent with his theory) ſuch immenſe maſſes of coal, ſulphur, and bitumen as muſt be ſuppoſed collected together, to procure by their inflammation a heat of energy ſufficient to melt all the ſtony ſubſtances now exiſting, I proceed to ſhew, that, conſiſtently with the laws of nature hitherto known to us, either no inflammation at all could be produced, or at leaſt none capable of producing the effects required by him. To produce inflammation the preſence of vital air is neceſſary; to produce an immenſe inflammation of energy ſufficient to melt ſtony ſubſtances, not only an immenſe collection of ſuch air, but of air of the greateſt purity, is required. To produce an inflammation, capable, if poſſible, of melting ſtony ſubſtances without effecting either calcination or any other change in them, an immenſe compreſſion muſt alſo be ſuppoſed. Now, granting to our author a collection of coal, bitumen, &c. ſufficient to produce a flame capable of ſuch mighty effects, where ſhall we find air to ſupport that flame? neither coal nor bitumen produce vital air. But ſuppoſe this coal or bitumen mixed with ſubſtances capable of giving out that air, ſtill that air would be ſo impure, from the mixture of the fixed and mephitic airs ariſing from the coal or bitumen, as to

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be incapable of forming a heat even equal to that of our common furnaces, as Mr. Dolomieu has clearly ſhewn to be the caſe with reſpect to volcanic heat; and again, allowing our author air of ſufficient purity to effect his purpoſes, what ſhall prevent the water, ſuperincumbent on his looſe, ſpongy ſtrata, and preſſing upon them with immeaſurable weight, from penetrating through theſe ſtrata, and extinguiſhing this flame, or at leaſt from preſſing down the incombuſtible ſtrata upon the inflamed coal, and, by intercepting all communication with the air, from immediately ſuppreſſing the flame? What ſhall prevent the air itſelf, rarefied by heat, from eſcaping through the looſe, and as yet, unconſolidated ſtrata? But to favour the author ſtill further, let us ſuppoſe the fire to originate in caverns formed within the coaly matter itſelf, and let us ſuppoſe ſuch vaults (contrary to all probability) capable of ſupporting the weight not only of a ſuperincumbent ocean, but of all the ſtrata ſubject to that ocean, which by heat are to be converted into ſtone, the air, thus confined in theſe caverns, either would admit of no combuſtion at all, if incapable of expanding, or would ſoon be ſo diminiſhed by the abſorbtion of its vital parts, as to admit the eſcape of fixed air from the ſubſtances acted upon (which would then be calcined) or ſo contaminated by mephitic and fixed airs as to be incapable of producing the violent and extreme effects required by this hypotheſis; then where ſhall we place the ſtrata to be acted on? if over the coaly vaults, they will not be in contact with the flame; if under or on one ſide, only their ſurface can be acted on. Thus, on whatever ſide

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we contemplate this hypothesis, it presents nothing but incompatibilities with our actual knowledge of the operation of fire.

SECONDLY, Our author's demand, that we admit a degree of heat acting with the greatest possible intensity, and yet producing no change in the substances acted upon, is not only gratuitous, as he himself seems to allow, but incompatible with all physical inquiry, and a mere *petito principii*. No cause can be traced but by its effects, that is, by the changes it has produced; if these are supposed null, all inquiry must cease. To avoid this objection, and countenance this supposition, our author further demands a degree of compression under which the action of heat could produce no change; but this being another new and independent supposition, should itself be proved to be probable, or at least possible, which our author neither has done, nor, as far as I can see, can do; and even, with the help of this double supposition, it cannot be proved that pure calcareous earths can at all be melted with or without emitting their fixed air, as this fusion has never yet been effected either by concentrated solar heat, or by the help of pure air, or by any other contrivance of art; and if it could, how could the shells, with which it is in sundry instances filled, escape fusion, and remain unblended with the common mass in which they were imbedded?

OUR author will probably reply, that many difficulties also accompany the supposition of an aqueous solution, and this I freely confess. In the actual constitution of things, both physical and moral, many inexplicable difficulties occur, but

must

muſt we not diſtinguiſh thoſe which *eſcape* our reaſon, from thoſe that formally *contradict* it? The former may *in time* be connected with our actual knowledge, the latter never. In our preſent view of nature all appearances point out a watery ſolution, or diffuſion and concretion in that fluid, though the means of effecting this ſolution are but imperfectly known; but they contradict the idea of an igneous ſolution, as we ſhall at preſent prove.

To reduce the perſpective of the mineral kingdom within the bounds of an academical diſſertation, we muſt neceſſarily confine it to the general claſſes under which minerals are commonly arranged, and a few ſpecies of each. And firſt, as to the calcareous claſs. Stones of this claſs, when perfectly pure, or nearly ſo, as ſpars and granular marbles, are abſolutely infuſible in any degree of heat yet known, as Lavoiſier, Geyer, and Ehrman have ſucceſſively ſhewn *. On the other hand, the perfect cryſtallization of the former, and the internal conſtitution of the latter, confeſſedly prove that they were once in a ſtate of perfect ſolution, and ſince they could not be ſo in the igneous, they muſt have been ſo to the aqueous fluid; if we ſuppoſe their particles to have been originally in that ſtate of diviſion which actual ſolution requires, which ſtate may as well be ſuppoſed to have been their primordial ſtate as any other, there will be no difficulty in ſuppoſing them diſſolved or ſuſpended

* Mem. Paris 1783. Schewed. Abhand. 1784. p. 127. Vers. Einer Schmelſkunſt. Von Ehrman.

ſuſpended in an aqueous fluid. As to the compact limeſtones and marbles, in which the teſtaceous exuviæ of marine animals abound, it is evident that if theſe ſtones were ever melted, thoſe would, with them, run into one common maſs, as we have already ſaid. Other ſtones of this claſs are more impure, and mixed with argill and ſilex in ſuch proportion as to be vitrifiable in ſuch heats as art can eaſily produce, yet we never find them in that ſtate; a circumſtance which clearly excludes all ſuſpicion of their ever having been expoſed to them.

In the *muriatic claſs*, we ſee ſteatites and pott-ſtone, which in their actual ſtate have a ſoft ſoapy feel, but harden when heated, vitrify in a ſtronger heat, and acquire a texture and hardneſs quite different from thoſe they before poſſeſſed. Steatites often contain 16 per cent. of air and water; theſe characters depoſe in favour of an aqueous origin: but ſerpentines, of which whole mountains often conſiſt, demand this origin more loudly; for they are infuſible in all but the moſt extreme degrees of heat, in which they vitrify and acquire the poliſh, texture, and luſtre of glaſs.

In the *argillaceous claſs*, we meet with argillaceous ſlates, hornblends, and trapps or baſalts; all of which are in a moderate heat converted into ſlags, whoſe appearances totally differ from that which theſe ſtones preſent in their natural ſtate; and hence they evidently diſclaim an igneous origin. Mica has been clearly proved to originate in water by Mr. Nauovarke, 1 *Chy. An.* 1786.

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In the *filiceous clafs* we have quartz or cryftal in various regular forms, which, if fufed at all, muft have been in the thinneft fufion, to be enabled to affume thofe fhapes. Now the ftrongeft heat that art can produce is fcarcely capable of producing the flighteft emollefcence in pure quartz; how then can we affume that nature, in the moft unfavourable circumftances, could produce a perfect fufion of that fubftance? Volcanos afford the moft intenfe natural heat with which we are acquainted; yet the moft fturdy volcanifts allow it to be infufible in thefe. In fact it is frequently found in circumftances in which it is impoffible, confiftently with the known laws of nature, to attribute its origin to igneous liquefaction; for inftance, it is frequently found cryftallized in company with calcareous fpar, fluors, lead ores, &c. on ftones of a mixed nature, as Petrofilex, Hornblends, &c. Now it is well known that though pure quartz or fpars will not melt alone, yet in company with ftones of another kind they will readily melt and unite into one common mafs; when, therefore, they are found in diftinct maffes, clofe by each other, it is evident that they were not formed by fufion, but in fome other manner; and there is no other than aqueous folution. Of this they bear the marks, for they decrepetate for the moft part when heated, and become opake from the lofs of their watery particles; though the quantity of thefe involved in their texture be exceeding minute. Have not fhells and chalk, and even water, been found inclofed in filex *? The impreffion of fhorls has

 often

* 41 Roz. 34. Mem. Dijon 1783 per Camus.

often been found on the quartz that inveloped it *. This laſt muſt therefore have been in a ſoft ſtate, while the ſhorl was in a hard ſtate; now this could not happen if the quartz were ſoftened by heat, for ſhorl, being infinely more fuſible, muſt have been in a ſoft ſtate alſo, and concrete long after the quartz: nay, if we credit Mr. Gerhard and others, cryſtal has been detected in a ſoft ſtate. We have already quoted Mr. Laſſone as an eye witneſs of the aqueous formation of ſiliceous ſtones. I ſhall only add, that petroſiliceous and other fuſible ſtones of this claſs have quite a different aſpect when they paſs through a ſtate of fuſion from that which they preſent in their natural ſtate. I alſo paſs over the moſſes and other vegetable and animal ſubſtances incloſed in agates, &c. as our author pretends to account for their preſervation in the midſt of the moſt raging heat by virtue of a compreſſion, originating, one knows not how, which prevents their combuſtion or charring.

Let us now examine the principal proofs which our author adduces in ſupport of his ſyſtem; that from the inſolubility of calcareous ſtones we have already obviated.

1ſt, " There are ſpecimens of foſſil wood which bear the " moſt evident marks of having been injected with a flinty " ſubſtance in fuſion. This appears from the wood being pene- " trated partially, ſome parts not having been penetrated at all. " In

* 2 Romé 267 in Note. 1 Chy. Ann. 1786. p. 174.

"In the limits of thofe two parts we have the moft convincing "proofs that it had been flint in a fimple fluid ftate, which had "penetrated the wood, and not in a ftate of folution." Why? "becaufe the flinty fubftance has proceeded to a certain length, "and no further; and there is no partial impregnation "nor gradation of the flintifying operation, as muft have "been the cafe if filiceous matter had been depofited from "a folution *." I own I am at a lofs to perceive the force of this argument, and can fee nothing in it but mere affertion.

2dly, "Sulphur is found naturally combined with almoft "all metallic fubftances, which are then faid to be mineralized. "Now no perfon, fkilled in chymiftry, will pretend to fay "that may be done by aqueous folution. The combination "of iron and fulphur, for inftance, may eafily be performed "by fufion; but, by aqueous folution, this combination is again "refolved, and forms a vitriol." That metals may combine with fulphur in the moift way is a fact which perhaps was but little known when our author wrote; it is however at prefent fufficiently eftablifhed. Water may be ftrongly impregnated with hepatic air; the fulphur is precipitated by almoft all metals from this water, and in the fubterraneous meanders where they meet, being protected from accefs to atmofpheric air, there is little danger of the converfion of the fulphurated metals into vitriols. That fulphurated ores may be formed, without the help of heat, is incontrovertibly proved by their having been

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* Page 233, 234.

found overlaying the tools of workmen in old galleries of mines. See the 3d Letter of Baron Trebra, in his Treatiſe on the Internal Structure of Mountains.

3dly, " Several metals have been found native." May not they have been ſo originally?

4thly, " Manganeſe has been found in a reguline ſtate by " Mr. De La Peyrouze, and in ſmall grains, as when produced " by fire." True; but it was mixed with a large quantity of iron, which is often found in that form without any ſuſpicion of fuſion. A fire capable of melting quartz might ſurely produce it in larger maſſes.

5thly, " Spar, quartz, pyrites, and other minerals, are " found variouſly intermixed, cryſtallized upon or near each " other, and adhering to coal, or mixed with bitumen, &c. " circumſtances that cannot be explained in the hypotheſis of " ſolution in the moiſt way." Not exactly, nor with certainty; which is not wonderful: but they are ſtill leſs explicable in the hypotheſis of dry ſolution, as muſt be apparent from what has been already ſaid. How coal, an infuſible ſubſtance, could be ſpread into ſtrata by mere heat, is to me incomprehenſible.

6thly, " Dr. Black found mineral alkali cryſtallized, yet " deſtitute of water of cryſtallization, which could not happen " unleſs it were cryſtallized by fuſion." What then will our author ſay of the vaſt maſſes of this ſalt which are found with their

their full portion of water of cryſtallization? The author refers us to the 71ſt volume of the Philoſophical Tranſactions for an account of Dr. Black's paper. However, in thoſe of the Royal Society of London (the only known by that title without addition) no ſuch paper is to be found. If the alkali were fuſed, the bodies in its neighbourhood were fuſed alſo; without ſome knowledge of their ſtate nothing more can be ſaid; the caſe is not fairly before us. I make no doubt, however, but Dr. Black has examined all circumſtances with that ſkill and accuracy which he is known to poſſeſs.

I DECLINE mentioning a few other diffuſe objections to the aqueous theory, which appear to me to ſhew nothing more than the difficulty of accurately explaining various circumſtances of the mineral kingdom. The only point to be conſidered is which of the two ſyſtems, the aqueous or the igneous, is, upon the whole, leaſt exceptionable, and on this head enough has been already ſaid. I cannot however omit noticing, for the ſake of the diſcuſſion it leads me to, that the application of our author's ſyſtem to the formation of granite is peculiarly unhappy. This rock is formed of ſtones of different degrees of fuſibility, which, in a heat capable of melting quartz, ſhould naturally run into each other; it moſt frequently contains mica, which, when melted, aſſumes an appearance very different from the plated ſtructure it naturally preſents; and, to crown all, can be formed in the *moiſt* way, but cannot in the *dry*: Here I have the misfortune of differing with another zealous patron of the Igneous Theory, equally ſkilled in mineralogy and chymiſtry, the

the learned Dr. Beddoes. In the Philosophical Transactions for 1791, *Page* 56, &c. he tells us, that "a mixture of different "earths, with more or less metallic matter, in returning from "a state of fusion to a solid consistence, may assume, sometimes, "the homogeneous basaltic, and sometimes the heterogeneous "granitic internal structure. No fact is more familiar than "that it depends altogether on the management of the fire, "and the time of cooling, whether a mass shall have the uniform "vitreous fracture, or an earthy broken grain arising from "a confused crystallization. The art of making Reaumur's "porcelain consists entirely in allowing the black glass time to "crystallize by a slow refrigeration, and the very same mass, ac-"cording as the heat is conducted, may without any alteration of "its chymical constitution be successively exhibited any number "of times, as glass, or as stony matter with a broken grain. "In the slagg of iron furnaces the same pieces generally ex-"hibits both these appearances." How far the same mass in fusion is capable of assuming sometimes, the basaltic and sometimes the granitic, we shall presently see. With respect to Reaumur's porcelain, it is certain that the changes of texture, mentioned by the learned author, may be produced in it, not by a slower or more rapid crystallization (for in fact there is no crystallization at all) but by the continuance of a higher or lower degree of heat. This is evident from the experiments of Dr. Lewis and Mr. Delaval. Now the effect of the highest heat of our furnaces, in this case, is to rob the glass of its saline part, as Dr. Lewis well observed; and hence it is not wonderful that the texture should be altered, and the mass at last become

loose

loose and porous. It cannot, therefore, be said that it retains the same chymical constitution as before; the case is quite different with respect to glasses formed of earthy substances without any salt, as I know from my own experience, when once they are perfectly vitrified, a second fusion makes no alteration whatsoever in them, though ever so slowly cooled. Thus, feltspars, garnets, shorls and basalts, being converted into glass by the heat of a furnace, remain glass even when exposed to the highest heat producible by art, namely, that arising from the action of pure air; nor will any retardation of their cooling produce the smallest change. As to the slagg of iron furnaces, it is a compound in which the metallic particles, being by far the most abundant, separate themselves, during fusion, from the earthy. These last then vitrify, vitrification being the effect of the heat to which they are then exposed, and not in consequence of their rapid refrigeration; the metallic particles, on the contrary, assume the grain that is peculiar to them, being incapable of vitrification; hence all analogy with basalt fails.

The Doctor, however sensible of the difficulty of supposing that a substance once uniformly fused, as he imagines granite to have been, should present us 2, 3, 4, 5 and 6 separate substances, as granites frequently do, further adds, "That "this difficulty does not press the igneous more than the opposite "hypothesis, since the constituent parts of granite are crystals, "the whole mass must have once existed in that state of entire "disunion of its particles which is necessary to crystallization.

Now

" Now whether ſuch a ſolution has been effected by the repulſive " force of fire, or the intervention of water, it is juſt as eaſy " to conceive heterogeneous earthy cryſtals, ſhooting from " different points of an uniform liquid, according to the former " ſuppoſition as the latter." It is true, by abſtract conſiderations, we may conceive any thing; but to form juſt conceptions of the operations of nature we muſt take *experience*, or, where this fails us, *analogy*, for our guides. Here both lead us to conceptions diſagreeing totally from the Doctor's. Experience tells us that granites, once perfectly fuſed, coaleſce in cooling into a *greeniſh white* or other *coloured glaſs* *, ſo different from baſalt, that the experimenter, from this experiment alone, was tempted to conclude that baſalt muſt have been produced in the moiſt way. Analogy ſuggeſts that as ſalts of different degrees of ſolubility, in a liquid menſtruum, being brought to cryſtallize, cryſtallize ſeparately, but if fuſed in fire never can; ſo ſtones of different degrees of ſolubility in a liquid menſtruum, being brought to cryſtallize, ſhould cryſtallize in ſeparate concretions. Even a *priori* cryſtallization into ſeparate heterogeneous maſſes is much more eaſily conceived in an aqueous than in the igneous fluid. This laſt occupies no perceptible ſpace, and all the particles it holds in ſolution are on that account crouded together, and in full contact with each other; in proportion as the igneous fluid decreaſes they loſe that facility of motion that is neceſſary for the union of the homogeneous parts and regularity of arrange-

* Per Hacquet 1 Crell. Beytr. 35, & Morveau in 1 Buff. Mineralogy p. 139, in 8vo.

arrangement; ſo that ſcarce any thing but a difference of ſpecific gravity can, while they are in full fuſion, produce a ſeparation. While cooling, ſuch a ſeparation cannot poſſibly take place, according to our conceptions. On the other hand, if ſtony maſſes be once conceived diſſolved in water, this fluid, occupying a much greater ſpace, will allow them full room to concrete in ſeparate maſſes, according to the laws of their various affinities; and this is ſo true, that if evaporation be carried too far, they cannot be properly ſeparated by cryſtallization.

To cloſe this controverſy, I ſhall only add, that granite, recently formed in the moiſt way, has frequently been found; but no inſtance can be produced of its formation by fire. Thus a mole, having been conſtructed in the Oder in the year 1722, 350 feet long, 54 feet in height, 144 feet broad at bottom, and 54 feet at the top, its ſides only were granite, without any other cement than moſs; the middle ſpace was entirely filled with granite ſand. In a ſhort time this concreted into a ſubſtance ſo compact as to be impenetrable by water *. Mr. Soulavie diſcovered an enormous fiſſure in a marble rock, filled with granite matter, which muſt have been in a liquid ſtate when the marble was already ſolid; elſe it would have mixed with it, and not have filled, as it was found to do, all the ſinuoſities of the calcareous rock †.

* Laſius Hartz.

† 1 Soulavie France Merid. 385.

A METHOD *of* PREPARING *a* SULPHUREOUS MEDICINAL WATER. *By the Rev.* EDWARD KENNEY.

CHEMISTS differ in opinion concerning the procefs of nature in the formation of fulphureous waters. Whilft all agree that fulphur by itfelf is not foluble in water, fome confider fulphureous waters as impregnated by the fumes only of fulphur: Others affert that thefe waters contain fulphur combined with an alkali; and each party thinks, and poffibly juftly, that its opinion refpecting particular waters is fupported by the analyzation of them. Artificial fulphureous waters have often been prepared on the former of thefe principles; and they have been prepared on the two principles combined by M. Le Roy of Montpellier, who applied a ftrong and continued heat to water mixed with fmall quantities of fulphur and magnefia, until the fumes of the fulphur had ftrongly impregnated the water. Read April 6, 1793.

My method is founded on the fecond principle. Its fimplicity, and the probability that it is fimilar to that purfued by nature in the formation of fome of the moft powerful fulphureous waters, induced me to make trial of it. The fame confiderations may poffibly be deemed by gentlemen of the medical faculty a recommendation of this artificial medicinal water for trial in the courfe of their practice.

The method is this: Mix fulphur and magnefia, in the proportion of four drachms of each, with one quart of cold water. Care muft be taken that every particle of the fulphur and magnefia be made fo wet as that none fhall float. Pour this mixture into a veffel in which it may be conveniently fhaken feveral times every day during the fpace of three weeks. Let it then fettle for two days, and rack off the Liquor. This, firft racked off from the fulphur and magnefia, will be of the colour of water, and free from any bad fmell. If a like quantity of water be poured into the veffel in which the magnefia and fulphur remain, and be frequently fhaken, it will in a fortnight be found to be as ftrongly impregnated as the former; and in like manner may many fucceffive impregnated liquors be obtained; but they will differ from the firft in having a yellow tinge and emitting a fœtid odour. However, in their component parts and medicinal properties, all thefe impregnated liquors feem to me, from the trials I have made of them, perfectly to agree. Thefe liquors almoft inftantly change the colour of filver. They are moft effectually decompofed by powdered nutgalls and alum, the alum being added a few a few minutes after the nutgall. In this procefs a very copious precipitation enfues.

Flowers of fulphur and magnefia are to be mixed with water in the proportion of four drachms of each to a quart of water. They fhould previoufly be ground together in a glafs mortar, for the purpofe of breaking all the fmall lumps of fulphur which would otherwife float on the water. They fhould then be gradually wetted with the water, and worked up with it by the hand. When fo mixed, as that none of the fulphur floats, the whole is to be poured into a clofe veffel, in which it may conveniently be fhaken two or three times every day for three weeks. After that time it is to fettle for two days, and then the liquid to be racked off fine. The fame ingredients will impregnate the like quantity of water two or three times, to an equal degree of ftrength, in a fpace of time fomewhat fhorter than the firft.

N. B. I have not found that the fineft, light, white, magnefia, fucceeds as well as a darker and heavier fort.

The liquid thus racked off contains in folution what may be named a magnefiac liver of fulphur.

Some powdered nutgalls being mixed with this liquid, and afterwards fome alum, the water is by their ftiptic quality rendered incapable of holding the magnefiac liver of fulphur in folution: the latter is therefore precipitated, but not decompofed.

One

One ounce of this ſolution of magneſiac liver of ſulphur, mixt with a quart of pure water free from any ſtiptic or acid mixture, makes a medicinal ſulphureous water fit for uſe. If an acid be added to it, it decompoſes the liver of ſulphur, uniting with the magneſia to form a ſal catharticus amarus. Fixed air would therefore be an improper addition to this medicinal water.

A grown perſon may take of this medicinal water, at firſt, half a noggin twice in the day; and gradually increaſe the quantity to three noggins in the day. I have not known it to cauſe the head-ach in any perſon except myſelf; and I have always been immediately relieved by taking ſix grains of camphor and ſix drops of ether in honey and water.

I have had ample experience of the efficacy of this medicinal water in the cure of thoſe diſorders which are ſometimes called the land ſcurvy, and ſometimes ſaid to proceed from impurities of the blood; ſuch as eruptions on the head; the herpes exedens; a white, dry, ſcaly ſcruf; and thoſe various infectious eruptions which in Scotland are named the ſibbens, and amongſt the common people of this country paſs under a variety of names.

The itch is alſo effectually cured by this water.

It has had remarkably good effects in the few caſes of ſcrofula in which I have had opportunity of trying it.

In

In every caſe of worms in which I tried it, and they have not been few, it has deſtroyed them; thoſe particularly called aſcarides. In ſome of thoſe caſes the patients were in a ſtate of high fever when they took this medicine. This is the only caſe in which I give this water whilſt ſymptoms of fever are perceivable.

I have alſo found this water to be very ſucceſsful in the cure of the chronic rheumatiſm.

I have thus, my dear Sir, noted down the particulars which you wiſhed me to commit to writing for you, and am

Your very affectionate,

Humble Servant,

EDWARD KENNEY.

The method of preparing the medicinal ſulphureous water from this ſtrongly impregnated liquor is very ſimple, being as follows, viz.

Mix one ounce of the impregnated liquor with twelve ounces of cold water.

This

THIS medicinal water ſhould be uſed with caution. Two ounces at a time may be, in general, a proper quantity for a perſon to begin with.

THE ſtrongly impregnated liquor, and the medicinal water prepared from it, may be kept a long time unimpaired.

Moviddy, *Cork*, *January* 28th, 1793.

EDWARD KENNEY.

On the SOLUTION of LEAD by LIME.

By ROBERT PERCEVAL, *M.D. M.R.I.A.*

IN the year 1787 I obferved that the lining of milled lead, which covered the infide of a water ciftern, was corroded, in fome places, fuperficially, in others, quite throughout, fo as to fuffer the water to efcape. Read June 1, 1793.

THE holes were fmall and ragged at their edges. The lead was about one-twelfth of an inch thick.

THE plumber, who was employed to repair it, imputed the accident to fome mortar which had accidentally fallen into the ciftern, and lain on its bottom a confiderable time; confidering this circumftance as worthy of fome inveftigation, I tried the following experiments with a view of afcertaining in what manner the corrofion took place, and particularly of determining how far it might be promoted by the contact of air.

These experiments were begun on the 26th of September 1788.

The following mixtures were then made:

N°. 1, 100 grains of lead filings with the fame quantity of lime and three ounces of diftilled water.—Thefe were put into a phial which was carefully corked, fo as to exclude the air.

N°. 2.—The fame quantities of lime, lead and water, in a bottle, which was left uncorked. A piece of lead wire, one-twelfth of an inch diameter, which weighed 30 grains, was put into each of thofe bottles.

N°. 3.—A fimilar piece of lead wire, with about two ounces of lime water, was put into a phial, which was corked. The phial contained fome air.

August 24th, 1790. The contents of thefe phials were examined. The furface of the wire in N°. 1. appeared bright and metallic; its weight was thirty grains exactly: Hepatic air, paffed through the liquor, fcarcely produced any tinge. The piece of wire in N°. 2. weighed twenty-eight grains; this was covered with a whitifh grey fcale, which was fcraped off before the lead was weighed; the water of the mixture had been, at fome time which I could not afcertain, fpilled by the fall of the phial; the lime at the bottom of the phial appeared flightly cauftic. The wire in N°. 3. was covered with a cruft like that in N°. 2; this cruft being feparated, though not completely, by bending the wire backwards and forwards, the wire weighed twenty-nine grains.

August

August 30th, 1790. The mixture of lime and lead in N°. 2, which was now dry, was triturated with one ounce of diſtilled water, and filtered; the liquor on being expoſed to air was ſoon covered with a pellicle like lime water; on paſſing hepatic air through it, it acquired a ſlight browniſh tinge.

August 6th, 1791. Six hundred grains of lime, and the ſame quantity of lead, cut ſmall, were put into a phial, with about five ounces of water; this was ſuffered to ſtand, corked, until the 9th of October 1792; the liquor was then poured off; when filtered it ſtruck a ſlight browniſh colour with hepatic air; eight ounces of diſtilled water were boiled with the reſiduum; the filtered liquor, by evaporation, yielded an extract of ſeven grains; marine acid was added to this extract: the ſolution was not complete; a powder, probably plumbum corneum, remaining at the bottom of the veſſel, which weighed two grains.

On Auguſt 6th, 1791, the ſame quantities of lead and lime, as in the former experiment, were made into a paſte with diſtilled water; this was ſuffered to dry in the open air, and the lime cake, containing lead, was examined on the 9th of October 1792; it was then dry, but during the abovementioned interval of fourteen months it had been wetted two or three times. When examined it weighed fifteen hundred and ninety grains; each ſmall particle of lead appeared ſurrounded with a yellowiſh ring, which extended to ſome diſtance in the lime

 cake;

cake; this cake was powdered and boiled with ſix ounces meaſures of diſtilled water for half an hour. The filtered liquor ſtruck a deep black colour, with hepatic air and ſolution of hepar ſulphuris; and depoſited a white precipitate on the addition of marine acid, and a ſolution of neutral arſenical ſalt. From the encreaſed weight of the cake it appears probable that it had attracted fixed air from the atmoſphere.

From the foregoing experiments it was inferred that lime acts imperfectly, perhaps not at all, upon lead, without the aſſiſtance of air to calcine the metal.

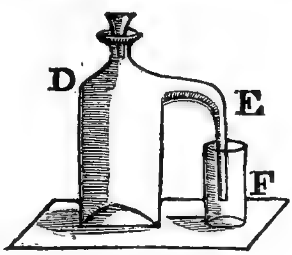
The following experiments were made in order to aſcertain the action of lime upon lead, in different ſtates of calcination. I took ten grains of finely powdered lithrage *a*, the ſame quantity of calx of lead precipitated from nitrous acid by volatile alcali *b*, and the ſame weight of minium *c*; to *a* and *b* were added four ounces by meaſure of lime water made by boiling lime with the pureſt diſtilled water. The ſpecific gravity of this lime water at the temperature 60 was 1003; to *c* were added four ounces and a half of the ſame lime water.

The mixtures were all put into phials which contained them exactly; to each of theſe ſtoppers were carefully adapted, ſo ſo as to exclude the air completely; the phials were placed in ſand, which, for ſeveral days together, was heated to one hundred degrees. After they had ſtood for thirty days, during which time they were often ſhaken, the liquors were filtered, and the powders that remained undiſſolved

were

were carefully collected on filtering papers; these, with the powders upon them, were dried and weighed; the papers were then exactly cleared of the powders, and again weighed; by this means the residuum of *a* was found to be 7, 9 grains; of *b* 6, 6 grains, and of *c* 8, 3 grains; so that four ounce measures of pure lime water dissolved of litharge 2, 1 grains, of calx 3, 4 grains, and four ounces and a half of the same liquor dissolved, 1, 7 grains of minium. All the filtered liquors struck a deep black colour with hepatic air, and let fall a white precipitate on the addition of marine acid.

To the production of the black precipitate, afforded by hepatic air, the presence of atmospheric, or rather vital air, appears to be necessary, as may be inferred from the following observation: Hepatic air was generated in the phial D, to the side of which was adapted the bent tube E, whose extremity, plunged to the depth of between two and three inches under the surface of the liquors above-mentioned, which were severally put into the small glass jar F. The phial being then stopped, the stream of hepatic air, issuing through the tube, passed through a considerable part of the liquors and escaped at their surface; there the black colour first appeared; the transparency of the lower parts was not disturbed, unless by the subsidence of the precipitate formed at the top.



The same conclusion is suggested by the following observation. On lifting up the tubulated phial, part of the liquor remained suspended fuſpended in the tube; this liquor abſorbed hepatic air at its upper ſurface, and therefore mounted in the tube. No diſcoloration was perceived until a bubble of atmoſpheric air ruſhed through the liquor to ſupply the vacuum in the phial. The liquor then immediately became black.

We know that hepatic air is decompoſed by vital air, and ſulphur is precipitated. May not this ſulphur, thus ſet at liberty, unite with the lead (reduced in part by the inflammable air of the elaſtic fluid) and thus form a kind of galena in the humid way?

Lime water, added to a ſolution of ſugar of lead, firſt produces a precipitate, which it afterwards rediſſolves: On ſtanding, laminated cryſtals of an olive colour are formed.

On a NEW KIND of PORTABLE BAROMETER for MEASURING HEIGHTS. By the Rev. JAMES ARCHIBALD HAMILTON, D.D. M. R. I. A.

THE acknowledged conveniency and accuracy of barometrical menſurations have induced thoſe who combine with a taſte for experimental philoſophy, mathematical ſkill and a talent for calculation, to aim at facilitating this work, by rendering the proceſs for determining the actual differences of altitudes within all poſſible limits, from barometrical obſervations made at different ſtations, ſimple and expeditious. Read Dec. 3, 1791.

THIS they have effected by a few general and obvious precepts and tables conſtructed on the baſis of a theory deduced from actual obſervation, and confirmed or amended by accurate and repeated trials; theſe helps, added to the variety of improvements made on the conſtruction of the portable barometer, and the excellent method methods contrived for its adjuſtment and uſe, have enabled enterpriſing and perſevering men to aſcertain facts with reſpect to the heights of mountains, which, though very material to the perfecting of geography and many other deſirable objects in natural philoſophy, would otherwiſe in all probability have long eluded our reſearches, from the inſupportable difficulty and fatigue that obſtruct all attempts at geometrical meaſurements of this kind; but the inſtruments and tables are ſufficiently accurate and complete to enable the mechanical and expert, who will not be debarred by a little neceſſary labour of previous adjuſtment and concommitant attention, from applying both in their preſent forms to attain their end. Yet even theſe require ſome conſiderable object to reward their pains, and there are, beſides, very many to whom a general ſection of the country they live in, and even the particular altitudes of their own grounds, would be both uſeful and deſirable; who, nevertheleſs, are debarred from the enquiry by an apprehenſion that a good deal of nice and delicate adjuſtment is an almoſt inſurmountable difficulty in unpractiſed hands, and that where ſo much is required of previous ſtudy and knowledge, the reſults are ſo liable to uncertainty as not to be perhaps worth the labour of the inveſtigation; admitting then that in the hands of a De Luc or a Sauſſure the accuracy and preciſion of the adjuſtments of a Ramſden will be done ample juſtice to, yet ſtill, as we have not every where ſuch hands, ſuch inſtruments, or ſuch objects as Mountblanc to employ them on, it cannot ſurely be denied that it may be very deſirable, even as a philoſophical amuſement, to put within every perſon's reach the buſineſs of making an accurate ſection of a whole county, province or kingdom, whoſe greateſt elevations do not exceed

two

two or three thoufand feet above the level of the fea, of marking regularly and truly the feveral inequalities of the grounds, and of fhewing their actual differences of heighth within a few inches, by an inftrument of no comparative price, that requires in no cafe whatever the fmalleft adjuftment, hardly any previous fkill in its application; is liable to fcarce any accident or error, and is not more cumbrous than a common walking ftaff.

If it fhall appear that fuch an inftrument, within the limits affigned, is capable of extreme accuracy, and anfwers equally well all the purpofes of the complicated, expenfive and operofe, I fhould hope that this improvement would be favourably received by the more fkilful and learned, and afford an incitement to others to enter upon an eafy experiment that may lead to many valuable difcoveries in the courfe of their refearches.

I shall now proceed to the manner of making and ufing this kind of portable barometer, and fhall add fome remarks on the peculiar and confiderable advantages of its conftruction. It may be proper to premife that the principle on which this inftrument acts is this; that corkwood is a fubftance, the pores of which afford a ready and free paffage to the particles of air, while at the fame time they are too fmall to fuffer thofe of quickfilver to to efcape, except indeed fome particular and powerful means are ufed to force them through its interftices.

The barometer confifts of a tube not much more than thirty inches long, an ivory cylinder about two inches in length, and upwards of one inch in diameter, open at one end, clofed at the other by a cover that is to be fitted on with a ſcrew, ſo fine and true as to prevent the eſcape of any quickſilver when the inſtrument is put together.

A SOUND, clean and porous cork, of about three-fourths of an inch in length, and one in diameter, ſhould be very nicely fitted to enter with a moderate preſſure at the bottom of the ivory cylinder, which ſhould be turned ſo truly throughout that the cork may be puſhed up to the extremity of the open, where there ſhould be left a ſmall ſhoulder to ſtop the farther progreſs of the cork, and to retain it in its proper place. When the cork is in this ſituation it ſhould be carefully bored with a circular file to receive the end of the glaſs tube tightly through its axis, ſo that the end of the tube may riſe beyond it, and project about half an inch into the empty part of the cylinder, and that the axis of the tube, and of the cylinder, may be exactly in the ſame right line.

THE tube ſhould be then carefully filled in the uſual manner, and the mercury poured over the end into the ivory cylinder till ſuch a quantity is admitted as may be ſufficient, when the lid is ſcrewed down tight, to cover the end of the glaſs tube in any poſſible poſition of the inſtrument: to wit, when held either parallel, oblique, or perpendicular to the horizon, a bored mahogany ſtaff with a braſs ſcale and vernier, a thermometer caſe, and caps of braſs to ſlide or ſcrew on each end, is to be prepared to receive the barometer and its attached thermometer, which being firmly and carefully introduced and fitted to their places, the whole is completed and fit for uſe.

FIGURE I.

A B reprefents a fection of the barometer longitudinally, when put together and ready for ufe. F the ivory cylinder. C D the fcale, with a vernier that flides fo as to cover the operture when the inftrument is put by. E the attached thermometer in its cafe, G G the brafs caps that fecures the ends.

FIGURE II.

A B reprefents a fection of the ivory cylinder with its cork C, and its tube T S the furface of the mercury. M the mafs of mercury, E E the fhoulders that keep the cork C in its place; and F F its bottom that fcrews on tight.

To ufe the inftrument, you have nothing to do, but taking it lightly between the finger and thumb of the right hand near D, fig. I. gently turn up the point A, and looking through oppofite to the light (for the outer cafe is to be cut to give this advantage, and alfo for the purpofe of a fcale and vernier divifion on each fide) you will obferve the mercury to fink gradually and gently to its due ftation; and with your left hand following the fubfiding furface of the mercury with the bottoms of the vernier flide (which are to be made exactly fquare and of the fame length) determine their contact with the top of the quickfilver when fettled, and finally read off the obfervation, on one or both ſides of the inſtrument. Note, for greater accuracy this proceſs may be repeated two or three times, and ſhould the ſeveral obſervations vary, a mean of all may be taken.

Remarks on the Conſtruction and Uſe of the Inſtrument.

REMARK I.

On the conſtruction of this inſtrument very little need be ſaid, as it is ſo obviouſly ſimple and eaſily to be apprehended. It may not, however, be uſeleſs to remark that the author has, by the uſe of a variety of theſe inſtruments for a number of years, ſatisfied himſelf and ſeveral ingenious and philoſophical friends, that the permeability of cork to air, and at the ſame time its reſiſtance to the paſſage of mercury, is moſt ſatisfactorily aſcertained, a point which cannot be too ſtrongly inſiſted on in this buſineſs, as the ſucceſs of the experiment depends entirely on the truth of this fact; in making the inſtrument, great care ſhould be taken to provide ſound, ſmooth, and ſpungy corks, to fit the round of both tube and cylinder very accurately, and to be careful not to force them into their places too tight, which would not only endanger breaking the tube, but alſo make the riſe and fall of the mercury, on inverting the barometer, too tedious. It will be alſo requiſite, in filling the ciſtern, to obſerve accurately the quantity of mercury that will ſuffice to keep the end of the tube covered in all poſitions, and at the ſame time

time leave the greateſt poſſible room for the reception of the falling quickſilver.

R E M A R K II.

On the Adjuſtment of the Inſtrument.

THIS Inſtrument is adjuſted, once for all, at the time of making it, in the following manner:

MEASURE very exactly the inner diameter of the ivory cylinder, which ſhould be turned throughout perfectly true; the inſide of the cover very ſhallow and of the ſame dimenſions with the reſt of the cylinder. You are likewiſe to meaſure the diameter of the aperture of the glaſs tube, which ſhould be alſo choſen truly cylindrical and well drawn. Theſe diameters being known, a very eaſy calculation will ſhew what the correction of the ſcale muſt be within any aſſigned limits; that is to ſay, what the elevation of a fluid in a cylinder of one inch diameter will be by pouring into it the contents of another cylinder of one-tenth of an inch in diameter, and three inches or any other given quantity in height. There will in the preſent caſe be required another correction (viz.) an allowance for the ſpace occupied in the cylinder by the projecting part of the glaſs tube. Theſe calculations being made for every particular barometer, its ſcale ſhould be accompanied

panied with a ſmall table of corrections for converting the obſerved differences of altitude into the true to be applied at reading off the obſervations.

But as ſome perſons may chuſe to conſtruct theſe barometers who could not rely on their ſkill in making theſe calculations, they may find the above correction mechanically, as follows: before you put the inſtrument together, let the tube exceed the required length, perhaps three or four inches: break off three inches, and reſerve them till the barometer is finiſhed; apply it in its caſe to the ſcale, and carefully note the height it ſtands at.

For greater ſecurity, let this obſervation be made three or four times, then take it out of the caſe or mounting, and opening the cylinder, without loſing any of the mercury, fill your reſerved tube of three inches, and pouring the contents into the cylinder, replace all in the mounting. Now obſerve the height the mercury ſtands at; the difference of this and the former height gives preciſely the effect on the level of the mercury in the ciſtern, occaſioned by the addition of three inches of the contents of the tube; and this quantity will ſerve as an argument to conſtruct a ſcale of correction for the inſtrument which cannot err.

It is to be obſerved, that when you have thus attained what may be called the error of the ſcale, you ſhould withdraw the mercury poured in, to leave the more room in the ciſtern, if wanted.

Example

Example of the above Process.

On constructing a barometer, with a cylinder of one inch in diameter, I observe the mercury to stand at 29, 0. I open the cylinder, and pour in three inches from a piece of the reserved tube, replacing the whole as before; I observe that the mercury now stands at 29, 12. It is obvious, from this experiment, that had I carried the barometer, previous to this infusion of mercury, to an height which would have occasioned the descent of three inches of mercury into the cistern, that the effect would have been precisely similar, *i. e.* that the mercury would then stand at 26, 12; and of course, in estimating all heights, the interval found by this barometer is in the first instance to be increased in the proportion of ,04 of an inch for every inch the mercury falls in the tube. It is easy to apprehend that a table may be readily formed to make these allowances at sight, as correct as can be desired.

On the advantage of this adjustment it is to be observed, that being once found, it remains perfectly free from any possibility of alteration, as neither the form or capacity of the tube or cistern are liable to alter. It saves the trouble of pouring mercury in or out of the cistern; the inconvenience of leather bags, that are constantly liable to go out of order and waste the mercury; and, besides, gets rid of all the errors of adjustment, arising either from friction or reading off; which the observer is liable

liable to in the uſe of the floating guage, or any other contrivance ſubſtituted in its room, that requires to be adjuſted at every ſeparate obſervation.

R E M A R K III.

On the Dimenſions of the Inſtrument.

As ſimplicity and portableneſs are the great advantages of this inſtrument, I would recommend the keeping the width of the aperture of the tube as much under as poſſible, as well to prevent accidental breaking from the weight of a thick column of mercury, as alſo to leave the more room for the deſcending mercury in the ciſtern. If the diameter of the tube is a full tenth of an inch, and that of the ciſtern 1, 2, there will be ample room to meaſure a height of 3000 feet, which is enough for any thing intended to be accompliſhed by this inſtrument. The ſhell of the glaſs ſhould be of ſtrong and tough metal, and the ſealing well annealed, as there is nothing but the gradual admiſſion of the air through the cork to check the force of the mercury againſt the top of the barometer on inverting it. It is alſo plain that the length of the tube ſhould be as little more than that of the greateſt uſual height of the mercury at the level of the ſea as poſſible, becauſe when the barometer is uſed all the ſuperfluous mercury that runs down from above the height will contract the neceſſary ſpace in the ciſtern.

REMARK IV.

On the Mercury.

THE mercury ſhould be perfectly well cleaned; this is beſt done by repeatedly waſhing it in a vial with ſucceſſive freſh waters, and when dried pouring it through a pin-hole made in a white paper cone.

THE mercury may (if required) be boiled in the tube; but I apprehend there are ſome conſiderable objections againſt this mode.

REMARK V.

On the Thermometers.

THE attention to the ſtate of the attached thermometer, ſo neceſſary in the uſe of any barometer for meaſuring heights, is particularly ſo in this; as from the ſize and material of the ciſtern or cylinder its dimenſions are liable to poſſible changes. To obviate this objection the ſides of the cylinder ſhould be made pretty thick and ſtrong, as otherwiſe they might be acted upon in ſome degree like Mr. De Luc's hygrometers, and a

 change

change in their dimenſions from expanſion or contraction might defeat the accuracy of obſervation. It were at all times deſirable, with every ſpecies of barometer, that the ſeparate obſervations ſhould as nearly as poſſible be made at the ſame temperature of the annexed thermometer. If this circumſtance is attended to the ſource of errors depending on this cauſe will entirely vaniſh.

REMARK VI.

On carrying and uſing the Barometer.

The only ſafe way of carrying this inſtrument is with the point downwards; the attached thermometer is to be looſe in its caſe with a quill ſcale, and a bit of cork or cotton within the cap for its bulb to reſt on; in this way I have both on horſeback and in carriages conveyed this inſtrument ſafely for many hundred miles.

From the conſtruction it is evident that, if accurately made, they will hang truly plumb when inverted and held lightly between the fore-finger and thumb. But this obvious advantage does not preclude many contrivances which might be thought of to hang them in gimmals, or ſuſpend them in any manner that might be thought more adviſable. This and a great many varieties in the conſtruction, which might be adopted without interfering with the ſimplicity of the principle, I leave at large to to the curious, and ſhall only add, that from continued and cautious experience it is manifeſtly certain that theſe barometers are as ſenſible and ſhew the ſmalleſt changes in the weight of the atmoſphere as accurately as thoſe whoſe ciſterns are actually open, and that I have tried them repeatedly againſt ſome of the ingenious and accurate Mr. Ramſden's provided with floating guages, verniers, &c. and the reſults have never varied two inches from each other in altitudes of above three hundred feet.

On Barometrical Menſuration.

THE inſtruments required for this purpoſe are two good barometers of a proper and ſimilar conſtruction, with two thermometers of Farenheit's ſcale to each, one attached to the barometer and covered in its caſe as near the mercury as poſſible, to determine the actual heat or cold and the conſequent expanſion or contraction of the mercury in the inſtrument. The other detached for obſerving the temperature of the air in the ſhade, and from thence to deduce the value in length of a column thereof equal in height to a given column of mercury in the barometrical tube.

☞ IF the heights to be meaſured are but ſmall, and the different ſtations acceſſible in a ſhort interval of time, one obſerver by going from place to place may determine them with ſufficient accuracy; but if either the heights or the intervals of the ſtations in diſtance make a conſiderable portion of time

 neceſſary

neceſſary to complete the obſervers, they ſhould unqueſtionably be made by ſeparate obſervations at the ſame inſtant of time, to be aſcertained either by ſignals or by a previous compariſon and adjuſtment of their ſeveral time-keepers.

The Things to be done are,

1ſt, To obſerve accurately the heights of the mercury in the barometer at the reſpective places of obſervation, and carefully to note the differences.

2d, To obſerve the temperature of the mercury in the barometer, by conſulting the attached thermometer of each barometer at the different places of obſervation.

3d, To note the temperature of the air, by obſerving alſo at each ſtation the ſtate of the detached thermometer in the ſhade.

THESE obſervations being carefully made, and the neceſſary allowances and calculations gone through, the reſult will give very correctly the difference of the actual height of *the two ſtations.*

To

To equate a given Column of Mercury in the Barometer ſo as to aſcertain its proportionate Length to a Column of 30 Inches with a Temperature of 55° of FARENHEIT'S *Scale.*

As the mercury in the barometer expands and contracts itſelf in proportion to the heat and cold of the atmoſphere, ſo that the ſpecific gravity of the metal is in fact different at different times, it is evident that the actual height of a column of the atmoſphere being given, if the temperature of the air as to heat and cold varies, the length of the rod of mercury ſupported by ſuch a given column will be longer or ſhorter in proportion to the greater or leſſer degree of atmoſpheric heat. To aſcertain therefore the variations in columns of mercury of different lengths, ariſing from the different temperatures of the atmoſphere, ſome given length and given temperature muſt be fixed on, as the term or ſtandard of compariſon. 30 inches of mercury, and 55° of Farenheit's thermometer, have been generally choſen for this ſtandard, as the one is pretty nearly the mean height of the mercury in the barometer at the level of the ſea, the other the point of the ſcale of Farenheit's thermometer, uſually in theſe climates marked temperate. If either or both of thoſe terms vary, the obſerved length of any column muſt be equated to reduce it to what it would be if the mercury ſtood at 30 in a barometer at the level of the ſea, and the thermometer at 55°. It has been proved, by very exact and repeated experiments, that the barometer, ſtanding at 30 inches, the expanſion produced in the whole column by a change of one degree of heat in the thermometer, is equal to ,00304 of an inch. On this calculation, for a variation of 33° of the thermometer above or below 55° you muſt

muſt retrench or add ,1 of an inch from or to the obſerved height of a column of 30 inches, and on the ſame principle it will be found that a variation of each degree of the thermometer above or below 55° expands or contracts the mercury ,00101333 of an inch in every column of the length of ten inches. The mercury therefore ſtanding at 30 inches, the correction for each degree of variation of temperature above or below 55° is in the proportion of ,1 of an inch to a variation of 33°; call this correction C. If the height of the mercury in the barometer alſo varies from 30, call the obſerved height A, and the correction ſought X, then ſay 30 : A :: C : X, therefore $\frac{AC}{30}=X$. To abridge theſe calculations a table has been conſtructed on the foregoing principles, which ſhews in decimals of an inch the effect of the expanſion or contraction for each ſingle degree of the thermometer above or below 55° upon the number of inches of ☿ marked in the firſt column.

TABLE I.

Inches of ☿	
10	,001013
20	,002027
30	,003040
40	,004053
50	,005067
60	,006080
70	,007093
80	,008107
90	,009120

To

To ufe this table, write out the decimals correfponding to the given height of the barometer in inches and decimal parts, lowering the places of the decimals for the odd inches and tenths above or below 30 inches. Add all together, and multiply the fum by the difference in degrees of the attached thermometer from 55°. This product applied to the obferved height—or+as the temperature of the barometer was above or below 55°, will give the correct height for the mean temperature. Note, this correction is to be regulated by the attached thermometer at every ftation and obfervation; and is intended with a view to afcertain and allow for the actual temperature of the mercury in the feveral barometers.

As the expanfive power of heat and the contraction of cold do alfo fo affect the atmofphere, that as thefe caufes vary a longer or fhorter column of the atmofphere, and of courfe different intervals of height, will be indicated at different times by a column of mercury of the fame length, and reduced as above to a ftandard temperature.

It therefore becomes neceffary to correct the differences of heights fhewn by the reduced columns of mercury at the different ftations, by a calculation founded on the effects of heat and cold on the atmofphere. The argument for this calculation is the mean temperature of the atmofphere, obtained by adding together the heights of the mercury in the detached thermometers at the different ftations, and dividing the fum by 2. This mean may be called an imaginary temperature.

TABLE

TABLE II.

1	,0024
2	,0048
3	,0072
4	,0097
5	,0121
6	,0148
7	,0170
8	,0194
9	,0218

As the ratio of the decreafe of denfity in the atmofphere is only conformable to the tabular logarithms and Englifh meafure in the temperature of 32° nearly of Farenheit's fcale, table the fecond is calculated to fhew the correction required for the rarefaction of each foot of the atmofphere for a fingle degree of heat above 32° in decimals of a foot, and may be applied to any number of feet, as for 4444,4

Example 4000	9,72000
400	,97200
40	,09720
4	,00972
,4	,00097

FROM

From the preceding obſervations and tables are deduced the following rules to determine the actual differences of the heights of the places of obſervation.

RULE I.

Reduce the temperature of the mercury in each barometer to the mean temperature at each ſtation.

RULE II.

Reduce the obſerved temperatures of the atmoſphere at the different ſtations to the imaginary uniform temperature.

RULE III.

Seek the common logarithms of each obſerved height, corrected by rule I. in inches and tenths, and reject the index.

Cut off the firſt four figures with a comma, and place the logarithms one under the other. The differences of the parts of theſe logarithms, preceding the comma, ſhew the actual differences of the heights of the ſtations in Engliſh fathoms; and the differences to the right of the comma in decimals of a fathom, provided the mean temperature of the atmoſphere (that

(that is, the imaginary uniform temperature) be nearly 32°; otherwife call this refult the approximate height. Multiply the approximate height by 6 to reduce it to feet.

RULE IV.

SEEK in the table, for correction of rarefraction of the atmofphere, the numbers anfwering to the numbers of feet in the approximate height. Multiply the fum of thefe numbers by the difference in degrees and decimals of a degree between the imaginary uniform temperature and 32°. If the imaginary unform temperature exceed 32°. add this product to the approximate height, and the fum will be the actual difference of the heights of the ftations in Englifh feet. This method of inveftigation is deduced from a paper of Dr. Mafkelyne's, founded on the calculation of Mr. De Luc and Sir George Shuckburgh; and to make it more practicable and lefs complicated the algebraic demonftrations are omitted. If General Roy's calculations of the effects of heat and cold on the atmofphere and on the mercury be more accurate than any former ones, they may be eafily adapted to thefe *formule*, and tables calculated from them.

POSTSCRIPT.

THIS communication on the fubject of the portable barometer was fome time fince fubmitted to the confideration of the learned and

and accurate friend whoſe valuable correſpondence accompanies it; and whatever its intrinſic value may be, the author has no doubt that, from its having ſuggeſted the following curious hints and obſervations, it is well worth being offered to the notice of the Academy. He is himſelf convinced, by the experience of many years, that it will act effectually and well in the form he deſcribes, which, as being by far the moſt ſimple, he therefore propoſes as moſt eligible. He muſt confeſs that in making theſe inſtruments a conſiderable degree of nicety is required, to adjuſt the cork to the box and tube in ſuch a manner, as to allow the air a ready acceſs to the ſurface of the mercury, and at the ſame time completely confine it in its box; and it has often occurred that by compreſſing the cork too tightly the riſe and fall of the mercury have been more gradual than were to be wiſhed. To remedy this, the author thought of, and put in practice, ſome contrivances that were fully ſufficient; but as he thinks none of them ſo complete as that ſuggeſted by his correſpondent, he will not increaſe the length of this paper by inſerting any of them. Having made numbers of the portable barometers in their ſimpleſt forms, compared their variations with thoſe of the very beſt open barometers, and found them to correſpond exactly; having alſo carried them ſome thouſands of miles, moſtly in a carriage, but often on horſeback, without injury to any of them, he is inclined to think that, in the improved form, they ſhould be adopted for general uſe, and may be readily and univerſally made upon the plan of a corrected ſcale, as ſet forth in the following letter by his ingenious friend. Should any perſon wiſh to try the experiment therein mentioned, relative to the abſorption of air by mercury, it will make it ſtill more deciſive and ſatisfactory. If after the ciſtern is finally cloſed, it, and about half an inch of the tube immediately adjoining it, be dipped in melted wax and ſuffered to cool, and this repeated three or four times.

A LETTER *to the* AUTHOR *of the preceding* PAPER, *with* REMARKS *and* HINTS *for the* FURTHER IMPROVEMENT *of* BAROMETERS.

By H. HAMILTON, *D. D. Dean of Armagh, F.R.S.* *and M. R. I. A.*

DEAR SIR,

I HAVE read the account you ſent me of your portable barometer, and as you deſire my opinion of that inſtrument I ſhall give it very freely. The form and ſtructure of your barometer is as ſimple and convenient as can be. The ivory box is ſo cloſed by a cork, through which the tube paſſes, that the mercury cannot get out, however the inſtrument is placed or agitated. But it ſeems to me that the cloſeneſs of the cork, which is ſufficient to prevent the mercury from eſcaping, will alſo prevent the free communication that ought to be between the Read Dec. 1, 1792.

the outward air and that in the box. And even ſuppoſing the pores of the cork were at firſt ſufficiently permeable by air, yet they may be in time obſtructed by duſt, the cork may imbibe moiſture which will contract or ſtop its pores; and, as there is no hole to drop in a floating gage, you cannot at any time meaſure accurately the height of the mercury, or be ſure it is the ſame that it would be were the barometer open. I would therefore recommend that, inſtead of a cork, the top of your box ſhould be of ivory, with a hole to drop in a floating gage, which is the caſe in all other portable barometers. This hole you may occaſionally ſtop with a peg or ſcrew, and then the inſtrument will be ſafely portable: or perhaps it might be better to have a cover to ſcrew over the top of the box, and a hole in it to correſpond with the one in the box. When theſe two holes are together the box is open; and it is ſhut when the holes are removed from each other by turning the cover and ſcrewing it tight to the top of the box, and if there be a plate of ſoft leather between them, it will be ſufficient to keep in the mercury when the inſtrument is agitated by carriage. That I might let you know whether this ſcheme would ſucceed I have had a barometer made in this form, and find it anſwers all the purpoſes of an open and of a portable one. The tube is not incloſed, like your's, in a mahogany ſtaff, but fitted in a frame of the uſual form. There have been various other methods propoſed for making barometers portable, but all thoſe I have met with are of a conſtruction more complex than is neceſſary. I have ſeen one made for the late Doctor Uſsher by Nairne and Blount,

Blount, in the manner ſaid to be moſt approved of by the Royal Society. In this inſtrument the box has a leathern bag or moveable bottom, which being ſcrewed up raiſes the mercury till it fills both the box and the tube; then the hole made for admitting the floating gage is ſtopped and the inſtrument becomes portable. Theſe contrivances for keeping the box and tube full of mercury ſeem to have been thought neceſſary from a miſtaken notion, that if air was included in the box its elaſticity would (when the inſtrument was ſuddenly inclined) force the mercury againſt the top of the tube ſo violently as to break it, which has often happened in an open barometer; but this is not the caſe, for I have ſeen your cloſe barometer ſuddenly inclined, and the included air did not make the mercury ſtrike the top of the tube with any violence. I am therefore of opinion that your barometer, if the box was made to be occaſionally opened or ſhut, would have the moſt ſimple and convenient form, and would be leſs liable than any other to be put out of order, or to require readjuſtment or repairs, as I am told Doctor Uſsher's barometer, now in the obſervatory, frequently does. The true altitude of the mercury, in a barometer, is the diſtance between the ſurface of the mercury in the tube and in the box; when therefore the ſurface in the box is ſo large that it will not riſe or fall ſenſibly, as the mercury falls or riſes in the tube, the common ſcale, if rightly adjuſted at firſt to the height of the mercury, will continue to point out its true height afterwards. This is the caſe in fixed barometers, which have uſually very large veſſels to hold the ſtagnant

ſtagnant mercury. But in theſe portable barometers with narrow boxes, though the common ſcale be adjuſted at firſt to the true height of the mercury, it will not ſhew its true height afterwards when the mercury has riſen above, or fallen below, that point or diviſion of the ſcale where it ſtood at firſt. For as the box is narrow, the height of the mercury will vary in the box whenever it varies in the tube, and it is the ſum of theſe two variations that gives the true variation which has taken place in the height of the elevated mercury. Now the ſcale annexed to the tube only ſhews one of theſe variations; and therefore when the mercury ſtands above or below the diviſion of the ſcale to which its real height was at firſt adjuſted, we cannot tell, by merely inſpecting the ſcale, how many aliquot parts of an inch the height of the mercury has varied, or how much it differs from the height it had when it ſtood at that diviſion to which it was adjuſted at firſt. Conſequently when the mercury departs from that diviſion the ſcale will not ſhew its true altitude in inches and aliquot parts of an inch.

To correct this error of the ſcale, by which the variations in the height of the mercury alway appear leſs than they really are, you propoſe that tables ſhould be formed which may ſhew what additions ought to be made to each particular variation. This however might be done in an eaſier way than by tables previouſly calculated: For when you have found the proportion between the ſurface of the mercury in the box and that in the tube, ſay as the ſurface in the box is to the ſum of the

the two ſurfaces, ſo is the apparent variation in the tube to the ſum of the variations in the tube and box, which gives the true variation. But as applying this correction to all the ſeveral variations in a ſeries of obſervations would be trouble-ſome and tedious, I think it would be much better to form, at once a ſcale which ſhould need no correction; and this may be done by reducing the common ſcale of inches, that is, by making a ſcale whoſe diviſions ſhall be leſs than the cor-reſpondent diviſions of the common ſcale, in the ſame proportion that the apparent variation in the tube of your barometer is leſs than the true one; and this proportion is always conſtant in the ſame barometer; for it is that proportion which the ſurface of the mercury in the box bears to the ſum of its ſurfaces in box and tube. If this contracted ſcale be annexed to the tube of the portable barometer, it is evident that, when the mercury has varied through any of the contracted diviſions of this ſcale, it will have varied, at the ſame time, through the correſponding diviſions of the common ſcale annexed to the tube of a fixed barometer. Therefore this contracted ſcale will always point out the variations and altitudes of the mercury truly, or ſuch as the common *inch-ſcale* ſhews them to be at the time in a fixed barometer whoſe box is of the largeſt dimenſions. To illuſtrate this by an example: ſuppoſe that in a portable barometer the ſurface of the mercury in the box is to that in the tube as 49 to 1, then it will be to the ſum of the ſurfaces as 49 to 50; and when the mercury in the tube falls through $\frac{49}{50}$ of an inch, it will riſe in the box $\frac{1}{50}$; ſo that its true fall, at that time, will be one inch. If then

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to this barometer a ſcale be adapted in which a line $\frac{49}{50}$ of an inch be made to repreſent one inch, when the mercury falls through the length of this line its altitude will be really leſſened by one inch. And thus the diviſions of this ſcale will repreſent the true variations and altitudes of the mercury in inches, as correctly as the common ſcale can do in any large fixed barometer. This corrected ſcale is to be divided into aliquot parts ſimilar to thoſe in the common *ſcale of inches*; and to its diviſions are to be annexed the ſame figures or numbers that are annexed to the correſponding diviſions of the common ſcale.

The eaſieſt, and, I believe, the moſt accurate method of forming a correct ſcale for a portable barometer, is this: put it up by a fixed barometer, whoſe veſſel, for the ſtagnant mercury, is ſo large that you may be ſure the ſurface of the mercury in it will not riſe or fall perceptibly on its riſing or falling in the tube; ſo that the common ſcale, annexed to this large barometer, will always point out the true variations and altitudes of the mercury in the tube. Mark, at the ſame time, the points at which the mercury ſtands in the tube of each barometer. When you find that the mercury in the fixed barometer has varied through any given ſpace, ſuppoſe one inch, then take accurately the length of the ſpace through which it has varied at that time in the portable barometer; this will be the length of a line which is to repreſent one inch in the correct ſcale for that portable barometer. That this obſervation may be accurate it ſhould be repeated often. In this

way

way of making a ſcale we avoid the trouble of meaſuring exactly the diameters of the box and of the tube, and of its orifice or bore, and of finding out from thence what is to be the length of our corrected ſcale. Inſtead of this we have only the length of one ſpace or line to meaſure, and this gives the length of our ſcale without any calculation. It is ſo convenient to have a correct ſcale, ſuch as I have mentioned, for a barometer, and the method of making one is ſo ſimple and obvious, that we may wonder it has not long ſince been known and practiſed.

We ſee that, according to this ſcheme, every portable barometer muſt have a ſcale made purpoſely for itſelf, and a vernier adapted to that ſcale; ſo that to get ſuch a ſcale made we muſt beſpeak it, and tell the proportion we would have its aliquot parts bear to thoſe of the common *inch-ſcale.* If it be thought that this is any inconvenience, and that it would be deſirable that all portable barometers ſhould uſe one common ſcale, which might be had ready made with a vernier adapted; this is a thing that may be eaſily effected, if it was generally agreed what the length of that common ſcale ſhould be. I would therefore propoſe, for inſtance, that the length of the ſcale ſhould be $\frac{1}{50}$ leſs than the ſcale of three inches now in uſe, which would be no great diminution. And in this caſe an artificer would have a very eaſy rule by which he might ſo conſtruct his barometers, that the ſcale now propoſed ſhould anſwer for them all. The rule is this, meaſure the external diameter of the tube you intend to uſe, and the diameter of its orifice or bore;

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make a right-angled triangle, one ſide of which ſhall be equal to the diameter of the tube, and the other ſide ſeven times the diameter of the bore, the hypotenuſe will be the proper diameter for the box, ſo that the ſcale now propoſed ſhall be a correct ſcale for that barometer. The reaſon of this is plain: For in the barometer, thus conſtructed, the ſquare of the diameter of the box is equal to the ſquare of the diameter of the tube, and alſo to 49 times the ſquare of the diameter of the bore; therefore (ſince circles are as the ſquares of their diameters) the area of the box is equal to the area occupied by the lower end of the glaſs tube, and to 49 times the area of the bore of the tube. And therefore the annular area in the box, occupied by the ſurface of the mercury, is 49 times the ſurface of the mercury in the tube, conſequently it is to the ſum of theſe two ſurfaces as 49 to 50, and therefore it follows, from what has been ſaid, that the propoſed ſcale, whoſe length is to that of the common ſcale as 49 to 50, will be the proper correct ſcale for this barometer.

The foregoing rule, when expreſſed in general terms, will direct us how to conſtruct a portable barometer, whoſe contracted or correct ſcale ſhall bear any given proportion we pleaſe to the common ſcale of inches. Take two numbers, differing by a unit, the leſſer of which ſhall be to the greater in the proportion we intend the contracted ſcale ſhall have to the common one: then as a unit is to the leſſer of theſe numbers, ſo let the diameter of the bore of the tube be to another line; between this line and the diameter of the bore find a mean proportional,

proportional, and make it one ſide of a right-angled triangle, and let the other ſide be equal to the diameter of the tube. The hypotenuſe will be the diameter that the box of the barometer ought to have, in order that the propoſed ſcale may be the proper ſcale for it.

Portable barometers have the advantage of being filled with leſs trouble than the common ones; for when the tube is filled, we have nothing more to do than to pour into the box as much mercury as we are ſure will cover the orifice of the tube, in whatever poſitions the inſtrument may be placed, and then ſcrew the cover on the bottom of the box with a collar of leather to prevent the mercury from getting at the threads of the ſcrew. The upper part of the box, which is ſolid, ought not to be leſs than $\frac{3}{4}$ of an inch in length, that it may take a ſufficient hold of the tube cemented into it. The end of the tube ſhould go into the cavity of the box ſo far as the middle of its length, and we ought to pour into the box as much mercury as will leave only $\frac{1}{4}$ of an inch in length to be occupied by the air when the barometer is erect; this ſpace will be ſufficient to allow the mercury in the tube to fall through ten or twelve inches, which will be full enough for meaſuring the heights of any places to which we uſually have acceſs, and we may be then ſure we have put in as much mercury as will cover the orifice of the tube in any poſition of the inſtrument. One reaſon, I believe, why it was thought neceſſary that air ſhould be excluded from the box of a barometer, while it was carried from one place to another, was, that the mercury would

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be more apt to imbibe the air into its pores when they were agitated together by the carriage. If, on this account, it be thought beſt to prevent ſuch agitation, it may be done more eaſily than by any of the contrivances I have met with; for, when the gage-hole is ſtopped, invert the inſtrument, unſcrew the bottom of the box, and put in a piece of cork that may fill the ſpace which was occupied by the air, and the cover being ſcrewed on again will keep all tight. The cork having a thread put through it will be eaſily removed, and it ought to go into the box ſo eaſily as to let the air paſs out by its ſides. I have not met with any experiments made to ſhew what quantity of air mercury will abſorb after being well purged of air. An experiment for this purpoſe may be conveniently and accurately made in the following manner: As ſoon as a portable barometer is filled with mercury, well purged of air, let it be hung up along with a thermometer in a cool place, where the temperature of the air is not like to vary; and, when the mercury has attained the temperature of the place, ſhut the box of the barometer and mark the height at which the mercury then ſtands. On this occaſion not more than $\frac{1}{10}$ of an inch in length ſhould be left for the air in the box. When the barometer has remained in this ſituation for ſome time (during which the mercury in it ſhould be now and then agitated), if it has imbibed any proportion of the included air, ſuppoſe $\frac{1}{10}$, the air will then have loſt $\frac{1}{10}$ of its elaſticity, and conſequently the column of mercury ſuſtained will loſe $\frac{1}{10}$ of its height, or will have deſcended in the tube about three inches. Thus the deſcent of the mercury will ſhew accurately the proportion of the air that has been abſorbed.

As

As you have turned my attention to this ſubject, I now ſend you ſuch remarks as have occurred to me; ſome of which may poſſibly be uſeful to thoſe who are employed in barometrical obſervations.

I am, dear Sir,
Your's, &c.
H. HAMILTON.

Dublin, February 6th, 1792.
To the Rev. Doctor J. A. Hamilton.

POSTSCRIPT.

I FIND the mercury in my portable barometer (now a conſiderable time in uſe) varies as freely, when the cover is ſcrewed cloſe to the top of the box, as it could do in any open barometer; for I never could perceive the leaſt alteration in the height of the mercury upon opening the hole in the box after it had been a long time cloſed, ſo that the air muſt have free acceſs to the box though it is cloſe enough to retain the mercury perfectly. The ſame thing may happen in other cloſe barometers, and when it does happen it is an advantage, as it ſaves the trouble of turning the cover and bringing together the holes in it and the box, whenever we would know the height of the mercury. I therefore thought this circumſtance worth mentioning. This kind of barometer will ſerve juſt as well at ſea as at land, and will ſupply what has been much wanted; as none of the contrivances for a marine barometer have been found to anſwer the purpoſe ſufficiently.

What are the MANURES *most advantageously applicable to the* VARIOUS SORTS *of* SOILS, *and what are the* CAUSES *of their* BENEFICIAL EFFECT *in each* PARTICULAR INSTANCE.

——— IDONEUS PATRIÆ, SIT UTILIS AGRIS.
JUVEN. SAT. 14.

AGRICULTURE is the art of making the earth produce the largest crop of useful vegetables at the smallest expense; it has often been remarked that, amidst the various improvements which most of the practical arts have derived from the progress lately made in natural philosophy and chemistry, none have fallen to the share of agriculture, but that it remains nearly in the same state in which it existed two thousand years ago. Read Jan. 4, 1794.

I am far from allowing the truth of this obfervation taken in its totality; to refute it we need only compare the writings of Cato, Columella or Pliny, with many modern Tracts, or ftill better, with the modern practice of our beft farmers; it muft be granted, however, that vague and fortuitous experience has contributed much more to the prefent flourifhing ftate of this art than any general principles deduced from our late acquired knowledge, either of the procefs of vegetation, or of the nature of foils; but the fkill thus fortuitoufly acquired is neceffarily partial, and generally local; the very terms employed by the perfons who moft eminently poffefs it are generally of a vague and uncertain fignification. Thus Mr. Young, to whofe labours the world is more indebted for the diffufion of agricultural knowledge than to any writer who has as yet appeared, remarks that in fome parts of England, where hufbandry is fuccefsfully practifed, any loofe clay is called marle *; in others marle is called chalk †, in others clay is called loam ‡. Philofophic refearches have been made, not yet fufficiently noticed; much information may be derived from Monfieur Du Hamel, and much more from the well-directed experiments of Mr. Tillet §. Immenfe ftrides have been made in this career by the illuftrious Bergman; Dr. Prieftley's experiments have thrown a new light on this as well as on every other object of natural philofophy. Mr. Lavoifier's new theory explains many circumftances before inexplicable;

* Firft Eaftern Tour 178.
† 2 Bath. Mem. 192. 220.
‡ 2 Bath. Mem. 137.
§ Mem. Par. 1772.

inexplicable; diſcoveries of great importance have been made by Mr. Senebier and Dr. Ingenhouz; even Mr. Young has not always confined his attention to the mere practical part, but ſometimes happily extended it to objects of a more general and ſpeculative nature; but the fulleſt light, perhaps, has been thrown on this ſubject by the late diſcoveries of Mr. Haſſenfraz *.

If the exact connection of effects, with their cauſes, has not been ſo fully and ſo extenſively traced in this as in other ſubjects, we muſt attribute it to the peculiar difficulties of the inveſtigation; in other ſubjects expoſed to the joint operation of many cauſes, the effect of each, ſingly and excluſively taken, may be particularly examined; the experimentor may work in his laboratory with the object always in his view; but the ſecret proceſſes of vegetation take place in the dark, expoſed to the various and indeterminable influences of the atmoſphere, and require at leaſt half a year for their completion; hence the difficulty of determining on what peculiar circumſtance ſucceſs or failure depends; the diverſified experience of many years can alone afford a rational foundation for ſolid ſpecific concluſions. It cannot therefore be expected that new, deciſive and direct experiments ſhould be laid before the Academy within the time preſcribed for anſwering this queſtion. The reſolution of the firſt part muſt be deduced from a ſtatement

* Annales Chymigues, Vol. 13. 14.

of facts long established by multiplied experience; and that of the second by the application of more general principles to the explanation of those facts.—But before we proceed to either branch of this question the distinctions and denominations both of soils and manures must be exactly settled and accurately defined.

CHAP. I.

Of SOILS *and* MANURES.

SECT. I. *Of* SOILS.

LAND, considered as the basis of vegetation, is called *soil.*

SOILS consist of different combinations of two or more of the four primitive earths, namely, the calcareous (which I sometims call mild calx), magnesia, argill, and the siliceous. For a more accurate description of these I must refer to books of mineralogy, and shall only remark that by calcareous earths are meant chalk, and all stones that burn to lime; they are easily distinguished by their property of effervescing with acids.

MAGNESIA

Magnesia is never found alone; its diſtinguiſhing character conſiſts in affording a bitter ſalt, generally called Epſom ſalt, when combined with the vitriolic acid.

Argill is that part of clay to which this owes its property of feeling ſoft and unctuous, and of hardening in fire; it is difficultly ſoluble in acids, and ſcarce ever effervesces with them; when combined with the vitriolic acid it forms alum.

Siliceous earth is often found in a ſtony form, ſuch as flint or quartz, and ſtill more frequently in that of a very fine ſand, ſuch as that whereof glaſs is made; it does not efferveſce, nor is it ſoluble in any of the common acids.

To theſe we may add Iron, in that imperfect ſtate in which it exiſts when reduced to ruſt, and commonly called calx of iron.

The ſoils moſt frequently met with, and which deſerve a diſtinct conſideration, are clay, chalk, ſand and gravel, clayey loam, chalkey loam, ſandy loam, gravelly loam, ferruginous loam, boggy ſoil, and heathy ſoil, or mountain, as it is often called.

Clay is of various colours, for we meet with white, grey, browniſh red, browniſh black, yellowiſh or bluiſh clays; it feels ſmooth and ſomewhat unctuous; if moiſt, it adheres to the fingers, and if ſufficiently ſo it becomes tough and ductile.

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If dry it adheres more or lefs to the tongue, if thrown into water it gradually diffufes itfelf through it, and flowly feparates from it. It does not ufually effervefce with acids, unlefs a ftrong heat be applied, or that it contains a few calcareous particles or magnefia. If heated, it hardens and burns to a brick.

It confifts of argill and fine fand, ufually of the filiceous kind, in various proportions, and more or lefs ferruginous. The argill forms generally from 20 to 75 per cwt. of the whole mafs, the fand and calx of iron the remainder. Thefe are perfectly feparable by boiling in ftrong vitriolic acid.

Chalk, if not very impure, is of a white colour, moderate confiftence, and dufty furface, ftains the fingers, adheres flightly to the tongue, does not harden when heated, but, on the contrary, in a ftrong heat burns to lime, and lofes about $\frac{4}{10}$ of its weight; it effervefces with acids and diffolves almoft entirely therein. I fhall alfo add that this folution is not difturbed by cauftic volatile alkali, as this circumftance diftinguifhes it from magnefia—it promotes putrefaction.

Sand. By this is meant fmall loofe grains of great hardnefs not cohering with water, nor foftened by it; it is generally of the filiceous kind, and therefore infoluble in acids.

Gravel differs from fand chiefly in fize; however, ftones of a calcareous nature, when fmall and rounded, are often comprehended under that denomination.

Loam

LOAM denotes any ſoil moderately coheſive, that is, leſs ſo than clay, and more ſo than looſe chalk; by the author of the body of agriculture it is ſaid to be a clay mixed with ſand. Doctor Hill defines it an earth compoſed of diſſimilar particles, hard, ſtiff, denſe, harſh and rough to the touch, not eaſily ductile while moiſt, readily diffuſible in water, and compoſed of ſand and a tough viſcid clay. The definition I have given ſeems moſt ſuited to the different ſpecies I ſhall now enumerate.

CLAYEY LOAM denotes a compound ſoil, moderately coheſive, in which the argillaceous ingredient predominates. Its coherence is then greater than that of any other loam, but leſs than that of pure clay; the other ingredient is a *coarſe* ſand, with or without ſmall mixture of the calcareous ingredient. It is this which farmers generally call *ſtrong*, *ſtiff*, *cold* and *heavy* loam, in proportion as the clay abounds in it.

CHALKEY LOAM. This term indicates a loam formed of clay, coarſe ſand and chalk, in which, however, the calcareous ingredient or chalk much predominates. It is leſs coheſive than clayey loams.

SANDY LOAM denotes a loam in which ſand predominates; it is leſs coherent than either the abovementioned. Sand, partly coarſe and partly fine, forms from 80 to 90 per cent. of this compound.

GRAVELLY LOAM differs from the laſt only in containing a larger mixture of coarſe ſand or pebbles. This and the two

laſt

laſt are generally called by farmers, *light* or *hungry* ſoils; particularly when they have but little depth.

Ferruginous Loam, or *Till.* This is generally of a dark brown, or reddiſh colour, and much harder than any of the preceding; it conſiſts of clay and calces of iron more or leſs intimately mixed; it may be diſtinguiſhed not only by its colour, but alſo by its ſuperior weight; it ſometimes effervefces with acids, and ſometimes not; when it does, much of the irony part may be ſeparated by pouring it, when well dried, into ſpirit of ſalt, from which the iron may afterwards be ſeparated by alkalis or chalk.

——— Akin. To this are certain *vitriolic ſoils*, which, when ſteeped in water, impart to it the power of reddening ſyrup of violets. Theſe are generally of a blue colour, but redden when heated.

Boggy Soil, or *Boggs*, conſiſt chiefly of ligneous roots of decayed vegetables mixed with earth, moſtly argillaceous, and ſand, and a coaly ſubſtance derived from decayed vegetables. Of boggs there are two ſorts; the black, which contain a a larger proportion of clay and of roots more perfectly decayed, with mineral oil; in the red the roots ſeem leſs perfectly decayed, and to form the principal part.

Heathy Soil is that which is naturally productive of heath.

SECTION

SECTION II.

Of Manures.

Manure denotes any ſubſtance or operation by which a ſoil is improved. To improve a ſoil is to render it capable of producing corn, legumens, and the moſt uſeful graſſes.

The ſubſtances principally uſed as manures, are chalk, lime, clay, ſand, marle, gypſum, aſhes, ſtable dung, mucks, farm-yard dung, pounded bones, ſea-weed, ſweepings of ditches, old ditches. Other manures or top-dreſſings, as they are employed chiefly to promote the growth of vegetables, and not merely with a view of improving the ſoil, I omit.

The operations uſed to improve ſoils, are fallows, draining, paring and burning.

Of chalk, clays and ſand we have already treated.

Lime is a ſubſtance whoſe external characters and mode of production are well known. It differs from chalk and powdered limeſtone chiefly by the abſence of fixed air, which is expelled from theſe during their calcination. This air it greedily re-abſorbs from the atmoſphere, and all other bodies with which it comes in contact, and which can furniſh it; but it cannot unite with the air unleſs it is previouſly moiſtened. 100 parts quick-lime abſorb about 28 of water. It is ſoluble in about 700 parts of this fluid. To regain its full portion of air from the atmoſphere it requires a year or more, if not purpoſely ſpread out; it reſiſts putrefaction; but with the aſſiſtance of moiſture it reſolves organic ſubſtances into a mucus.

Marle is of three ſorts, calcareous, argillaceous, and ſiliceous or ſandy. All are mixtures of mild calx (*i. e.* chalk) with clay, in ſuch a manner as to fall to pieces by expoſure to the atmoſphere, more or leſs readily.

Calcareous Marle is that which is moſt commonly underſtood by the term *Marle* without addition. It is generally of a yellowiſh white, or yellowiſh grey colour, rarely brown or lead coloured. It is ſeldom found on the ſurface of land, but commonly a few feet under it, and on the ſides of hills, or rivers that flow through calcareous countries, or under turf in boggs—frequently of a looſe texture, ſometimes moderately coherent, rarely of a ſtoney hardneſs, and hence called *ſtone-marle*; ſometimes of a compact, ſometimes of a lamellar texture, often ſo thin as to be called *paper-marle*; it often abounds with ſhells, and then is called *ſhell-marle*, which is looked upon as the beſt ſort—when in powder it feels dry between the fingers,—put in water it quickly falls to pieces or powder, and does not form a viſcid maſs—it chips and moulders by expoſure to the air and moiſture, ſooner or later, according to its hardneſs and the proportion of its ingredients; if heated it will not form a brick, but rather lime; it effervеſces with all acids; it conſiſts of of from 33 to 80 per cent. of mild calx, and from 66 to 20 per cent. of clay.

To find its compoſition, pour a few ounces of weak, but pure ſpirit of nitre or common ſalt into a florence flaſk; place them in a ſcale and let them be balanced; then reduce a few ounces of dry marle into powder, and let this powder be carefully and gradually thrown into the flaſk, until after repeated agitation no effervefcence is any longer perceived; let the remainder of the powdered marle be then weighed, by which the quantity projected will be known; let the balance be then reſtored; the difference of weight between the quantity projected and that requiſite to reſtore the balance will diſcover the weight of air loſt during the effervefcence; if the loſs amounts to 13 per cwt. of the quantity of marle projected, or from 13 to 32 per cwt. the marle eſſayed is calcareous marle. This experiment is deciſive when we are aſſured by the external characters abovementioned that the ſubſtance employed is marle of any kind; otherwiſe ſome ſorts of the ſparry iron ore may be miſtaken for marle. The experiments to diſcover the argillaceous ingredient, being too difficult for farmers, I omit. The reſidue left, after ſolution, being well waſhed, will when duly heated, generally harden into a brick.

Argillaceous Marle contains from 68 to 80 per cwt. of clay, and conſequently from 32 to 20 per cwt. of aerated calx—its colour is grey, or brown, or reddiſh brown, or yellowiſh or bluiſh grey—it feels more unctuous than the former, and adheres to the tongue—its hardneſs generally much greater—in water

 it

it falls to pieces, more flowly, and often into fquare pieces—it alfo more flowly moulders by expofure to the air and moifture, if of a loofe confiftence; it hardens when heated, and forms an imperfect brick.—It effervefces with fpirit of nitre or common falt, but frequently refufes to do fo with vinegar—when dried and projected into fpirit of nitre in a florence flafk, with the attentions abovementioned, it is found to lofe from 8 to 10 per cwt. of its weight. The undiffolved part, well wafhed, will, when duly heated, harden into a brick.

Siliceous or Sandy Marles are thofe whofe clayey part contains an excefs of fand, for, if treated with acids in the manner abovementioned, the refiduum or clayey part will be found to contain above 75 per cwt. of fand—confequently chalk and fand are the predominant ingredients.

The colour of this marle is brownifh grey, or lead-coloured—generally friable and flakey, but fometimes forms very hard lumps,—it does not readily fall to pieces in water—it chips and moulders by expofure to the air and moifture, but flowly—it effervefces with acids, but the refiduum after folution will not form a brick.

Limestone Gravel: This is a marle mixed with large lumps of limeftone; the marle may be either calcareous or argillaceous; but moft commonly the former; the fandy part is alfo commonly calcareous.

Gypsum is a compound of calcareous earth and vitriolic acid; it forms a diſtinct ſpecies of the calcareous genus of foſſils, of which ſpecies there are ſix families.

The general character of this ſpecies are

1. *Solubility* in about 500 times its weight of water, in the temperature of 60°.

2. *Precipitability* therefrom by all mild alkalis, and alſo by cauſtic fixed, but not by cauſtic volatile alkali.

3. *Inefferveſcence* with acids if the gypſum be pure; but ſome families of this ſpecies, being contaminated with mild calx, ſlightly efferveſce.

4. *Inſolubility*, or nearly ſo in the nitrous acid, in the uſual temperature of the atmoſphere.

5. A *ſpecific gravity*, reaching from 2,16 to 2,31.

6. A degree of *hardneſs*, ſuch as to admit being ſcraped by the nail.

7. When heated nearly to redneſs it calcines, and if then it be ſlightly ſprinkled with water it again concretes and hardens.

8. It promotes putrefaction in a high degree.

Of

Of the ſix families of this ſpecies I ſhall deſcribe only one, namely that which has been moſt advantageouſly employed as a manure. Deſcriptions of the other five ſhould be found in treatiſes of mineralogy. It is called *fibrous gypſum.*

Its colours are grey, yellowiſh or reddiſh, or ſilvery white, or light red, or browniſh yellow, or ſtriped with one or more of theſe dark colours. It is compoſed of fibres or ſtriæ either ſtraight or curved, parallel or converging to a common centre, ſometimes thick, ſometimes fine and ſubtile, adhering to each other and very brittle—its hardneſs ſuch as to admit being ſcraped with the nail—commonly ſemitranſparent, in ſome often in a high degree.

Ashes. Sifted coal aſhes, thoſe of peat, and white turf aſhes, have been found uſeful. Red turf aſhes uſeleſs and generally hurtful. Wood-aſhes have alſo been employed advantageouſly in many caſes; they contain either the four primitive earths, as Mr. Bergman aſſerts, or calcareous earth chiefly, according to Achard, or calcareous and magneſia, according to D'Arcet. They alſo contain ſome proportion of phoſphorated ſelenite, *i. e.* calcareous earth united to the phoſphoric acid. Almoſt all contain alſo a ſmall and variable proportion of common ſalt, and Glauber's ſalt, and terrene ſalts, which, when in a ſmall doſe, all accelerate putrefaction; alſo ſmall bits of charcoal.

Charcoal is a ſubſtance well known; it has frequently and ſucceſsfully been uſed as a manure. 1ſt Young's Annals, 152, &c.

Soap

Soapboilers Waste forms an excellent manure for ſome ſoils; it contains, by Mr. Ruckert's Analyſis, 57 per cwt. of mild calx, 11 of magneſia, 6 of argill, and 21 of ſilex.

Stable Dung. This is uſed either freſh or putrified; the firſt is called *long*, the other *ſhort dung*; it abounds in animal matter, eaſily runs into putrefaction, and when putrified ſerves as a leaven to haſten the decay of other dead vegetable ſubſtances; its fermentation is promoted by frequent agitation and expoſure to the air: yet it ſhould be covered to prevent water from carrying off moſt of its important ingredients, or at leaſt the water that imbibes them ſhould not be loſt.

Farm-yard Dung conſiſts of various vegetables, as ſtraw, weeds, leaves, fern, &c. impregnated with animal matter; it ferments more ſlowly than the former; ſhould be piled in heaps, and ſtirred from time to time.—Fern putrefies very ſlowly—the water that iſſues from it ſhould be preſerved.

Some of theſe manures have been analyzed.

Table

Table of Contents of Manures.

105 lb.	Heavy Inflam. Air. Cub. Inchs.	Fixed Air. Cub. Inch.	Water. lb.	Coal. lb.	Calx and Magnesia. lb.	Argill. lb.	Silex. lb.	Vol. Alk. lb.	Fixed Salts. lb.
Fresh Cow-dung * -	— —	— —	— —	3,75	1, 2	0,15	2, 4	— —	0, 6
Fresh Horse-dung * -	— —	— —	88	10,2	1, 5	0, 5	3	— —	0, 21
Sheeps Dung * - -	— —	— —	— —	25,0	9,28 Calx. 1, Magn.	3	29	— —	0, 72
Rotten Cow-dung * -	1360	120	81	10	3.	0, 6	5.	0.65	Gypf. 0, 9 F. Salts 0, 24
Earth resulting from rotten Horse-dung †	1. 64	1.	Water and Oil 38,15	18,75	6, 2	1, 5	23,43		
Soapboilers Waste * -	— —	— —	— —	—	57 Calx. 11 Magn.	6,	21,		

* 2 Ruckert. † In Encycloped. Art. Vegetation. *Hassenfraz.*

HENCE

Hence they ſhoud be applied, not indiſcriminately, but according to circumſtances, to be indicated in the ſequel.

Pounded bones form alſo a manure much uſed in the neighbourhood of great towns. They gradually depoſite their oily part, which contains a large proportion of animal coal which is extricated by putrefaction, and phoſphorated calx. Hence Bone-aſh is alſo uſeful.

Sea-weed, particularly if mixed with earth, ſoon putrefies and makes a good manure.

Sweepings of Ditches abound with putrid matter from decayed vegetables, and hence form a manure.

Old Ditches, expoſing a large ſurface to vegetation, contain, when deſtroyed, a quantity of decayed vegetables, which putrefy and make a good manure; but in this and the former caſe, it may be proper to diſtinguiſh of what ſoil they are compoſed, for reaſons that will hereafter appear.

Fallowing is the principal operation by which exhauſted lands are reſtored to fertility; its uſe ſeems to me to conſiſt in expoſing the roots of vegetables to decay, whereby food for a freſh growth is prepared; the atmoſphere alſo depoſites fixed air and carbonaceous ſubſtance on earth long expoſed to it.

Draining is an operation equally neceſſary and well known, on which no more need be ſaid here.

PARING *and* BURNING reduces the roots of vegetables to coal and aſhes, and thus prepares both a ſtimulant and nutriment for plants, as will be ſeen hereafter.

CHAP. II.

Of the Food of Plants, and the Compoſition of fertile Soils.

HAVING in the preceding chapter explained the nature of the different ſoils known in agriculture, and of the different manures whoſe general utility has been aſcertained by long experience, we are now to enquire which of thoſe manures are moſt advantageouſly applicable to each of thoſe particular ſoils, and what are the cauſes of their beneficial effect in each particular inſtance.

To proceed with order in this enquiry, we muſt obſerve that the general effect expected from the application of manure is fertility; that is, the moſt copious production of corn and graſſes; and, ſince fertility is itſelf the reſult of the due adminiſtration of the food of thoſe vegetables, we muſt firſt ſee what that food is, and of what ingredients a ſoil ought to be compoſed in order to contain or adminiſter it; after which we

ſhall

ſhall indicate by what manures each particular ſort of ſoil is brought into a fertile ſtate, which is the beneficial effect expected from them, and how in each particular caſe they contribute to the due adminiſtration of the vegetable food which is the cauſe of their beneficial effect.

SECTION I.

Of the Food of Plants.

To diſcover the food of plants, particularly of thoſe which form the object of our preſent inquiry, we muſt examine the nature and proportion of the ſubſtances in which they grow, and of thoſe which they themſelves contain; thus we ſhall be enabled to ſee which of the latter are derived from the former.

First, All plants (except the ſubaqueous) grow in a mixed earth moiſtened with rain and dew, and expoſed to the atmoſphere; if this earth be chemically examined it will be found to conſiſt of ſiliceous, calcareous and argillaceous particles, often alſo of magneſia, in various proportions, a very conſiderable quantity of water, and ſome fixed air. The moſt fertile alſo contain a ſmall proportion of oil, roots of decayed vegetables, a coaly ſubſtance ariſing from putrefaction, ſome traces of marine acid and gypſum *. On the other hand, if vegetables be

 analyzed,

* Home, 15 Mem. D'Agriculture, Par. 1790. Encycloped. *Vegetation*, p. 277.

analyzed, they will be found to contain a large proportion of water and charcoal; alſo fat and eſſential oils, reſins, gums, and vegetable acids, all of which are reducible to water, pure air, inflammable air and charcoal; a ſmall proportion of fixed alkali is alſo found, ſome neutral ſalts, moſt commonly gypſum, tartar vitriolate, common ſalt, and ſalt of ſylvius. In corn, and particularly wheat, phoſphorated ſelenite is alſo found.

Hence we ſee that, on the laſt analyſis, the only ſubſtance, common to the growing vegetables, and the ſoils in which they grow, are water, coal, different earths, and ſalts: Theſe, therefore, are the true food of vegetables; to them we ſhould alſo add fixed air, though by reaſon of its decompoſition it may not be diſtinctly found in them, or at leaſt not diſtinguiſhable from that newly found during *their decompoſition.*

I shall now examine the ſeparate functions of each of theſe ingredients.

Of Water.

The agency of water in the proceſs of vegetation has never been doubted, though the manner in which it contributes to it has not, until of late, been diſtinctly perceived. Doctor Hales has ſhewn that in the ſummer months a ſun-flower, weighing three pounds avoirdupoize, and regularly watered every day, paſſed through it or perſpired 22 ozs. each day, that is, nearly $\frac{1}{2}$ its weight. He alſo found that a cabbage plant, weighing 1 lb. 9 ozs. ſometimes perſpired 1 lb. 3 ozs.

but

but at a medium about half its weight *. Doctor Woodward found that a fprig of common fpearmint, a plant that thrives moft in moift foils, weighing only 28,25 grs. paffed through it 3004 grs. in 77 days, between July and October, that is, fomewhat more than its own weight each day. He did more, for he found that in that fpace of time the plant increafed 17 grs. in weight, and yet had no other food but pure rain water. But he alfo found that it increafed more in weight when it lived on fpring-water, and ftill more when its food was Thames water †. From whence we may deduce that graffes and corn, during the time of their growth, abforb about one half their weight of water each day if the weather be favourable. Secondly, That the water they thus pafs nourifhes them merely as water, without taking any foreign fubftance into the account; for 3000 grs. of rain-water, in Doctor Woodward's experiment, afforded an increafe of 17 grains, whereas by Margraaff's experiments 5760 grs. of that water contain only $\frac{1}{3}$ gr. of earth ‡. But, Thirdly, It alfo follows, that water contributes ftill more to their nourifhment when it conveys to them earthy and faline particles, as fpring and Thames waters do.

The manner in which pure water contributes to the nourifhment of plants, befides the fervice it renders them in diftributing the nutritive parts throughout their whole ftructure, and forming, itfelf, a conftituent part of all of them, may be underftood from modern experiments. Doctor Ingenhouz and Mr.

* 1 Hales, 9. 10. 15. † 2 Phil. Tranf. Abr. 716. ‡ 2 Margr. 6. 70.

Mr. Senebier have ſhewn that the leaves of plants expoſed to the ſun produce pure air; now water has of late been proved to contain about 87 per cwt. of pure air, the remainder being inflammable air. Water is then decompoſed by the aſſiſtance of light within the vegetable; its inflammable part is employed in the formation of oils, reſins, gums, &c.; its pure air is partly applied to the production of vegetable acids, and partly expelled as an excrement.

Many indeed have aſſerted that water is the ſole food of vegetables; and among the experiments adduced to prove it, that of Van Helmont, quoted by the illuſtrious Mr. Boyle *, is by far the moſt ſpecious. He planted a trunk of willow weighing 5 lb. in an earthen veſſel filled with earth dried in an oven, and then moiſtened with rain water; this veſſel it appears he ſunk in the earth, and watered partly with rain water and occaſionally with diſtilled; after five years he found the tree to weigh 169 lb. and the earth in which it was planted, being again dried, to have loſt only 2 oz. of its former weight, though the tree received an increaſe amounting to 164 lb.

Before I proceed to the explication of this experiment, I muſt remark ſome circumſtances attending it: Firſt, that the weight of the earth contained in the veſſel at the commencement and at the end of five years could not be exactly compared,

* 2d Shaw's Boyle, 240.

pared, becaufe the fame degrees of deficcation could not be exactly afcertained, and becaufe many of the fibrillæ of the roots of the tree muft have remained in the earth after the tree was taken out of the veffel, and thefe muft have prevented the true lofs of earth from being perceived. Secondly, that the earthen veffel muft have frequently abforbed water impregnated with whatever fubftance it might contain from the furrounding earth in which it was inferted, for unglazed earthen veffels eafily tranfmit moifture, 1ft Hales 5, and Tiller's Mem. Par. 1772, page 298, 304, 8vo. Thirdly, as it appears that the pot was funk in the earth and received rain water, it is probable that diftilled water was feldom ufed.

These circumftances being confidered, it will eafily be made to appear that the rain water abforbed by the tree contained as much earth as the tree can be fuppofed to contain.

First, The willow increafed in weight 164 lb. in five years, that is, at the rate of 2,7 lb. nearly per month, and it being an aquatic it cannot be fuppofed to pafs lefs than its own weight of water each day during the fix vegetating months. In the firft month therefore it abforbed and paffed $5 \times 30 = 150$ lb. and as each pound of rain water contains $\frac{1}{3}$ gr. of earth, 50 grs. of earth muft have been depofited in the plant, and allowing no more than 50 grains for the depofite of each of the fix months, we fhall have $50 \times 6 = 300$ for the depofite of the firft year; but at the end of the firft year the plant gains an acceffion of 32 lb. therefore

fore in each of the fix fummer months of the fucceeding year it paffes × 3730=110 lb. of water, and receives a depofite of 310 grains, and at the end of the fecond year the depofite amounts to 2220 grains. At the commencement of the third year the tree gaining a farther acceffion of 32 lb. muft weigh 69lb. and pafs in each of the fummer months 69×30=270 lb. of water, and receive a depofite of 690 grains, which multiplied into 6=4140 grains. At the commencement of the fourth year the tree ftill gaining 32 lb. muft weigh 101 lb. and if it paffes 101×30 in each of the fummer months it muft gain a depofite in each of 1010 grains of earth, and at the end of the year 6060. At the commencement of the fifth year it weighs 133 lb. and gains at the end of the fix months 23940 grains of earth. The quantities of earth depofited each year exceed 5 lb. avoirdupoife, a quantity equal to that which 169 lb. of willow can be fuppofed to contain; for the commiffioners employed to infpect the fabrication of falt-petre in France, having examined the quantities of afhes afforded by trees of various kinds, found that 1000 lb. of fally, a tree much refembling the willow, afforded 28lb. of afhes, and confequently 169lb. fhould produce 4,7 ‡. I do not give this calculation however as rigoroufly exact; it is certain that if the depofite left at the end of every month were exactly taken the total would exceed the quantity juft mentioned, but that found even by this rude mode fufficiently proves that water conveys a portion of earth into vegetables equal to any that the experiments hitherto made can prove to exift in them.

As

‡ 3d Tranf. Royal Irifh Acad.

As to the coal or carbonaceous principle which this willow muſt alſo have contained, it is probable that much of it exiſted in the earth in which the willow grew; ſome is contained in all moulds or vegetable earth, and as we are not told what ſort of earth Van Helmont uſed, we may well ſuppoſe it was good vegetable earth, its quantity amounting to 200lb. This principle may alſo have been contained in the water, for the pureſt rain water contains ſome oleaginous particles, though in an exceeding ſmall proportion, as Mr. Margraaf has obſerved†, and all oil contains coal. Some alſo may have paſſed from the ſurrounding vegetable earth through the pores of the earthen veſſel. All the other experiments, adduced to prove that water is the ſole food of plants, may be explained in the ſame manner. Grains of wheat have been made to grow on cotton moiſtened with water; each produced an ear, but that ear contained but one grain*. Here the carbonaceous ſubſtance was derived from the grain and afterwards diffuſed and tranſported through the whole plant by the water abſorbed; for it muſt be obſerved that grain, like an egg, contains much of the nouriſhment of its future offſpring—it is thus that tulips, hyacinths and other plants, expand and grow in mere water.

The earth contained in rain-water is united partly with the nitrous and marine acids, as Margraaf has ſhewn, but far the greater part only with fixed air; for the feeble traces of the two former acids could not hold in ſolution the 100 grains of earth which he found in 300lb. of rain-water.

† 2d Marg. 15, 90. * 2d Young's Annals, 487.

By far the greateſt proportion of vegetable ſubſtances conſiſts of water; according to Mr. Young and Ruckert graſs loſes about $\frac{2}{3}$ of its weight on being dried into hay *. Dr. Hales found a ſun-flower plant, which weighed 48 ounces, to loſe 36 ounces by drying in the air during thirty days †, and conſequently to have loſt $\frac{3}{4}$ of its weight: even vegetables, to appearance thoroughly dry, contain from $\frac{3}{5}$ to $\frac{3}{4}$ of their weight of water ‡. This water is not all in a liquid ſtate, but by the loſs of much of its ſpecific heat is in great meaſure ſolidified.

Of Coal, or the Carbonic Subſtance.

To Mr. Haſſenfraz we owe the diſcovery, that coal is an eſſential ingredient in the food of all vegetables; though hitherto little attended to, it appears to be one of the primæval principles, as antient as the preſent conſtitution of our globe: for it is found in fixed air, of which it conſtitutes above $\frac{1}{4}$ part; and fixed air exiſts in limeſtones and other ſubſtances, which date from the firſt origin of things.

Coal not only forms the reſiduum of all vegetable ſubſtances, that have undergone a ſlow and ſmothered combuſtion, that is, to which the free acceſs of air has been prevented, but alſo of all putrid vegetable and animal bodies; hence it is found in vegetable and animal manures that have undergone putrefaction, and

* 2d Young's An. 26. 2d. Ruckert 139.
† 1ſt Hales, 8.
‡ Ruckert 28. Seneb. Encyclop. Vegetation, 52.

and is the true basis of their amelliorating powers; if the water that passes through a putrefying dunghill be examined it will be found of a brown colour, and if subjected to evaporation the principal part of the residuum will be found to consist of coal *. All soils steeped in water communicate the same colour to it in proportion to their fertility, and this water being evaporated leaves also a coal, as Mr. Hassenfraz and Fourcroy attest †. They also observed that shavings of wood being left in a moist place for nine or ten months began to receive the fermentative motion, and being then spread on land putrefied after some time and proved an excellent manure ‡. Coal, however, cannot produce its beneficial effects but in as much as it is soluble in water; the means of rendering it soluble are not as yet well ascertained; nevertheless it is even now used as a manure, and with good effect §. In truth the fertilizing power of putrid, animal and vegetable substances were fully known even in the remotest ages, but most speculatists have hitherto attributed them to the oleaginous, mucilaginous, or saline particles then developed, forgetting that land is fertilized by *paring* and *burning*, though the oleaginous and mucilaginous particles are thereby consumed or reduced to a coal, and that the quantity of mucilage oil or salt in fertile land is so small that it could not contribute the 1000th part of the weight of any vegetable, whereas coal is supplied not only by the land but also by the fixed air combined with the earths, and also by that which is constantly set loose by various processes, and soon precipitates by the superiority of its specific gravity, and is then condensed in, or mechanically

 absorbed

* 14 An. Chy. 56. † Ibid. ‡ Ibid. § Young's Annals.

abſorbed by ſoils, or contained in dew. Lands, which contain iron in a ſemicalcined ſtate, are thereby enabled to decompoſe fixed air, the iron, by the help of water, gradually attracting the pure air which enters into the compoſition of fixed air, as Mr. Gadolin has ſhewn *, a diſcovery which appears to me among the moſt important of theſe later times; but theſe calces of iron may again be reſtored to their former ſtate by union with oleaginous ſubſtances, as Mr. Beaumè has noticed, and this is one of the benefits reſulting from the application of dung before it has fully putrefied †. Hence we may underſtand how ſoils become effete and exhauſted, this effect ariſing in great meaſure from the gradual loſs of the carbonic principle depoſited by vegetable and animal manures, and from them paſſing into the growing vegetables, and alſo from the loſs of the fixed air contained in the argillaceous part of the ſoil, which is decompoſed by vegetables, and from the calcination of the ferruginous particles contained in the ſoil. I ſay in *great meaſure*, becauſe other cauſes contribute to the diminution of fertility, which ſhall preſently be mentioned. Hence alſo we ſee why lands paſtured remain longer fertile than thoſe whoſe vegetable crop is carried off, as much of the carbonaceous principle is reſtored by the excrements of the paſturing animals - - - - why ſome crops exhauſt more than others, becauſe corn, and particularly wheat, contains more of the carbonic principle than graſſes, and very little of its exuviæ are left behind - - - - why fallows are of ſome uſe, as the putrefaction of the roots of weeds and the abſorption of fixed air by clays, are thereby promoted - - - - why vegetables thrive

* 1ſt Chym. Ann. 1791. 53.

† The affinities of coal and iron to pure air vary with the temperature.

thrive most in the vicinity of towns, becaufe the carbonic principle is copioufly difperfed by the fmoak of the various combuftibles confumed in inhabited places—why foot is fo powerful a manure—why burning the clods of graffy land contributes fo much to its fertility, and then only when the fire is fmothered and coal produced, befides many other agricultural phænomena too tedious to relate; but I muft not omit that the phofphoric acid is found in coal, and this enters into the compofition of many vegetables.

THE quantity of coal in vegetables is various according to their various fpecies, age and degrees of perfection; wood and corn contain moft, graffes leaft. Wiegleb found dry beech wood to contain about $\frac{1}{5}$ of its weight of coal*. Weftromb found *trifolium pratenfe*, a fort of clover, to contain about $\frac{1}{7}$; hence after water it is the moft copious ingredient in vegetables.

Of Earths.

THE next moft important ingredient to the nourifhment of plants is earth; and of the different earths the calcareous feems the moft neceffary, as it is contained in rain-water; and, abfolutely fpeaking, many plants may grow without imbibing any other. Mr. Tillet found corn would grow in pounded glafs †; Mr. Succow in pounded fluor fpar, or ponderous fpar, or gypfum ‡; but Tillet owns it grew very ill; and Haffenfraz, who repeated this experiment, found it fcarcely grow at all when the glafs

* Uber die alkalis, p. 76. † Mem. par. 1772. 301. 8vo. ‡ 1ft 1ft Chym. An. 1784.

glaſs or ſand were contained in pots that had no hole in the bottom through which other nutritive matter might be conveyed. It is certain, at leaſt from common experience, that neither graſſes nor corn grow well either in mere clay, ſand or chalk, and that in vegetables that grow moſt vigorouſly, and in a proper ſoil, three or four of the ſimple earths are found. Mr. Bergman, on the other hand, aſſures us he extracted the four earths, the ſiliceous, argillaceous, calcareous and muriatic, in different proportions from the different ſorts of corn *. Mr. Ruckert, who has analyzed moſt ſpecies of corn and graſſes, found alſo the four above-mentioned earths in various proportions in all of them. Of his analyſis I ſhall here give a ſpecimen, comprehending however the calcareous and muriatic in the ſame column, as this laſt ſcarcely deſerves particular notice:

One hundred parts of

The lixiviated aſhes	contained of -	Silex.	Calx.	Argill.
of Wheat -	- -	48 parts	37	15
Oats -	- -	68	26	6
Barley -	- -	69	16	15
Bere -	- -	65	25	10
Rye - -	- -	63	21	16
Potatoes -	- -	4	66	30
Red Clover -	- -	37	33	30

Mr.

* 5 Bergman, 94, 98. Schœffer Worles, ſec. 172.

Mr. Ruckert is perſuaded that earth and water in proper proportions form the ſole nutriment of plants; but Mr. Giobert has clearly ſhewn the contrary, for having mixed pure earth of alum, ſilex, calcareous earth and magneſia in various proportions, and moiſtened them with water, he found that no grain would grow in them; but when they were moiſtened with water from a dunghill corn grew in them proſperouſly *. Hence the neceſſity of the carbonic principle is apparent.

The abſolute quantity of earth in vegetables is very ſmall. Dr. Watſon informs us that 106 avoirdupoiſe pound = 1696 ozs. of oak, being carefully burned, left but 19 ozs. of aſhes, and from theſe we muſt deduct 1,5 for ſalt, then the earthy part amounts only to 17,5, that is, little more than one per cwt. The commiſſioners appointed to inſpect the ſaltpetre manufactory found nearly the ſame reſult, namely 1,2 per cwt. in beech 0,453, and in fir only 0,003. Hence we need not wonder at trees growing among rocks where ſcarce any earth is to be ſeen; but in the ſtalks of Turkey wheat or maize they found 7 per cwt. of earth, in ſun-flower plant 3,7 †; ſo that, upon the whole, weeds and culmiferous plants contain more earth than trees do. Mr. Weſtromb found *trifolium pratenſe* to contain about 4,7 per cwt. of earth, of which 2 per cwt. was mild calx, nearly 2 more ſilex, 0,7 argill, together with a ſmall proportion of phoſphorated iron, calx of iron and manganeſe ‡.

Since

* Encyclop. *Vegetation*, 274.

† See 3 Tranſ. Royal Iriſh Academy.

‡ 1ſt Chy. An. 1787.

Since plants derive ſome proportion of earth from the ſoil on which they grow we cannot be ſurpriſed that theſe ſoils ſhould at length be exhauſted by crops that are carried off, ſuch as thoſe of corn and hay, particularly the former; even lands paſtured muſt at laſt be exhauſted, as the excrements of animals do not reſtore the exact quantity that the animals have conſumed; and hence the utility of mucks, as the reſtoration is performed by more animals than have been employed in the conſumption. Hence alſo a ſucceſſion of different crops injures land leſs than a ſucceſſion of crops of the ſame kind, as different proportions of the different earths are taken up by the different vegetables. Finally, we may hence derive the utility of marling land, as the deficient earths are thereby replaced. This ſubject admits of more preciſion than has been hitherto imagined, and may even be ſubjected to calculation. The abſolute quantity and relative proportions of the various earths in an acre of land may be determined, ſo may that in the crops of different vegetables, and by comparing both, the *time* alſo may be found in which the land muſt be exhauſted, unleſs renovated by various manures; thus the neceſſity of marling, the kind of marle or other manures, and the quantity neceſſary to an acre of land may be very nearly aſcertained.

Earths cannot enter into plants but in a ſtate of ſolution, or at leaſt only when ſuſpended in water in a ſtate of diviſion as minute as if they had been really diſſolved. That ſiliceous earth may be ſuſpended in ſuch a ſtate of diviſion appears from various experiments, particularly thoſe of Mr. Bergman, who found it thus diffuſed in the pureſt waters of Upſal; and it is equally certain

tain that it enters copiously into vegetables. Both his experiments, and especially those of Mr. Macie, establish this point beyond contradiction*. Argillaceous earth may also be so finely diffused as to pass through the best filters, so also may calx, as appears from the quantity Margraaf found in the purest rain water. This earth is even soluble by means of an excess of fixed air in about 1500 times its weight of water. It may also be and most frequently is converted into gypsum by the vitriolic acid which most clays contain, as Mr. Morveau has shewn‡, and then it is soluble in 500 times its weight of water.

Vegetables not only require food, but also that this food be duly administered to them; a surfeit is as fatal to them as absolute privation. Doctor Hales observed that a young pear-tree, whose roots were set in water, absorbed a smaller quantity of it every day, the sap vessels being saturated and clogged by it; and Mr. Miller found that too much water rotted the young fibres of the roots as fast as they pushed out†. Saturated solutions of dung appeared to Mr. Du Hamel equally hurtful §. Now the preservation and due administration of this liquid food is effected by due proportions of the simple earths and their loose or condensed state. Their situation in other respects being the same, those that abound in the argillaceous principle are the most retentive of water: those that abound in the coarse siliceous, least—the calcareous being intermediate between both; various species of

* Phil. Transf. 1791. ‡ 1st Encycloped. Chymic. 123.

† 1st Hales, 17. § Mem. Par. 1748.

of vegetables requiring various quantities of water and other food; hence it is that every ſort of ſoil bears vegetables peculiarly adapted to it, while others do not grow at all or but ill in it. By the experiments of Mr. Bergman we find that

Argill takes up	2, 5 times its weight of water when ſaturated ſo as to let none drop.
Magneſia -	1,05
Chalk -	0, 5
Siliceous ſand	0,25

Fixed Air.

THAT plants do not thrive, but moſt frequently periſh, when ſurrounded by an atmoſphere of fixed air, has long been obſerved by that great explorer of the moſt hidden proceſſes of nature, Doctor Prieſtley; but that fixed air imbibed by the roots is favourable to their growth ſeems well eſtabliſhed by the experiments of Doctor Perceval of Mancheſter, and fully confirmed by thoſe of Mr. Ruckert. This laſt-mentioned philoſopher planted two beans in pots of equal dimenſions filled with garden mould. The one was watered almoſt daily with diſtilled, the other with water impregnated with fixed air in the proportion of ½ cubic inch to an ounce of water; both were expoſed to all the influences of the atmoſphere except rain. The bean treated with aerated water appeared over ground nine days ſooner than that moiſtened with diſtilled water, and produced 25 beans, whereas

whereas the other pot produced only 15. The ſame experiment was made on ſtock july-flowers and other plants with equal ſucceſs *. The manner in which fixed air acts in promoting vegetation ſeems well explained by Mr. Senebier: he firſt diſcovered that freſh leaves expoſed to the ſun in ſpring-water, or water ſlightly impregnated with fixed air, always produce pure air as long as this impregnation laſts; but as ſoon as it is exhauſted, or if the leaves be placed in water out of which this air has been expelled by boiling, they no longer afford pure air †; from whence he infers that fixed air is decompoſed, its carbonic principle retained by the plant, and its pure air expelled. It appears to me alſo, by acting as a ſtimulant, to help the decompoſition of water. Mr. Haſſenfraz, indeed, denies its decompoſition, but his arguments do not appear to me concluſive, for reaſons too tedious and technical to mention here. The vitriolic acid contained in various clays brought into multiplied contact with calcareous earth by the agitation of ſoils in agricultural operations, and the motion of the roots, gradually ſets looſe the fixed air contained in this laſt mentioned earth; that portion alſo of this earth, which is by water introduced into the plant, is decompoſed, and its air ſet looſe by the vegetable acids of the plant.

Of Saline Subſtances.

SALINE ſubſtances (gypſum and phoſphorated calx excepted) ſeem to ſerve vegetables as they do animals, rather as a *condi-*

 mentum,

* 2d Chy. An. 1788, 399. † Sur l'influence de la Lumiere & 41 Roſier, 206.

mentum, or promoter of digeſtion, than as a *pabulum*. This idea is ſuggeſted by the ſmallneſs of their quantity, and the offices they are known to perform. Their quantity is always ſmaller than that of earth, and this we have already ſeen to be exceeding ſmall.

Thus one thouſand pound of	lb.
Oak gives of ſaline matter only -	1,5
Elm - - -	3,9
Beech - - -	1,27
Fir - -	0,45
Vine branches - -	5,5
Fern - -	4,25
Stalks of Turkey wheat - -	17,5
Wormwood - -	73,
Fumitory - -	79,
Trifolium pratenſe - -	0,78
Vetches * - - -	27,5
Beans with their ſtalks * -	20,

In all the experiments hitherto made the proportion of ſaline matter to the earthy has been found ſmalleſt in woods. In other

* 3 Ruck. 49.

other plants generally as 1 to 1,3, 1,5, or 2; however, Mr. Ruckert has marked ſome exceptions, which I ſhall mention as worthy of notice.

Proportion of Saline Subſtances to the Earthy.

In Hemp	-	- as	1	to	8.
Flax	-	-	1	to	1,7 nearly.
Parſnips	-	-	1,1	to	1.
Potatoes	-	-	1	to	1,3
Turnips	-	-	1	to	3,33
Wheat	-	-	1	to	3.
Rye	-	-	1	to	8.
Oats	-	-	1	to	8.

These proportions have ſome analogy with the quantity and ſort of manure proper to be employed in the cultivation of theſe plants and the ſucceſſion of crops. But I ſhall enter no farther into this ſubject, as it would lead me too far from the preſent object of enquiry.

The ſalts generally extracted from the aſhes of vegetables are tartar vitriolate, Glauber's ſalt, common ſalt, ſalt of ſylvius, gypſum, phoſphorated calx, and fixed alkalis.

Alkalis

Alkalis ſeem to be the product of the vegetable proceſs, for either none or ſcarce any is found in the ſoils, or in rain-water, while in the vegetable they are moſt probably neutralized, partly by vegetable acids which are decompoſed in the proceſs of combuſtion, and partly by the vitriolic and marine acids. Weſtromb found tartar vitriolate and digeſtive ſalts in the juices of trifolium.

Gypsum probably exiſts in greater quantity in plants than it appears to amount to after combuſtion and lixiviation; much of it muſt be decompoſed during combuſtion, and ſtill more during lixiviation, by the alkalis exiſting in the ſolution. Thus the apparent quantity of tartar vitriolate is increaſed.

Phosphorated Calx is found in greateſt quantity in wheat where it contributes to the formation of the animal gluten. Hence in rainy years the quantity of gluten in wheat has been obſerved to be ſmaller *. Hence the excellence of bone-aſhes as a manure for wheat, and hence wheat ſucceeds beſt after clover if the clover be fed off, but not if it be mowed †, as much of the phoſphoric acid is communicated by the dung of animals.

The chief uſe of tartar vitriolate ſeems to be, that it promotes the decompoſition of water, as Mr. Senebier has obſerved ‡.

* 2d Witwer's Diſſertations, 103.
† 2d Young's Annals, 36, 37.
‡ Sur la Lumiere, p. 130.

SECTION

SECTION II.

Of the Conſtitution of fertile Soils, and the Method of eſtimating their Fertility.

THE moſt fertile ſoil is that which contains the greateſt quantity of the food of thoſe vegetables that nouriſh men and uſeful animals, and adminiſters it to them with due œconomy.

THE firſt eſſential requiſite therefore to a fruitful ſoil is, that it contain a ſufficient quantity of the three or four ſimple earths abovementioned, and of the ſoluble carbonaceous principle. The other requiſites are, that the proportion of each, and general texture of the ſoil, be ſuch as to enable it to admit and retain as much water as is neceſſary to vegetation, and no more.

NOW we have already ſeen that the retentive powers of moiſture are very different in the ſimple earths: therefore the proportions in which the fertility of a ſoil requires them to be mixed muſt be different in climates and countries that differ conſiderably in moiſture; in the *drier* they muſt be ſuch as are moſt retentive, in the *moiſter* ſuch as ſuffer it to paſs or evaporate more eaſily.

THE ſame remark extends to ſituation. Lands on a plain ſhould be ſo conſtituted as to be leſs retentive of water than thoſe ſituated on a declivity, as is very evident.

So

So lands that have a retentive or impermeable ſubſoil ſhould be differently conſtituted from thoſe that have one leſs retentive or more permeable. The time of the year in which rain moſt abundantly falls may alſo be worthy of notice.

These circumſtances muſt undoubtedly modify the concluſions that may be drawn from the experiments I ſhall now relate.

Analyſis of a fertile Soil in a very rainy Climate.

Mr. Giobert has communicated to the public the analyſis of a fertile ſoil in the vicinity of Turin, where it rains yearly above 40 inches on the ſquare foot. He found 1 lb. of it to contain from 20 to 30 grains of extractive matter which flamed and burned, and therefore was a coal ſoluble in water; 26 lb. of it contained 1808 grains of water. The ſimple earths were in the following proportion per cwt *.

Silex,	from	—	77	to	79
Argill	—	—	9	—	14
Calx	—	—	5	—	12

Hence the pound ſhould contain †,

			grs.
Carbonic matter	—	—	25
Water	—	— —	70

Silex,

* Encyclop. *Vegetation*, 276.

† The Turin medicinal pound is divided like the Troy, and contains the ſame number of grains.

Silex,	from	4362	to	4475
Argill	—	509	—	793
Calx	—	283	—	679

He alſo found it to contain a great deal of air (about 19 grains) of which $\frac{1}{3}$ was fixed, and the remainder heavy inflammable air; but no volatile alkali.

The weight of a cubic foot of this ſoil does not appear, nor is its ſpecific gravity given; hence neither its texture, nor the quantity of each ingredient, can be directly aſcertained; yet from the neceſſity of its being in ſome degree open, and the weights of good ſoil found by Mr. Fabroni *, I conclude its ſpecific gravity cannot exceed 1,58; then a cubic foot of it ſhould weigh about 120lb. troy, or 100 avoirdupois.

In leſs fertile ſoils Mr. Giobert found the proportions of

Silex	from	48	to	80
Argill	—	7	—	22
Calx	—	6	—	11

Hence the troy pound contained of

Silex	from	2716	to	4528
Argill	—	396	—	1245
Calx	—	339	—	622

allowing 100 grains for moiſture, as either the calx or argill exceeds the proportions in more fertile lands.

* 8 Young's Annals, 174.

The ſpecific gravity of theſe ſoils is not given, but it probably exceeds or falls ſhort of that of the more fertile ſoils.

In Barren Soils.

The proportions of Silex	from	42	to	88
Argil	—	20	—	30
Calx	—	4	—	20

Hence the troy pound contained, allowing for water 120 grains,

Silex	from	2368	to	4963
Argil	—	1128	—	1692
Calx	—	225	—	620

The ſpecific gravity of theſe ſoils is not given, but it probably is either much above or much below that of the former, as they are either too cloſe or too open. Mr. Fabroni found that of barren ſandy land 2,21.

Note alſo, that if the proportion of water be different from that here ſuppoſed, the contents of the troy pound will alſo be different, but may eaſily be rectified.

Analyſis of a fertile Soil, where the fall of Rain is 24 Inches.

Mr. Bergman found that a fertile ſoil, ſituated on a plain, where the yearly fall of rain amounts to 15 Swediſh (that is 23,9 Engliſh inches) contained four parts clay, three of ſiliceous ſand,

two

two of calcareous earth, and one of magnesia (in all ten parts), but the last not being of absolute necessity, may be annexed to the calcareous.

The composition of the clay he does not expressly mention, but we may suppose it such as most frequently occurs, containing 66 per cwt. of fine siliceous sand and 34 of of mere argill, consequently 0,40 of it contain nearly 14 of mere argill, and 0,26 of fine siliceous sand.

The siliceous sand, mentioned by Mr. Bergman, is what we call gravel (consisting of stone from the size of a pea, or less, to that of a nut) and thus he himself explains it *; this amounts to 30 per cwt.

Hence we may state the proportions thus:

Coarse Silex	-	-	30	
Finer	—	-	26	
				56 parts
Argill	—	-		14
Calx	—	-		30
				100

The use of the gravel is to keep the soil open and loose, a circumstance absolutely necessary, as I have before observed.

 The

* 5 Bergman, 102, 103.

The ſpecific gravity is not given, but ſhould not much exceed I ſuppoſe 1 600. Muſchenbrock found that of garden mould 1,630. The carbonic matter was not known to Mr. Bergman.

The proportion in a troy pound, ſuppoſing the quantity of water and coal not to exceed 100 grains, ſtands thus, omitting fractions:

Gravel	-	-	1698	
Fine ſand	-	-	1471	
				3169
Argill	-	-	-	792
Calx	-	-	-	1698

Here we ſee the quantity of calx much greater than in the ſoil of Turin, where the fall of rain is greater; for in the drier climates there is a neceſſity to retain the rain, and the argill if increaſed would retain it too long and too much; and, beſides, enters very ſparingly into the conſtitution of plants.

The following experiments were made by Mr. Tillet at Paris, where the fall of rain amounts to 20 inches at an average.

He filled with mixtures of different earths a number of pots twelve inches in diameter at the top, ten at bottom, and ſeven or eight deep; it appears alſo that they were ſo porous as to abſorb moiſture, and that they were perforated at the bottom; theſe

theſe he buried up to the ſurface in a garden, ſowed in each ſome grains of wheat, and then abandoned them to nature.

Fertile Mixtures.

1. The firſt mixture he found fertile conſiſted of $\frac{3}{8}$ of the potters clay of Gentilly = 0,375 —— $\frac{3}{8}$ of the parings of limeſtone and $\frac{2}{8}$ of river ſand = 0,25. In this the corn grew very well for three years, that is, as long as the experiment laſted.

As potters clay is not pure argill, and as Mr. Tillet does not mention the proportion the mere argillaceous part bore to the ſiliceous, I muſt ſupply this defect, by ſuppoſing this clay to contain near $\frac{1}{2}$ its weight of pure argill, as it is clay of this ſort that potters generally chuſe, and that of Gentilly is eſteemed one of the beſt. Both the clay and limeſtone, he tells us, were pulverized, that they might more exactly incorporate when mixed. Then the centeſimal proportions will ſtand thus :

Coarſe Silex - -	25	
Finer — - -	21	
		46
Argill — - -		16,5
Calx — - -		37,5
		100

The

The quantities in the troy pound, ſuppoſing the water, &c. to amount to 100 grains, are,

Coarſe ſand	- -	1415	
Finer	— - - - -	1188	
			2603
Argill	- - - -		934
Calx	- - - -		2122
			5659

2d. This contained $\frac{2}{8}$ of potters clay, $\frac{3}{8}$ parings of limeſtone, and $\frac{3}{8}$ coarſe ſand. The centeſimal proportions are,

Coarſe ſand	- -	37,5	
Finer	- - -	14	
			51,5
Argill	- - -		11
Calx	- - -		37,5
			100

In

In the troy pound, ſuppoſing the quantity of water to amount to 100 grains, the quantities of the three earths will be,

Coarſe Silex -	2122	
Finer - -	792	
		2914
Argill - - -		622
Calx - - -		2122
		5658

Hence we ſee that in the drier countries, where the fall of rain is but 20 inches, the ſoil, to be fertile, muſt be cloſer, and the quantity of calcareous earth much increaſed, and that of the ſiliceous much diminiſhed. Thus, in the climate of Turin, where the fall of rain exceeds 40 inches, the proportion of ſiliceous earth is from 77 to 80 per cwt. and that of calcareous from 9 to 14, to ſuffer this exceſs of rain more eaſily to evaporate. In the climate of Upſal, where the fall of rain is 24 inches, the proportion of ſilex is only 56 per cwt. but that of calx is 30; and in the climate of Paris, which is ſtill drier, the proportion of ſilex is only from 46 to 51, and that of calx 37,5 per cwt. and hence we may perceive the neceſſity of attending to the average quantity of rain to judge of the proper conſtitution of fertile lands on fixed principles. The quantity of rain differs much in different parts of the ſame kingdom, but in general in Ireland I believe it to be between 24 and 28 inches on an average.

In the two laſt mixtures the proportions vary conſiderably: The firſt may ſerve as a model for the heavier ſoils, and the ſecond for the lighter. In theſe and the following experiments the carbonic principle ſeems to have been extracted from the ſurrounding garden mould with which the pots communicated by means of their perforation at bottom.

Barren Mixtures

First.

Mr. Tillet, in his ſixth and eighth experiment, mixed $\frac{3}{8}$ of potters clay with $\frac{3}{8}$ of parings of limeſtone and $\frac{2}{8}$ of *fine* ſand; the only difference between this mixture and that of the firſt experiment was, that in the firſt experiment *coarſe ſand* was uſed and in this *fine*, yet the former was fruitful in the higheſt degree; but in this the grain proſpered indeed the firſt year, but ſickened in the ſecond, and failed in the third—the proportions have been already ſtated. Here we have a clear proof of the neceſſity of an open texture in ſoils, without which the beſt proportions are uſeleſs.

Second.

In his thirteenth experiment he employed a mixture of $\frac{2}{8}$ potters clay, $\frac{4}{8}$ coarſe ſand and $\frac{2}{8}$ marle. The corn grew well the firſt year, poorly the ſecond, and decayed the third. The compoſition of the marle is not mentioned; but ſuppoſing it to contain 70 per

per cwt. of calx, and 30 of clay, of which the one-half is argill, it would form one of the richest sorts of marles. The centesimal proportions of this mixture should be,

Silex - -	50 † 14=64
Argill -	11 † 8=19
Calx - - -	17
	100

And in the troy pound, supposing the water, &c. to amount to 100 grains, the quantities will be,

Silex - -	3622
Argill - -	1075
Calx - -	962
	5659

The sterility of this mixture seems to proceed from a defect of calcareous earth. If we suppose the marle poorer in that earth the defect will be still greater. The retentive powers of the different earths with respect to water being expressed by the quantities which each can retain without suffering any to drop, as above said, and the quantities retained by the mixed mass of these earths being proportional to the respective quantities of each,

 it

it ſhould ſeem that in fertile ſoils, where the fall of rain is from 20 to 30 inches, this power ſhould not exceed 70 nor fall ſhort of 50 per cent. It were of great conſequence to ſettle this point with preciſion, but to do this would require more numerous experiments. To explain my meaning I ſhall give one example.

Of the retentive Power of the fertile Soil mentioned by Mr. Bergman.

THIS ſoil contains, as we have already ſeen, Silex - 56
Argill - 14
Calx - 30

Now the retentive power of 100 parts Silex = 25
Argill = 250
Calx = 50

Conſequently the retentive power of 56 parts Silex = 13
14 — Argill = 35
30 — Calx = 15
—— 63

THE conſtitution of the Iriſh fertile ſoils has not been aſcertained, nor has the average annual quantity of rain been determined here; indeed the ſolution of the queſtion propoſed by the Academy does not ſtrictly require it ſhould, not having been limited to any particular country; but I ſhould ſuppoſe its beſt ſoil foil to approach to the nature of that of Upfal, the fall of rain being probably between 24 and 28 inches. In 1792, which was reckoned remarkably wet, it was 30½ inches in Dublin.

Before I quit the experiments of Mr. Tillet it will be proper to mention a few made by him, which feem to invalidate the neceffity of the prefence of the three fimple earths in fertile foils.

1mo. In his 26th experiment he tells us he employed only pure fand, fuch as is ufed for making glafs, yet corn grew well in it the firft year, indifferently the fecond, and nearly failed in the third. Mr. Haffenfraz having repeated the experiment in pots unperforated did not find it to fucceed even the firft year, therefore the fuccefs of Mr. Tillet's was owing to the perforation at the bottom of his pot through which water impregnated with the different earths, and coal muft have paffed. In fact Mr. Tillet's conclufion is contradicted by univerfal experience.

2o. In his 28th experiment, in which powdered limeftone only was employed, the corn fown profpered exceedingly during the three years. To the caufe mentioned, in treating of the 26th, I muft add, that the limeftone he ufed was that of St. Leu, which contains clay, and confequently filex and argill; it is fo porous as to admit from $\frac{3}{19}$ to $\frac{1}{5}$ of its weight of water, as Mr. Briffon has fhewn, and thus is eafily decompofed. The coarfe powder to which it was reduced anfwered the fame purpofe as coarfe filex, and the finer might nourifh the plants.

3°. In his 30th experiment he employed mere potters clay; the grain grew tolerably well the firſt year, but periſhed the ſecond; on the third it flouriſhed moſt. It is hard to draw any ſpecific concluſion from this experiment, for it is plain that if the texture were not much looſer than that of clay, the corn could not grow at all, as was the caſe in his 6th and 8th experiments, already mentioned, and as Mr. Haſſenfraz, who repeated this experiment, obſerved. Rain water might however ſupply a ſmall quantity of calx ſufficient for a ſmall produce of corn.

I pass over his experiments on old mortar, as the three earths were evidently contained in it, though in unknown proportions.

Soils on the declivity of hills ought to be more retentive of water than thoſe on plains, as is evident.

C H A P. III.

To determine the Compoſition of a Soil.

1mo. In dry weather, when the ſoil is not overmoiſt nor dry, let a ſurface of 16 inches ſquare be cut through to the depth of 8 inches; this may be effected by a right angled ſpade formed for this particular purpoſe. Of the parallelopiped thus dug up the two inches next the ſurface ſhould be cut off to get rid of the graſs and the greater part of the roots, we ſhall then have a ſolid 6 inches long and 16 ſquare at the end = 96 cubic inches.

Let

Let this be weighed †; its weight will ſerve to find the ſpecific gravity of the ſoil; for if 96 cubic inches weigh *n* pounds, 1728 (a cubic foot) ſhould weigh *x* pounds, and *x* divided by 75,954 will expreſs by the quotient the ſpecific gravity of the ſoil. To render this and the ſubſequent operations more intelligible I ſhall illuſtrate each by an example: Suppoſe the 96 cubic inches to weigh 6,66 pounds, then 1728 cubic inches ſhould weigh 120 lb. and $\frac{120}{75,954}=1,579$.

2°. The earth being weighed is next to be broken down and freed from all ſtony ſubſtances above the ſize of a pippin, and the remainder well mixed together to render the whole as homogeneous as poſſible; then weigh the ſtones that were picked out, and find the proportion belonging to each pound of the reſiduary earth; call this the ſtony *ſupplement*, and denote it by *S*.—Thus if the ſtones weigh 1 lb.=12 oz. the remainder or mere earth muſt weigh 5,66 lb. and if to 5,66 lb. there belong 12 ozs. of ſtone, to 1 lb. muſt belong 2,12014 ozs. or 2 ozs. 57,66 grs.=1017,66 grs. This then is the ſtony ſupplement of each ſucceeding pound=*S*.

3°. Of the earth thus freed from ſtony matter take 1 lb.—*S*. (that is the above caſe 1 lb.—2 oz. 57$\frac{2}{3}$ grs.) heat it nearly to redneſs in a flat veſſel, often ſtirring it for half an hour, and weigh it again when cold. Its loſs of weight will indicate the quantity

† Troy weights are generally more exactly made than avoirdupois, and therefore ſhould be preferred. A cubic foot of pure water weighs 75,954 troy, very nearly, or 62,5 avoirdupois pounds, at the temperature 62°.

quantity of water contained in 1 lb. of the foil; note this lofs, and call it the *watry fupplement* = *W*, fuppofe it in this cafe 100 grains.

4°. Take another pound of the above mafs freed from ftones, deducting the ftony and watry fupplements, that is 1 lb.—S—W, or in the above cafe 1 lb.—2 ozs. $57\frac{2}{3}$ grs. for ftone, and—100 grs. for water, confequently 1 lb.—2 ozs. $157\frac{1}{3}$ grs. reduce it to powder, boil it in four times its weight of diftilled water for half an hour; when cool pour it off, firft into a coarfe linen filtre to catch the fibrous particles of roots, and then through paper to catch the finer clayey particles diffufed through it; fet by the clear water, add what remains on the filtre to the boiled mafs; if it be infipid, as I fuppofe it to be, then weigh the fibrous matter, and call it the *fibrous fupplement* = F; fuppofe it in the example in hand to weigh 10 grs.

5°. Take two other pounds of the mafs freed from ftony matter, No. II. fubftracting from them the weight of the ftony, watry and fibrous fubftances already found, that is 2 lb.—2S—2W—2F; pour twice their weight of warm diftilled water on them and let them ftand twenty-four hours or longer, that is until the water has acquired a colour, then pour it off and add more water as long as it changes colour, afterwards filtre the coloured water and evaporate it to a pint or half a pint, fet it in a cool place for three days, then take out the faline matter, if any be found, and fet it by.

6°. Examine

6°· EXAMINE the liquor out of which the falts have been taken; if it does not effervefce with the marine acid, evaporate it to drynefs and weigh the refidiuum;—if it does effervefce with acids, faturate it with the vitriolic or marine and evaporate it to $\frac{1}{4}$ of the whole; when cool take out the faline refiduum, evaporate the remainder to drynefs and weigh it, this gives the coaly matter, which may be tried by projecting it on melted nitre, with which it will deflagrate; the $\frac{1}{2}$ of this coaly matter call the *coaly fupplement* of 1 lb. I fhall fuppofe it to amount to 12 grs. and denote it by C.

7°· THE filtred water, No. IV. is next to be gently evaporated to nearly one pint, and then fuffered to reft for three days in a cool place that it may depofite its faline contents, if it contains any, and thefe being taken out the remainder muft be evaporated nearly to drynefs, and its faline and other contents examined. How this fhould be done I fhall not mention, the methods being too various, tedious and of too little confequence; few falts occur except gypfum, which is eafily diftinguifhed; the water may be examined as to its faline contents when it is evaporated to a pint; if any falts be found, call them the *faline fupplement*, and denote them by S'; I fhall fuppofe them here=4 grains.

8°· WE now return to the boiled earthy refiduum, No. IV. which we fhall fuppofe fully freed from its faline matter, as, if it be not, it may eafily be rendered fo by adding more hot water; let it then be dried as in No. III. is mentioned. Of this earthy matter thus dried weigh off one ounce, deducting one-

twelfth

twelfth part of each of the ſupplements *S. W. F. C.* and *S′*, that is in this caſe $\frac{1017,66}{12} = 84,405 + \frac{100}{12} = 8,333 + \frac{10}{12} = 8,333 + \frac{12}{12} = 1 + \frac{4}{12} = 0,3333 = 95$ grains in all — then 480—95 = 385 grains will remain, and repreſent the mere earthy matter in an ounce of the ſoil.

9°. Let this remainder be gradually thrown into a Florence flaſk holding one and an half as much ſpirit of nitre as the earth weighs, and alſo diluted with its own weight of water; (the acids employed ſhould be freed from all contamination of the vitriolic acid); the next day the flaſk with its contents being again weighed, the difference between the weights of the ingredients and the weights now found will expreſs the quantity of air that eſcaped during the ſolution;—thus in the above caſe the earth weighing 385 grains and the acid 577,5 grains, and the water 577,5 grains, in all 1540 grains, the weight after ſolution ſhould alſo be 1540 if nothing eſcaped; but if the ſoil contains calcareous matter a loſs will always be found after ſolution; let us ſuppoſe it to amount to 60 grains.

The weight of air that eſcaped furniſhes us with one method of eſtimating the quantity of calcareous matter contained in the earth eſſayed, for mild calx generally contains 40 per cent of air; then if 40 parts air indicate 100 of calcareous matter, 60 parts air will indicate 150*.

10°. The

* I take no account of magneſia, as in agriculture I believe it of little importance.

10°. The ſolution is then to be carefully poured off, and the undiſſolved maſs waſhed and ſhaken in diſtilled water; the whole thrown on a filtre and ſweetened as long as the water that paſſes through has any taſte; the contents of this water ſhould be precipitated by a ſolution of mild mineral alkali; this precipitate alſo being waſhed and dried in a heat below redneſs ſhould then be weighed; thus we have another method of finding the weight of the calcareous matter.

11°. The undiſſolved maſs is next to be dried in the heat already mentioned, and the difference between its weight and the weight of the whole earthy maſs before ſolution ſhould be noted, as it furniſhes a third method of diſcovering the weight of the calcareous matter of which it is now deprived. Suppoſing this to amount to 150 grains, the weight of the undiſſolved reſiduum ſhould in the above caſe be 385—150=235 grains.

12°. Reduce the dried maſs into the fineſt powder, throw it into a Florence flaſk or glaſs retort and pour on it three times its weight of pure oil of vitriol, digeſt in a ſtrong ſand heat, and at laſt raiſe the heat ſo as to make the acid boil; afterwards let it evaporate nearly to dryneſs; when cold pour on it gradually ſix or eight times its weight of diſtilled water, and after ſome hours pour off the ſolution on a filtre; the filtre ſhould previouſly be weighed and its edges ſoaked in melted tallow *; the ſubſtance

* An ingenious contrivance of Dr. Black's.

found on the filtre being weighed (ſubſtracting the weight of the filtre) gives the quantity of ſiliceous matter; and this weight ſubſtracted from that of the dried maſs gives that of the argill; in this caſe I will ſuppoſe the ſiliceous maſs to weigh 140 grains, then the argillaceous ſhould weigh 95 grains.

Then the compoſition of one pound of the ſoil is as follows:

Stony matter - - - -		1017,66
Water - - - - -		100
Fibres of roots - - - -		10
Soluble coal - - - - -		12
Saline matter - - - -		4
Silex - - -	140 × 12 =	1680
Argill - - -	95 × 12 =	1140
Mild calx - - -	150 × 12 =	1800
		5763,66 *

And in centeſimal proportion

Stony matter -	18	
Fine ſiliceous -	29	
		47
Argill - -		22
Mild calx - -		31
		100

Its

† An error of 3,66 grains for decimals omitted in ſubſtractions.

Its retentive power is 82,25; hence I ſhould judge it to be unfertile in this climate, unleſs ſituated on a declivity with an unimpeded fall; it may be called a *clayey loam.*

Mr. Young diſcovered a remarkable circumſtance attendant on fertile ſoils; he found that equal weights of different ſoils being dried and reduced to powder, afforded quantities of air by diſtillation ſomewhat correſponding to the ratios of their values. This air was a mixture of fixed and inflammable airs, both proceeding moſt probably from the decompoſition of water by the coaly matter in the ſoil; the diſtillation ſhould be made from a retort glazed on the outſide—he found an ounce of dry ſoil, value five ſhillings - - produced - - ten ounce meaſures,

of from	5 to 12	produced	28
	12 — 20		42
above	20		66

This appears to be a good method of eſtimating the proportion of coaly matter in ſoils that are in full heart, that is, not exhauſted, and freed from roots, &c. Another mark of the goodneſs of a ſoil is the length of the roots of wheat growing in it, for theſe are in inverſe proportion to each other, as, if the land be poor, the wheat will extend its roots to a great diſtance in queſt of food, whereas if it be rich they will not extend above five or ſix inches; but of theſe and ſome other empyrical marks I ſhall ſay no more, as they do not tell us the defects of the ſoils.

CHAP. IV.

Of the Manures moſt advantageouſly applicable to the different Soils, and of the Cauſes of their beneficial Effect in each Inſtance.

THE ſolution of the firſt part of this problem can only be derived from the general practice of the moſt ſkilful farmers, corrected however and improved by the more precise determinations and reſtrictions of theory. That of the ſecond I ſhall endeavour to deduce ſolely from the theory eſtabliſhed in the two laſt chapters. The whole is grounded on this ſimple propoſition, that *manures are applied to ſupply either the defective ingredients of a ſoil, or improve its texture or correct its vices.*

I NOW proceed to conſider each ſoil in particular.

Of Clayey Soils.

THE beſt manure for clayey ſoils is marle; in this all the books of agriculture are unanimous†; and of the different ſorts of marle that which is moſt calcareous is beſt; the ſiliceous next beſt; limeſtone gravel beſt of all; and argillaceous marle leaſt advantageous ‡.

CLAYEY

† 4th Young's Eaſtern Tour, 404. 1ſt Body of Agriculture, 104, 108. ‡ Ibid. 108.

CLAYEY ſoils are defective both in conſtitution and in texture; they want the calcareous ingredient and coarſe ſand. Calcareous marle ſupplies the calcareous ingredient chiefly: limeſtone gravel both. The other marles ſupply them in a leſſer degree. If the clay be analyzed, and its proportion of ſand and argill known, the ſpecies of marle moſt advantageouſly applicable may be determined ſtill better; for inſtance, if the argill notably exceeds or even amounts to the proportion of 40 or 50 per cwt. calcareous marle or limeſtone gravel are the beſt improving manures, as they contain moſt of the calcareous ingredient; but if the ſiliceous ingredient amounts to 75 or 80 per cwt. as it ſometimes does, argillaceous marle is moſt ſuitable.

A mixture of marle and dung is ſtill more advantageous *, becauſe the dung ſupplies the carbonaceous ingredient. But the marle muſt be uſed in the ſame quantity as if dung had not been applied, otherwiſe the operation muſt be more frequently repeated. How the quantity of marle or other manure can be eſtimated I ſhall preſently ſhew.

IF marle cannot be had a mixture of coarſe ſand and lime perfectly effete or extinguiſhed, or chalk, will anſwer the ſame purpoſe, as it will ſupply the defective ingredient and open the texture of the clay; ſo alſo ſand alone, or chalk, or powdered limeſtone, may anſwer, though leſs advantageouſly. Lime alone appears to me leſs proper, as it is apt to cake and does not ſufficiently open the ſoil.

WHERE

* 4th Young's Eaſtern Tour, 404.

WHERE thefe manures cannot be had, coal-afhes, chips of wood, burned clay, brick-duft, gravel, or even pebbles, are ufeful *; for all thefe improve the texture, and the former fupply alfo the carbonaceous ingredient.

BEFORE I advance farther, to prevent fuperfluous repetition I fhall lay down a fecond general maxim, which is, that *dung is a proper ingredient in the appropriated manures of all forts of foils*, as it fupplies the carbonaceous principle.

Of Clayey Loam.

THIS foil is defective either in the calcareous ingredient, or in the fandy, or in both; if in the firft, the proper manure is chalk †; if in the fecond, fand; if in both, filiceous marle or lime-ftone gravel, or effete lime and fand.

THE quantity of chalk that fhould be employed, confidered abftractedly, fhould be directly proportional to the defect of calcareous matter; but as fuch a quantity cannot be added without diminifhing the proportion of one of the other ingredients, a much fmaller quantity muft be employed, or elfe a fubftance which may convey fome proportion of the other ingredient. The fame obfervation holds alfo with refpect to fand; thus we have feen, in the laft chapter, a clayey loam, in which the fandy ingredient

* 5 Bergman, 107. Young's Irifh Tour, 249, 129, 136.
† 1ft Young's Eaft. Tour, 395.

ingredient was defective, and the argillaceous ſuperabundant, but the calcareous exact; its compoſition ſtood thus:

Sand and gravel	-	-	47
Argill	-	-	22
Mild calx	-	-	31

HERE the ſandy part wants 10 per cwt. the argill is ſuperabundant, but we cannot increaſe the proportion of ſand without diminiſhing that of calx. Hence we muſt either uſe a ſmaller proportion of the ſandy ingredient than its defect requires, or apply a ſubſtance that would ſupply ſome proportion of the calcareous ingredient alſo; ſuch are limeſtone gravel, ſiliceous marle, effete lime, mixed with ſand or pounded limeſtone. Suppoſe the proportion of the ſubſtance to be employed were ſix per cwt. that is ſix pound for every hundred pounds of the ſoil, then the quantity requiſite for an acre may be calculated thus: a ſquare foot of this ſoil, cut down to the depth of 14 inches, and paring off the two uppermoſt as conſiſting chiefly of roots, &c. weighs, as we have ſeen, 120lb.; and if 100lb. requires ſix of the manure, 120lb. will require 7,2; therefore every ſquare foot of the ſoil will require 7,2 of the manure: now an Engliſh acre contains 43560 ſquare feet, and conſequently 43560 multiplied into 7,2 of the manure = 313632lb. or 208 cart loads, reckoning 1500lb. to the cart load.

Chalkey

Chalkey Soil.

THIS ſoil wants both the argillaceous and the ſtony, ſandy or gravelly ingredients; therefore the beſt manure for it is clayey loam, or ſandy loam *, but when the chalk is ſo hard, as it frequently is in England, and ſo difficultly reducible to impalpable powder as to keep of itſelf the ſoil ſufficiently open, then clay is the beſt manure †, as in ſuch caſes the coarſe ſand or gravelly ingredients of loams are of no uſe. Some think, it is true, that pebbles in a field ſerve to preſerve or communicate heat; this uſe however is not ſufficiently aſcertained.

Chalkey Loam.

THE beſt manure for this ſoil is clay, or argillaceous marle ‡ if clay cannot be had; becauſe this ſoil is defective principally in the argillaceous ingredient: in Ireland chalkey ſoils or loams ſeldom occur, but light limeſtone ſoils frequently, and theſe do not differ eſſentially from chalkey loams poor in argill; clay therefore, and often the ſoil of boggs, ſhould ſerve as a manure for them.

Sandy Soils.

THE beſt manure for theſe is calcareous marle §, which exactly correſponds with our theory, for theſe ſoils want both argillaceous and the calcareous ingredients, and this marle ſupplies both; the next beſt is argillaceous marle, and next to theſe clay mixed with

* 5 Bergman, 107.

† Young's Eaſtern Tour.

‡ 4th Young's Eaſtern Tour, 404.

§ 4th Young's Eaſtern Tour, 401, 412.

with lime, or calcareous or clayey loams. In Norfolk they ſeem to value clay more than marle, probably becauſe their ſandy ſoils already contain calcareous parts; poſſibly alſo they miſname marle, calling mere chalk by that name. Lime or chalk are leſs proper, as they do not give ſufficient coherence to the ſoil; however, when mixed with earth or dung, they anſwer well *, becauſe they form a ſort of marle or compound, comprehending the defective ingredients.

Sandy Loams.

THESE are defective chiefly in the calcareous ingredient, and in ſome degree alſo in the argillaceous; their texture too is imperfect, as they abound both in fine and coarſe ſand; chalk or lime would ſupply the firſt defect, but would leave the texture unamended; hence they are uſed when better cannot be had †. Yet calcareous or argillaceous marles are moſt proper ‡. Clay, after land has been chalked, anſwers, as we are told, remarkably well, becauſe it remedies the texture §.

Gravelly Loams.

THESE ſoils are benefited by the application of marle, whether argillaceous or calcareous ‖, for reaſons which I ſuppoſe are now apparent; if the gravel be calcareous, clay may be employed ¶. A mixture of effete lime and clay ſhould anſwer in all caſes.

* Young's Eaſtern Tour, 397. † 4th Ibid. 398.
‡ Ibid. 402. § 4th Young's Annals, 413.
‖ 4th Young's Eaſtern Tour, 404. 406. ¶ 1ſt Eaſt. Tour, 494.

Till and Vitriolic Soils.

THESE neceſſarily require the calcareous ingredient to neutralize their peccant acid; hence chalk, limeſtone gravel, lime and calcareous marle, are moſt advantageouſly applied to them. *Home* 35.

Boggs or Boggy Soils.

WHEN theſe are well dried by ſufficient drains, the nature of their ſoil ſhould be explored by analyſis, and an appropriate manure applied; in general they ſhould firſt be burned if capable of that operation, then graveled; if their upper parts contain a ſufficiency of the carbonaceous principle, as often happens, they need not be burned. Limeſtone gravel will anſwer beſt or lime mixed with coarſe ſand or gravel, becauſe in general they are of a clayey nature; if more ſandy, lime may anſwer well, or calcareous marle; the preference in theſe caſes muſt be decided by analyſis *.

Heathy Soils.

THESE ſhould firſt be burned to deſtroy the heath and increaſe the carbonaceous principle; they ſhould then be analyzed and the defective principles ſupplied; lime is ſaid to deſtroy heath, and ſo is limeſtone gravel †; this is fitteſt when the ſoil is clayey, lime when it is gravelly ‡. Gypſum alſo anſwers remarkably well when the ſoils are dry.

Of

* Young's Iriſh Tour, 233, 223.

† 4th Young's Eaſt. Tour, 396.

‡ Iriſh Tour, 212.

Of some particular Manures.

We have now stated most of the known soils, and mentioned the manures which tend most to their improvement; there are, however, some others whose mode of action is not generally understood, and whose nature it will therefore be proper to explain.

Of Paring and Burning.

This mode of improvement is not particular to any species of soil, though poor soils that have few vegetables growing in them will certainly profit least by it.

Its principal advantages are,

First, that it converts vegetables and their roots into coal. Hence it is that agricultural writers tell us, though without knowing the reason, that all violence of fire is to be avoided, and that a slow smothering fire is best *.

Secondly, that it destroys the old sickly roots, and thus leaves room for others younger and more vigorous.

Many have imagined that it diminishes and consumes the soil, but repeated experience has shewn the contrary: I need only mention that of Colonel St. Leger in Yorkshire, related by Mr. Young in the 1st volume of his Eastern Tour, p. 182.

* 1st Body of Agriculture, 210, 211.

It is well known that clays and loams are rather hardened than confumed by heat. However, unlefs frefh feeds be committed, the foil will be unproductive for a number of years; the coaly principle may alfo be exhaufted by too many crops.

Of Gypfum.

THIS manure was difcovered by Mr. Mayer, a German clergyman of uncommon merit, in the year 1768; it has fince been applied with fignal fuccefs in Germany, Switzerland, France and America. If in England it has not been fo much approved of, it muft be becaufe the calcareous principle prevails there almoft univerfally; clayey lands are moft improved by it; the time for fpreading it is February or March, and then it is to be thinly ftrewed on the land at the rate of about eight bufhels to the acre; more would be hurtful; the rationale of its effects may be deduced from its extraordinary feptic power, for it is found to accelerate putrefaction in a higher degree than any other fubftance *; and hence it is not ploughed in like other manures, but barely ftrewed on the furface of the land, and in the month of February, to convert the old grafs quickly into coal to nourifh the young growth.

2dly. FROM its being itfelf no inconfiderable part of the food of many plants, particularly of clover, pulfe and corn, but the land on which it is ftrewed muft be dry, fuch as would naturally fuit clover, &c. otherwife it would be ufelefs.

THUS

* Hiftoire de la Putrefaction, 36.

Thus far I have endeavoured to illuftrate the important fubject propofed by the Academy, collecting and reflecting upon it the fcattered rays refulting from the lateft chemical refearches. The intimate connection between many of thefe, feemingly the moft abftract and remote, with the hidden proceffes of nature, may now be clearly perceived. Thefe grand and complicated operations, like a well fortified town, cannot be maftered by ftorm or a coup de main; the approaches muft be made at a diftance, and almoft unfeen—hence we may infer how little can be expected from agricultural focieties that do not unite chemiftry and meteorology with their principal object.

With refpect to the queftion at prefent before us, the great defiderata feem to be, *how to render charcoal foluble in water for the purpofes of vegetation?* and *to difcover that compofition of the different earths beft fuited to detain or exhale the due proportion of the average quantity of moifture that falls in each particular country?* On this relation or adaptation we have feen that the fertility of each effentially depends; we muft alfo have perceived that to a regular and fyftematic improvement of foils a knowledge of their defects and of the *quantum* of their defects is abfolutely neceffary. This information can be conveyed only by a chemical analyfis. Country farmers (at leaft as long as the prefent abfurd mode of education prevails) cannot be expected to poffefs fufficient fkill to execute the neceffary proceffes, but country apothecaries certainly may. The profit arifing from fuch experiments (fhould the public encourage them) would fufficiently excite them to acquire a branch of knowledge fo nearly allied with their pro-

feffion.

feffion. In the mean time foils might be fent to fome fkilful perfons in the capital by country gentlemen, who would thus be enabled to afcertain and appreciate the advantages attending fuch refearches, and enlighten and encourage their more ignorant and diffident neighbours. Many of them might perhaps themfelves feel a tafte for occupations of this nature, occupations which not only fully fuffice to fill up the many vacant hours and days which the folitude of a country life muft frequently leave them, but are moreover fweetened by the pleafing recollection, that of all others they tend moft directly to the general happinefs of mankind.

AGRICOLA.

On the NATURE *and* LIMITS *of* CERTAINTY *and* PROBABILITY. *By the Reverend* GEORGE MILLER, *F.T.C.D and M.R.I.A.*

THE rash and unsuccessful efforts which bold speculators have made in metaphysical enquiry have brought discredit upon every attempt to examine the first principles of human knowledge. The sober part of mankind, alarmed at the perplexities which have generally been the result of such enquiries, withdraw their attention from every disquisition professing to be metaphysical as from a fruitless pursuit. If this persuasion operated universally, perhaps no very bad consequences would follow from it; but whilst the sincere friends of truth shun the direct enquiries of metaphysics, they are less prepared to escape from the subtleties with which the sceptic endeavours to ensnare them, or the errors into which in their own researches they may sometimes be betrayed. It becomes necessary, therefore, to establish principles by which they may be directed, and the authority of Lord Bacon should encourage us to hope that

Read May 4, 1793.

that the attempt is not impracticable. In his Novum Organum he cautions us against being discouraged by the failure of physical experiments, observing that a negative sometimes brings more light than an affirmative instance. The errors and perplexities of metaphysicians are the *negative instances* of metaphysics. They inform us in what cases the powers of the human mind have failed, and perhaps it may appear that this attempt to ascertain the limits of those powers derives from them an experimental confirmation.

To enquire into the original, certainty and extent of human knowledge, together with the grounds and degrees of belief, opinion and assent, was the purpose of the celebrated Locke, and in the prosecution of it he has made many valuable observations; but it cannot be surprizing that in his early essay there should be some deficiencies which a century of philosophic researches might enable us to supply. His general plan seems to be right, but some parts of it appear to be erroneous, and others to want the distinctness necessary for their application. In the second class of probable propositions he has placed *the manner of operation in most parts of the works of nature* (book iv. chap. 16); and yet I believe upon examination it will be found that *the manner of operation* does not enter into any of the probabilities which he has mentioned as examples, and that it lies beyond the reach even of conjecture. He has been deficient in not giving with sufficient distinctness a general description of all those propositions in which demon-

strative

ſtrative certainty is unattainable, and in not pointing out the cauſe which in thoſe propoſitions renders certainty hopeleſs. He alſo appears to have been erroneous in his favourite poſition, that moral truths are capable of ſtrict demonſtration.

To ſupply this want of diſtinctneſs, and to correct theſe errors, I would divide all our enquiries into three claſſes; the firſt of which ſhould comprehend all thoſe in which our ideas are compared together, without being conſidered as connected by the relation of cauſe and effect, but merely regarded as independent objects of thought, correſponding in ſome particulars which enter into the compoſition of each idea; the ſecond ſhould include thoſe in which a conſideration of the relation of cauſe and effect is directly or indirectly involved, limited however to the mere fact of their connection, and not extending to the nature of that connection or *manner of operation;* and the third ſhould conſiſt of enquiries into the nature of cauſes or the manner of operation.

By the word *cauſes* I mean not only phyſical but moral cauſes, without pretending to determine the quantity of the influence of the latter. If motives are allowed to have any influence, it is ſufficient to entitle them to be placed amongſt cauſes. Of thoſe three claſſes the firſt appears to me to be the region of demonſtrative certainty, the ſecond to be that in which probability alone is attainable, and the third to be that of abſolute ignorance. In this diviſion they have been arranged in what ſeemed the moſt natural order; but as the conſideration of the third claſs will point out the circumſtance which renders certainty unattainable in the ſecond, it will be convenient to examine thoſe two claſſes in a contrary order.

WHEN we compare two ideas without conſidering them as being connected by the relation of cauſe and effect, but merely regarding them as two independent objects of thought, it is obvious that in many caſes we may be capable of diſcovering with certainty their agreement or diſagreement. Thus the mathematician may with certainty diſcover that a triangle correſponds in extent to a rectangle of the ſame altitude, and whoſe baſe is half of that of the triangle, or that they are equal. Each idea is a certain modification of extenſion, and he may diſcover that they correſpond as to the quantity of that extenſion.

PROBABILITY is not however excluded from propoſitions compoſed of ideas in this manner compared. The propoſition, whoſe truth might be eſtabliſhed by demonſtration, may be received on teſtimony; and a mathematician, when inveſtigating the conſtruction of a geometric problem, is guided by the probability of the exiſtence of various relations ariſing from the reſemblance which the caſe bears to others in which ſuch relations have been known to exiſt.

THIS clafs has been illuftrated only by a mathematical example; and I believe that it would be difficult to produce an inftance of complete demonftrative certainty, containing any thing more than verbal information, which belonged to any other department of human knowledge *. Locke was indeed defirous of convincing his readers that morality was capable of demonftration; but he does not appear to have eftablifhed this favourite principle. He has fpoken in general of our relation towards God as a foundation on which moral rules might be built; but even though mankind fhould agree in the principle of obligation, a fubject which has wearied and exhaufted controverfy, the detail of moral duties muft neceffarily involve a complicated confideration of the motives and confequences of human actions, that is, of the operation of phyfical and moral caufes. This principle then, though admitted, would be of little importance unlefs the operation of caufes were within our cognizance. He has not been more fuccefsful in his examples than in his general obfervations. *Where there is no property there is no injuftice*, and, *No government allows abfolute liberty*, are, as Paley has already obferved, merely verbal propofitions; for the notion of injuftice fuppofes the exiftence of

 property,

* Mr. Hume has in a note on the fixth fection of his Enquiry concerning Human Underftanding, propofed a middle clafs of arguments, which he calls proofs, meaning fuch arguments from experience as leave no room for doubt or oppofition. This diftinction may, as he fays, *conform our language more to common ufe;* but fuch arguments philofophically confidered are only of the higheft degree of probability, and are ranked as fuch by Locke.

property, and that of government is a reſtriction of abſolute liberty. But though moral rules are not poſſeſſed of that ſtrict and abſolute certainty which ſeems to be the pre-eminent diſtinction of mathematical ſpeculation, they are yet founded on an aſſurance ſo ſtrong that their proofs have been ſometimes miſtaken for demonſtrations. If there be any other part of human knowledge beſides the mathematical ſciences which can claim the privilege of demonſtration, it muſt be logic. The relations of abſtract reaſoning may be of ſuch a kind as that the mind may be certain of their truth, ſince no conſideration of cauſe and effect is involved. Metaphyſics however muſt be excluded; they are really a branch of natural philoſophy conſidered in an extended ſenſe; they are the natural philoſophy of the mind.

Agreeably to the plan already mentioned, the third claſs of inquiries ſhall now be conſidered. This claſs, which conſiſts of inquiries into the nature of cauſes or their manner of operation, is moſt flattering to the pride of the underſtanding; but whether we conſult reaſon or experience we ſhall have little inducement to hope that this pride could be gratified by the reſult of ſuch inquiries. Thoſe who think that all the operations of nature are performed immediately in conſequence of the determination of the Divine Will, and that God literally *upholdeth all things by the word of his power*, will not make an inquiry which ſuppoſes a communicated efficiency. Thoſe, on the other hand, who think that he has communicated efficiency to the created world, will

perceive,

perceive, if they reflect for a moment, that effects alone lie within our cognizance. By our senses we discover that a change has happened in our own bodies, or in those by which they are surrounded; and by the faculty of perception we perceive that an idea is present to the mind; but in neither case does any circumstance suggest to us the manner in which the effect has been produced. We may in either case observe a continued series of effects happening in a regular order, which may induce us to conjecture that they are connected amongst themselves as causes and effects; but however probable such a conjecture may be, since it is founded merely on the observation of the order in which one follows another, there is not any circumstance which can guide us to any conjecture concerning the nature of that connection.

NATURAL philosophers do indeed enquire into the laws according to which forces act, but the law of a force does not point out its manner of operating. The law only tells us what variety there would be in the effect in consequence of a certain variety in the circumstances under which the cause operated. Thus the law of gravity is, that one body is attracted by another with a force inversely as the square of the distance. This only tells us that the quantities of the effects produced by the unknown cause called gravity, at different distances, are inversely as the squares of those distances.

SIR Isaac Newton has suggested that the gravitation of bodies is probably the effect of the repulsion of a very subtil elastic fluid.

fluid. Repulfion is hereby fubftituted in the place of attraction, but repulfion and attraction, as to the manner of operating, are equally unknown. I would not, however, be thought to undervalue fuch a difcovery. Though it could not give us any conception of the nature of the active caufe, it would unfold to us a new and comprehenfive analogy of effects, by tracing to one common caufe effects which appear to be of the moft oppofite natures.

We are equally in the dark with refpect to moral and intellectual agency. That operation of the mind which is called confcioufnefs will not give us in this refpect any affiftance towards the difcovery of the nature of our own minds, or of the operation of moral caufes. The operation of confcioufnefs may be diftinguifhed into two parts. The one is merely a perception that the mind is actually thinking, and this is, as Locke obferves, effential to thinking. This is evidently a mere perception that certain ideas or combinations of ideas are prefent to the mind, and confequently does not give us any intimation of the powers by which they had been introduced. In the other part the mind is more active, it being a deliberate furvey which the mind makes of its own operations, but it is only a recollection of the train of ideas previoufly perceived for the purpofe of obferving their order and conjecturing their connection. In neither application does the word confcioufnefs imply any obfervation of the mode of operating. It is in the one the prefent perception of each effect when it happens, in the other the recollection of a feries of effects in the order in which they had happened. How-

ever,

ever, as in the material, ſo in the intellectual world, ſuch obſervations of the connection of effects are of conſiderable importance. Though we cannot penetrate the eſſence of the mind, and diſcover how it thinks, we may learn many uſeful leſſons with regard to the conduct of our underſtandings and the regulation of our paſſions; as in the material world we may diſcover methods of rendering the powers of nature ſubſervient to our convenience, whilſt their manner of operating is wholly unknown.

HAD attention been given to this diſtinction we might perhaps have eſcaped the intricate inquiries for which metaphyſics have been ſo diſtinguiſhed. The queſtions concerning matter and ſpirit and human liberty appear to belong to the claſs of inquiries into the nature of cauſes or their manner of operating, which baffle the reſtleſs curioſity of human ſpeculation. Perhaps, however, this obſervation may be retorted, and it may be ſaid that Doctor Prieſtley, who has lately revived them, has ſufficiently ſhewn that thoſe ſubjects do not lie beyond our comprehenſion. It will, therefore, be neceſſary to aſſign reaſons for the purpoſe of proving the arguments alleged by Doctor Prieſtley to be inconcluſive.

To his argument, in proof of the Materiality of the Soul, I will content myſelf with oppoſing Berkeley's argument in favour of Spirit. Such is our ignorance of cauſes that we are incapable of diſcovering any eſſential diſtinction amongſt them. If we begin by acknowledging Matter, we are led by the ordinary rules of reaſoning reafoning to conclude with Doctor Prieftley, that all caufes are material. If we begin by acknowledging Spirit, we are led to conclude with Berkeley, that all caufes are fpiritual or immaterial. Nor let it be thought that thefe two metaphyficians, fetting out from different points, meet in one conclufion. Doctor Prieftley does indeed exclude Solidity from his idea of Body; but this, however it may facilitate conviction, by removing the objection of the fuppofed imcompatibility of folidity and thought, as properties of the fame fubftance, does not by any means appear effential to his argument. Befides, he afcribes efficiency to beings incapable of thought, and to thinking beings he afcribes other powers befides that of thinking or fuggefting ideas.

WITH regard to the queftion of Human Liberty, Doctor Prieftley's argument appears to reft intirely upon the mere fuppofition of the truth of a principle the oppofite to that which I have endeavoured to eftablifh, namely, that the fubject is within the reach of the human underftanding; fince he fupports the doctrine of neceffity only by the impoffibility of maintaining that of free-agency. If this fhould appear to be the ftate of the argument, it cannot be ufed to prove that very fuppofition.

IN the fecond fection of his Illuftrations of Philofophical Neceffity, he fays, that to evade the force of his great argument of caufe and effect, it is faid, "that though in a given ftate of "mind two different determinations may take place, neither of "them can be faid to be without a fufficient caufe; for that "in this cafe the caufe is *the mind itfelf*, which makes the deter- "mination in a manner independent of all motives." This, which

which Doctor Prieftley introduces as an argument ufed to evade the force of reafoning by which the doctrine of philofophical neceffity had been maintained, is really the ftatement of the opinion of thofe who embrace the doctrine of free-agency; and until it fhall have been overturned no decifive progrefs can have been made in the eftablifhment of the oppofite doctrine. On this point then the whole queftion turns. What is Doctor Prieftley's anfwer? "That the mind itfelf, independent of the "influence of every thing that comes under the defcription of "motive, bearing an equal relation to both the determinations, "cannot poffibly be confidered as a caufe with refpect to either "of them in preference to the other; becaufe, exclufive of what "may properly be called motive, there is no imaginable dif-"ference in the circumftances immediately preceding the deter-"minations. Every thing tending to produce the leaft degree "of inclination to one of the determinations more than the "other muft make a difference in the ftate of mind with refpect "to them, which by the ftating of the cafe is exprefsly ex-"cluded; *and I will venture to fay that no perfon, let his bias in* "*favour of a fyftem be ever fo great, will chufe to fay in fupport* "*of it that the mind can poffibly take one of two determinations* "*without having for it fomething that may at leaft be called an* "*inclination for it in preference to the other*; and that inclination, "or whatever elfe it be called, muft have had a caufe producing "it in fome previous affection of the mind." I will not, indeed, chufe to fay that the mind can take one of two determinations without being itfelf determined by fome preceding circumftance, becaufe I will not chufe to affume the queftion of the freedom of the will; but I do not fee that, becaufe I decline to affume the truth of one ſide of this queſtion, Doctor Prieſtley is therefore authorized to aſſume the oppoſite. This argument of cauſe and effect is, he ſays, *the great and moſt concluſive argument* for his doctrine. To what does it amount? The queſtion is fairly ſtated, and if in this dark and doubtful conteſt no metaphyſician boldly maintains the doctrine of free-agency, the victory of philoſophical neceſſity is at once proclaimed. Were the enquiry commenſurate to the human underſtanding this would be a fair appeal to the common ſenſe of mankind; but I have already aſſigned a general reaſon for thinking that all enquiries into cauſes are beyond our comprehenſion.

It may poſſibly be thought that this is an enquiry only into the *connection* of cauſes and effects, ſince it only propoſes to determine whether the operation of moral cauſes is neceſſary or contingent, and that it therefore is reducible to the claſs of thoſe in which probability is attainable; but the argument of Doctor Prieſtley is derived from a general conſideration of the nature of the human mind, and not from any experimental obſervation of facts. Not that I think ſuch obſervation could ſupport his ſyſtem; even though it were certain that man is a free-agent, he could not have any experimental proof of his freedom, ſince he could not know by experience that he could in any inſtance have acted in a manner different from that in which he then choſe to act. Since therefore a free-agent could not by experience diſcover his freedom, it cannot be proved from experience that a being acts neceſſarily. Experience muſt in both caſes be the ſame, and therefore cannot eſtabliſh the truth of either.

Mr.

Mr. Hume has defended the doctrine of neceſſity on a ground different from either of thoſe which have been mentioned: He is of opinion "that men begin at the wrong end of this queſtion "when they enter upon it by examining the faculties of the "ſoul," and propoſes to determine it by an obſervation of the general conduct of mankind. "It is," he ſays, "univerſally "acknowledged that there is a great uniformity among the "actions of men in all nations and ages, and that human na-"ture remains ſtill the ſame in its principles and operations. "The ſame motives always produce the ſame actions; the ſame "events follow from the ſame cauſes." Hence he contends that there is the ſame conſtant *conjunction* in the voluntary actions of men and in the operations of mind as in the material world, and we are therefore required to acknowledge the ſame neceſſity in the one as in the other. In anſwer to this it muſt be obſerved, that the caſe oppoſed by Mr. Hume to that of ſtrict neceſſity is a total diſregard to motives, but this is a caſe for which the advocates of liberty do not contend. They allow that motives do very generally influence the conduct of mankind, and only maintain that the mind has a power of reſiſting and rejecting them: They do therefore acknowledge *that there is a great uniformity among the actions of men*; and Mr. Hume himſelf admits that "it is poſſible to find ſome actions, which ſeem to "have no regular connection with any known motives, and are "exceptions to all the meaſures of conduct which have ever "been eſtabliſhed for the government of men." For theſe anomalous caſes Mr. Hume does indeed endeavour to account, by ſaying, that as in the material world a philoſopher concludes that "a ſeeming uncertainty in ſome inſtances proceeds from the

" the ſecret oppoſition of contrary cauſes," he muſt, if he be conſiſtent, " apply the ſame reaſoning to the actions and voli-" tions of intelligent agents." This, however, appears to be merely what logicians call *begging the queſtion.* The queſtion is, whether human actions are regulated by the ſame neceſſity which connects material cauſes and effects? The proof is, that we ought to argue about the former on the ſame principle as about the latter. But Mr. Hume ſeems conſcious that he had in this inſtance unfairly begged the queſtion, for in the words immediately following he renounces his advantage by giving up the queſtion: " Or even," he ſays, " when an action, as ſometimes " happens, cannot be particularly accounted for, either by the " perſon himſelf or others, we know, in general, that the cha-" racters of men are, to a certain degree, inconſtant and irregular. " This is in a manner the conſtant character of human nature; " though it be applicable in a more particular manner to ſome " perſons who have no fixed rule for their conduct, but proceed " in a continued courſe of caprice and inconſtancy." In theſe words he appears to me to abandon his firſt principle, that *the ſame motives always produce the ſame actions*; to acknowledge that there is in all, but more particularly in ſome men, an inconſtancy of character which renders it impoſſible to account for their conduct in all caſes, and conſequently to leave the queſtion of neceſſity in its original uncertainty.

The doctrine of liberty has lately been maintained by Doctor Gregory, who has undertaken to eſtabliſh it, by proving, on phyſical and mathematical principles, that the doctrine of Neceſſity is abſurd. The doctrine of Neceſſity, as he has ſtated it, he has, I think,

think, clearly refuted; but he has not ftated it in the fame manner with Doctor Prieftley. He fuppofes that when a number of motives are prefent to the mind, each fhould, if the doctrine of Neceffity were true, have its determined effect, and he fhews that the refults arifing from their combinations would not be fuch as are obferved to happen. In page 600 he fays, "let the *manner* "of the *conjunction* of caufe and effect in phyfics be fuppofed as "different as poffible from the *manner* of the *conjunction* of mo-"tive and action; only let the conjunction in both relations be "*conftant*, and the whole of my reafoning from the dilemma and "axioms to the laft inference muft remain unfhaken, and all my "conclufions will be found fuch as may be tried experimentally." If then this be not fuppofed by the advocates of Neceffity, the reafoning of this writer is by his own conceffion without foundation. It, I think, appears from Doctor Prieftley's treatife, that he regarded all the confiderations prefent to the mind as forming *one motive*, and that to this collective view he attributed the neceffary determination of the will. The queftion, therefore, on which he argues is, whether the mind can, in the fame combination of circumftances, form different determinations, whilft the queftion on which Doctor Gregory argues is, whether it is not abfurd to conceive that in every combination each diftinct confideration fhould be connected with a correfponding effect. That Doctor Prieftley did not confider each diftinct motive as connected with its correfponding action, will, I think, appear from the following paffages.

In the firſt ſection of his treatiſe on the doctrine of Neceſſity he ſtates his opinion in theſe words: "I maintain that there is ſome "fixed law of nature reſpecting the will as well as the other "powers of the mind, and every thing elſe in the conſtitution of "nature; and conſequently, that it is never determined without "ſome real or apparent cauſe foreign to itſelf, i. e. without ſome "motive of choice, or that motives influence us in ſome definite "or invariable manner, *ſo that every volition or choice is conſtantly* "*regulated and determined by what precedes it*; and this conſtant "determination of mind, according to the motives preſented to "it, is all that I mean by its neceſſary determination." And in the ſixth ſection he ſays, "If we always chooſe that object or "that action, which, on whatever account, appears preferable at "the moment of making the choice, it will always be determined "by ſome invariable rule depending upon the ſtate of the mind "and the ideas preſent to it; *and it will never be equally in our* "*power to chooſe two things, when all the previous circumſtances* "*are the very ſame.*" In the ſecond ſection he ſays, that "to "eſtabliſh the concluſion defined in the preceding ſection, nothing "is neceſſary but that, throughout all nature, the ſame conſe- "quences ſhould invariably reſult *from the ſame circumſtances*. For, "if this be admitted, it will neceſſarily follow, that at the com- "mencement of any ſyſtem, ſince the ſeveral parts of it, and "their reſpective ſituations, were appointed by the Deity, the firſt "change would take place according to a certain rule eſtabliſhed "by himſelf, the reſult of which would be *a new ſituation*; after "which, the ſame laws continuing, another change would ſuc- "ceed, according to the ſame rules, and ſo on for ever; *every new*

"*ſituation*

" *ſituation invariably leading to another*, and every event, from the " commencement to the termination of the ſyſtem, being ſtrictly " connected; ſo that, unleſs the fundamental laws of the ſyſtem " were changed, it would be impoſſible that any event ſhould " have been otherwiſe than it was." In the ſame ſection he expreſsly calls the collective view of all the conſiderations ſuggeſted to the mind the motive. " In every determination of " mind, or in caſes where volition or choice is concerned, all the " previous circumſtances to be conſidered are, the ſtate of mind " (including every thing belonging to the will itſelf) and the views " of things preſented to it; *the latter of which is generally called* " *the motive, though under this Term ſome writers comprehend them* " *both*." And he expreſſes himſelf in the ſame manner in the following page: " A particular determination of mind could not " have been otherwiſe than it was, if the laws of nature reſpect- " ing the mind be ſuch *as that the ſame determination ſhall con-* " *ſtantly follow the ſame ſtate of mind and the ſame views of things*." And in the fourth ſection he ſays, " whenever any perſon makes " a choice, or comes to any reſolution, there are two circum- " ſtances which are evidently concerned in it, viz. what we call " the previous diſpoſition of the mind with reſpect to love or " hatred; for example, approbation or diſapprobation of certain " objects, &c. and the ideas of external objects then preſent to " the mind, that is, *the view of the objects* which the choice or " reſolution reſpects." Doctor Gregory, as the reſult of his argument, has determined, that a motive is not a phyſical cauſe impelling a man to act, but *that for the ſake of which* a man acts;

and

and confidered this as the diftinction between the doctrines of Neceffity and Liberty; but Doctor Prieftley has fpoken of it as a diftinction wholly unimportant, according to his notion of Neceffity. In the fecond fection he fays, "No lefs fallacious is it to "fay that motives do not impel or determine a man to act; but "that a man, from the view of the motives, determines himfelf "to act." And in the fourth fection he fays, "Every volition is "nothing more than a defire, viz. *a defire to accomplifh fome end*, "which end may be confidered as the object of the paffion or "affection." In the following words he has guarded againft any miftake which might arife from his comparifon of the mind to a balance. "It is acknowledged that the mechanifm of the balance "is of one kind and that of the mind of another, and therefore "it may be convenient to denominate them by different words; "as for inftance, that of the balance may be termed a *phyfical*, "and that of the mind a *moral mechanifm*. But ftill if there be "a real mechanifm in both cafes, *fo that there can be only one* "*refult from the fame previous circumftances*, there will be a real "neceffity, enforcing an abfolute certainty in the event."

Mr. Hume has not expreffed himfelf with fo much clearnefs as Doctor Prieftley; but his ambiguity renders his opinion equally fecure from the attacks of Doctor Gregory, fince his expreffions are at leaft equally applicable to the opinion of Doctor Prieftley as to that which Doctor Gregory has controverted. In his Effay on Liberty and Neceffity he fays, that the inferences concerning human actions "are founded on the experienced union of like ac-

"tions

" tions with like motives, inclinations and circumſtances." In the beginning of his Eſſay he had ſaid, that " the ſame motives " always produce the ſame actions;" but he afterwards explains this aſſertion. " We muſt not," he ſays, " however expect that " this uniformity of human actions ſhall be carried to ſuch a " length as that all men in the ſame circumſtances will always act " preciſely in the ſame manner, without making any allowance " for the diverſity of characters, prejudices and opinions."

THE greateſt efforts in metaphyſical inquiry appear then, by the difficulties in which they are involved, to give confirmation to the opinion, that the nature of cauſes and their manner of operating are hid from us in impenetrable obſcurity. The attempts made by Doctor Prieſtley and Mr. Hume to eſtabliſh the doctrine of Neceſſity, have, I imagine, been ſhewn to belong to that claſs of inconcluſive reaſoning which logicians denominate *Petitio Principii*, and Doctor Gregory's attempt to overthrow it to belong to the claſs called *Ignoratio Elanchi*; whilſt on the queſtion of Materialiſm Doctor Prieſtley and Biſhop Berkeley refute each other by contradictory arguments. Between this claſs of inquiries and that in which we are capable of arriving at certainty lies the claſs of mere Probability. In this middle claſs all the practical, and conſequently all the immediately uſeful, knowledge of mankind is to be found. Mathematical ſpeculations and the abſtract rules of logical reaſoning may boaſt the high privilege of abſolute certainty, but they are only uſeful as far as they are capable of being applied to human actions; and in this application the mind of man muſt be content with an aſſurance of leſs ſtrength.

The ſecond claſs I defined to be that in which a conſideration of the *connection* of cauſes and effects is directly or indirectly involved. That where ſuch a connection is the object of inquiry probability only is attainable, is a direct inference from what has been mentioned with regard to the third claſs. If we are wholly ignorant of the nature of all cauſes and their modes of operating, we cannot be in any caſe certain of the connection of effects with each other, or with thoſe cauſes to which they are aſcribed.

Mr. Hume has indeed, from this principle, drawn a much more extenſive concluſion. From our ignorance of the *nature* of the connection of cauſe and effect he has inferred, that we cannot reaſon about the *exiſtence* of ſuch a connection; and that our ſuppoſition of its exiſtence is only the reſult of a cuſtomary tranſition of the mind from the one object to the other. This inference, which is the foundation of his ſcepticiſm, is ſupported by the following argument. Between theſe two propoſitions *I have found that ſuch an object has always been attended with ſuch an effect*, and *I foreſee that other objects, which are, in appearance, ſimilar, will be attended with ſimilar effects*, the connection is not intuitive. There is therefore required a medium which may enable the mind to draw ſuch an inference, if indeed it be drawn by reaſoning and argument. But there is not any ſuch medium, ſince the idea of ſuch a connection cannot be ſuggeſted by any ſingle inſtance, and there is nothing, in a number of inſtances, different from any ſingle inſtance which is ſuppoſed to be exactly ſimilar, except only that after a repetition of ſimilar inſtances the mind is carried by habit, upon the appearance of one event, to expect

expect its usual attendant, and to believe that it will exist. In answering this argument all that is necessary is to produce that medium which he declared passed his apprehension. It, I think, consists of two propositions. The former is that *those things which begin to exist have not an independent existence*; the latter, *those things which do not exist of themselves, or independently, must derive their existence from some other things.* These two abstract principles are to me self-evident. *Perpetuity of existence* is inseparably connected with *Necessity of existence*; and the notion of *derived existence* is inseparably connected with the notion of that which is not *necessary.* The former is the principle of the first proposition, the latter of the second. We must therefore acknowledge that every thing which begins to exist has derived its existence from some other being as its cause. This has, indeed, been acknowledged by Mr. Hume himself. "It is," he says, "universally "allowed that nothing exists without a cause of its existence."

IGNORANT as we are of the nature of causes, we are, indeed, unable to determine whether all Effects should be ascribed immediately to the first cause; or whether, by the appointment of that first cause, there has been established a connection between created things. On the latter supposition we might conclude that there is a real *connection*, where we have observed an uniform *conjunction*; but even on the first we are authorized to infer the probability of a similar conjunction of effects in similar cases yet unobserved by us. It is agreeable to the opinion, that all things derive their existence immediately from one great author, to believe that there should be a simplicity and uniformity in this continued system of creation.

The probability of any particular inference on either fuppofition will be proportioned to what we conceive to be the extent of our obfervation of the analogies of nature. Mr. Hume has faid, that if any intricate or profound argument be produced, it is in a manner giving up the queftion, becaufe it fhould be obvious to the capacity of an infant; but, though the former fuppofition be indeed too profound, the latter has no abftrufenefs, except what it derives from the abftract form in which it has been propofed; and other maxims, which, expreffed abftractedly, would be as difficult to an infant, are yet readily admitted in their application. It would not be eafy to convince an infant that *the whole is greater than a part*, and yet he would not find any difficulty in a particular inftance. But if after even this abatement it fhould ftill be thought too difficult, there is not any reafon why we fhould not fuppofe that the infant is influenced by the acknowledged principle of the affociation of ideas; and that what in him is affociation is in the man affociation corrected and ftrengthened by reafoning.

This clafs, which has for its object the connection of caufes and effects, evidently contains all the enquiries of natural philofophy; and what has been faid under the firft head of the divifion has, I fuppofe, made it appear that morality is alfo comprifed within it. It remains to be fhewn that it includes thofe propofitions which are fupported by the evidence of teftimony, or which relate to the computations of chance.

All

All enquiries with regard to teſtimony may be reduced to two heads: In proportion as we are ſatisfied that a witneſs has not been influenced by any deſire of deceiving, and has not himſelf been deceived, we give credit to his teſtimony. The examination then by which we eſtimate the credit due to teſtimony conſiſts of two parts, and if it ſhall appear that each is a conſideration of the *connection of cauſe and effect*, it will be allowed that all propoſitions whoſe credit reſts upon teſtimony are rightly claſſed.

When we wiſh to determine whether a witneſs has been influenced by a deſire of deceiving, we conſider what motives could have induced him to wiſh to deceive us, or whether the means which he employed could promiſe him ſucceſs in a ſcheme of deception. The former conſideration is evidently an enquiry into the operation of motives, that is, of moral cauſes on his mind; and the latter will, I think, appear after a little conſiſideration to be an enquiry of the ſame kind, though ſomewhat more complicated. An enquiry into his judgment of the probability of his ſucceſs, is an enquiry into the operation of a view of the circumſtances in which he was placed, conſidered as a motive which ſhould determine him in the formation of his plan of action. It is therefore an enquiry of the ſame kind. It is however a more complicated enquiry, becauſe it is made for the purpoſe of enabling us to form a judgment of his judgment of his ſituation. He deliberates about the operation of motives on the minds of others in diſpoſing them to concur with his ſcheme or to oppoſe it; but we, from our view of his ſituation, deliberate about the expectation which he muſt have

entertained

entertained concerning the probability of concurrence or oppoſition. When, on the other hand, we would determine whether a witneſs has been himſelf deceived, we conſider the ſtate of his mind at the time when he ſuppoſed the fact, of which he has given teſtimony, to have happened. This again is a conſideration of the influence of moral cauſes. I cannot give a fuller illuſtration of what I have ſaid than by referring to Lord Lyttleton's celebrated Obſervations on the Converſion of Saint Paul, from which indeed this diviſion of the enquiry into the credibility of a witneſs has been taken. It is not at all neceſſary to my purpoſe that the queſtion of human liberty ſhould be examined. That motives have ſome influence on the mind will not be denied by thoſe who maintain its freedom, and the deficiency of their influence muſt be ſupplied by the ſelf-determining power of the mind, which is a cauſe whoſe manner of operating is equally remote from our comprehenſion.

Doctor Campbell, in his very able Examination of Mr. Hume's Eſſay on Miracles, has contended " that teſtimony hath " a natural and original influence on belief antecedent to ex- " perience," and in the ſenſe in which he has aſſerted this propoſition it appears to be true. Teſtimony has an influence on belief *antecedent to inferences from the conduct of others*, but this influence is founded on the conſciouſneſs which a child has of his own veracity. When he does not ſpeak for enquiry, he ſpeaks to communicate his own ideas. Subſequent experience of the conduct of others may teach him that the noble gift of ſpeech is ſometimes abuſed, or he may learn the ſame leſſon of diſtruſt from the artifices which he himſelf is ſometimes induced to adopt; but the original and genuine uſe of ſpeech he feels

is

is to communicate the real thoughts of his mind. The credit of teſtimony is therefore founded on our original experience of our own veracity, though our eſtimates of it are afterwards corrected by an enlarged view of the general conduct of mankind.

THE probabilities of chance are included within this general deſcription of probability. Doctor Reid has very properly obſerved, " that we attribute ſome events to chance, becauſe we " know only the remote cauſe which muſt produce ſome one " event of a number, but know not the more immediate cauſe " which determines a particular event of that number in pre- " ference to the others." This he has illuſtrated by obſerving, " that in throwing a juſt die upon a table we ſay it is an equal " chance which of the ſix ſides ſhall be turned up, becauſe " neither the perſon who throws, nor the by-ſtanders, know the " preciſe meaſure of force and direction neceſſary to turn up " any one ſide rather than another." Eſſay 7. ch. 3. The eſtimate of chance appears therefore to be founded in a conſideration of the connection of cauſe and effect. When we are unable to diſtinguiſh thoſe circumſtances of the cauſe which will determine the event in a particular manner, we proceed as if all the events which might poſſibly ariſe from the ſame general cauſe, acting in various circumſtances, were equally probable, and make our computation merely from the number. The example mentioned by Doctor Reid belongs to that claſs of probable propoſitions which relates to the operation of phyſical cauſes. If the ſubject of computation were the contingency of human conduct it would belong to the claſs of moral cauſes.

BEFORE

Before I conclude this eſſay, I would remark a peculiarity in the probability of teſtimony which ſeems to add to it conſiderable force: This peculiarity is derived from the ſucceſſive nature of the acts of the mind. In the material world cauſes and effects co-exiſt; and as we conjecture the connection between them only from their correſpondence, it may frequently be difficult to determine to which of two co-exiſting objects we ſhould aſcribe an effect. The queſtion concerning phlogiſton, as ſtated in Nicholſon's Firſt Principles of Chemiſtry, may afford an example of ſuch a difficulty. "The great queſtion," according to this writer," now is, whether inflammable air be con-"tained in all combuſtible bodies, ſince they do not all emit it "by mere heat; and it is evident, that if combuſtion can be "effected without it in any one inſtance, it cannot be the in-"diſpenſable and univerſal principle of inflammability. Its "exiſtence is denied in ſulphur, phoſphorus, charcoal, metals, "and ſome other ſubſtances. It may however be obtained by "heating thoſe if water be preſent: *whether it is afforded by the* "*ſubſtance under examination, or by the water, is therefore the ſubject* "*of controverſy*." Book 2. ſec. 1. chap. 2. In the operations of the mind this ambiguity cannot prevail in the ſame degree. We judge of the connection of moral cauſes and their effects by their order of ſucceſſion, and as the mind cannot at the ſame time give conſiderable attention to more than one motive, we are not liable to much ambiguity in our obſervation of the tendency of that motive; beſides, though in ſome caſes ſeveral motives may conſpire to influence the mind to the ſame determination, yet in others they operate ſeparately. In thoſe other caſes we may learn the natural tendency of thoſe motives, and we may apply the reſult of thoſe obſervations in caſes more complicated

complicated. We conclude for inſtance that in a certain caſe the conſideration of pecuniary intereſt has had a certain influence on the mind, becauſe no other motive appeared in that inſtance to be preſent to the mind, and we could ſcarcely miſtake in attributing the effect to the *ſingle* cauſe with which it appeared to have connection. In like manner we conclude that the hopes of credit or power, and the deſire of gratifying paſſion, produce certain tendencies, and that different ſtates of mind diſpoſe men variouſly with regard to the reception of truth. It were eaſy to ſelect from the ſacred writings examples of ſuch caſes. This peculiarity in the operation of moral cauſes appears to give conſiderable force to concluſions concerning their influence in particular caſes, and to balance any diſadvantage which might ariſe from our inability to determine the queſtion of their neceſſary operation. If we could aſcertain that moral cauſes act neceſſarily, this ſucceſſive nature of the operations of the mind might perhaps in ſome caſes, not too much complicated, enable us to arrive at certainty; but as I conceive that this queſtion is beyond the limits even of probable conjecture, I conclude that the credit of teſtimony can never riſe above probability. Our ignorance of the nature of all cauſes, moral as well as phyſical, muſt baniſh ſtrict and abſolute certainty from every enquiry into the material or intellectual world.

I HAVE now finiſhed what I propoſed; and if it ſhall appear that I have more accurately deſcribed the nature and boundaries of certainty and probability, and diſtinguiſhed both from that region into which the human mind is unable to penetrate, I ſhall think that I have done ſome ſervice to the cauſe of truth; if I have failed, this eſſay will only be one *negative inſtance* more, and may with others ſerve to guide ſome future experimenter.

METEOROLOGICAL OBSERVATIONS *in* IRELAND *in the Year* 1793. *By* RICHARD KIRWAN, *Esq; LL.D. F. R. S. and M. R. I. A.*

IN my former papers on this subject I have laid down the rules of probability or measures of expectation of the three most important seasons of the year, as far as they could be established by an experience of forty-one years, and determined the limits of each with as much precision as the *data* I could collect would admit. The seasons that are conformable to these I shall therefore call *regular*, and those that deviate from them *anomolous*, until a still longer experience instructs us to alter or improve these rules. It will therefore be the business of the meteorologist who chuses to follow this method to exhibit every year a view of the seasons of that immediately preceding, and examine their conformity with these

Read Jan. 25, 1794.

theſe rules. To effect this more eaſily, and without recurring to anterior volumes of our Academy, it may be proper to exhibit a yearly ſhort view of the ſeaſons under their reſpective denominations, and alſo of the meaſures of expectation.

FIRST TABLE.

Seaſons.

Spring 61 Days. Rain.			Summer 92 Days. Rain.			Autumn 61 Days.		
	Inches.	Days.		Inches.	Days.		Inches.	Days.
Wet -	3,78+	36+	Wet -	5,67+	54	Wet -	3,78+	36,+
Variable	3,15±	30±	Variable	4,72±	45±	Variable	3,15±	36,±
Dry -	2,52—	24-	Dry -	3,78—	36-	Dry -	2,52—	24-

SECOND TABLE.

Probabilities at the Beginning of a Year.

1. Spring.		2. Summer.		3. Autumn.	
Wet - -	6/41	Wet - -	20/41	Wet - -	11/41
Variable -	13/41	Variable -	5/41	Variable -	19/41
Dry - -	22/41	Dry - -	16/41	Dry - -	11/41

OF

Of Spring I have as yet no prognoſtics, but it is poſſible that in time the mean height of the barometer in March will furniſh ſome. The mean of March 1792 was 29,707, and the Spring was wet. That of March 1793 was 29,96, and the Spring was variable.

THIRD TABLE.

Probabilities of Summer.

Prognostics.			
Spring	Wet.	Variable.	Dry.
Wet - - -	$\frac{5}{6}$	$\frac{1}{6}$	0
Variable - - -	$\frac{7}{13}$	$\frac{1}{13}$	$\frac{5}{13}$
Dry - - -	$\frac{8}{22}$	$\frac{3}{22}$	$\frac{11}{22}$

FOURTH TABLE.

Probabilities of Autumn.

Prognostics.			
Summer	Wet.	Variable.	Dry.
Wet - - -	$\frac{3}{20}$	$\frac{12}{20}$	$\frac{5}{20}$
Variable - - -	$\frac{3}{5}$	$\frac{1}{5}$	$\frac{1}{5}$
Dry - - -	$\frac{5}{16}$	$\frac{6}{16}$	$\frac{5}{16}$

FIFTH

FIFTH TABLE.

Probabilities of Autumn.

PROGNOSTICS.		Wet.	Variable.	Dry.
Wet Spring and	Wet Summer -	$\frac{1}{5}$	$\frac{2}{5}$	$\frac{2}{5}$
	Variable -	$\frac{0}{41}$	$\frac{0}{41}$	$\frac{1}{41}$
	Dry - -	$\frac{0}{41}$	$\frac{0}{41}$	$\frac{0}{41}$
Variable Spring and	Wet Summer -	$\frac{1}{7}$	$\frac{5}{7}$	$\frac{1}{7}$
	Variable -	$\frac{1}{41}$	$\frac{0}{41}$	$\frac{0}{41}$
	Dry - -	$\frac{0}{41}$	$\frac{2}{41}$	$\frac{3}{41}$
Dry Spring and	Wet Summer -	$\frac{0}{41}$	$\frac{6}{8}$	$\frac{2}{8}$
	Variable -	$\frac{2}{3}$	$\frac{1}{3}$	$\frac{0}{41}$
	Dry - -	$\frac{4}{11}$	$\frac{4}{11}$	$\frac{3}{11}$

Of the Diſtinctions of Variable.

THE modification called *variable* being intermediate between *dry* and *wet*, may ſometimes approach very nearly (that is, within one or two-tenths) to the one, and ſometimes to the other; and hence I ſhall diſtinguiſh *variable inclining to dry*, and *variable inclining to wet:* it is reaſonable to conclude that when this modification occurs as a *prognoſtic* it ſhould be deemed to participate but in a leſſer degree of the foreboding properties of that modification to which it approaches moſt; and alſo indicate a *leſſer degree* of the modification foreboded, by the prognoſtic to which it approaches. As the prognoſtications however founded on theſe diſtinctions are not the reſult of immediate obſervation, I ſhall comprize them in ſeparate tables, that their validity may be eſſayed by future experience. If found uſeful, they may be enlarged.

SIXTH TABLE.

Probabilities of Summer.

PROGNOSTICS.

Spring.	Wet.	Variable Wet.	Variable.	Variable Dry.	Dry.
Wet - -	$\frac{5}{6}$	$\frac{4}{6}$	$\frac{1}{6}$	$\frac{1}{41}$	0
Variable - -	$\frac{1}{13}$	$\frac{6}{13}$	$\frac{1}{13}$	$\frac{4}{13}$	$\frac{5}{13}$
Dry - -	$\frac{8}{22}$	$\frac{6}{22}$	$\frac{3}{22}$	$\frac{10}{22}$	$\frac{11}{22}$

SEVENTH TABLE.

Probabilities of Summer.

PROGNOSTICS.

Spring.	Wet.	Variable.	Dry.
Variable wet - - - - -	$\frac{4}{6}$	$\frac{6}{100}$	$\frac{1}{13}$
Dry - - - - -	$\frac{7}{22}$	$\frac{2}{22}$	$\frac{9}{22}$

EIGHTH

EIGHTH TABLE.

Probabilities of Autumn.

	PROGNOSTICS.				
Summer.	Wet.	Variable Wet.	Variable.	Variable Dry.	Dry.
Wet - -	$\frac{3}{20}$	$\frac{6}{20}$	$\frac{12}{20}$	$\frac{7}{20}$	$\frac{5}{20}$
Variable - -	$\frac{3}{5}$	$\frac{2}{5}$	$\frac{1}{5}$	$\frac{1}{5}$	$\frac{1}{5}$
Dry - -	$\frac{5}{16}$	$\frac{4}{16}$	$\frac{6}{16}$	$\frac{4}{16}$	$\frac{5}{16}$

NINTH TABLE.

Probabilities of Autumn.

	PROGNOSTICS.		
Summer.	Wet.	Variable.	Dry.
Variable wet - - -	$\frac{2}{20}$	$\frac{10}{20}$	$\frac{4}{20}$
Variable dry - - - -	$\frac{4}{20}$	$\frac{5}{16}$	$\frac{4}{16}$

TENTH TABLE.

Probabilities of Autumn.

PROGNOSTICS.	Wet.	Variable.	Dry.
Wet spring, and summer variable wet	$\frac{1}{6}$	$\frac{1}{41}$	$\frac{1}{5}$
Summer variable dry	$\frac{1}{41}$	$\frac{1}{41}$	$\frac{1}{41}$
Spring var. wet, and summer variable wet	$\frac{1}{7}$	$\frac{1}{41}$	$\frac{1}{6}$
Summer variable dry	$\frac{2}{41}$	$\frac{2}{41}$	$\frac{2}{41}$
Spring var. dry, and summer variable wet	$\frac{2}{41}$	$\frac{5}{8}$	$\frac{2}{8}$
Summer variable dry	$\frac{1}{41}$	$\frac{1}{41}$	$\frac{1}{41}$
Dry spring, and summer variable wet	$\frac{1}{41}$	$\frac{5}{8}$	$\frac{1}{8}$
Summer variable dry	$\frac{3}{11}$	$\frac{3}{11}$	[illegible]/11

A View

A View of the Weather in 1793.

	Barometer.			Thermometer.			Rain.	
	Higheft.	Loweft.	Mean.	Higheft.	Loweft.	Mean.	Days.	Inches.
January -	30,68	29,05	30,12	52,	28,	39,32	20	1,8911
February -	30,23	29,14	29,92	55,5	29,	42,17	18	2,1281
March -	30,45	29,33	29,96	55,	31,5	38,27	18	2,0887
April -	30,57	29,42	30,05	60,	31,	44,87	18	2,3645
May - -	30,60	29,27	30,30	67,5	41,	52,06	11	0,6305
June - -	30,47	29,56	30,11	69,5	43,	56,95	22	1,6157
July - -	30,35	29,81	30,16	80,	48,	63,98	20	2,0093
Auguft -	30,29	29,65	30,05	75,5	48,5	61,51	23	2,0093
September	30,57	29,30	30,16	67,	40,	54,65	14	2,4828
October -	30,68	29,41	30,11	67,	33,	54,04	16	1,1034
November	30,63	29,22	29,90	54,	30,	44,35	17	2,7192
December	30,60	28,68	29,81	55,	32,	43,51	17	1,8128
			30,054			49,64	Total 214	Total 22,8554

THE

THE greateſt height of the barometer, and conſequently the higheſt atmoſpheric tide, was in *October*, the loweſt in *December*; the month during which its mean height was greateſt was *May*; that during which it was loweſt was *December*.

IN 1792 its greateſt height was in *September*, its loweſt in *January*, and the month during which it was higheſt on a mean was *June*, and that in which the mean was loweſt was *March*.

View of the Seaſons.

SPRING.	Rain. Inches.	Days.	SUMMER.	Rain. Inches.	Days.	AUTUMN.	Rain. Inches.	Days.
			June	1,6157	22			
April	2,3645	18				September	2,4828	14
			July	2,0093	20			
May	0,6305	11	Auguſt	2,0093	23	October	1,1034	16
	2,9950	29		5,6343	65		3,5862	30

HENCE we ſee the *ſpring was variable*, whether we conſider the quantity of rain or number of days.

THE *ſummer was variable inclining to wet*, if we conſider the quantity of rain, or even *wet*, if we conſider the number of rainy days.

THE

THE *autumn was variable ſlightly inclining to wet*, if we conſider the quantity of rain, but *ſtrictly variable* if we attend only to the number of rainy days.

Compariſon of the Seaſons, with the Rules of Prognoſtication.

1°. THE ſpring being variable, the probability of a *wet* ſummer was the greateſt by the third and ſixth table, being $\frac{7}{13}$, but that of a *variable inclining to wet* was the next greateſt by the ſixth table, being $\frac{6}{13}$, and actually took place.

2°. THE ſummer being variable, the probability of a *wet* autmun was the greateſt by the fourth and eighth table, being $\frac{3}{5}$; but as the ſummer was *variable inclining to wet*, the probability of a variable autumn was alſo the greateſt, by the ninth table, being $\frac{10}{20}$.

3°. A VARIABLE ſpring ſucceeded by a variable ſummer occurred but once in 41 years by Dr. Rutty's obſervations, and theſe were ſucceeded by a wet autumn, therefore its probability ſtood ſingle, and was but $\frac{1}{41}$ by the fifth table; but variable ſprings were ſeven times followed by wet ſummers, and theſe were followed five times out of ſeven by variable autumns, as appears alſo by the fifth table; therefore as this ſummer was variable inclining to wet, the probability that it would be followed by a variable autumn alſo inclining to wet, was the greateſt

greateſt. Hence we may perceive the neceſſity of the diſtinctions of variable, and of enlarging the tables by their admiſſion, ſtill further.

Compariſon of the Years 1792 *and* 1793.

	Rain.		Months.				
	Inches.	Days.	3 Welteſt.	3 Dryeſt.	Dryeſt.	Mean.	Barometer.
In 1792	28,793 *	228	Auguſt September December	February June November	November	Heat. 50,5	Mean. 29,95
In 1793	22,85	214	November September April	May October June	May	49,6	30,05

In 1792 the winds in March blew 19 days from the W. or S. moſtly from the 12th to the end of the month. In 1793 it blew towards the end of the month chiefly from the eaſt. It is remarkable that though the quantity of rain was different in theſe two years, yet the number of rainy days did not differ much, they being only fewer in 1793 by 14.

In

* By a miſtake this was 30,7 in my laſt paper.

In 1792 they were to the whole year as 10 to 16, and in 1793 as 10 to 17.

It may now be proper to attempt to gain prognoftics of the different feafons from the ftate of the winter months that precede them. If we call *winter* thofe three months in which the greateft cold ufually prevails and vegetation is arrefted, we may reckon five in every year; three at its beginning, January, February and March, and two at its clofe, November and December. March indeed may be reckoned intermediate between winter and fpring, but it partakes more of winter; thefe five months precede the fucceeding feafons, I fhall therefore confider them together under thofe heads which appear to me moft likely to furnifh prognoftics.

Of the Winter preceding the Seafons of 1792.

1791.	Rain. Inches.	Days.	Mean of Barometer.	Mean Heat.	Storms.
November -	2,1088	22	29,74	43,21	1 W. N. W.
December -	1,8910	18	29,72	36,34	0
	3,9990	40	29,73	39,7	1
1792.					
January -	2,679	21	29,72	39,92	0
February -	1,576*	19	30,01	43,78	0
March - -	1,655†	25	29,70	44,09	9 all S.W. or S. or S. S.W.
	5,910	65	29,81	42,8	9
Total -	9,909	105	29,77	41,66	10

Enfuing

* By error 2,8240 in my laft paper.

† By error 2,3644 in my laft paper.

Enſuing Seaſons, Spring *wet*, Summer *wet*, Autumn *wet.*

Of the Winter preceding the Seaſons of 1793.

1792.	Rain. Inches.	Days.	Mean. Barometer.	Heat.	Storms.
November -	0,3940	14	30,05	48	0
December -	2,9163	17	29,986	42,4	5 W. N. W. or W. S. W.
	3,3103	31	30,01	45,2	5
1793.					
January -	1,8911	20	30,12	39,32	0
February -	2,1281	18	29,92	42,17	2 S. W. N. W.
March -	2,0887	18	29,96	38,27	2 S. W. before the 3d.
	6,1079	56	30, Mean	39,92	4
Total -	9,4182	87	30, Mean	42,5	9

Ensuing feaſons, ſpring *variable*, Summer *variable inclining to wet*, autumn *variable ſlightly inclining to wet.*

Among all the years obſerved by Dr. Rutty from 1725 to 1765, there occurs but one ſimilar to 1792, viz. the year 1755; in that the three ſeaſons, ſpring, ſummer and autumn, were wet; and by comparing my journal with his account, I find many other points of reſemblance; it were perhaps worth examining how far they reſembled each other with reſpect to human health. The

The year 1756 bore alfo fome refemblance to 1793, for the fpring was variable, the fummer wet, and the autumn variable.

Mr. Barker of Lyndon in England remarked that 1792 was the wetteft year that occurred fince 1782. The mean height of the barometer at Lyndon is 29,4 and the mean annual rain is about 23; but this year there fell 29,4 inches. The mean height of the barometer in March was about $\frac{2}{10}$ below its ftandard height.

EXPERIMENTS *on a* NEW EARTH *found near* STRONTHIAN *in* SCOTLAND. *By* RICHARD KIRWAN, *Esq; LL.D. F.R.S. and M.R.I.A.*

IN the mineral kingdom there are many fubftances whofe diftinctions are obvious; no one can be at a lofs to diftinguifh earths from ftones, or ftones from metals, or the various metals from each other. The utility of thefe diftinctions is as evident as the difference of characters on which they are founded is ftriking; but of late years lines of difcrimination have been traced between fubftances, moft of whofe characters refemble each other fo nearly, that they have ever before been deemed homogeneous. The difcovery of thefe latent diftinctions is often as important as that of the moft obvious, and much more difficultly effected; thus the difcovery of the difference between plumbago and molybdena led to the true knowledge of mineral coal; that of the difference between iron and manganefe led

Read Jan. 9, 1794.

to the more perfect knowledge of ſteel and iron ores; that of the difference between barytic and common lime-ſtone led to the knowledge of a ſubſtance that is now a capital inſtrument in chemical analyſis, and evinced the futility of that theory which deduced the origin of all ſubſtances that burned to lime from ſea ſhells. The ſubſtance I now announce to the Academy affords a farther proof of the danger of too ſtrict a reliance on general theories, and of the poſſibility of detecting many ſubſtances nearly allied to, but in reality differing from, thoſe with which we are already acquainted. It is only after ſome years of diligent but fruitleſs reſearch after ſuch aſſimilating ſubſtances that this *poſſibility* may be deemed an *improbability*.

The firſt account I received of this ſubſtance, which I ſhall call Stronthianite, was from Doctor Crawford in the year 1790; he was ſo obliging as to ſend me a ſpecimen, accompanied with a letter, informing me that from ſome experiments he made it appeared to him to contain a new earth; what theſe experiments were he did not mention.

Shortly after, however, it attracted ſome attention; in the Miner's Journal of February 1791 a good deſcription of its external appearance, and ſome account of its chemical properties, are given from the obſervations of Mr. Sulzer. I had not leiſure to examine it until laſt October; from the experiments I ſince made, in which I was aſſiſted by Mr. Higgins, ſuperintendent of our Apothecaries Hall, whoſe chemical abilities

ties are well known, and likely to be eminently uſeful to this country, it plainly appears to be a new earth, intermediate between the barytic and common limeſtone.

External Characters.

Its colour is whitiſh or light green.

Its luſtre common.

Its tranſparency intermediate between the ſemitranſparent and opake.

Its fracture ſtriated, preſenting oblong diſtinct concretions, ſomewhat uneven and bent.

Its hardneſs moderate, being eaſily ſcraped. Very brittle.

Its ſpecific gravity from 3,4 to 3,644.

SECTION

SECTION FIRST.

Its Relation to Heat and Fixed Air.

THIS ſtone expoſed to a heat of 130° Wedgwood, in common clay crucibles, vitrifies very readily when in contact with the crucible, but the interior part remains unchanged. Having heated two ounces of it in a black lead crucible, only a very ſmall part of it was vitrified; the remainder was converted into lime by a heat of 140°, and loſt 194 grs. that is, little more than 20 per cent. of its weight; ſubſequent experiments ſhewed that the weight thus loſt is fixed air, and that the loſs is ſtill greater than this experiment indicates, as by reaſon of the partial vitrification it cannot be perfectly aſcertained.

EQUAL parts quartz and Stronthian lime, melted in a heat of 138°, partly into an amber yellow glaſs, and partly into a black and white enamel, the ſurface preſented a ſtrong lead-coloured metallic glaze, which was communicated even to the interior of the crucible and to its cover.

Two parts of this lime and one of magneſia being heated to 138°, the lime vitrified with that part of the crucible with which it was in contact into a porcelain maſs, and acquired a purpliſh and greeniſh colour; the magneſia remained unaltered.

FOUR

Four parts Stronthian lime, and one of silex, heated to 138°, partly hardened and partly melted; but as the crucible was considerably acted upon, the genuine effects of these proportions is not clear.

Three parts Stronthian lime and one part argill, heated to 150°, melted into a black compact mass, of which the upper part was an enamel, and the lower a porcelain, not having been sufficiently heated. The same experiment made with common lime produced only a porcelain.

Two parts Stronthian lime and one of argill, heated to 150°, melted only where in contact with the crucible, the interior parts retained its powdery state; common lime used instead of Stronthian remained also in powder.

Equal parts Stronthian lime and magnesia retained their powdery state, except where in contact with the crucible.

Four parts silex and one of Stronthian remained unaltered at 147°, though the influx of melted coal seemed to convert them into a greyish black compact porcelain.

A compound formed of 67 parts silex, 23 of argill, and 10 of Stronthian lime, melted at so low a heat as 114° into a greyish white porcelain, which in a heat of 147° only became porous. When common lime was used instead of Stronthian the compound melted at 145° into a semitransparent frothy enamel.

mel. Hence we ſee this ſtone may be advantageouſly ſubſtituted for lime in pottery and vitrification, and, in metallurgy, as a flux for certain refractory ores.

WATER poured on Stronthian lime heats more violently than with the ſame proportion of common lime; it alſo diſſolves it much more copiouſly, 200 parts of water diſſolving one of this lime, or rather more; for a troy pound of water, temperature 60°, diſſolves 36 grs. of this lime.

THE moſt remarkable property of this lime is that it is capable of cryſtallizing; a ſaturate ſolution of it, being ſuffered to ſtand for one day in a cool place, ſhot into tranſparent rhomboidal cryſtals, two of whoſe oppoſite angles were very acute, and the other two conſequently very obtuſe; theſe cryſtals do not readily effloreſce by expoſure to the air of the temperature of 66°, but placed on a hot iron they fall into powder which is ſtill in the ſtate of lime; the water deprived of them forms a pellicle on the ſurface like common lime-water; the cryſtals themſelves are alſo ſoluble with the aſſiſtance of heat.

THE lime-water has a ſtronger taſte, though of the ſame kind as that of the common; like this, it precipitates metallic ſolutions, and particularly that of ſublimate corroſive with the ſame colour, but much more copiouſly than the common.

IT is a much better teſt of fixed air than common lime-water, being precipitated much more abundantly by the ſmalleſt particle of that air.

WATER

Water, thus impregnated, abſorbs hepatic air in great quantity, and thus forms a Stronthian hepar. Marine acid, added to this liquid hepar, produced a pale bluiſh precipitate, accompanied with ſome effervеſcence.

Stronthian-Lime is precipitated from its ſolution in water like the common by ſpirit of wine.

SECTION SECOND.

Of its Relation to Acids.

To diſcover its rank in the ſeries of bodies ſubjected to the action of acids, I found it neceſſary previouſly to examine ſome anomalous experiments relative to the powers of common quick-lime, which, if left undetermined, would diffuſe their obſcurity over thoſe which I meant to inſtitute on Stronthian lime.

About the year 1779, Mr. Sage of the Royal Academy of Paris, and Doctor Demeſte, aſſerted that quick-lime was a different earth from the calcareous; and, to prove this difference, they affirmed that lime-water precipitated a ſolution of gypſum, and alſo of lime-ſtone in the nitrous and marine acids. M. Morveau, in examining the nature of various calcareous compounds, allowed the truth of this experiment, and at that time

 attributed

attributed this precipitation to the phlogifton of lime *. This explanation not appearing to me fufficiently fatisfactory, I made the following experiments:

1°. Having diluted fome quantities both of nitrous and marine acids with diftilled water, I faturated both with Carrara marble; another portion of this marble I converted into lime, and of this lime I formed lime-water.

2°. To fmall portions of the folutions of this marble in each of the above-mentioned acids I added lime-water; in each a flaky and fomewhat brownifh white precipitate appeared.

3°. This precipitate was not foluble by an addition of pure diftilled water.

4°. Neither was it increafed by an addition of more lime-water; yet it was fo fmall that I could not conceive it to contain all the calcareous earth in the nitrous and marine folutions.

5°. The liquor in which this precipitate appeared being filtered, and the precipitate, thus feparated, more lime-water was added to the filtered liquor, but no precipitate appeared; yet on dropping into this liquor a frefh quantity of marine felenite a cloud was immediately difcernible.

* See 17 Rozier for 1781, p. 218, 224 and 227.

6°. The

6°. The ſolution of marine ſelenite being ſlightly boiled, lime-water was added to it; a cloud ſtill appeared, but the precipitate was much leſs copious than when the unboiled ſolution was uſed.

7°. To a ſolution in the nitrous acid, not of marble, but of lime formed of marble, lime-water was added; no precipitate or cloudineſs enſued.

Hence it is clear that the precipitation, formed by the addition of lime-water to the acid ſolutions of Carrara marble, conſiſted of the lime itſelf contained in the lime-water, and not of that united to the mineral acids, being occaſioned by the fixed air abſorbed by thoſe acid ſolutions after or during its extrication in the act of ſolution; for this precipitate muſt be either argill, magneſia or calcareous earth; if it were argill or magneſia the precipitate would be as copious from a boiled as from the unboiled ſolution, the contrary of which we have ſeen in the 6th experiment; it ſhould alſo be found in the acid ſolutions of lime, which is contradicted by the 7th experiment; if it were an earth ſeparated from an acid it ſhould be in the ſtate of lime, and conſequently ſoluble by an addition of more water, contrary to the 3d experiment; but if we ſuppoſe it a calcareous earth, precipitated from the lime-water by the fixed air contained in the acid ſolutions, all the phenomena exhibited by theſe experiments muſt naturally occur. This air will precipitate the lime in the lime-water added, as in the 2d experiment.

riment. This precipitate will be insoluble in water as in the 3d; the addition of more lime-water will not increase it, as is seen in the 4th, all the fixed air being already taken up; but on adding to this mixed liquor more of the acid solutions a precipitate will appear as these acid solutions convey an additional quantity of fixed air, which acts on the lime-water contained in the mixed liquor, as in the 5th experiment; the precipitate will be less copious in the boiled solutions, as much of the fixed air is expelled by the boiling, as in the 6th experiment; and finally, no precipitate will be formed in the acid solutions of lime, as in that case no fixed air can exist.

This source of ambiguity being removed, I now proceed to the experiments made to discover the relation of Stronthianite to acids.

To the Marine.

Stronthianite dissolves very readily in the marine acid, whether concentrated or diluted, and with considerable effervescence; 100 grains of Stronthianite lose by solution in acids 26.5. Common lime-stones, equally free from foreign mixture, contain much more fixed air; barytic lime-stones much less, and are more difficultly soluble.

The acids of vitriol, tartar and sugar, being successively dropped into this muriatic solution, instantly produced copious precipitates still more insoluble than those that arise from their union with common calcareous earth.

This

THIS ſolution is alſo precipitable by mild alkalis, whether fixed or volatile, and apparently ſo by the fixed cauſtic vegetable alkali; but the cauſtic volatile produced only a ſlight cloud, proceeding, as I imagine, from its not being perfectly cauſtic.

MILD calx cauſed alſo a precipitation of Stronthian from this ſolution, but mild barytes none.

COMMON lime-water produced a precipitate in this ſolution from the cauſes already mentioned; but barytic lime-water cauſed a copious precipitate.

ALL neutral ſalts, formed by the vitriolic acid united to any baſis, ſuch as the ſolutions of tartar vitriolate, glauber, vitriolic ammoniac, ſelenite, epſom, alum, and of the vitriols of zinc, lead, mercury, tin, biſmuth, regulus of antimony, produced copious white precipitates, that of iron a brown.

BUT neither iron, copper, tin or lime, in their metallic forms, cauſed any change in our ſolution, at leaſt in a ſhort time. This ſolution, being evaporated nearly to $\frac{1}{2}$ its bulk, ſhot into cryſtals ſoluble in their own weight of water of the temperature of 68°. At 78° or a ſtill higher heat they effloreſce.

To the Nitrous Acid.

THIS ſtone is ſcarce at all attacked by nitrous acid whoſe ſpecific gravity is 1,4, but if to this acid half its weight of

water

water be added, ſo that its ſpecific gravity be about 1,3, it is gently ſoluble; but if nearly its own weight of water be added to it, ſo that its ſpecific gravity be 1,22, it efferveſces violently, whereas barytes is nearly inſoluble in an acid even thus diluted. This ſolution alſo cryſtallizes; and, if the evaporation be ſlow, into large cryſtals; by ſpontaneous evaporation I have obtained ſome of the weight of 14 grains, and if the quantities were large their ſize would undoubtedly be greater. Their ſhape was that of flat hexahedral lamellæ imbricated, that is, ſuperimpoſed on each other, as ſlates on the roof of an houſe.

These cryſtals are ſoluble in ſomewhat more than their weight of water heated to 66°. Placed on a red-hot iron they decrepitate like common ſalt, and fuſe in a ſtill ſtronger heat.

The ſaturate ſolution of this earth does not diſcolour litmus, as that of barytes does.

To the Acetous Acid.

This ſtone is alſo ſoluble, though much more ſlowly, in diſtilled vinegar. The ſolution being carefully evaporated ſhoots into ſtelliform cryſtals, of an acid taſte; they effloreſce by expoſure to the air.

To the Vitriolic Acid.

This acid, when concentrated, has ſcarce any action on this ſtone, whether mild or calcined. When much diluted I found 10,000 parts of it to diſſolve one of this ſtone.

SECTION

SECTION THIRD.

Of the Affinities of Stronthian.

STRONTHIAN lime-water, poured on a ſolution of tartar vitriolate, immediately formed a precipitate, and ſo it did in the ſolution of glauber's ſalt and vitriolic ammoniac; hence its affinity to vitriolic acid is ſuperior to that which any alkali bears to this acid.

IT alſo formed a precipitate in the ſolutions of common ſelenite, epſom and allum; hence the affinities of common lime, magneſia and argill, to the vitriolic acid, are inferior to thoſe of this earth.

BUT barytic lime-water decompoſes the compound of vitriolic acid and Stronthian, and alſo the combinations of this earth with the nitrous, muriatic and acetous acids.

HENCE the affinities of Stronthian ſeem to be the ſame as thoſe of barytes, but inferior in degree, though ſuperior to thoſe of common calx.

OBSERVATIONS *on* RAIN GAGES.

By THOMAS GARNETT, *M. D. Member of the Royal Medical, Royal Phyſical, and Natural Hiſtory Societies of Edinburgh, of the Medical Society of London, and the Literary and Philoſophical Society of Mancheſter.*

THE theory of rain has long engaged the attention of philoſophers, and many ingenious and plauſible conjectures on the nature of this meteor have been given to the public; but the facts of which we are at preſent poſſeſſed ſeem to me to be too few in number, and to have been made at places too remote from each other, either to refute or confirm the theories in queſtion. This conſideration induced me to collect all the obſervations on this ſubject I could; and in the laſt volume of the Memoirs of the Literary and Philoſophical Society of Mancheſter is an Eſſay of mine containing a number of obſervations made on the weſtern coaſt of this iſland. Since the publication of that Memoir I have received journals from different parts of the kingdom, Read Jan. 25, 1794.

kingdom, and have prevailed on ſeveral of my philoſophical friends in different counties to keep exact regiſters of the barometer, thermometer and rain gage. By theſe means I am induced to hope that we ſhall be furniſhed with a part of the natural hiſtory of this iſland as yet little known, and which will not merely be gratifying to curioſity, but applicable to the moſt uſeful purpoſes, and which promiſes to ſupply the deficiency of obſervation, and enable the philoſopher to correct his theory by facts.

The barometer and thermometer are inſtruments which are liable to little error when carefully made and in the moſt ſimple form; but rain gages are very imperfect inſtruments. My attention was directed to the methods of remedying their imperfections as much as poſſible, from having obſerved that the journals of two gentlemen at Kendal in Weſtmoreland, whoſe accuracy in obſervation could not be ſuſpected, differed conſiderably, though their gages were ſimilarly expoſed, but I do not know whether ſimilar in their conſtruction.

Rain Gages are imperfect on two accounts. Firſt, on account of the evaporation which very commonly takes place on the interior ſurface of the funnel during wet weather in ſummer; for the air is for the moſt part in a condition to abſorb more water than it contains, though our humid atmoſphere is ſometimes ſo perfectly ſaturated as to depoſite part of its vapours on furniture within doors, even during the months of July and Auguſt, provided the weather be very wet; but water will frequently evaporate from the ſurfaces of many bodies, particularly from

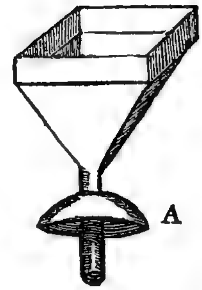
from metallic fubftances, as I have found by experiment, while rain is falling in fummer, or dew forming in an evening; for if a veffel of tinned iron be rubbed over with a wet fpunge, and then fufpended with its mouth downwards, its inner furface will foon become dry again, though rain be falling or dew forming at the time.

As this evaporation cannot be entirely prevented by any method of conftructing the gage, if the quantity of water loft this way could be determined, this imperfection would be corrected; and I fhould think that this quantity might be determined by two contiguous gages. For, let A and a = the areas of the apertures of the two gages; B and b = the curve furfaces of the funnels; S, s = the quantities of water collected by them in a given time in grains; X and x the quantities loft by evaporation in the fame time; then S + X and $s + x$, being the quantities received by the gages, we have $A : a :: S + X : s + x$, and $x = \frac{a\,S - A\,s + a\,X}{A}$; but fince the quantities evaporated in the fame time are as thofe furfaces, $B : b :: X : x$, and $x = \frac{b\,X}{B} = \frac{a\,S - A\,s + a\,X}{A}$; hence $X = \frac{B\,a\,S - B\,A\,s}{A\,b - B\,a}$, and the quantity corrected $= S + X = \frac{A\,b\,S - B\,A\,s}{A\,b - B\,a}$; but the cones muft not be fimilar, for in that cafe both the numerator and denominator would be $= 0$, and confequently nothing could be determined. Indeed, if it was worth the while, it might even be determined in this cafe, by taking the fluxion of the quantity.

 THE

The fecond imperfection arifes from the lofs of water occafioned by drops of rain burfting when they are driven obliquely by a breeze, and ftrike the fides of the gage: in fuch cafes they difperfe into a number of minute drops, many of which never defcend into the receiver, but efcape over the margin of the funnel. This depends on principles too fimple to require any experimental proof. It is difficult, if not impoffible, to prevent entirely this lofs of water by difperfion; all that we can do is to diminifh the caufe of it as much as we can. An ingenious friend of mine, Mr. Gough of Kendal, in a letter which I lately received from him, propofes the following method of remedying this imperfection.

A linen ftrainer, he fays, of a conical figure, fhould be fitted exactly to the mouth of the gage; this flexible funnel fhould be ftretched by a weight or ftring fixed to its apex within the veffel; the drops ftriking on this yielding fubftance would receive a moderate concuffion, and the particles of water would be entangled in the threads of the cloth. It is evident that this would greatly prevent the lofs occafioned by difperfion, but would much increafe the evaporation, by detaining a quantity of water in the funnel, expofing a much greater furface to the air. A better way of remedying this imperfection is to have a perpendicular rim, an inch or two high, fixed to the rim of the funnel. I have here given the form of rain-gages which I have had conftructed for my friends.



In gages of this form, efpecially when made fufficiently large, Mr. Copland of Dumfries informs me that he found the lofs from difperfion nearly, if not entirely, corrected. The area of the aperture of one of his funnels contains 144 fquare inches, and the other 288. He has compared this with one of 16 inches, and always found a fmaller than proportional refult from this laft in windy weather. He fays he has obferved his large fquare gages in ftormy falls, and could obferve nothing driven over after it had ftruck the infide, and was furprifed to fee fo little loft even during a hail fhower. He recommends gages with fquare apertures in preference to circular or cylindrical, for " from the rotatory motion which the air always takes, when forced over the end of a tranfverfely truncated cylinder, and which emits, for that reafon, a whiftling noife, the rain will be carried over the edges of the cylinder, and be almoft entirely prevented from falling into the gage." He foon found, after ufing fquare ones, that the refults from them were much more ample than from fome others that were kept in the neighbourhood, which were of a cylindrical form. I generally have a little cup with its mouth downwards, fitted to the neck of the funnel as at A, which will go over the mouth of the bottle; becaufe it is evident that when rain is driven againft the outfide of the funnel, or in confequence of the condenfation of dew upon the outer or under fide of it, more water would be collected by the receiver than falls within the area of the funnel, if it was not prevented by a contrivance of this kind.

I WAS

I WAS induced to trouble the Academy with the preceding obſervations, in hopes that they might direct the attention of philoſophers in our ſiſter kingdom to this ſubject; for this part of the natural hiſtory of Ireland is yet very imperfectly known; but I am happy to find it has engaged the attention of that excellent philoſopher Mr. Kirwan, by whom it may be expected that much light will be thrown on it. If theſe hints ſhould meet with a favourable reception, I intend from time to time to ſend to the Academy an account of any philoſophical diſcoveries or obſervations that may occur to me.

I HAVE ſubjoined a table containing the quantity of rain in inches and parts which has fallen within five years; the places of obſervation were chiefly on the weſtern coaſt of this iſland, and may eaſily be compared with the quantities which fall on correſponding parts of the eaſtern coaſt of Ireland.

Years	Dumfries.	Kendal.	Keſwick.	Fellfoot.	Lancaſter.	Salford.	Youngſbury.	London.
1788	26,423	39,2575	34,3057	42,06	29,45		17,676	14,892
1789	48,093	69,835	72,2449	66,52	51,01		29,493	21,976
1790	39,354	66,263	64,7439	58,48	46,61	42,75	22,970	16,052
1791	39,2817	62,200	73,5522				24,200	
1792	47,5130	84,884	84,6051			54,75		19,5

N. B.

N. B. Kendal bears N. 30°. W. from London, diftant 226 Englifh miles, meafured on a great circle of the earth; and, according to the obfervations of Mr. Dalton, the town is elevated about 46 yards above the level of the fea. Kefwick bears N. 30°. W. from Kendal, 22 Englifh miles, meafured on a great circle; and, according to Mr. Crofthwaite, is elevated about 76 yards above the level of the fea.

Fellfoot lies at the fouth end of Winandermere, where the lake contracts into a river, and is about 26 yards above the level of the fea. Thefe places are furrounded with high hills, fome of them elevated confiderably more than a thoufand yards above the level of the fea. Salford joins Manchefter, and Youngfbury is near Ware in Hertfordfhire, 20 miles north of London.

The difference in the quantities of rain which fall at thefe different places is furprizing. Much more falls in hilly than in level countries.

T. GARNETT.

Harrogate, December 10, 1793.

LETTER *from the Reverend Doctor* YOUNG, *Senior Fellow of Trinity College, Dublin, and M. R. I. A. to the Right Honourable the* EARL *of* CHARLEMONT, *President of the Royal Irish Academy.*

MY LORD,

AS the manner of extracting coal from mines is not generally known in detail in this country, perhaps the following circumstantial account of the noted mines of Whitehaven in Cumberland by a gentleman of that country may be thought fit to be inserted in the Transactions of the Academy. Read Dec. 7, 1793.

I have the honour to be,

MY LORD,

With the greatest respect,

Your Lordship's most obedient,

Humble servant,

MATTHEW YOUNG.

OBSERVATIONS *and* INQUIRIES *made upon and concerning the* COAL WORKS *at* WHITEHAVEN *in the County of* CUMBERLAND *in the Year* 1793. *By* JOSEPH FISHER, *M. D. Fellow of the Royal Physical Society in Edinburgh.*

IN the neighbourhood of Whitehaven are two coal works or collieries, called Howguill and Whinguill. The firft lies on the fouth weft part of the town, and the prefent works extend from the town towards the fouth about two miles and a half, reaching nearly to the valley called Sandwith, and in breadth about one mile and a half, viz. from a rivulet called the Powbeck on the eaft fide to about nine hundred yards under the fea towards the weft, making in area about two thoufand four hundred acres. This is the extent of the prefent workings, and is afferted to be the moft extenfive colliery in Great Britain.

IN

In this colliery are now difcovered five workable feams or bands of coal, befides feveral fmaller feams which are not worth the working.

In the pit named King-pit, which is the deepeft pit in this colliery or in Great Britain, the firft feam or band is called the Crow Coal, which is two feet two inches thick. It lies at the depth of fixty yards.

The fecond feam or band is called the Yard-band, in thicknefs four feet fix inches, and lies at the depth of one hundred and fixty yards.

The third feam is called the Bannock-band, about eight feet thick, including two metals, which are about twelve inches thick. It lies at the depth of two hundred yards.

The fourth feam is called the Main or Prior-band, which is from ten to twelve feet thick, and about two hundred and forty yards deep.

The fifth feam is called the Six-quarters Coal, about five feet thick. It lies at the depth of three hundred and twenty yards. No part of this laft feam has been yet wrought.

What other feams lie below thefe is yet unknown. No trial has been made above twenty yards below the fifth feam, which makes the greateft perpendicular depth hitherto funk to to be three hundred and forty yards below the earth's surface.

It would not be difficult to perceive, before any coals were got, that this tract of land contained seams or bands of coal, because the Bannock or third seam, and the Main-band or fourth seam, before mentioned, have burst out, as it is termed at Whitehaven; that is, they shew themselves in several places on the sloping surface of the earth, on the west side of the valley leading from Whitehaven to St. Bees. To the southward of this colliery these seams of coal are also thrown much nearer the surface by what is called upcast dykes (words which will be hereafter explained) the largest of which is about forty yards.

At a pit called Wilson's pit, which is the most southern pit in this colliery, the main band or fourth seam before-mentioned lies only about one hundred and forty yards below the surface; whereas at King-pit, as before stated, it lies one hundred yards deeper, or about two hundred and forty yards.

It appears that at the first beginning to work this colliery a level or watercourse has been driven from the rivulet called Powbeck, near the copperas work, to the south of the town about three hundred yards.

The course of this level is to the full dip or descent of the colliery, which is nearly due west, until it cuts or intersects the

the Bannock-band or third ſeam of coal before-mentioned. This level effectually drained about three hundred yards in length, and about one hundred yards in breadth, water level courſe, in this ſeam. The extent of coals thus drained is called a winning. The depth of the pits in this winning or extent is from twenty to ſixty yards.

The ſecond winning or extent drained has been effected by driving a level from the ſurface of the Powbeck near a farm-houſe called Thicket, further ſouthward than the firſt winning. By continuing this level to the weſtward they have cut or interſected the main band or fourth ſeam before-mentioned about four hundred yards to the dip or weſt of the outburſt or appearance of this coal at the ſurface.

This level drained about a thouſand yards in length water level courſe, and four hundred yards in breadth or dip and riſe courſe, and alſo ſomething more in breadth in the Bannoch-band ſeam of coal.

The coals obtained from theſe two winnings or extents muſt have been very conſiderable.

At that time the coals were drawn out of the pits by men with jackrolls or windlaſſes, and laid up in banks, from whence they were carried to the ſhips upon the backs of little horſes in pack loads, each pack-load containing what is called a Cumberland buſhel, conſiſting of twenty-four gallons, and each weighing about fourteen ſtones.

HAVING obtained as much coal as they could by these two levels, the third winning was made at a place now called Ginns, which is a village or hamlet near Whitehaven on the south west.

HORIZONTAL *vertical* wheels were erected here, called Ginns, by which they drew the coals with horses out of the pits, which before was done by men with windlasses or jack-rolls.

A FEW houses being built here, in consequence, for the colliers and workmen, became a considerable village, now known by the name of Ginns.

WITH these ginns or vertical wheels both water and coals were drawn from the pits; but drawing the water thus by horses and these vertical wheels became too expensive, so that the coals drawn would not pay for the expenses incurred. To remedy this, the late Sir James Lowther purchased the materials of an engine in London, which had been formerly used there for raising water to supply the city. Report says that this was the second steam-engine which was erected in England. The materials were sent in a ship from London to Whitehaven, where they were put together and fixed upon a pit near Ginns. The depth of this pit is about fifty-six yards from the earth's surface to the main band or fourth seam of coals. This engine had a copper boiler about ten feet in diameter, with a lead top, a brass cylinder twenty-eight inches in diameter, and wooden pumps eight inches in diameter, with a brass working barrel.

As

As the number of pits was increafed the water augmented, until at length it was judged neceffary to erect another engine with greater powers than the firft. By thefe two engines the water was drained from a confiderable extent of the Yard-band, Bannock-band and Main-band, feams of coals, which, being thus laid water free, fupplied the town and export market for many years.

The pit, called Parker's-pit, about half a mile from what is called the Staith, (a place to hold a large quantity of coals) which is near the harbour, was won in the Yard-band feam by thefe engines.

It was from this pit that the firft waggon-way (as it is called) was laid in this county. A waggon-way is a road for a waggon with four wheels to run upon. It is made with wood laid down faft on each fide of the road at a proper diftance for the folid iron wheels of the waggon to move upon; the wheels are confined from running off from the wood by a protuberant rim of iron on the interior fide of each wheel. The road is made fo as to have a gentle defcent along its whole length, fo that the loaden waggon runs from the pit to the ftaith without any horfe to draw it; where the defcent is fo much that the motion would be too quick, a man, who is mounted behind the waggon, by preffing down upon one wheel a piece of wood, called the convoy, which is fixed to the waggon for that purpofe, can reftrain the too rapid motion and regulate it properly.

A HORSE is ufed to draw the empty waggon back again to the pit from the ftaith by an eafy afcent along another fimilar waggon-way, laid along the fide of the former at about three feet diftance: thus it is fo contrived that the loaden and empty waggons never meet or interfere with each other.

THE ftaith is a large wooden building on the weft fide of the town adjoining to the habour and covered in. In this ftaith are fixed five hurries or fpouts, at fuch a diftance from each other that a fhip of three hundred tons burthen can lie under each hurry and receive a loading at one time. The ftaith is about thirty-feven feet above the level of the quay, and when the waggons arrive there the bottom of each waggon is drawn out and the coals are dropped from thence into the hurry or fpout under it, through which they run down into the fhip laid below to receive her loading. The hurries or fpouts lie with an inclining flope of about forty-five degrees.

WHEN there are no fhips ready to receive coals they are depofited in the ftaith, which will contain about fix thoufand tons, Dublin meafure, or three thoufand waggon loads. Thefe coals thus depofited are once more put into waggons and dropped through the hurries or fpouts into fhips, when there are more veffels than the ufual daily fupply of coals will load. There have been two hundred waggon loads, or four hundred Dublin tons, fhipped from the pits in one day, and an equal quantity on the fame day from the ftaith, making in the whole about eight hundred tons, Dublin meafure.

BY

By the contrivance of waggons and waggon-roads, one horſe carries as much coals at once as twenty-four horſes uſed to do upon their backs before this invention.

The fourth winning or extent of coal drained was made about eighty years ago, at a place called Saltom near the ſea, about a mile ſouth weſt of Whitehaven. This was a very expenſive undertaking; it was, however, deemed abſolutely neceſſaay, as on the completion of this depended the future ſucceſs of this colliery. A fire engine was therefore erected here with a twelve feet boiler, a cylinder forty inches in diameter, and a pump ſeven inches in diameter. The pumps were divided into four ſets or lifts, the pit being one hundred and fifty-two yards in perpendicular depth. It was perceived neceſſary, however, a few years afterwards, to erect a ſecond ſteam-engine in this place of the ſame dimenſions as the firſt, becauſe the water was increaſed very conſiderably by ſinking ſeveral new pits.

The fields of coal already drained by theſe two engines have been explored from north to ſouth about three miles, and may probably be extended about three miles more when wanted. The coal now drained, and ready to be wrought in the ſeveral working pits at preſent, will ſerve for about twenty years, according to the quantity now drawn. Pits, however, being in ſome time naturally exhauſted, it is thought prudent now and then to drive what is called trial drifts, in order to explore the fields of coal, and to find proper places where to make new pits, when the ſame may be wanted.

 About

About twelve years ago, thefe two engines being nearly worn out, a new one was erected at Saltom, capable of drawing more water than the two old ones. It has two boilers, each fifteen feet in diameter, a cylinder feventy inches in diameter, and a working barrel eleven inches and a half. It can draw all the water in eight hours which is produced in fummer in twenty-four hours, but in winter it requires double that time as there is double the quantity of water. This engine was repaired about three years ago at a very great expenfe, with a new cylinder, new regulating beam, and new cylinder and fpring beams. At this time it is admitted, by feveral profeffional men who have examined it, to be the beft engine of the fize within the kingdom. Its maximum in working is fifteen ftrokes, each fix feet and a half long in a minute; each ftroke draws twenty-feven gallons of water, that is, four hundred and five gallons per minute, or nine thoufand two hundred and forty hogfheads every hour.

All the bands or feams of coal in this colliery dip or defcend nearly due weft, floping towards the horizon with a defcent of one yard in eight to one in twelve, and the feams are always and invariably equally diftant from each other, whatever be the depth. However, though thefe feams of coal are thus always equally diftant from each other, yet they are not equally deep from the earth's furface. The feams, as before-mentioned, conftantly dip or defcend towards the weft, and rife towards the eaft, till at length they fhew themfelves in fome places on or near the earth's furface.

Besides

Besides this general defcent or afcent, the feams are in fome places abruptly broken off by a bed of ftone or other matter of a confiderable thicknefs, betwixt the coal and which there is generally a cavity or hollow called at Whitehaven a Gut. When a feam of coal is thus interrupted by the interpofition of other matter the workmen know that they will find the fame feam either above or below this place, or, as they term it, they know that the feam is thrown either upward or downward. In order to know whether the feam of coal will be found above or below, they endeavour to difcover which way the ftone or other feparating matter hangs or flopes. If it recedes from the coal, floping ever fo little upwards, they conclude that the feam of coal is thrown upwards (as they call it), that is, in fuch a cafe the feam is always found above the break: If the flope be hanging over the coals, floping towards the furface, then the feam of coal is faid to be thrown downwards, and is found below the break. The real fact is, that in fome former time there has been fome great convulfion of the earth, in which all the fuperincumbent covering matter, confifting of feams or beds of ftone, coals or other materials, have been moved upwards in all fuch chafms or breaks, leaving the feam or bed of coal below, in one part, where it was at the time the dreadful convulfive motion happened: Hanging over and floping upwards or downwards are only relative terms, depending upon which fide of the interpofed matter you arrive at. Where any feam or field coal feems thus to end, the interpofing matter hangs or flopes one way on one fide of the matter and the contrary on the other

 fide,

fide, fo that the fuperincumbent matter with the feam of coals has been invariably thrown upwards by the convulfion, whilft the remaining part of the bed has been left as it was before the motion.

Whitehaven collieries abound with what they there call Dykes, that is, beds of ftone of a confiderable thicknefs, which feparates one field of coal from another. The principal ones run in a direction nearly eaft and weft. They divide the feams of coal into fields, as they are called, that is, feparate tracts of coal almoft like the fields or inclofures of a farm. Thefe dykes or feparations are very ufeful, by reftraining the water or inflammable air from flowing out of any adjoining field of coal, where no works are going on, into another where men are working, until it is found convenient to cut through or work a new field. Without thefe dykes it would frequently be very difficult to keep the works from being overcharged with water, but it is fometimes very troublefome and expenfive to cut through them, being of a confiderable thicknefs. Where the covering of fuperincumbent matter is not of fo great a thicknefs, which is towards the rife of the feam or field, there pillars of coal are left from five to ten yards fquare, and the workings are from three to four yards wide, fo that about one-half of the coal is taken away, and the other half left to fupport the earth above. Where the coals lie from one hundred and fifty to three hundred yards deep, and efpecially where the coal is drawn from under the fea, the pillars are left from fixteen to twenty yards fquare, fo that about

about one-third part of the coal is taken, and two-third parts are left to ſupport the roof.

WHITEHAVEN colliery is not ſo much loaden with water as the collieries about Newcaſtle and other flat countries are, where they are not able, by what is called day levels, to take away the top water, called ſurface feeds, as is practiſed at Whitehaven.

THE coal works at Whitehaven have produced and ſtill do produce greater quantities of inflammable air, commonly called damp, than any other coal work known. This ſeems to ariſe from the coal lying at a greater depth below the level of the ſea than any other known colliery. This obſervation holds invariably true both here and about Newcaſtle, that in all coal works lying above the level of the ſea little or no inflammable air is perceived, except in the guts of the dykes, that is, in the cavities or hollows betwixt the fields of coal and the dykes or beds of ſtone which ſeparate the fields. The quantity of inflammable air appears to bear proportion to the depth of the works below the level of the ſea.

WHEN they began to ſink the coal pits at Whitehaven ſo deep that coals were drawn from below the level of the ſea, inflammable air was found in ſuch quantities that it frequently took fire from the flame of the candles uſed by the workmen under ground, which cauſed violent and dangerous exploſions, by which numbers of the workmen were burned and maimed, and

by

by which ſeveral loſt their lives. Mr. Spedding, a late eminent engineer and director of the coal works at Whitehaven, diſcovered that ſparks produced from flint and ſteel were not nearly ſo productive of theſe exploſions, by kindling the inflammable air, as the flame of candles was. He therefore contrived a machine, compoſed ſo that by being turned about by a wheel it ſtruck a great number of flints againſt ſteel in perpetual ſucceſſion. This gives light ſufficient for the workmen to work by in ſuch depths as the inflammable air abounds in, whereby the danger is greatly abated. Without this or ſome ſimilar contrivance the deepeſt coal works would probably before this have been totally given up, as being ſo dangerous to the men employed.

It is now about one hundred and fifty years ſince coals are ſuppoſed to have been firſt raiſed here for exportation. What the quantity exported has been at different periods cannot now be well aſcertained. Within the laſt twenty years the export trade has increaſed about one-third part of what it now is. Whitehaven colliery has produced for a few years laſt paſt from one hundred thouſand to one hundred and twenty thouſand tons, Dublin meaſure, yearly. Two tons contain about a chaldron and a quarter, London meaſure. In general a Whitehaven waggon of coals contains two Dublin tons, each ton weighing from twenty-one to twenty-two hundred weight. The beſt coals are invariably the lighteſt. One-third part of the main band ſeam, which lies in the middle thereof, would, if ſeparated, be as good as the beſt Newcaſtle coal. The bank or bottom is worſe in quality, but when mixed they are allowed to be the beſt coals raiſed in the county of Cumberland.

On

On the ſouth-weſt ſide of Whitehaven, in the part called Preſton Iſle, there appears to be coal enough to ſupply exportation at the preſent rate for near two hundred years to come. There are three day holes, called Bear-mouths, where the men and horſes go from the ſurface down a ſloping cavern to the works; they are made into the different ſeams of coal. By theſe entrances horſes are daily brought down, to draw the coals from the places where they are hewn, in waggons, along a waggon way under the ground, made as beforementioned, to the bottom of the reſpective pits, where they are put into baſkets, and drawn perpendicularly up to the earth's ſurface by ſteam engines, through a ſpace of near three hundred yards in depth in ſome places. Each engine performs what twenty-four horſes uſed to do formerly. The men alſo walk up and down theſe caverns to and from their work, which is much eaſier and leſs troubleſome than being let down and drawn up through the pits each night and morning, which was formerly done. In ſhort every thing is moſt wonderfully contrived to ſave labour and expenſe.

On the FISH *enclosed in* STONE *of* MONTE BOLCA.

By the Rev. GEORGE GRAYDON, *LL.B. M.R.I.A.*

and Secretary of foreign Correspondence.

IT is impossible for those who have not seen and examined the fossil fish of Monte Bolca to form an adequate idea of that most curious phenomenon of natural history: In this, as in every thing else where the sensible qualities of bodies are concerned, it is well known that description, however exact, must come far short of conveying the impressions that are given immediately by the senses *. That such is remarkably the case in the present instance, I can testify from experience; for

Read March 1, 1794.

* For this reason, and to supply the defects of my pen, I have, with the permission of the Academy, placed a few small specimens of these fish in their museum. To them I beg to refer such members as wish for more accurate ideas than it is possible for words alone to convey. An engraving of some of these is annexed to this paper.

for though I was not unacquainted with theſe ſingular productions by deſcription, I was not leſs forcibly ſtruck, on firſt ſeeing a collection of them at Verona in January 1791, than if I had never heard of them before. As I had not, at that time, leiſure to pay much attention to them, or to make the minute inquiries which they ſeemed ſo well to deſerve, I determined, if I ſhould again paſs through this part of Italy at a favourable ſeaſon for the purpoſe, to viſit, if poſſible, the place where they were found, as well to verify the fact, as, from an examination of the ſituation and other particularities of the ſpot, to endeavour to form ſome conjecture as to the origin of a circumſtance ſo extraordinary.

THIS opportunity occurring in the month of June following, I took up my reſidence for ſome days at the town of Arzignan in the Vicentine, by the recommendation of the well-known Abate Alberto Fortis *, and from thence occaſionally made excurſions through the neighbouring hills, under the guidance of

* I was much indebted to the hoſpitality and kind attention of the Abate Fortis to my accommodation in every reſpect, during the ſhort time that I had it in my power to ſpend in examining the Euganean hills, and other curious diſtricts of the Venetian territory. It is much to be regretted that the many intereſting papers which this gentleman has given to the world, at different times and in various ſhapes, on particular parts of the north of Italy, have never been collected into one publication. Many valuable works of Signior Giov. Arduino of Venice have alſo appeared in the ſame ſcattered manner, which, though written ſome years back, would form a moſt uſeful collection of accurate local deſcriptions and obſervations. At preſent it is very troubleſome and difficult to procure many of the writings of theſe authors, even in the countries where they were publiſhed

of peaſants well acquainted with the country. In one of theſe I had the pleaſure of ſeeing the quarries of foſſil fiſh at Monte Bolca. Of theſe, and ſuch circumſtances relating to them as the very ſhort time I could ſpend there enabled me to collect, I ſhall proceed to give the Academy the beſt account in my power; relating, firſt, the facts as I found them; and then the inferences which they ſuggeſted to my mind as to the poſſible immediate cauſes of this ſtriking phenomenon.

Monte Bolca lies on the border of the Veroneſe territory, about fifty miles W. N. W. of the Lagunes of Venice, which, I believe, is the neareſt ſea. I am not informed of its height, but it muſt be pretty conſiderable, as I underſtood from the inhabitants that the climate is too cold for the growth of the country fruits which are common about every peaſant's houſe in the lower grounds, ſuch as apricots, apples, cherries, &c. as well as vines. It forms one of the chain or ladder of ſecondary hills, which, from ſome diſtance within the adjoining Vicentine, riſe gradually above one another to the Alps of the Biſhopric of Trent.

Great part of this tract of country has been conſidered by many Italian, as well as other naturaliſts of eminence that have viſited it, as covered with productions of extinct volcanoes. The ſuppoſed lava of theſe diſtricts differs eſſentially from that of which the Euganean hills are compoſed; this latter is of a whitiſh, yellowiſh or browniſh grey, rough and coarſe in the grain, and mixed with numerous minute frag-

 ments

ments of what they denominate ſchorl and felt-ſpar; whence Sir John Strange, Monſieur de Dolomieu, and others, have called it granitical, though unlike granite in many important points: But the ſuppoſed compact lava of the Vicentine and Veroneſe is black, or dark blue, of a cloſe and rather fine grain, even, and almoſt homogeneous, except that it ſometimes contains a few fragments of ſchorl, and is wholly of the argillaceous genus, and of the trapp or homblend ſpecies; and in ſhort almoſt perfectly reſembles our baſaltic ſtone of the county of Antrim, and the N. W. of Scotland. The baſaltic columns of San Giovanni Ilarione, deſcribed by Sir John Strange, lie not many miles from Bolca, on the ſide of a valley that leads to it, and the ſummit of this hill itſelf was, many years ago, diſcovered by Abate Fortis to be crowned with a great maſs of tolerably defined columnar baſalte.

The whole of the hill, as far as I could obſerve, ſeems to be compoſed of ſimilar, or at leaſt of argillaceous matter, except the quarries in which the fiſh are found, which are calcareous, and lie at about half a mile from the ſummit. Beſides the diſſimilarity of theſe to the other materials of the hill, it is further important to remark that they do not form a continued ſtratum, but lie in great and wholly detached and diſtinct maſſes, as it were accidentally imbedded in the ſide of the hill, ſet in a looſe rubble of argillaceous, and the ſame kind of calcareous fragments, the whole more or leſs in a ſtate of decompoſition.

THE mafs that has been moft worked ftands near the point of an angle formed by two deep channels that have been worn by a greater and lefs torrent that meet there. The height of its face above the prefent furface is, as well as I can recollect, about fourteen or fifteen feet; but as it cannot be determined to what depth it may be buried in the ground, it is not eafy to judge what its pofitive heighth or breadth may be. The length of the face, I fhould fuppofe, for I did not meafure it, may be two hundred feet or upwards. The ftone is of a fchiftous or flag-like ftructure, the leaves lying in the fame direction, and parallel to each other; but this direction, it is to be remarked, is neither horizontal, nor coincident with the flope of the hill.

I GOT fome of the people that are ufually employed in working the quarry to bring their tools, and fhew me their manner of operation, as well to be informed in this, as, if poffible, to fee fome fifh actually opened in the ftone. Unfortunately the day proved very wet, which prevented my having more than two or three workmen, but from thefe I procured almoft all the information and fatisfaction I could expect. I fpent nearly three hours on the fpot, during which time I not only faw fome fifh, as well as a few remains of marine vegetables found by the men, but had the pleafure to find fome myfelf, opening with my own hands ftones which contained them. Thefe I collected, packed and brought home; and fome of them are now in the mufeum of the Academy: They are but fmall indeed, and in the mutilated ftate that accident prefented them; but, I believe, they

they are amply ſufficient to authenticate the principal fact, if any one ſhould be found to doubt it.

The manner of working theſe ſtones is by detaching from the face of the quarry moderate ſized blocks, which are then drawn out, ſet an edge, and quickly ſplit with ſharp-ſided hammers or wedges, the workmen glancing between the leaves, to obſerve if there be any mark of fiſh, or other organic ſubſtance; when they diſcover ſuch, if they happen to be ſhattered, as they generally are, by the rude manner of opening, and the fragile texture of the ſtone, they ſet about to collect all the fragments that compoſe the piece as carefully as poſſible, detaching alſo from the great ſtone ſuch parts as may remain adhering to it. When their day's work is finiſhed, they bring their collections to their houſes, until they happen to go, either to market, or on any other occaſion, to Verona, when they take them in baſkets, juſt as they are, to the proprietor of the ſoil, who is their landlord and employer; or frequently, I believe, to ſell underhand, for their own profit, to ſome naturaliſt there, or to ſome of the ſhops that vend theſe productions. Thoſe who receive them in this manner from the peaſants are then obliged to employ a ſkilful ſtone-cutter, to find and arrange together the ſeveral fragments that compoſe each piece, and fineer or cement them on another ſtone of the ſame kind, which is ſometimes done with ſuch art and exactneſs that it is not eaſy to diſcern where they have been joined; and thus the ſpecimens are made up for cabinets or for ſale.

Besides this principal quarry or maſs, from which almoſt all the fiſh yet found have been extracted, the workmen ſhew two or three others of the ſame ſtone ſeated in the ſides of the adjoining banks, ſome of which, they ſaid, had been diſcovered not long ſince: They had all been tried, and were found preciſely of the ſame kind, and equally containing fiſh; but the people being very poor, and no funds allowed for the buſineſs, which would require conſiderable expenſe to clear away the bearing, and prepare for working to advantage, nothing of conſequence had been done.

The ſoil of theſe quarries had been the property of a Signor Bozza, formerly an apothecary of Verona, who purchaſed, or took it on leaſe, many years ago, and whoſe cabinet is too well known to all the naturaliſts of Europe, and has been too long one of the principal objects of the attention and admiration of thoſe who paſs through this town, to make it neceſſary for me to enter into any detailed deſcription of it; but while I was there his whole collection, with the quarries, was agreed for, and, as I was informed, purchaſed, at a very conſiderable expenſe, by the Marquis Gazola, of that city. This gentleman had already a very fine collection of his own, containing many fiſh that were not in Bozza's. He was ſo kind as to give me a catalogue of theſe, in addition to Bozza's printed catalogue, both of which, as I have not ſeen them in any publication, I ſhall ſubjoin to this paper*. They will be found to contain together

* I give theſe catalogues juſt as I received them from Mr. Bozza and the Marquis Gazola, poſſeſſing neither ſufficient knowledge of the natural hiſtory of fiſhes

gether the ſcientific names of upwards of one hundred different ſpecies of fiſh, with diſtinct references to the authors by whom they have been deſcribed, and the plates in which they are repreſented; ſo that thoſe who will take the pains may, by actual compariſon, judge of the reſemblance and propriety of denomination. But what is moſt remarkable is, that theſe fiſh are deſcribed, by the authors referred to, as the modern natives of various ſeas, moſt remote from each other; and not of Europe only, but of Aſia, the Indian Ocean, the South Sea, Africa, North and South America; and in addition to theſe ſome few of ſweet water*.

THAT

fiſhes to enable me to judge whether the cabinet ſpecimens have been faithfully denominated or not, nor having had time or leiſure enough at Verona, though I had underſtood the ſubject, to go through ſuch an examination. I will not even venture to anſwer whether ſome tricks may not have been played by the ſtone-cutters who arrange theſe ſpecimens:—Whether, for inſtance, they may not, in ſome caſes, have contrived to ſuit pieces of different fiſh to each other in ſuch a manner as to form a whole that might correſpond with the plates in books of natural hiſtory. Such an idea has been ſuggeſted, perhaps founded on the well-known dexterity of Italian workmen in ſimilar fabrications; but in the preſent inſtance, though I will not ſay it is abſolutely impoſſible, I really think it ſo very improbable as to deſerve little ſerious attention.

* Mr. Bozza, in a paper publiſhed by him a few years ago, ſpeaks of his collection as follows: "In my cabinet, which contains upwards of ſix hundred fiſh "of different ſizes, all extracted from Bolca, there are more than one hundred "whoſe kinds are known, which differ from each other in genus and ſpecies, and "*many others beſides to which ſimilar living ones have not yet been diſcovered.*" In another paſſage he adds, "The firſt decade of fiſh publiſhed by M. Brouſſonet has "aſcertained to us that many of thoſe found at Bolca are natives of the South "Sea—of theſe I have four, which agree exactly in form, in proportions and in "fins

That all theſe ſhould be found, as is aſſerted, perfectly and unequivocally defined within the narrow compaſs of the quarry of Monte Bolca, muſt ſurely be conſidered a very aſtoniſhing circumſtance, and ſuch as I believe can ſcarcely be paralled in what has hitherto been diſcovered of the natural hiſtory of our globe.

It has been already obſerved, that the ſtone in which theſe fiſh are found is calcareous, and of a ſchiſtous ſtructure, capable of being ſplit into flags or laminæ of various thickneſs and dimenſions. Moſt mineralogiſts who have mentioned it call it a marle or marley ſchiſt. Its colour is whitiſh, yellowiſh or bluiſh grey; the grain, though not coarſe, is very dull and earthy; it varies a good deal in hardneſs, but in general eaſily yields to the knife, though not to the nail. Every part of the maſs, whether immediately ſurrounding fiſh or not, on being ſtruck or ſcraped hard, emits a peculiar kind of fetid ſmell which cannot eaſily be defined. It is ſomewhat of the kind, yet differs conſiderably from the ſmell of the lapis ſuillus or ſwineſtone. It is not properly hepatic, unleſs perhaps it might be called animal-hepatic *.

"fins with four deſcribed by M. Brouſſonet, which are peculiar to the ſea of "Otaheite, which are the *Polynemus plebeius*, or Emoi of the Otaheitans; the Gobius ſtrigatus, which they call Jaipoa; the Chætodon trioſtegus, and the Gobius "oſcellaris:"—Theſe perfectly correſpond with the fiſh given by Sir Joſeph Banks to M. Brouſſonet.

* Abate Fortis obſerves the ſame thing of the calcareous ſtone, containing many ſhells of the valley of Ronca in the Veroneſe, at no great diſtance from Bolca. "Every

The fiſh are of a dark-brown colour, and therefore appear very diſtinctly on the light ground of the ſtone; they lie flat between the laminæ; their profile, and their ſeveral parts, little, if at all, diſtorted from their natural ſhape and dimenſions, except that in ſome caſes the ſtone incloſing them ſeems to have ſuffered ſome little diſturbance, as if by ſettlement, after their incloſure, by which they are found, at times, ſomewhat fractured, and the parts a little disjoined. Their whole form is well defined, but the harder parts, ſuch as the head, fins, ſpine, with the bones that branch from it on either ſide, and indeed all the bones in general, as well as in ſome the ſcales, are remarkably well expreſſed. The dark-brown matter compoſing theſe fiſh remains diſtinct, and may be picked off from the ſtone, and projects in proportion to the thickneſs of each part in its natural ſtate. It is hard, brittle, and rather gloſſy through its ſubſtance, except in ſome of the groſſer bones, ſuch as the joints of the vertebræ, which, though of this appearance externally, are found, when broken, to conſiſt internally of laminar cryſtallized calcareous ſpar.

To thoſe who may not have an opportunity of ſeeing ſpecimens of theſe fiſh, it is further proper to mention, that when the leaves of ſtone that encloſe them are opened, the forms are

" Every ſtroke of the hammer or other ſolid body," ſays he " diſengages from this " ſtone a moſt fetid cadaverous odour, by no means bituminous, but a true ſmell " reſulting from decayed animal putrefactive matter. *Di vero fracidume animale.*" Fortis Della Valle Vulcanico-Marina di Ronca, p. 24.

are found equally announced on each of the oppofite fides, with this difference however, that more or lefs of the prominent brown matter of the bones, fins, and other parts, fometimes adheres to one leaf and fometimes to another, or frequently is divided between both; the prominences on one fide, when the pieces have been carefully and well put together, being exactly anfwered by correfponding hollows on the other; and thus the more valuable fpecimens are formed in duplicates. This, properly confidered, muft furely make the difficulty of fabrication, in fuch inftances at leaft, fo great, that it may well be deemed infurmountable; and if not from the nature of the cafe itfelf, yet decidedly fo at fuch an expenfe, as either the capital of the late proprietor, or the prices at which I underftood he fometimes parted with fpecimens, would bear; fome in his, as well as in Marquis Gazola's own cabinet, were of an immenfe fize; certainly, as the catalogue mentions, fully three feet long.

I HAVE now related all the facts worthy of notice which I recollect to have fallen within my obfervation relative to thefe curious foffils; and I truft I have done fo faithfully and without a view to any particular theory or fyftem of explanation: In fact, I vifited the fpot where they are found wholly unprejudiced as to the manner of accounting for the phænomenon, and indeed wholly uninformed, as I ftill am, of any attempt to account for it, except on vague and general principles *. What

 I fhall

* Whether the following attempt to affign immediate caufes for the production of the phænomena now defcribed can lay claim even to novelty, as a recommendation

I ſhall now venture to lay before the Academy on this ſubject, I can therefore ſay with confidence, is purely the reſult of inferences ſuggeſted to my mind by the appearances of the objects themſelves, and their ſeveral concomitant circumſtances.

I SUPPOSE no one that has attended to the deſcription of theſe foſſils, and ſtill leſs that has examined the ſpecimens themſelves, will doubt that the forms which they exhibit are the remains of once actually exiſting fiſh.

BUT theſe, it ſhould be well remarked, differ eſſentially from the forms of fiſh that are frequently found in argillaceous ſchiſtus, as well as from what are commonly called petrifactions of ſhells, &c. which abound in moſt lime-ſtone ſtrata. In this latter caſe we have ſeldom more than the mere external ſhapes preſerved; the ſubſtance is wholly changed, and what remains reſembles,

tion to the attention of the Academy, I cannot poſitively aſſert, my information in theſe ſubjects being very limited; I can only ſay, that nothing of the kind has fallen within my obſervation. I have conſulted ſome modern Italian writers of mineralogy and natural hiſtory, but find them very conciſe in what they ſay of theſe fiſh. P. Petrini, the very worthy mineralogical profeſſor of the Collegio Nazzareno at Rome, has an appendix to the 2d vol. of his Gabinetto Mineralogico del Collegio Nazzareno, publiſhed at Rome in 1792, which treats expreſsly of petrified remains of animal and vegetable ſubſtances, but contains nothing relative to theſe. Signior Bozza, in his paper above quoted, which is entitled, Della Rivoluzione ſofferta dal Globo Terraqueo, ſpeaks only of remote and general cauſes: And the Marquis Gazola, to whom I mentioned at Verona the outlines of the explanation here attempted, immediately after my viſit to Bolca, ſeemed wholly unacquainted with any thing ſimilar. Theſe circumſtances, particularly the laſt, led me to preſume that the ideas here given had not been anticipated, and emboldened me, on that ground, to ſubmit them to the Academy.

refembles, in general, a portion of the fame matter of which the mafs confifts, caft, as it were, into a mould formed by the outfide impreffion of the fhell*. In fome cafes the fhell-formed nucleus differs from the furrounding matter, by being of a brighter colour, and of a fparry texture; but, in almoft all, the original fhell that gave the impreffion exifts no longer as fuch, nor any certain remains of it. The fame holds equally in the former cafe, a bare impreffion of fifh only remaining, and feldom any thing that can be thought to refemble any part of the fubftance that gave it†; whereas in thefe of Monte Bolca, not only the

* It is to be obferved that I fpeak here only of the petrified fhells that are found imbedded in folid lime-ftone ftrata, and forming part of the ftone; yet even thefe fhells and their fragments fometimes, though rarely, retain their natural fubftance and appearances, as in the beautiful Lumachella di Carinthia; but in lefs compact beds, fuch as fand, marle, clay, &c. fhells are often found little changed from their original ftate.

† Impreffions of fifh, partly in argillaceous and partly in calcareous fchiftus, have been found in a variety of places: In Syria, between Batron and Diebail; in the mountains of Caftravan, near Baruth; in Antigua, nine hundred feet above the level of the fea; at Monte Viale in the Vicentine; at Scapezzano in the Campagna of Rome; in the valley of Glaris; at Mont Pilate, in the canton of Lucern; near Angers; and at Eichftedt in Franconia. Thefe are moftly fea fifh. River fifh are found in the copper flats of Eifleben, in the county of Mansfeld, near Pappenheim in the Pallatinate, and at Œningen in Suabia.—See P. Pinis Mem. fulle rivolugions del Globo terreftre, in the 5th vol. of the Societa Italiana of Verona, p. 238. Mr. Rafpe informs me that a Baron Francis Beroldingen has given, fome years ago, a very circumftantial and fatisfactory account of the fifh found at Œningen; but I have not feen his work. He alfo tells me he faw, many years ago, a fpecimen equal to any of the Bolca fifh in the poffeffion of the late Profeffor Green at Cambridge, found in fome part of Leicefterfhire, but he could not learn the exact place.—He adds that he has lately found numberlefs impreffions of fifh in

the forms are preferved uncommonly perfect, but even every refidue of animal matter that could be expected to refift the natural deftructive caufes, and the immenfe lapfe of time in any, the moft favourable circumftances, is found ftill exifting. The prominent brown matter with which all the harder and lefs corruptible parts of the animal are fo ftrongly marked in the ftone, and which may be detached from it with the point of a knife, infpection alone will determine to be of a nature wholly different from the inclofing fubftance, and as far as can be prefumed without analyfis, to be the actual dry remains of the animal bodies, in fuch a ftate as almoft to authorize their being called fifh-mummies‡.

But when we recollect of what very foft and corruptible materials the bodies of fifhes are compofed, not confifting of the firm

in bituminous fchiftus, in Caithnefs on the river Thurfo near Carfgo; and on the flope of Gerfton-hill. To thefe I will add a fifh engraved and fhortly mentioned by Doctor Nafh, in his Hiftory of Worcefterfhire, p. 236, and found in a ftone-pit of the parifh of Cleve or Clive in that county, for the communication of which I am indebted to the Bifhop of Dromore. With regard to moft of thefe the ingenious M. Volta, in his Elements of Mineralogy, publifhed in 8vo. at Cremona, 1787, p. 292, obferves, after fhortly mentioning the fifh of Mount Bolca, that in Germany and elfewhere flate and calcareous ftones are found containing the impreffions of the bones or fkeletons of various fpecies of fifh, the reft of the animal being deftroyed; and thefe impreffions he denominates Typolites, to diftinguifh them from thofe which he calls peculiarly Icthyolites, which term he confines to fuch fpecimens as exhibit the animal itfelf either dried or petrified; and of thefe laft the only inftance that he gives is the fifh of Bolca.

‡ M. Volta calls the manner in which thefe fifh are preferved a Natural Embalming.—Elem. Min. p.

firm muſcles and tendons that inveſt the bones of land animals; when we call to mind in how very ſhort a time ſuch of them as die, or remain dead in their own element, do actually corrupt, and run into ſuch complete diſſolution as to obliterate the whole form, the bones only remaining, a fragile and imperfect indication of the ſpecies to which they had belonged;—and further, when we find, in the caſes juſt now mentioned, that ſubſtances ſo hard and durable as ſhells have not been able to withſtand the corroding influence of time, muſt it not aſtoniſh us to find, that in this inſtance, nature has been able to effect more than perhaps the moſt ſtudied art could have accompliſhed, and will it not force us to have recourſe to more than ordinary cauſes to account for a phænomenon ſo extraordinary?

The very perfect preſervation of the living form which we ſee in theſe ſpecimens, I conſider as a certain proof that the animals could not have been long dead before they were encloſed in the matter that ſurrounds them; from the ſame circumſtance it follows equally, that this matter muſt have been in a very fine and pulverulent ſtate, ſuſpended in, or ſubſiding from the water in which the fiſh ſwam not long before. Here then we have next to a demonſtration, of two inferences of moſt important and fundamental facts, which are of material conſequence to our preſent enquiry: Firſt, that theſe animals were alive, and of courſe that the water in which they were was clear and fit for the ſupport of their life, at a very ſhort period before they were enveloped in the matter of their preſent ſtony encloſure; and ſecondly, that this matter muſt have been very ſuddenly diffuſed through that water in a pulverulent ſtate,

from

from whence ſpeedily ſubſiding, it caught and encloſed the fiſh now dead, and probably deriving their death from this very cauſe.

These inferences being admitted to follow, from the preſervation of the exact form as we ſee it in theſe fiſh, we may advance a ſtep farther, and obſerve, that as not only the form is preſerved, but a remarkable proportion of the very animal ſubſtance, to account for this effect we ſhall find it neceſſary to ſuppoſe, that the encloſing matter muſt have been of ſuch a nature, or in ſuch a ſtate, as to fit it for the ſpeedy abſorption of the ſofter and more pulpy parts of theſe fiſh, as faſt as they became abſorbable.

To ſee the neceſſity of this concluſion, let us in the firſt place recollect that the whole operation muſt, from the nature of the caſe, be conceived to have taken place in or under water; either then we muſt ſuppoſe that the dead fiſh continued floating at large, until the proceſs of putrefaction had taken place, in which caſe, indeed, the conveyance of the corrupting animal matter is eaſily accounted for; but, with it, the forms, not to ſay thoſe parts of their ſubſtance that are ſeen to remain, would be wholly loſt; or elſe we muſt imagine them arreſted, before putrefaction, in the depoſition of their preſent ſtony bed, by which all acceſs to the water as the vehicle of the ſoft putrefying parts is cut off, and we muſt look for ſome other mode of accounting for the ſpeedy removal of theſe parts; but ſuch removal, by ſome means or other, is abſolutely neceſſary to be ſuppoſed; for ſhould this tender animal matter reſt any time unremoved, it it would not only ſoon infect and involve all, but the moſt ſolid bones perhaps, in one complete corruption and diſſolution, and conſequently leave no other veſtige of the animals remaining; but, by the diſengagement of putrid air, or of the different ſpecies of gaſes produced by the putrefactive fermentation, either the ſurrounding matter muſt be puffed into cavities and air-bladders; or, if the medium was ſufficiently yielding to admit theſe gaſes to collect and force a paſſage upwards, ſuch an inteſtine motion muſt be produced, as would have greatly diſturbed, if not deſtroyed, not only the regular laminar ſtructure of the ſtone, but the very forms and ſubſtance of the fiſh as we ſee them, contrary to experience, and the actual ſtate of the facts.

From theſe conſiderations therefore it ſeems juſt to conclude that the bodies of theſe animals did not undergo any ſimultaneous putrefaction; but as it is clear that their oily, mucilaginous, and other ſoft parts muſt have been conveyed away, to have produced this effect, without general putrefaction, in the circumſtances here ſtated, I conceive can only be explained on the ſuppoſition of a rapid abſorption of theſe by the incloſing matter, as faſt as they become capable of it.

We are now to look for a cauſe adequate to the production of the ſeveral effects, which, from the above ſtatement, appear to have taken place,—a cauſe which ſhall account for the ſudden, and, as I may call it, unexpected diffuſion in a part of the ſea, of the kind of ſtony matter that we find incloſing theſe fiſh, in a pulverulent form, and in the immenſe quantity indicated by the bulk of the maſs; for their immediate loſs of life and ſpeedy incloſure in a bed of this matter—and for ſuch a ſtate of it as ſhould render it capable and fit, though immerſed in water, for the abſorption of the ſofter animal parts before fermentation could ariſe, as well as for leaving the harder and leſs abſorbable portions of their ſubſtance undeſtroyed, and in ſuch a ſtate as to reſiſt the no leſs deſtructive influence of ſucceeding time.

And here we are naturally led, by the quality and circumſtances of the incloſing ſtone, to a ſimple cauſe, which, though it might be too much to aſſert to be demonſtratively the real one, yet I believe will be allowed fully equal to the effects, and perhaps I might venture to ſay, almoſt excluſively ſo. This ſtone, it has already been obſerved, is wholly calcareous, of a light colour, of a grain dull though fine, and wholly devoid of any cryſtalline or ſparry appearance. Now it is well known that lime-ſtone, whatever its original colour may have been, becomes uniformly white or whitiſh, on being calcined or burnt more or leſs to a lime; that after this calcination it immediately ſlacks or falls into a powder, on being immerſed in water; and by agitation is eaſily diffuſed in this element, from which, if left in tranquillity, it ſoon ſubſides in a pulverulent ſtate; that this diffuſion of lime in water quickly deprives of life ſuch fiſh as happen to lie within its reach; and, in fine, there is every reaſon to believe, that a depoſition of this nature poſſeſſes remarkably the quality, juſt deſcribed, of quickly abſorbing, even in water, the oily and other ſoft parts of animals; and, when ſufficiently ſlacked,

ſlacked, and thus impregnated with animal matter, without deſtroying the harder and firmer parts*.

The application of theſe circumſtances is eaſily imagined, and will be found to correſpond remarkably with all the appearances in the caſe before us. This depoſition, gradually and ſucceſſively concreting at the bottom of the water, may naturally be expected to aſſume a flag-like or laminar ſtructure; the grain

 too

* To this, the cauſticity of lime, and its well-known application to dead bodies, with a view to their deſtruction, may be objected. On this account it may be looked upon by many as a *total deſtroyer* inſtead of a *partial preſerver* of animal ſubſtances. But beſides the diminution of this cauſticity by diffuſion in ſo great a body of water as our caſe ſuppoſes, we know that it muſt ſoon have acquired a ſufficiency of fixed air, or of the carbonic principle, from the abundance of animal juices furniſhed by the immenſe number of fiſh contained, to render it mild and prevent its preying on the firmer parts. This may eaſily be decided by actual experiment:—Mean time I beg to refer the Academy to the ſubjoined paper, which our learned and ingenious member, Dean Hamilton, who happened to be preſent at the firſt reading of this eſſay, has ſince done me the favour to communicate, and which contains facts and reaſoning ſo appoſite, that I believe they may be conſidered concluſive as to the objection now ſtated. That lime, though ſoaked in water, will ſtill greedily abſorb oils or other animal fluids, experience will determine, and its avidity for the carbonic principle will fully explain; add to this the weight, preſſure, and conſequent ſpeedy condenſation of the ſubſiding maſs, and I believe the effects ſtated may be regarded as highly probable. But if this be admitted, and ſhall be confirmed by ſuitable experiments, it will then deſerve to be conſidered, further, whether a greater or leſs degree of cauſticity of the incloſing calcareous matter, ſo far from an objection, may not prove to have been indiſpenſibly requiſite to the production of the effects, and, of courſe, whether the exiſtence of ſuch effects may not, in that caſe, be found a proof, and a ſtrong one, of that ſtate of cauſticity, and conſequently tend greatly to corroborate, if not to confirm, the whole of the account here given.

too of this new aggregate ſhould be wholly without luſtre, as well on account of its calcination, as of its formation by ſubſidence from, not in conſequence of ſolution in, a liquid menſtruum; in which laſt caſe alone cryſtals are known to be produced. This will further eaſily account for the formation of the calcareous ſpar found within the prominences occaſioned by the joints of the vertebræ, and the other groſſer bones; for, theſe being freſh and ſound at the firſt arrangement of the ſtone, of courſe excluded the ſubſiding matter; but in proceſs of time their hollows were filled, and by degrees, as it decayed, their ſubſtance replaced, by a ſucceſſive filtration of water holding calcareous matter in ſolution, which depoſited, plate after plate, its cryſtalline matter in theſe cavities. Of the abſorption already mentioned, the fetid ſmell emitted by every portion of this ſtone on being ſcraped, will ſurely be conſidered as affording a ſtrong preſumption, as it perfectly accords with, and would naturally follow from, the ſuppoſition, that the whole had imbibed, and been ſtrongly impregnated with, animal matter *.

In inveſtigating ſubjects of this kind, whoſe origin lies ſo very remote from any thing that either modern experience or hiſtoric record can produce, and particularly in a circumſtance like this,

* This impregnation would alſo probably contribute to the concretion of the ſtone, as it is known to do in cements, floors, compoſitions, &c. to which cheeſe, milk, blood, and other animal ſubſtances, are often added with this view. A mixture of curd, or whites of eggs, with lime, has been long uſed as a ſtrong cement to repair broken China.

this, which is ſo very rare in the hitherto diſcovered natural knowledge of our globe, the utmoſt that can be expected is a reaſonable degree of probability, deduced, as the caſe may admit, from more or leſs appoſite analogy. If ſuch analogical reaſoning be fairly applied, it is but juſt to expect that the conſequences reſulting from it be admitted, until their falſity ſhall be proved, or a ſuperior degree of probability eſtabliſhed on different and more ſolid grounds. For this reaſon I ſhall, for the preſent, venture to aſſume as proved what I have juſt now ſuggeſted, and proceed to another link in the chain of cauſes that may be ſuppoſed to have been concerned in producing the effects under examination.

Taking it then for granted that the ſudden diffuſion of lime in the water in which theſe fiſh happened to be, and its conſequent depoſition, was the immediate cauſe of their encloſure, and the origin of this curious quarry, we are next led to enquire in what manner this lime may have been ſo burned, and ſuddenly projected into water, which but juſt before was proper for the ſupport of the animals inhabiting it. And here it is ſo obvious to have recourſe to fire as a proper agent for the calcination of lime-ſtone, and, from the apparently rapid and unexpected projection of the immenſe quantity which the thickneſs of the ſtratum indicates, to ſuppoſe this fire to have been volcanic; that although no ſuſpicion had ever been entertained of the exiſtence of ſuch a cauſe in the neighbouring country, or of its operation on the adjacent ſoil, this ſingle caſe would ſeem ſufficient of itſelf to have excited ſuch an idea.

That

That, where volcanic fire exifts, it will burft out fuddenly, fometimes in one place and fometimes in another, as it happens to find more or lefs refiftance, is well known; it is alfo certain that the matter of its ejections muft be various, as the fubftances chance to be, that lie within the fphere of its activity, or fall within its vortex. That Vefuvius, for inftance, (not to fpeak of extinct and dubious volcanoes,) has at fome periods thrown out an immenfe quantity of marble, and other calcareous ftones, in various degrees of calcination, the beft naturalifts that have defcribed that volcano affert, and I can fully confirm, both from actual obfervation, and from the fpecimens which I collected there, and have had the honour to prefent to the Academy. Why then may we not fuppofe that other, and more ancient volcanoes, may have acted upon calcareous matter as well as Vefuvius, and in a greater quantity? The circumftances of the prefent cafe feem to demand fuch a fuppofition; and if it is not inadmiffible on ftrong negative grounds, it invites our affent by giving a clear and eafy explanation of the feveral effects in queftion.

Although, for reafons which I fhall mention, it appears to me evident that the prefent fituation of the fifh quarries of Bolca cannot be that of their original formation, yet the great bulk of the maffes that compofe them, will not allow us to believe, that they could ever have been feated very remote from their prefent beds. But as it is abundantly clear, that the place where they were formed muft have been covered with the fea, it feems reafonable to conclude that the fea did then approach

much

much nearer to Bolca than it does at preſent*, if it did not wholly cover that hill. But, this ſuppoſed, ſtill the immenſe number and variety of fiſh that are found incloſed in the very narrow compaſs of theſe quarries, ſo far exceeds any thing that in the ordinary courſe of nature is to be met with in any ſea of the world, that ſome uncommon cauſe muſt have occurred to aſſemble them thus, whether living or dead. Submarine volcanic commotions once admitted, (and that ſuch may exiſt the well-atteſted facts of new iſlands produced by them, in the Archipelago and elſewhere, ſufficiently prove,) may we not, in conformity with the other indications, account for this numerous aſſemblage of fiſh in one ſpot, by ſuppoſing that ſome new eruption of this kind might have driven them from their uſual haunts, to take refuge in ſome place, the moſt remote they could find, from the diſturbing cauſe; or, having deprived them of life, might have impelled or whirled their bodies into one pool. That the place where they were collected, whether living or dead, and in which they were buried in their calcareous incloſure, was not very remote from land, ſeems inferred by the mixture of river fiſh with thoſe of the ſea. This circumſtance, as it further proves the exiſtence of rivers at that period, evinces alſo that of hills raiſed much above the level of the ſea, from whence theſe rivers derived their ſources, and probably at no great

* I have already obſerved that the neareſt ſea is now upwards of fifty miles from Bolca.

great diſtance from this ſpot*, which ſtands at the foot of the Alps of the country of Trent. Theſe fiſh then poſſibly may have been forced or drifted into ſome receſs adjoining their preſent ſituation, into which the newly erupted volcano ſuddenly poured an immenſe quantity of calcined calcareous matter, and thus gave riſe to the ſeveral conſequences that I have already deſcribed.

It now only remains to trace the inferences that ariſe from the conſideration of the general circumſtances and ſituation of the maſſes that form the fiſh-quarries of Monte Bolca: Theſe, I have obſerved, lie in detached bulks, ſet in different ſpots of the ſide of this hill; but the ſoil of Bolca is wholly argillaceous, its native ſtone reſembling our baſalte of the county of Antrim; and as far as I could ſee, or learn from the inhabitants, beſide the quarries in queſtion, it does not contain any calcareous matter whatſoever. Now theſe, though near each other, lie at ſomewhat different heights, and different inclinations of their laminæ to the horizon, as well as to the ſurface of the hill, and coincide with neither of theſe directions; from whence alone it ſeems evident that they could not have been formed in the places where they are now found; add to this their flag or laminar

* P. Pini, in his memoir above quoted, p. 242, ſays, that with the fiſh of Bolca are often found impreſſions of ſprigs and leaves of various trees. How far this may be well founded I cannot pretend to ſay, not having myſelf ſeen at Bolca, or in the collections at Verona, impreſſions of any other than *marine* vegetables. *He* does not ſay either, *that he had ſeen* ſuch impreſſions. Soc. Ital. di Verona, tom. 5.

laminar ſtructure, the leaves all parallel to each other, and perfectly ſtraight or flat; their compoſition, and the nature of their contents, all ſtrongly atteſting formation in a horizontal poſition:—and their want of continuation, the extremities being ſuddenly and ſharply cut off; ſo many circumſtances concurring, and tending all to the ſame concluſion, give it a degree of evidence that may almoſt be called demonſtration.

But if it be admitted that the preſent ſituation of theſe maſſes muſt be regarded as foreign from their native place of formation, we have, in this, another object of inveſtigation, no leſs remarkable in itſelf than intereſting to the natural hiſtory of that country.

That theſe maſſes, in their original poſition, muſt have formed part of a continued, and horizontal ſtratum of ſome conſiderable extent, ſeems highly probable. If the account juſt now given of the origin of this ſtone be well founded, the thickneſs of the ſtratum evinces that the quantity of calcareous matter, ſuddenly poured into the ſuperincumbent water, muſt have been immenſe:—But this matter, immediately on its ſubſidence being pulverulent, or in the ſtate of a ſoft mud, muſt have lain to a great depth at the bottom of the water, and, from the levelling nature of that fluid, muſt have been ſpread out over a greater or leſs ſurface, as the ſhape of the ground, or bottom on which it lay, or the intervention of ſhores or other obſtacles may have permitted. At all events it is ſcarcely poſſible to

 imagine

imagine any circumſtances in which this ſtratum muſt not be conceived of a much greater extent than the very circumſcribed ſurface of the quarries of Bolca. Of ſuch a ſtratum then, theſe quarries are evidently to be conſidered as no more than portions or fragments, now completely disjoined from all connection with their native bed: We are next to enquire how theſe portions may have been ſo detached.

A NUMBER of circumſtances already mentioned leave not a doubt, that this muſt have happened after the conſolidation of the original ſtratum; for it is clear, that a disjunction of this kind could not have taken place, in any way, without ſuch a concuſſion and diſturbance of theſe maſſes, as, if their matter was in any reſpect ſoft or yielding, or in any other than a firmly compacted ſtate, muſt have greatly diſarranged their laminar ſtructure, as well as the forms of the fiſh contained. Their extremities too, inſtead of being ſharp and even, would have been left confuſed and ill-defined. This matter, therefore, muſt certainly have lain in its firſt bed and poſition, undiſturbed, ſo long at leaſt as was neceſſary to its perfect conſolidation. At ſome period ſubſequent to this, it would ſeem that the whole of the ſtratum was violently broken up, and immenſe fragments of it heaved from their natural ſituation, and diſperſed here and there, as in the inſtance before us. But when we calculate the prodigious forces required to produce the effects here deſcribed, we ſhall not find it eaſy to aſſign any other cauſe, fully adequate to them, but that which we have already had recourſe to,

namely,

namely, ſubterraneous fire and exploſion; and this we muſt conceive to have operated, in the preſent caſe, with an eruptive force much exceeding whatever has been experienced in the known hiſtory of volcanoes*.

I HAVE choſen to repreſent the ſeparation of theſe maſſes, from the ſtratum of which they formed a part, as brought about, rather by their having been heaved up to their preſent ſituation from an inferior one, than by their having been left where they are, while the reſt of the ſtratum was ſunk; but I ſhall not trouble the Academy with the reaſons that led me to the one concluſion in preference to the other, as the principal point of enquiry is the nature or quality of the cauſe, not its preciſe mode of operation. Now, whatever may have been the manner in which the effects were produced, the immediate cauſe muſt, in any caſe, have been one of great violence; and there are many reaſons, beſides ſufficiency of force, to conclude it volcanic.

IT is remarkable that, except theſe quarries, nothing of the ſtratum to which they belonged is left, not a trace having yet been diſcovered, either at Bolca or in its neighbourhood, to indicate where ſuch a ſtratum had ever exiſted; all is covered with argillaceous materials, the ſuppoſed lavas and other ejections of ancient ſubmarine volcanoes: but this circumſtance, until

 it

* If theſe effects are ſuppoſed to have been produced in air, the forces applied muſt have been immenſe; if under water, much leſs would have ſufficed.

it ſhall be better accounted for, admits of an eaſy explanation, by ſuppoſing that the ſame eruption which broke up the original calcareous bed, wherever it lay, and raiſed theſe fragments of it to their preſent ſituations, muſt, in ſo doing, not only have disfigured and wholly changed the appearance and ſhape of the former ſurface, but may naturally be conceived to have thrown out, at the ſame time, ſuch a quantity of argillaceous matter as was ſufficient completely to bury every other part and veſtige of it, except theſe maſſes which ſeem to have eſcaped, as it were, by accident.

That eruptions, and of the kind here ſuppoſed, did exiſt in the diſtrict in queſtion at ſome very early period, naturaliſts of great eminence have, long ſince, attempted to deduce from other conſiderations than thoſe contained in this paper; but as their reaſonings in proof of this are immediately connected with a very important mineralogical queſtion, which, though much diſcuſſed, remains ſtill in controverſy *, I ſhall not avail myſelf of their authority, reſpectable as ſome names are which I might otherwiſe adduce in ſupport of this opinion. It is not my intention, nor is it, I believe, at all neceſſary to my preſent ſubject, to introduce here any conſideration of the extenſive and difficult queſtion to which I allude; for whether the baſaltic columns, and other analogous covering of this and ſimilar countries, ſhall be attributed to the immediate agency of fire or of water, the conſequences which I have endeavoured to trace, from

* It will eaſily be perceived that the queſtion alluded to is that *on the origin of baſaltes.*

from the examination of the fiſh-quarries of Bolca, may ſtill hold whatever degree of probability they may be thought entitled to; theſe conſequences, being deduced from local and partial circumſtances, and ſuch as are, perhaps, peculiar to that ſpot, and reſting principally on internal evidence, require to be judged ſpecially, and on their own merits, without being ſubjected to any determination that may be formed, as to the reality of other effects, imputed to the ſame general cauſe.

I HAVE now given the Academy the principal obſervations which aroſe in my mind from the inſpection of theſe curious quarries, and ſhall only add, that, as far as they have any weight, they go to infer the remote exiſtence of two diſtinct æras of great volcanic exploſion in this place, one of which at leaſt ſeems decidedly to have been ſubmarine; the firſt, when the fiſh were caught and incloſed in calcined calcareous matter; and the ſecond, after the complete concretion of this matter into ſtone, when its horizontal ſtratum was torn up and diſperſed.

BEFORE I conclude this paper I cannot omit taking ſome further notice of a circumſtance already mentioned, which ſeems to hold out a ſubject of inveſtigation the moſt ſtriking, and to many the moſt intereſting, that occurs, perhaps, in the whole range of natural hiſtory: I mean the great variety of fiſh collected in one ſpot, which, from the catalogues, appear to correſpond with ſpecies now only to be found in ſeas and climates the moſt remote from the Italian ſhores. It would be ſuperfluous to dwell on the analogy which this remarkable circum-

ſtance

ftance bears to the many difcoveries that have been made, not only of fhells but of horns, teeth, bones, and other remains, and even of entire fkeletons, of various land-animals, partly known and partly unknown, in countries where fimilar living fpecies have never been obferved to exift, and often in climates now wholly unfuited to their conftitutions. Thefe extraordinary facts have been long known, and have long excited the attention, and exercifed the ingenuity, of naturalifts of the moft diftinguifhed talents: But, unfortunately, experience has fhewn that the paths of fpeculation to which they directly lead have too often conducted thefe great men into labyrinths, from which all the efforts of their genius have not been able to extricate them. Hence it would feem, that natural knowledge is not yet fufficiently advanced, nor a fufficient ftock of well-attefted phenomena yet formed, to enable us to profecute fuch extenfive and difficult enquiries with good effect. On this account, perhaps, thofe who really wifh to contribute to the fubftantial improvement of the fcience, might employ their talents more beneficially, in the humble tafk of collecting facts, and inveftigating partial and immediate caufes, than in giving the reins to their imaginations, and foaring in purfuit of vifionary theories. Of more remote and general caufes, pofterity, better informed by new facts and obfervations, in addition to thofe which we now poffefs, may poffibly form a better judgment than we can afpire to, if fuch a judgment really lies within the limits of human attainable knowledge. But, at all events, it fhould not be forgotten, that fpeculations of this kind are regarded by men of the foundeft underftandings, rather as amufements of the mind,

mind, and relaxations from ſeverer ſtudies, than as purſuits of much intrinſic importance; and that, at beſt, they are to be conſidered as contributing but remotely to the more uſeful and ſerious objects of life: But when applied, as we know they have been too often, to excite and diffuſe doubts of the moſt eſſential truths, and ultimately to ſap the foundations of religion, and, with it, of both private and public virtue, order and happineſs, and indeed of the very exiſtence of civil ſociety, as too fatal modern experience has ſhewn, it is not eaſy to ſay whether we ſhall be moſt ſtruck with the vanity and preſumption, the folly, or the wickedneſs of the attempt, to raiſe ſo daring a ſuperſtructure on ſo ſlender a baſe *.

* Since this paper was read, Mr. Raſpe has pointed out to me a paſſage in his Preface to Ferber's Letters, tranſlated and publiſhed by him in 1776, which had before eſcaped my notice: Speaking of ſubmarine volcanoes, he ſays "The petrified "fiſhes are monuments of their heat;" and adds, "ſome unnatural revolution in "their own element muſt have killed and involved them at once in the ſediments of "the troubled ocean; on this account, *many argillaceous ſlate rocks*, filled with petri-"fied fiſhes, are to be conſidered as ſubmarine or ſubaqueous volcanic productions; "nay, *many calcareous ſlates*, ſuch as thoſe at *Bolca, Pappenheim, Eichſtaed, Altheim* "and *Mont Libanon*, are, *for the ſame reaſon*, to be ranked amongſt them."

I tranſcribe this paſſage, as it ſeems to bear ſome reſemblance, though very generally expreſſed, to the account above given.

The deſcription of Monte Bolca, which we meet with in pages 49 and 50 of the ſame work, is erroneous in almoſt every particular. It is as follows: "Bolca is a "ſteep barren hill at twenty miles diſtance from Verona, *for the moſt part* of *ſtratified limeſtone*, but *now and then* interrupted by ancient *volcanic craters*. The lime-"ſtone contains *variegated flints, of a red, black, green and white colour*. In this hill "are found the famous impreſſions of plants and fiſhes."

Catalogo

Catalogo Sistematico, dei piu' rari ictioliti del Monte Bolca che si conservano nel gabinetto privato del Sig. Vincenzo Bozza, in Verona, nel quale vi sono piu' di 500 esemplari di pesci fossili dello stesso monte, una gran parte ancora da riconoscersi e denominarsi.

ORDINE I.

Pesci dei Mari dell' Europa.

Ophidium barbatum. WILLOUGB. *Icth. Tab. G. 7. fig.* 6.
Squalus stellaris. LINN. *Syst. nat. Edit.* 13. *pag.* 399.
Scomber colias. WILLOUGB. *L. C. Tab. M.* 1. *fig.* 1.
——— *scomber.* WILLOUGB. *l. c. Tab. M.* 3.
——— *pelamis.* SALVIAN. *de Aquatil. fig.* 98.
——— *thynnus.* ARTED. *Icth. Gen.* 31. *Synon.* 49.
Scorpæna porcus. BLOCH. *Icth. VI. Tab.* 181.
——— *scorpius.* WILLOUGB. *l. c. Tab.* 12. *X.*
——— *scrofa.* BLOCH. *l. c. Tab.* 182.
——— *Salviani.* WILLOUGB. *l. c. Tab. X.* 13.
Biennius ocellarius. SALVIAN. *l. c. fig. P.* 84.
Gadus carbonarius. BELLON. *de Aquat. Lib. I. pag.* 134.
——— *virens.* VILL. *l. c. Tab. L.* 1. *fig.* 3.
——— *merlucius.* BELLON. *l. c. Tab. L.* 1. *fig.* 123.
Pleuronectes limand. WILLOUGB. *l. c. Tab. F.* 4.
Sparus aurata. GRONOV. *Mus.* 1. *n.* 90.
——— *chromis.* LINN. *l. c. pag.* 470.
——— *pagrus.* ARTED. *l. c. Gen.* 36. *Syn.* 64.

Trigla cuculus. WILLOUGB. *l. c. Tab.* 5. 2. *fig.* 2.
Esox sphyræna. LINN. *l. c. pag.* 515.
Muræna myrus. ARTED. *l. c. Gen.* 24. *Synon.* 40.
Lophius piscatorius. SALVIAN. *l. c. fig.* 47.

Raja Postinaca *Muræna Helena* ——— *Serpens* ——— *Cœca* *Gadus Mediterraneus* *Blenius Lumpenus* *Zeus Faber* *Sparus Sargus* *Labrus Turdus* *Clupea Harengus.*	MARQUIS GAZOLA.

ORDINE II.

Pesci dei Mari dell' Asia.

Chætodon vespertilio. BLOCH ICTH. *VI. Tab.* 199. *fig.* 2.
——— *bisfasciatus.* SEBA. *Mus.* 3. *Tab.* 26. *fig.* 23.
——— *pinnatus.* SEBA. *l. c. Tab.* 25. *fig.* 15.
——— *niger.* SEBA. *l. c. Tab.* 25. *fig.* 5. *a.*
——— *canescens.* SEBA. *l. c. Tab.* 25. *fig.* 7.
——— *lineatus.* SEBA. *l. c. Tab.* 25. *fig.* 1.
——— *fuscus.* SEBA. *l. c. Tab.* 26. *fig.* 22.
——— *striatus.* BLOCH. *li. c. Tab.* 205. *fig.* 1.
——— *macrolepidotus.* SEBA. *l. c. Tab.* 25. *fig.* 8.
Fistularia chinensis. VALENT. *Ind.* 3. *fig.* 23.
Pegasus natans. BLOCH. *Icth. IV. Tab.* 121. *fig.* 3. 4.
——— *volans.* LINN. *l. c. pag.* 418.

Polynemus paradiseus. LINN. *l. c. pag.* 522.
Zeus ciliaris? BLOCH. *Icth. VI. Tab.* 191.
—— *triurus. Huic valde adfinis Zeus faber* LINN.
Tetraodon lagocephalus. SEBA. *l. c. Tab.* 23. *fig.* 5. 6.
Clupea Thrissa. BROUSSON. *Icth. Dec. I. Tab.* 10.
Perca unicolor. SEBA. *l. c. Tab.* 27. *fig.* 10.
Esox amboinensis. RUYSCH. *Amboin. Tab.* 14. *fig.* 2.
Diodon reticulatus. SEBA. *l. c. Tab.* 23. *fig.* 3. 4.
Labrus ferrugineus. SEBA. *l. c. Tab.* 31. *fig.* 5. 6.
Muræna serpens. WILLOUGB. *l. c. Tab. G.* 10. *fig.* 1.
Callyonimus indicus. LINN. *l. c. pag.* 434.
Sparus argenteus. SEBA. *l. c. Tab.* 27. *fig.* 13.

Pegasus Draconis.—MARQUIS GAZOLA.

ORDINE III.

Pesci dei Mari dell' Africa.

Sparus dentex. WILLOUGB. *lib. c. Tab. X.* 7. *fig.* 6.

Ostracion Turritus.—MARQUIS GAZOLA.

ORDINE IV.

Pesci Marini dell' America meridionale.

Raja muricata. MARCGR. *Brasil. pag.* 175.
Scomber cordyla. WILLOUGB. *l. c. Tab.* 5. 18. *fig.* 1.
——— *coorza Pisonis.* WILL. *l. c. Tab. M.* 5. *fig.* 2.
Esox brasiliensis. MARCGR. *l. c. pag.* 168.

Chætodon

Chætodon arcuatus. BLOCH. VI. *Tab.* 201. *fig.* 2.
——— *trioſtegus.* SEBA. *l. c. Tab.* 25. *fig.* 4.
——— *acarauna.* WILL. *l. c. Tab.* o. 5.
——— *fuſiformis. An. Ch. rhomboides?* BLOCH. *l. c. Tab.* 209.
Polynemus quinquarius. SEBA. *l. e. Tab.* 27. *fig.* 2.
——— *plebejus.* BROUSSON. *l. c. Tab.* 8.
Loricaria plecoſtomus. MARCGR. *l. c. pag.* 166.
Silurus bagre. SEBA. *l. c. Tab.* 29. *fig.* 2.
Gobius ſtrigatus. BROUSSON. *l. c. Tab.* 1.
Zeus vomer. BLOCH VI. *Tab.* 193. *fig.* 2.

Diodon Orbicularis *Mulus Gigas* *Exocetus Evolans* *Sparus Argenteus* *Coryphena Hippurus* *Chætodon Saxatilis* ——— *faſciatus* ——— *ciliaris?* ——— *curacao* ——— *nigricans* ——— *cornutus* ——— *lanceolatus* ——— *orbis* ——— *arcuatus* ——— *aculeatus* *Silurus Faſciatus.*	MARQUIS GAZOLA.

ORDINE V.

Pesci marini dell' America settentrionale.

Balistes monoceros. CATESBY. *Carol.* 21. *Tab.* 19.
Chætodon chirurgus. BLOCH. *VI. Tab.* 208.
Esox umbla minor. CATESB. *l. c. Tab.* 1. *fig.* 1.
Fistularia tabacaria. WILLOUGB. *l. c. Tab. P.* 6. *fig.* 4.
Exocetus evolans. CATESB. *l. c. Tab.* 8. *fig.* 1.
Gasterosteus carolinus. LINN. *l. c. pag.* 490.
Gadus tau. WILLOUGB. *l. c. Tab. N.* 12. *fig.* 3.

Gasterosteus Canadus
——— *Volitans*
Perca Venenosa
—— *Punctata*
—— *Trifurca*
Scomber Fasciatus
Clops Saurus.
} MARQUIS GAZOLA.

ORDINE VI.

Pesci di acqua dolce, esotici.

Tetraodon ocellatus. Ex Indiis. SEBA. *l. c. Tab.* 23. *fig.* 7. 8.
Chætodon argus ex Indiis. BLOCH. *l. c. Tab.* 204. *fig.* 1.
Gobius ocellaris. Ex Ins. otheit. BROUSSON. *lib. c. Tab.* 2.
Clupea cyprinoides. Ez Brasilia. BROUSSON. *lib. c. Tab.* 9.
Zeus insidiator. Ex Surate. BLOCH. *l. c. Tab.* 102. *fig.* 23.

Tetraodon Lagocephalus
Chætodon Glaucus
Sparus aurata ex America.
} MARQUIS GAZOLA.

La grandezza di alcuni Pesci è rimarcabile, essendovene di quelli che giungono alli 40 pollici di lunghezza. Non si notano poi, nel presente Catalogo, le singolarità di ciascheduna petrificazione, per servire alla brevità propria di un Indice.

Nel Gabinetto prefato, oltre ai Pesci fossili, si conservano le seguenti petrificazioni di altre Classi di corpi organizzati, cioè

I. Fitoliti di fuchi e felci europee ed esotiche. Del Monte Bolca.

II. Zoofiti di Madrepore, Isidi, Millepore ecc. dei Monti Veronesi.

III. Elmintoliti di Conchiglie univalvi, e bivalvi di molti mari, con echiniti, stelle marine, e congierie delle medesime. Delle Montagne predette.

IV. Entomoliti rari di granchi, insetti apteri, e dipteri d'Europa, e di America. Del Monte Bolca.

V. Osteoliti di Animali ruminanti, e di altri quadrupedi d'insigne grandezza. Di Romagnano, e di altre parti del Veronese.

On the POWER of FIXED CAUSTIC ALKALINE SALTS to preſerve the FLESH of ANIMALS from PUTREFACTION. In a Letter to the Reverend GEORGE GRAYDON, &c. from the Reverend HUGH HAMILTON, D. D. &c.

SIR,

THE ingenious paper you communicated to the Royal Iriſh Academy, on the fiſhes that have been found encloſed in ſtone in the quarry at Monte Bolca, brought to my recollection ſome obſervations I had made many years ago, on the power that alkaline ſalts, even when highly cauſtic, have to preſerve animal ſubſtances. I mentioned to you ſome of theſe obſervations, and you have deſired I ſhould give you a fuller account of them, as you thought they in ſome meaſure coincided with the theory you had delivered, concerning the preſervation of the more ſolid parts of the fiſhes found in quarries of lime-ſtone. Read April 5, 1794.

I CAME

I CAME to the knowledge of this power of alkaline ſalts, I may ſay, accidentally. I had a wiſh to procure ſome kind of alkaline liquor that might be ſafely taken, for the purpoſe of correcting acidities in the ſtomach. I knew that a ſolution of ſalt of tartar was exceedingly offenſive to the taſte, and if it was of ſtrength ſufficient to neutralize any quantity of acid in the ſtomach, it could not be ſwallowed without danger to the paſſages, from its cauſticity. It occurred to me, that its cauſticity might probably ariſe from its having a ſtrong affinity to ſomething or other, to get at which it burned or deſtroyed the texture of the fleſh. If this ſhould be the caſe, it was natural to ſuppoſe, that this ſalt, if intimately mixed with fleſh, would ſaturate itſelf with whatever it was that it had ſuch a ſtrong appetite for, and, being ſo ſaturated, it would act no further on our fleſh, and might, without danger, be taken inwardly. To try this, I firſt encloſed ſome bits of lean raw mutton in a vial with a ſtrong ſolution of ſalt of tartar; but, after ſtanding ſeveral days, no ſuch alteration as I expected appeared in the liquor. I was willing to account for this, by ſuppoſing the ſalt had a greater affinity to the water than to any thing in the fleſh; I therefore cut ſome fleſh from the breaſt of a turkey, roaſted the day before, and made it as dry as I could; this I pounded in a mortar, adding by degrees ſome dry and finely powdered ſalt of tartar *, until I thought there was enough, for I had no rule to judge by; the mixture grew moiſt,

* This ſalt had been ſent to me rendered cauſtic by quick-lime, though I had not deſired it.

moift, and when it was fufficiently pounded, I fpread it into a thin cake on an earthen difh, and fet it before the fire, and it foon became quite dry. I found it had then a faponaceous mild tafte, for the tafte of the falt was fcarcely perceptible. Having macerated this flefh in warm water, and poured off the clear liquor, I found it effervefced with vinegar, which fhewed, that the falt was not fo far neutralized, but that it would unite itfelf with an acid, fo that I confidered it as a mild alkaline liquor, fuch as I fought for: However, that I might have an opinion from a perfon of fkill on the fubject, I wrote to my late worthy and ingenious friend, Doctor Mc. Bride, and acquainted him with the preparation I had made, and the intention of it. In his anfwer, he was pleafed to fay he approved of the idea, and would make fome of the liquor I defcribed, and let me know what he thought of it. He afterwards wrote to me, and faid he had tried the alkaline liquor, and thought it might prove a ufeful medicine, particularly as it might be mixed with milk and given to children, who have often acids in their ftomachs. He alfo mentioned a phyfician then in Dublin, to whom he recommended the liquor, and who had found great benefit from it. I firft made this liquor in the year 1771, and in the year 1777, being then at Bath, I met with an account of fome experiments made by Mr. Bewly, an ingenious chymift, which plainly proved that fixed air is an acid, and faturates alkaline falts; this at once informed me, what it was in the flefh of an animal, that alkaline falts had fuch a ftrong affinity to. At the fame time I got from London one of Doctor Nooth's glafs machines for impregnating water with fixed air, and to the

 water

water I added ſalt of tartar; after this, you may ſuppoſe, I thought no more of my alkaline broth, having got a way of obtaining what I wanted in a much more elegant manner.

I WOULD not have given you this long detail of a matter now unintereſting, had you not deſired me to write the whole of what I had told you in our converſation. The only thing now worth attention in the experiment I have related is, that it diſcovered a power in even cauſtic alkaline ſalt to preſerve fleſh, I may ſay, incorruptible; though it has been generally imagined that ſuch ſalts would couſume it. I have ſome fleſh prepared with theſe ſalts in the year 1772; for, finding ſome bits made the year before had continued unaltered, I made ſome more, and laid it by to ſee how long it would keep, and what alterations it would undergo. I made it into a cake, and when quite dry I cut it into round bits about the ſize of half a crown, and put them into a drawer in my deſk; I ſhewed ſome of them to Mr. Kirwan the ſummer before laſt, when I had the honour of receiving a viſit from him at Armagh, and a few months ago I found ſeveral pieces in another drawer, *where they have lain near two and twenty years, and remain unaltered; when they are broken, the pieces hang together by fibres, and look like a piece of plaſter taken from a wall; the fibrous or ſtringy parts of the fleſh do not ſeem to have been corroded or diſſolved by the ſalt.*

AFTER I knew that fixed air was an acid, and ſaturated alkaline ſalts, I began to form conjectures about the means by which

which these salts had so entirely prevented putrefaction in the flesh to which they were united. Animal substances afford much volatile alkali, and now they are known to contain also a volatile acid gas. While these two volatile principles continue united with each other, they may prevent any material change from taking place in the substance; but if one of them by any means escapes, the other will follow; the acid seems to be the most volatile, and escapes first, though we may not be sensible of its escape, because it has no such strong smell as the alkali has. The letting loose these volatile principles seems to be the beginning of putrefaction. If this be the case, we may see the reason why flesh, growing putrid, is restored to sweetness by fixed air, that acid replacing what had escaped, and retaining the volatile alkali. It is probably on this account that the ærial acid is found to be of use in stopping the progress of some putrid diseases; it seems to act as a sort of pickle. If vinegar preserves flesh by keeping its volatile alkali united with this acid which is not volatile, we may expect a fixed alkali will have a like effect in preserving flesh, by expelling the weaker volatile alkali, and uniting itself to the volatile acid, which will therefore be retained. This I found to be really the case; for, while the flesh and alkali were combining in the mortar, a very strong smell arose like that of *sal volatile*; and at one time that I used a brass or metal mortar, I perceived its edges to be tinged with blue, which shewed the metal had been affected by a volatile alkali.

 THERE

There seems to be a good reason why fixed alkaline salts should preserve flesh much longer than any fluid acid, such as vinegar, can do; for when the alkaline salt combines with the flesh, it expells what is volatile, the mass grows hard, and it is easily reduced to a state of dryness, in which no sort of fermentation or any intestine motion can take place, and therefore there is nothing that can effect a change in this compound substance: whereas when an animal or vegetable substance is immersed in vinegar, a very heterogeneous mixture is formed, which, in length of time, will be very apt to run into a sort of fermentation, with an intestine motion among the minute particles, and this will bring on some change in the texture of the substance, and every fermentation, when long continued, ends in putrefaction, which, indeed, is said to be the last stage of fermentation.

Whether the conjectures I have offered on this subject be well or ill-founded is but of little consequence; you may rely on the facts I have mentioned, and if you think they throw any light on your theory, you may, if you think proper, submit to the consideration of the Royal Academy this paper as an appendix to your's.

I am, Sir,

Your very humble servant,

Dublin,
April 2, 1794.

HUGH HAMILTON.

EXTRACT *from a* PAPER *on* SURVEYING. *By* THOMAS MEAGHER, *near Palace Grene in the County of Limerick.*

On a new Division of the Compass for Land Surveying.

THE present division of the compass into degrees, &c. originally intended for the convenience of the navigator, is not necessary for the land surveyor. To him a division which would give by inspection the sine and cosine of the angle measured would be of much greater importance. Such a division would preclude the necessity of having recourse to a table of natural sines, and very often give the sine or cosine accurately in only two or three places of figures, which would be of considerable use in facilitating the computation of great surveys. This division might be accomplished with the same ease to the instrument-maker as the present one, and would in every case afford equal if not greater accuracy in the result.

LET

Let the periphery of the circle be divided into forty parts, or the quadrant into ten, and each of theſe again into ten parts, in ſuch a manner that the ſines anſwering to the extremity of each diviſion may be ,01 ,02 ,03 ,04 ,05 ,06, ,07 ,08 ,09 ,1,11, &c. radius being unity. Let alſo the periphery be divided in the ſame manner in a contrary direction. The principal diviſions may be numbered 1, 2, 3, 4, &c. but the ſub-diviſions need not be numbered, leſt the numbers ſhould be confuſed. If the magnitude of the inſtrument admits it, each of theſe ſubdiviſions may be again ſubdivided by the eye into 5. Every compaſs uſed in ſurveying ought to be large enough to admit this, otherwiſe the neceſſary accuracy could not be attained, whether it be divided by the old method or by the one now propoſed.

Hence it is evident that by inſpection we can have the ſine and coſine of the bearing pointed out by the needle to three places of figures, and near the end of the quadrant even to four, which will in every caſe give the area with as much or greater accuracy as the method by the common compaſs. It may be objected that the ſubdiviſions to be performed or computed by the eye ought not to be equal; but, although they are not accurately ſo, yet they are ſo nearly equal, that no error will ariſe except for the ſines of arches near the end of the quadrant. Theſe ſubdiviſions, as they include large arches, may be accurately ſubdivided with great eaſe by the inſtrument-maker; or inſtead of ſubdividing theſe a ſmall table may be uſed for finding the ſines of large or coſines of ſmall arches; the tabular number to be entered

with

with the cofine of the large, or fine of the fmall arch. The fubdivifion between ,94 and ,95 includes 1° 44′, and therefore from this to the end of the quadrant it will be better to have the fubdivifions accurately marked by the inftrument-maker, or perhaps better ftill to have a fmall table for this part.

THE annexed figure fhews the divifion propofed; but only one of the principal divifions is fubdivided. The outer or upper edge of the ring is to be divided in one direction, and the inner edge in the contrary one.

A CONSIDERABLE advantage arifes in this method of dividing the compafs from the check which the two readings afford.

POLITE LITERATURE.

POLITE LITERATURE.

CONTENTS.

The COMPARATIVE AUTHENTICITY *of* TACITUS *and* SUETONIUS, *illustrated by the Question,* "*Whether* NERO "*was the* AUTHOR *of the* MEMORABLE CONFLA-"GRATION *at* ROME?" *By* ARTHUR BROWNE, *L.L.D. S.F.T.C.D. and M.R.I.A.*

SO much has been said of the candour of Suetonius, and of his work being the most accurate narration extant of the lives of the Emperors, that it is worth the pains to enquire how far these praises are due. Others are said to have been actuated by hatred, or slaves to adulation; he is represented alone as fair and uninfluenced *. For my own part I so much differ from this opinion, that I have ever considered the rank allotted to Suetonius, in the scale of historical merit, as elevated much beyond his deserts. I am not inclined to trust either his candour or his accuracy, particularly when opposed to, or compared with Read June 8, 1793.

* See the encomiums collected by Pitiscus, in the preface to his edition of Suetonius.

with his rival hiſtorian. We are accuſtomed, I know not how, at an early age, from cotemporary ſtudies, to unite the names of cotemporary hiſtorians, and from thence perhaps inſenſibly to infer a ſimilarity of excellence. The authors peruſed treat of the ſame facts, they are read at the ſame time, and the mind is yet too young for accurate diſcrimination. May not ſuch aſſociations have had ſome effect with reſpect to Suetonius and Tacitus? But the exerciſe of maturer judgment readily ſeparates ſuch unions, and detects the apparent paralleliſm of objects, which, ſufficiently purſued, will be found in time infinitely to diverge. This judgment, however, is in many caſes never exerciſed at all.

A PREMATURE peruſal of the claſſics often prevents a ſubſequent cool reviſal of their beauties and their merits, impels the man to conſider the ſubjects of the ſtudies of the boy as trifling and diſguſting, and indolently to acquieſce in firſt impreſſions, rather than retrace ſteps which appeared unpleaſant becauſe involuntary. But he who at maturer years is led by taſte or inclination to examine and compare the lights of antiquity, will be aſtoniſhed at their numerous detections of his errors firſt imbibed, and corrections of the implicit faith which he has put in ſome of its oracles; and perhaps no where will he find leſs reaſon for confidence than in the ſecretary of Adrian (for ſuch was Suetonius), however high his poſt or good his means of information.

THE

The title of this Eſſay indicates my intention to confine my obſervations to the comparative fidelity in narration of the celebrated writers therein mentioned, without touching on their other relative perfections or imperfections. The inſtance which I have ſelected to illuſtrate this point (for abundance of them might be found *) may to ſome appear trifling; and it may be aſked, who, in the eighteenth century, can be intereſted in the queſtion, whether Rome, in the firſt, was burnt by the hand of her natural protector, or of what utility is the diſcuſſion which tends to waſh away one ſpot from the bloody garb of Nero? The objection ſhould not come from the theoretic lover of truth, never deſpiſing enquiry and diſcrimination; nor will the expulſion of falſehood from hiſtory ever appear trifling to its practical admirers. The queſtion too is not totally unconnected with the well-known controverſy in morals, on the exiſtence of gratuitous malevolence, as any alleged motives for this ſuppoſed conduct of the tyrant are utterly unſatisfactory to the

* Such as Suetonius' aſſertion, that Tiberius aboliſhed the privilege of ſanctuary, when the contrary, which is aſſerted by Tacitus, is proved beyond a doubt, by coins ſubſequent to his reign; his making Germanicus conquer a king of Armenia, when Armenia had no king, and was not at war with Rome; his repreſentation of the character of Nero, in many reſpects differing from the traits given by Tacitus and others; his mentioning the loſs of an army in Aſia, when from Tacitus it appears it was only the rumour of ſuch a loſs. Surely theſe variances would not have appeared trifling to Lipſius, who took ſuch pains to reconcile theſe authors, when differing in the point, Whether Agrippa Poſthumus was killed by a *centurion* or a *tribune* of the ſoldiers. Joſephus obſerves, that no man's character has been more miſrepreſented, from adulation on the one ſide and prejudice on the other, than Nero's.

the rational mind *: But its chief importance refts on the grounds I have premifed. If we detect an hiftorian in any one inftance, in a peremptory and dogmatical affertion of a difputed, nay improbable charge, have we not caufe to view his writings with general fufpicion, and fcrutinize with jealous eye his accuracy or his candour? And we cannot felect a better example than that of a direct and unqualified allegation of a plain and fimple fact, into which, if falfe, the writer could not from any circumftances be fuppofed to be innocently or unwittingly betrayed.

Suetonius, then, directly and circumftancially afcribes the conflagration at Rome in the time of Nero to that detefted Emperor, while Tacitus only fays, *forte an dolo Imperatoris incertum.* The authority of the former feems to have prevailed, and few traditions have been more ftrongly believed, or fayings more frequently applied, than "that Nero fiddled while Rome "was burning." I apprehend therefore that the following arguments to the contrary will have at leaft the recommendation of novelty, as the oppofite opinion has never been hinted by any writer whom I have met, except the Abbé Millot, who annexes no reafons for his doubts.

The

* The defire of feeing the refemblance of Troy in flames is too childifh to be imputed even to the fantaftical mind of Nero, and the defign of burning a great city in order to improve and rebuild it, if indeed neceffary, in the plenitude of his power, for fuch object (while under our moderate government fimilar improvement is without difficulty attained on valuing the houfes pulled down) does not feem to be confirmed by his fubfequent actions.

The reader, who recollects the idle calumnies, which, upon a similar occasion, were thrown out against a Prince of our own, Charles the Second, and the numberless insinuations of opposite parties at that period, branding each other with the name of incendiaries, will not incautiously assent to the rumour bred by inflamed imaginations, ascribing to malice the offspring of accident.

Whoever has implicitly believed that Rome was burnt by Nero, will find, to his surprise, on the first peep into Tacitus, this passage, *Hoc tempore, Nero Antii agens*, the paragraph which first indeed, by exciting my wonder, drew my attention to this subject. The man, who is depicted as sitting on a lofty tower of his palace, attuning to the harp the poet's numbers on the destruction of Troy, in the midst of the imperial city, with whose fires his eyes were feasted, was not, at their commencement at least, in Rome at all. This should seem almost to terminate the question: but, no! the critic will say, Antium was only ten miles from Rome, and the Emperor had ample time to arrive there long before the extinction of the flames; in fact he did so, when he found that the most vigorous orders which he had issued from Antium had no effect. Such orders he had issued, and it shews his alacrity in trying to have the fire extinguished before his arrival. Let us see then how he acted after his arrival. During the very confusion and terror of the conflagration it may have been difficult to ascertain the conduct of the Prince; and it is during that period that Suetonius charges him with

with encouraging the flames and cheriſhing the incendiaries. "Voices of men," ſays he, "were heard, exclaiming that they "acted by orders from the Emperor, and emiſſaries from his very "houſehold might have been apprehended in the act of ſpreading "the flames." That the Emperor ſhould have been abſurd enough to furniſh incendiaries with the authority of his name is incredible; but let us remember that within three years paſt the deſtroyers of the caſtles of the nobility in France pleaded authority from that King whoſe throne they were on the point of overturning. To theſe idle tales I oppoſe the acknowledged behaviour of Nero, after the extinction of the fire, when it ſtands unveiled by that cloud of confuſion and rumour which always attends preſent calamity. He opened his gardens for the ſufferers, he pitched tents for them, he laboured to provide them with neceſſaries, he cheapened the price of corn; ſuch are the teſtimonies of Tacitus. On his previous abſence, on his ſubſequent conduct, I might perhaps then reſt his innocence; but it is confirmed by ſome other ſtrong arguments, to which I now proceed.

The Emperor is charged with ſetting fire to the city, that he might enjoy the beauty of the ſight. It appears from Tacitus, that ſo far from coveting the ſpectacle, his fault was, indolent reluctance to move from Antium. He iſſued from thence the moſt vigorous orders for extinguiſhing the flames, but he refuſed to ſtir till his own palace was on fire. It was in this ſituation that he muſt be ſuppoſed to have run up with his harp, immediately on his arrival, to the top of the tower of Mæcenas; a ſtation where he ſtood a very reaſonable chance of being broiled for

for his pains. The ſuppoſition is too ludicrous to admit a doubt of its falſehood; and this being as confidently aſſerted as any circumſtance, muſt make us doubt of the truth of all the reſt. Let us combine, then, the abſence of the Emperor from the capital when the fire began, his active orders before he left Antium, his unwillingneſs to leave it, the ſituation of the city on his arrival, and his behaviour after the conflagration, and ſee where we can find the leaſt probable trace of the tale of Suetonius.

The ſpot where the fire broke out affords another very ſtrong argument of want of deſign; *In prædiis Tigellini Æmilianis proruperat*, ſays Tacitus. He obſerves, indeed, that *plus infamiæ incendium habuit*, for that reaſon, that is, becauſe it was on the eſtate of Tigellinus; but where were theſe Prædia? in the diſtrict called the Æmiliana. Now this diſtrict was quite without the city, as any one will find upon conſulting the plan of ancient Rome. *Eorum ædificia qui habitant extra Portam Frumentariam, aut in Æmilianis*, ſays Varro, lib. iii. *De re Ruſtica.* What could have induced the Emperor, whoſe abilities do not ſeem to have been contemptible, to have adopted ſuch an extraordinary method of firing the city, by kindling the flame in its remoteſt ſuburbs? "He was accuſed," ſays Tacitus, "of having been "actuated with a deſire of founding a new city, and calling it by "his name." Did he do ſo? And what prevented him? The conſequence did not follow, and the imputed means were abſurdly diſproportionate to the motive.

THAT the fire in the Æmiliana was accidental will become more than probable, when we find that it was a quarter where dangerous and entenſive conflagrations had happened before. It appears from Suetonius, in his account of the reign of Claudius, chap. 18. that one had obſtinately raged in this region during the life of that Prince : *Ubi Æmiliana pertinacius arderent.* And it appears that it was of conſequence enough to call for the preſence and inceſſant labour of the Emperor himſelf and his whole court : We may reaſonably conjecture, therefore, that it was a part of the ſuburbs, for ſome reaſon or other, perhaps by being the ſite of hazardous manufactures, particularly expoſed and obnoxious to theſe calamities.

IT is true that Tacitus, in another place, ſays, with a ſeeming contradiction, *Initium in ea parte Circi ortum, quæ Palatino Cælioque Montibus contigua eſt* ; and Fleury, in his Eccleſiaſtical Hiſtory, founding the aſſertion on this paſſage, ſays it broke out in ſome ſhops about the Circus, without taking notice of the other alleged ſite of its commencement.

THE commentators on Tacitus have endeavoured to reconcile the difference, and inſiſt that it broke out in two places, the Circus and the Æmiliana. Now, as to the Circus, Tacitus himſelf accounts for its riſe and progreſs there, *Ubi per tabernas, quibus id Mercimonium inerat quo flamma alitur cæptus ignis.* The fire began in certain ſhops filled with inflammable materials, and naturally calculated to originate and diffuſe the flames. Where they could ſo eaſily be accounted for, who would have ſeen, reflected by their light,

light, the deadly visage of the tyrant, but those whose horrors of his crimes and terror of his wickedness raised on every occasion the imperial phantom before their alarmed imaginations. Let us not fear that by deducting this little burthen of guilt we shall leave too small a portion of infamy to satiate resentment and deter imitation. The bloody roll of Nero's crimes will scarcely appear diminished by expunging this inferior title to abhorrence.

It is an inferior circumstance, yet not entirely unworthy of note, that the rumours which had reached the ears of the two historians, as to Nero's conduct, essentially varied. To the one he had been represented as going openly and publicly to the summit of Mæcenas's tower to sing the fate of Troy, while to the other he was depicted as retiring into his private apartments *(in domesticam scenam)*, there secretly to enjoy the devastation of his groaning country. Uncertainty and contradiction are the sisters of unfounded report.

From the account given us of this event by Tacitus, we find that the Emperor's object, in at length leaving Antium to go to Rome, was to save his palace. Now in this he did not succeed. The palace was destroyed, and yet he is afterwards accused of constructing a new palace of wonderful magnificence, out of the ruins of his country *(Usus est patriæ ruinis*, says Tacitus), not without insinuation that such might have been partly the object of the antecedent devastation. There is nothing in his previous conduct to support the suspicion, for he was anxious to save his former residence, and to prevent the necessity of erecting a new one.

The anxiety of Nero to avoid the charge is utterly incompatible with the narration of Suetonius. *Incendit urbem tam palam,* ſays that hiſtorian, *Ut plerique Conſulares, Cubicularios ejus, cum ſtupa tædaque, in prædiis ſuis deprehenſos non attigerint.* Is it credible that he, who ſo much dreaded the imputation, ſhould have committed the fact without diſguiſe. That he uſed every exertion to avert the charge appears from Tacitus—by anxious and active care to expedite the rebuilding of the city—by princely largeſſes to the ſufferers—by ſupplications and atoning ſacrifices to the gods, he laboured to extricate himſelf from the infamy. It is true he was not ſucceſsful. Such was the odium againſt him. *Non ope humana, non largitionibus principis, aut deûm placamentis decedebat infamia.* He then endeavoured to throw the ſuſpicion on the Chriſtians, ſince he found the world too prejudiced to aſcribe the event to accident—with equal want of ſucceſs indeed. But all which I wiſh to infer is, that this extreme anxiety confutes the notion of his raſh unguarded promotion of the calamity; and that he was particularly diſtreſſed at this rumour appears from his known character, which was, in general, to deſpiſe all rumours, *Nihil patientius quam maledicta et convitia hominum tulit.*—Suetonius, p. 258.

The extent of the power of prejudice againſt this miſerable Prince at this period cannot be more ſtrongly exemplified than in the murmurs which Tacitus mentions, occaſioned by his opening the city and widening the ſtreets, becauſe, as was alleged, the old narrow ſtreets and lofty houſes contributed exceedingly

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to the salubrity of Rome, by protecting the passenger from the heat of the sun. I will even draw an argument from the virulence of Suetonius. "He would not suffer," says that writer, "the bodies of the dead, who perished in the fire, to be burnt "by their friends, nor the ruins of the edifices to be removed by "the owners, but took the charge upon himself, for the sake of "plunder." Whether those who were burnt already required to be burnt again I know not; but does not the ill-nature of the remark proclaim the inclination of the author? Is it not more natural to suppose, that the fear of pestilence, from the exposition of bodies left to the random care of individuals, in a time of general distraction, required the interposition of government and the adoption of public regulations, to prevent the possibility of private negligence? And was it not right in the governing power of the state to refuse to trust to the weakness or indolence of the subject, the office of removing rubbish and ruins, whose immense heaps forbad improvement and postponed renovation?

The truth is, when Suetonius wrote, invective against the race of Cæsar opened the way to honour and preferment. Abuse of the Augustan family was the fashion of succeeding times, and the instrument of flattery with succeeding Emperors. With infinite caution, therefore, are we to admit the adulatory invective of the writers of the age of Trajan. The fidelity of history was made to bow to the etiquette of courts and the interests of historians.

This propenſity to blacken the Cæſars, received, in the particular inſtance of Nero, additional height in later times from the enmity of the Chriſtians. His cruel perſecution of Chriſtianity, and his inordinate wickedneſs, in averting upon its votaries the calumny thrown upon himſelf, with the ſignal martyrdoms of St. Peter and St. Paul, under his dominion, have ſtamped him with the moſt ſanguinary dye in the annals of religion. It was natural to ſurmiſe that the man who ſo unjuſtly accuſed others, had not been unjuſtly accuſed himſelf. His innocence was ſuppoſed to include their crimination; and as the empire became Chriſtian, it became in a manner impious to doubt his guilt.

On whom does the authority of this legend reſt? As appears to me, on the authority of Suetonius alone. The careful peruſer of Tacitus will, I think, agree with me, that he did not believe the tale; he wrote before Suetonius, and poſſeſſed earlier and better channels of enquiry. Suetonius was ſecretary to Adrian, whoſe reign was preceded by the death of Tacitus *. The next author who mentions the charge with confidence is Dio. Caſſius, who lived in the reign of Alexander Severus, two hundred years after the event; no teſtimony can go beyond its firſt original; the tribe of ſervile copyers add not a jot of weight to the evidence.

Aurelius Victor, Eutropius, Marcus Aurelius Caſſiodorus and Jornandes, the only ſubſequent Latin writers who repeat the clamour,

* As is generally ſuppoſed.

clamour, merely echo the aſſertions of Suetonius and Dio. They could not be much better judges of the matter than we at this day, had they even taken the trouble to weigh the evidence. Aurelius Victor and Eutropius lived at a period three hundred years diſtant from the time of the conflagration, in the reigns of Julian and Valentinian; Caſſiodorus was conſul under Theodoric, and born in 476; and Jornandes, in Juſtinian's age, was ſecretary to a king of the Goths. As to the principal modern writers who aſſert and inſiſt on the fact, and particularly the eccleſiaſtical hiſtorians, Xiphilinus, Vitranius and Sulpicius, though they lived earlier than Fleury, who in the preſent century ſupports their opinion, their aſſertions can have no more weight than his, nor their knowledge of the facts be greater than ours. Xiphilinus was the profeſſed abridger of Dio. Caſſius. Dio. repeated from Suetonius, and upon the foundation of Suetonius's authority the whole fabric muſt ultimately depend. If any thing has been added, it has probably been the work of exuberant imagination, like that of Karholtus of Hamburgh, a modern eccleſiaſtical writer, who repreſents the Emperor at a banquet ſending forth troops of incendiaries, and ſitting to hear at intervals the triumphant tale of their horrid exploits, a picture of which he could not have found the leaſt trait in any ancient hiſtorian. It remains only to obſerve, that Suetonius, the father of this tale, could not have been unwittingly deceived into this aſſertion.

Thus have I endeavoured to ſcrutinize, in this inſtance, the accuracy and authenticity of Suetonius, which may be a clue to his general character as a writer, the only object perhaps which could

could have juſtified my calling the attention of this revered aſſembly to a queſtion ſo remote, and ſeemingly ſo unintereſting. Always, as I have ſaid, has that hiſtorian appeared to me to be over-rated; the indecency of his deſcriptions has been often condemned, and it was well obſerved, that Suetonius wrote the lives of the Emperors with the ſame licentiouſneſs with which they lived. Were I to compare Suetonius with any writer of our own time, in point of credit due to his narration, I would ſcarcely aſſign him a place ſuperior to Smollet's; I mean not with reſpect to compoſition, but as to authenticity and materials. Both of them ſeem to have compiled from the *actus diurni*, or newſpapers of the day, and to merit equal authority with thoſe crude and haſty chronicles. If the one has lived for eighteen centuries, while the other poſſibly may not for one, it has perhaps been owing to the charms of his compoſition, not to the dignity of his hiſtory.

If theſe remarks ſhall in any degree tend to aſcertain the rank of this famed hiſtorian in the ſcale of hiſtory, or rather by calling the attention of more accurate obſervers to the general complection of his works, to induce them to aſcertain it, they will have an importance which at this remote time they could not borrow from the ſubject itſelf. They may perhaps alſo derive ſome additional claim to attention, from the circumſtance of a celebrated attack having been lately made by Mr. Whitaker of Mancheſter, on the authenticity of his rival hiſtorian, in a Compariſon between Tacitus and Gibbon.

An ESSAY *on the* ORIGIN *and* NATURE *of our* IDEA *of the* SUBLIME. *By the Reverend* GEORGE MILLER, *F.T.C.D and M.R.I.A.*

——— αγαθη (γαρ κατα τον Ησιοδον) ερις ηδε βροτοισι.
Και τω οντι καλος ουτος, και αξιονικοτατος ευκλειας αγων τε
και στεφανος, εν ω και το ηττασθαι των προγενεστερων ουκ αδοξον.

LONGINUS.

THE various opinions which have been entertained concerning the origin and nature of our idea of the ſublime afford a ſtrong proof of the difficulty of penetrating into our own minds. We are not only urged to the inquiry by that ſcientific curioſity which prompts us to analyze our modes of thinking, but elegance of taſte conſpires to engage us in a reſearch which has for its object all that is great or elevated, and yet the origin and nature of the ſublime are ſtill ſubjects of controverſy. According to Longinus, the ſublime conſiſts in a proud elevation of mind; according to the ingenious author of the Philoſophical Inquiry into the Origin of our Ideas of the Sublime and Beautiful, it conſiſts in terror; Doctor Prieſtly places it in an awful ſtillneſs; and Lord Kaims derives it from the magnitude or elevation of viſible objects, (Read July 13, 1793.)

objects, and from whatever caufes an agreeable emotion refembling thofe which are excited by great or elevated objects of fight. Doctor Blair profeffes himfelf inclined to think that mighty force or power, whether accompanied with terror or not, whether employed in protecting or alarming us, has a better title than any thing that had yet been mentioned to be the fundamental quality of the fublime, but does not infift upon it as fufficient to found a general theory. This controverfy about the principle of the fublime has naturally extended to its application, and we fee the fame paffages applauded for this quality by fome critics and rejected as deftitute of it by others. Longinus quotes as fublime the Ode of Sappho, which Lord Kaims, whilft he admits it to be beautiful, excludes from the clafs of fublimity. The celebrated defcription of the creation of light, which has been produced by the great critic of antiquity as an illuftrious inftance of the fublime, has not had a better fate. A French critic has difputed his judgment, and Lord Kaims thinks that the opinion of the latter may be defended as the more folid, though he acknowledges that the mind is affected by it with a momentary emotion of fublimity. The principles of tafte are indeed fixed in our nature, and whatever corresponds to them muft pleafe, though we fhould not be able to affign its proper clafs. However, to difcriminate thofe claffes from each other, whilft it furnifhes an elegant amufement, appears to conduce to a refinement of our apprehenfion of their refpective qualities.

Perhaps it would not be difficult to account for the variety of fentiment on this fubject. Some peculiar bias may poffibly be

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diſcovered to have operated on each writer, and have cauſed him to deviate from the opinions of the reſt. The love of ſyſtem appears to have narrowed the view of the philoſophical enquirer. He wiſhed to ſimplify the principle of ſublimity, and perceiving that ſome terrible objects were highly ſublime, he concluded that terror was that principle. The ſame diſpoſition ſeems to have operated, though in a different manner, on Lord Kaims. Anxious to unite in one elegant ſyſtem the principles of taſte and morality, and to diſcover in every part of our nature a new ſource of happineſs, he was led to pronounce that no diſagreeable paſſion could produce the ſublime; and to diſtinguiſh it from beauty, the other ſpecies of agreeableneſs, only by a ſingle circumſtance, that of greatneſs. Doctor Prieſtley, with the cool obſervation of a metaphyſician, conſiders merely the degree of exerciſe which great objects give to our faculties: and Longinus, in the ardour of literary compoſition, attending only to the tranſport which a writer experiences when a noble idea preſents itſelf to his view, has deſcribed the ſublime by ſaying, "that the mind is elevated "by it, and ſo ſenſibly affected as to ſwell in tranſport and "inward pride, as if what is only heard or read were its own "invention." A deſire of reconciling the various ſyſtems of former writers, ſuggeſted to Doctor Blair the conjecture, that power was the fundamental quality, but he himſelf doubted of its ſufficiency. Theſe opinions I propoſe to conſider, and perhaps the reſult may be a ſyſtem which, though leſs ſimple, ſhall have the advantage, to uſe the language of philoſophy, of explaining all the phænomena.

 DOCTOR

Doctor Blair has obſerved that Longinus has frequently uſed the word ſublime to ſignify any remarkable and diſtinguiſhing excellence of compoſition. In his celebrated treatiſe, accordingly, we find that he has quoted paſſages of great beauty and ſome of true ſublimity, but it gives us no aſſiſtance in the diſcovery of diſcriminating principles. This inaccuracy appears to have ariſen, as I have already mentioned, from the motive which induced the author to compoſe it. He tells his friend Poſthumius that the treatiſe of a preceding writer was deficient with regard to that which appeared to him the more important part of a treatiſe on the ſubject of any art, the method by which ſkill in that art might be acquired. This deficiency he undertakes to ſupply, and propoſing to give practical precepts of compoſition, he enters into the feelings of a writer. In this view he ſees the mind animated by the conſciouſneſs of vivid conceptions, and not conſidering that conceptions of very different kinds might give the mind occaſion to triumph in the conſciouſneſs of its own powers, he defines the ſublime by its analogy to that flattering ſenſation. But though we cannot learn from Longinus the nature of the ſublime, as diſtinguiſhed from other ſpecies of compoſition, it would not be difficult to illuſtrate it by examples from his writings.

The author of the philoſophical enquiry has not been thus deficient in preciſion. According to him terror is the ruling principle of the ſublime. That terror is in many caſes a conſtituent principle I am not diſpoſed to deny; but I conceive that there is not any claſs of ſublime objects which may not ſuggeſt the emotion of grandeur independently of terror, and that there

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is one clafs whofe grandeur, in fome cafes, confifts in its abfence. In the noble defcription of a firm and intrepid mind, which the patriotifm of Horace has given us, every circumftance of terror is indeed introduced, but only for the purpofe of more confpicuoufly difplaying unfhaken magnanimity. To fay that terror is the principle of the fublimity of this defcription which exhibits to us a mind unmoved by the menaces of a mob or a tyrant, by the violence of natural caufes and even by the power of the Divinity, would, in my opinion, be to ftrain in fupport of an hypothefis. Conceive thofe circumftances of terror to have their effect, and the capital object in the picture, the moral fublimity of a great mind, is annihilated. Befides fuch inftances, in which the abfence of terror appears to conftitute the fublime, there are others which have no apparent connection with terror, as the view of a fpacious plain or of the ftarry heaven; and in many painful and terrible objects, as Doctor Blair has obferved, there is no fort of grandeur.

The emotion of terror, which this author confiders as the ruling principle of the fublime, is, by Doctor Prieftley, wholly excluded. "The pure fublime," according to him, "tends to fix the attention and to keep the mind in a kind of *awful ftillnefs*, whereas it is the nature of every fpecies of the pathetic to throw it into an *agitation*." In this he appears to me to have committed an error fimilar to that of the writer whom I have laft confidered, by extending to the fublime in general that difpofition of mind with which fome fublime objects are contemplated. I do not difpute the fublimity which he attributes to Young's defcription of night, but I think that cafes might be mentioned

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in which the mind has a ſtrong perception of ſublimity, whoſe direct operation is to agitate in a conſiderable degree the affections. In proof of this obſervation I will venture to mention one of his own inſtances of the ſublime. When Ajax (for Doctor Prieſtley has by miſtake attributed the prayer to Diomede) prays that Jupiter would give him day and then deſtroy him, the object preſented to the mind does not ſeem fitted to ſink it into an *awful ſtillneſs*; on the contrary, it is animated by a ſympathetic heroiſm. I will mention another inſtance of moral ſublimity, which may perhaps more fully illuſtrate the obſervation. When Mr. Burke, in the glowing colours of his eloquence, paints to us the *circumnavigation of charity*, when he contraſts the ordinary purſuits of travellers with the conduct of him who deſcended into dungeons *to take the ſcale of human miſery*; I do not think that the effect of this ſublime image of active benevolence is an *awful ſtillneſs*. The better principles of our nature triumph in the view, and we balance our ſorrow for the follies and vices of mankind by our exulting admiration of the philanthropy of a Howard. Doctor Prieſtley has, in confirmation of his opinion, obſerved that "deep and ſlow notes in muſic bear a nearer relation to "the ſublime than ſhrill and quick ſounds." Perhaps the moſt ſublime effect which muſic is capable of producing, is produced by ſlow and ſolemn notes; but I think it cannot be denied that there is much grandeur and elevation in the tumultuary parts of the choruſes of Handel, and ſome of my friends of muſical ſkill, whom I have conſulted, agree that the effect of what is conſidered as ſublime in it is not in almoſt any caſe an *awful ſtillneſs*.

LORD

Lord Kaims appears to me to have adopted the true principle of inveſtigation, though, as I have already obſerved, a love of ſyſtem prevented him from tracing all its conſequences. He defines figurative ſublimity by the reſemblance of the emotion which it excites, to that which is cauſed by the grandeur or elevation of viſible objects. In this he appears to have followed nature, for he is ſupported by the analogy of language; and had he conſidered the different emotions which great and elevated viſible objects occaſion in different circumſtances, this eſſay ſhould not have been written; but he has attended only to the chearful emotion of ſublimity. A huge impending rock Lord Kaims muſt have admitted to be poſſeſſed both of grandeur and elevation, and yet I apprehend that the view of ſuch an object derives much of its effect from its influence in ſinking the mind of the ſpectator. A gothic church is mentioned by him as an inſtance of the ſublime amongſt the works of art, and ſurely the gloomy depreſſion which is occaſioned by its *darkneſs viſible* is not a diminution of its grandeur. Had Lord Kaims conſidered this difference amongſt the emotions which ſublime viſible objects excite, he would not I think have given up to Huet the judgment of Longinus. The mind does indeed "ſink down "into humility and veneration for a being ſo far exalted above "groveling mortals," but the ſublime object preſented in this magnificent deſcription of creation is not the human mind. The all-powerful Creator is the object, and the mind of a Newton cannot contemplate him without humiliation.

It remains that I ſhould make ſome remarks on the conjecture of Doctor Blair. He tells us that after the review which he he had taken, there did not occur to him any ſublime object, into the idea of which power, ſtrength and force either enter not directly or are not at leaſt intimately aſſociated with the idea, by leading our thoughts to ſome aſtoniſhing power, as concerned in the production of the object. The reflecting mind of Doctor Blair may be led by the conſideration of each of the objects which he has mentioned to the contemplation of power, but I apprehend that in ſome of thoſe caſes the ſublimity of the object may be perceived without any ſuch reflection. The view of an extended plain may expand with admiration the mind of him who does not think of the power that formed it; and endleſs numbers, which Doctor Blair conſiders as filling the mind with great ideas, do not neceſſarily lead us to a metaphyſical view of the powers of the underſtanding by which the modes of number are combined. With regard to the moral or ſentimental ſublime, Doctor Blair would ſay, that we are affected by the *energy* of character which we obſerve in our fellow-creatures; but I am inclined to think that the ordinary conſideration of ſuch examples of mental ſuperiority reaches no farther than a moral approbation of what is eſteemed worthy of the dignity of our nature.

Such are the accounts which have hitherto been given of the origin and nature of the ſublime. That of Longinus gives us no aſſiſtance, but each of the others, though imperfect, appears to be founded in nature. I agree ſo far with the author of the philoſophical enquiry as to think that in ſome caſes terror may heighten our perception of ſublimity. I ſo far agree with Doctor

Prieſtley

Prieftley as to admit that in fome cafes the effect of the fublime is to keep the mind in a kind of *awful ftillnefs*, and that extreme agitation is inconfiftent with it. With Lord Kaims, I think that the true method of inveftigating its principle is to trace the analogy between the effects of vifible and mental objects; and with Doctor Blair, I am of opinion that mighty force or power is frequently a caufe of the fublime, though in fome cafes the confideration of it appears to be rather a philofophical inference than a part of the fenfation. If it be true that thefe feveral fyftems have a foundation in nature, and the examples adduced by their refpective authors appear fufficiently to warrant the opinion, a confiftent fcheme which fhould reconcile them with each other would have fome pretenfion to be confidered as giving a true account of the fublime.

I HAVE already obferved that Lord Kaims appears to have adopted the true method of inveftigating the principle of the fublime. In every language the name of that emotion, by whatfoever object it may have been excited, has been derived from the magnitude or elevation of vifible objects. His view of vifible fublimity appears however to have been confined. He defcribes the emotion excited by it as extremely pleafant, though diftinguifhed from that occafioned by beauty in being rather ferious than gay, and confiders the qualities that contribute to beauty as effential to it. It is, according to his idea, beauty on a larger fcale. From this idea he has however departed in the example of figurative fublimity, which he has taken from Offian. In the conflict of Lochlin and Innisfail, in *the troubled ocean* and *the thundering heaven*, to whofe noife that of the battle is compared, we fhall in vain look for order, regularity or proportion. There is much fublimity in the defcription, but no beauty. Let us confider great or elevated vifible objects as exciting different emotions, as raifing or depreffing the mind; let us combine the ftormy grandeur of the fky and ocean with the regular magnificence which in framing his fyftem Lord Kaims appears to have exclufively confidered, and I imagine that we fhall have a bafis fufficiently broad for the ftructure of figurative fublimity.

THERE appear to me to be three claffes of fublime objects—external fenfible objects, thofe that excite the emotion which Doctor Blair has called the moral or fentimental fublime, and fuperior beings. I have called the firft clafs that of external fenfible rather than that of vifible objects, that I might include within it the fublime of found. " The burft of thunder or of cannon, " the roaring of winds, the fhouting of multitudes, the found of " vaft cataracts of water," are all, as Doctor Blair has obferved, " inconteftibly grand objects;" and they appear to me to excite emotions fimilar to thofe with which we are affected by the magnitude or elevation of objects of fight. The latter have been already in fome meafure confidered. It has I think appeared that the emotions excited by them are not all of the fame kind. The ftarry firmament, and the tempeftuous fky illuminated only by the blaze of lightning, are both fublime, but furely the emotions with which they are beheld are different. The pious

admiration

admiration of the Great Author of the Univerfe with which the view of the former infpires us, is not the difpofition with which we behold the *thought-executing fires* of the latter. In the awful fublime of nature terror then may have place; but that terror, if very great, will be deftructive of the fublime, by withdrawing our attention from the object. An example will beft illuftrate this variety of our emotions. The unbounded view of a calm fea will fill the mind with the pleafing emotion of the fublime. If the fea be agitated by a violent ftorm, whilft the fpectator is fecurely placed on a promontory, the emotion of fublimity will, I think, be increafed by the idea of irrefiftible force which its agitation will fuggeft; but as that force is exhibited to us in circumftances of danger to thofe who fhould be expofed to it, the emotion will now become of the more awful kind. If the fpectator behold a fhip in thofe circumftances of danger, his terror will become much more lively, but his fympathy with the unfortunate fufferers will no longer permit him to contemplate the wild magnificence of the ocean. If he is himfelf in danger, his attention is ftill more effectually withdrawn from it and directed to one fingle object, the means of efcaping. It appears then that the fublime of nature may be heightened by terror, fo far as that terror does not prevent us from attending to the whole of the great object which infpires it. This does not confine within fuch narrow limits the fublime of defcription as that of nature. The affections are principles defigned for action, and mere defcription will not fo eafily excite them to a degree inconfiftent with that felf-poffeffion which is requifite to the perception

ception of the ſublime. As the emotions excited by the ſublimity of viſible objects are of different kinds, the qualities which excite them muſt be different. Greatneſs or elevation are the general characteriſtics, which, to produce emotions of the chearful kind, muſt be accompanied by ſome degree of thoſe qualities which conſtitute beauty, as regularity, proportion, order and colour. To excite ſenſations of awful apprehenſion muſt be attended by circumſtances which indicate mighty power, or which tend to alarm the mind, as darkneſs, ſolitude and ſilence. The regularity which is required in the former caſe muſt however not be very exact. "In things which are ſtrictly regular," Doctor Blair has obſerved, "we feel ourſelves confined, and there is no room "for the mind's exerting any great effort."

To this claſs of ſublime objects I muſt annex the ideas of number and duration, though not objects of ſenſe. They evidently excite emotions ſimilar to that produced by the contemplation of wide-extended ſpace. Belonging to all kinds of beings, and yet containing in them nothing of intelligence, they appear to be moſt properly claſſed with the inanimate objects of nature. Perhaps the whole might be included in the general deſcription of inanimate or unintelligent ſublime objects.

The ſecond claſs has been defined by Doctor Blair, as "ariſing "from certain exertions of the human mind, from certain af- "fections and actions of our fellow-creatures." "Theſe," he has obſerved, "produce an effect extremely ſimilar to what is pro-

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" duced by the view of grand objects in nature, filling the mind " with admiration and elevating it above itself." From this class terror appears to me to be excluded. The affections and actions which it comprizes are not those of a mind alarmed by apprehension, nor are they fitted to excite sentiments of fear. An heroic disregard of danger, a cool and firm presence of mind in difficult and embarrassing circumstances, a disinterested and expanded benevolence, with a strong sense of every generous feeling, and a principle of virtue superior to the opinions of weak and corrupt men, and to the inordinate propensities of our nature, are the moral qualities which form the sublime of human character. To these perhaps should be added those qualities which are considered as belonging to the imagination or understanding. Shakespeare's description of poetic fancy will, I think, justify its admission, and the character of sublimity will scarcely be denied to the intellectual powers of Newton. When I say that terror is excluded from this class, I would be understood to speak only with reference to a manly mind. Habitual servility may possibly efface the recollection of the common nature of the species, and cause a man to look up to his fellow-man with sentiments of awful submission; but he who possesses a manly mind will separate the consideration of the individual from that of his station, and whilst he will shew for the latter that deference which the well-being of society requires, he will feel for the former no other sentiments of respect than those which are due to qualities which exalt and adorn the character of man. Sublime objects of this class inspire us with more elevated emotions

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than thofe of inanimate nature. In the Ode of Horace, to which I have already alluded, we fee every circumftance of political and natural terror, and even the wrath of fuch a Deity as Paganifm could form, introduced merely as fubordinate to the difplay of the firm intrepidity of the juft man. We are not fo much affected by the great image of a broken world as by that of him who receives the fhock undaunted.

The laft and higheft clafs of fublime objects comprehends fuperior beings, and more efpecially the Supreme Being. This, like the firft, includes objects which excite emotions of different kinds. Superior beings may excite in us emotions of fublimity, either by circumftances of terror, or by a difplay of unwearied goodnefs employed in the protection of mortal weaknefs. As an example of the fublime of this kind, divefted of all terror, I will refer to the morning hymn of our firft anceftors, in which with *holy rapture* they addrefs the *Parent of Good*, and call on all nature to join in his praife. Doctor Blair has quoted from the Prophet Habakkuk a defcription of the appearance of God, heightened by every circumftance of terror: " He ftood and meafured the " earth; he beheld and drove afunder the nations; the everlaft- " ing mountains were fcattered; the perpetual hills did bow; " his ways are everlafting. The mountains faw thee and they " trembled; the overflowing of the water paffed by; the deep " uttered his voice and lifted up his hands on high." The fame qualities, which have been already mentioned in treating of the other claffes, muft furnifh us with our beft conceptions of fuperior beings.

beings. They can be exhibited to us only by a difplay of moral and intellectual perfection, or by the gracious or terrible effects of their power.

I HAVE now confidered the different fyftems which have been propofed with regard to the emotion excited by fublime objects, and having pointed out in what refpect each was imperfect, have followed to its whole extent the method propofed by Lord Kaims, and have, I hope, fupplied the deficiency of thofe fyftems. The general error appears to me to have been a fuppofition of a fimplicity in the emotion. The emotion is indeed always of a grave kind, but with fome variation. I have endeavoured to fhew that, though the fublime of human character excites only an admiration for the great or good qualities of our nature, yet the fublimity of vifible objects, as well as that of fuperior beings, may be perceived fometimes with an awful apprehenfion occafioned by circumftances of terror, fometimes with a chearful expanfion of the mind filled by the union of beauty and greatnefs. If this reafoning be admitted, it will enable us to determine in what refpects the pathetic is connected with the fublime, and to form a judgment concerning the nature of the merit of thofe admired paffages whofe pretenfions to the clafs of fublimity have been difputed.

WITH regard to the connection of the pathethic and the fublime, two different inquiries may be propofed; the one of which is, whether the effects produced by them are of the fame kind;

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the other, whether a reprefentation of paffion can form a fublime object.

In anfwer to the former I would obferve, that the fublime operates directly on the affections, and therefore muft be confidered as producing effects of the fame kind with thofe which are produced by the pathetic. Admiration and terror are the effects of the fublime. Doctor Prieftley does indeed profefs to exclude from them every kind of *agitation*; but I fuppofe this term not to be ufed in its ftricteft application, for he has derived the fublimity which he attributes to the ideas of wealth, honour and power from thofe circumftances which enable them to fill and *charm* the foul. It has indeed been cuftomary to fpeak of the emotion of fublimity, and Lord Kaims has diftinguifhed emotion from paffion: "An internal emotion or agitation of the "mind," he fays, "when it paffeth away without defire, is deno-"minated an emotion; when defire follows, the motion or agi-"tation is denominated a paffion." This diftinction may be of importance to a moralift, but cannot be of any in the prefent enquiry. Emotion and paffion, according to the examples by which Lord Kaims illuftrates his diftinction, differ only in degree, and the prefent queftion is about the kind and not about the degree of the impreffion made by a fublime object; but this diftinction may be admitted, and yet the fublime and the pathetic be confidered as producing effects of the fame kind. The emotion excited by the fublime of eloquence is frequently of an active kind. The celebrated oath of Demofthenes, by which he deified thofe ancient patriots who had fallen in the plain of

Marathon probably contributed not a little to his acquittal. We may, I think, venture to ſay that this image of heroic fortitude had no ſmall ſhare in cauſing that decree by which the Athenians refuſed to condemn the friend of Grecian liberty for the unfortunate iſſue of his counſels. The emotion of ſublimity muſt then be allowed ſometimes to be attended by deſire operating on the will, and there cannot be any reaſon for conſidering that emotion in any caſe as entirely diſtinct from the effect of the pathetic, which will not equally affect what is acknowledged to belong to the latter. The emotion produced by the pathos of dramatic diſtreſs is not often ſo ſtrong as to inſpire an active wiſh of relieving the unfortunate hero or heroine; and all agree in ſaying, that tragedies repreſenting thoſe ſufferings with which we ſo inactively ſympathize are yet pathetic. The emotions then both of the ſublime and the pathetic are ſometimes of an active and ſometimes of an inactive kind: There can be no reaſon for diſtinguiſhing them from each other in this reſpect; and if the ſublime be conſidered as deeply intereſting us, and therefore in a greater or leſs degree agitating the mind, it cannot be improper to regard it as a part of that general claſs which includes all the cauſes of agitation. The diviſion of the claſſes of ſublime objects will enable us to determine which are the emotions attendant on the perception of ſublimity. The deſcription or conception of ſuperior beings may, as in the examples already given, be attended by reverential love and gratitude, or by terror. The ſublime of human character produces emotions of love and reſpect by a diſplay of all the nobler qualities of our nature;

and admiration or terror is the impreſſion made by natural ſublimity.

The other queſtion concerning the repreſentation of paſſion we ſhall alſo be enabled to determine by the principles already ſtated. The claſs of moral ſublimity has been deſcribed as comprehending whatever is conſidered as ennobling the human character, the ſuperior energy of intellectual or moral qualities. The agitation of paſſion may indeed give occaſion to the exertion of that mental vigour which ſtruggles to ſubdue it, or to the diſplay of that elevation of mind to which it ſuggeſts ſplendid and glowing images of great objects, but it does not appear that the tumult of paſſion is itſelf the object of our admiration. Doctor Stack, in an eſſay publiſhed in the Tranſactions of the Academy, has obſerved, that ſome of thoſe paſſages which exhibit the agitations of the reſolute character of Othello may be called ſublime, and I agree with him in the obſervation, whilſt I differ from him in the principle; they are ſublime, not becauſe they are paſſionate, but becauſe they evince the habitual heroiſm of Othello. When he wiſhes to brave the utmoſt violence of ſtorms, *if after every tempeſt come ſuch calms*, the ſublimity of his character does not conſiſt in the warmth of his love, which might be felt as much by a feebler nature, but in the magnanimity, which proves his attachment by the dangers for which he would conſider ſuch a meeting as an adequate reward. In the ſame manner, his *farewell* to thoſe great objects which had once employed his thoughts is indeed ſublime, becauſe it exhibits to us a great mind, even when ſinking under the attacks of paſſion,

paſſion, ſtill recurring to the magnificent circumſtances of its former purſuits. In the ſpeech in which he deliberates about the admiſſion of Emilia, there is much paſſion, and yet little ſublimity. The perplexed agitation of his mind, in the former part, gives me no ſenſation which deſerves that name; but, in the latter part, the greatneſs of his mind makes him think that all nature ſhould ſympathize with the horror of the ſcene. It appears then, I think, that the grandeur of thoſe paſſages conſiſts in the general elevation of Othello's mind, and not in the violence of the paſſion under which he is repreſented as labouring. The emotions of love and jealouſy are not more ſtrongly drawn by Shakeſpeare than by Sappho, but in the character of Othello they are repreſented as operating on a generous and heroic mind; and though we deny the praiſe of ſublimity to unreſiſting weakneſs, however violently agitated, we view with admiration the ſtruggles of magnanimity. That it is not the paſſion, but the magnanimity which ſtruggles againſt it, that gives us the idea of ſublimity, may perhaps receive a further confirmation, if we conſider the ſoliloquy of Othello when he is preparing for the murder of his wife. The generoſity of his mind had been ſubdued, and he no longer endeavours to repreſs the attacks of paſſion: love and jealouſy have entire dominion over him; and, as Lord Kaims has obſerved, every thing is done to reconcile the two oppoſite paſſions; he is reſolved to put her to death, but he will not ſhed her blood, nor ſo much as ruffle her ſkin. Nothing can be more natural or more pathetic, but ſurely this is not ſublime. The concluſions which I would infer from theſe obſervations are, that where the character is too feeble to make any ſtruggle, as in the Ode of Sappho, or where the ſtruggle of a generous mind has ceaſed, and the conflict of paſſion alone remains, as in the ſoliloquy of Othello, the expreſſion of the paſſions does not produce the emotion of the ſublime, and that it only contributes indirectly to this effect by exciting a diſplay of mental vigour.

Lord Kaims, conformably to his notion of the ſublime, has obſerved that no diſagreeable paſſion can produce it, and has propoſed the ſoliloquy of Antony wailing over the body of Cæſar, as a teſt by which it ſhould be determined with regard to the paſſion of revenge. Doctor Stack, on the other hand, has declared that he eſteems moſt parts of this paſſage truly ſublime. In this, as in the inſtances already mentioned, I agree with Doctor Stack in opinion, that the paſſage is ſublime, but its ſublimity conſiſts in diſplaying the generous elevation of the mind of Antony. It may perhaps be thought that elevation of mind is without reaſon aſcribed to him whoſe thirſt of power induced him artfully to ſtimulate the people againſt the conſpirators, and afterwards to ſacrifice his uncle to the reſentment of Octavius, whoſe cruel vengeance prompted him to exult over the bloody head and hands of Cicero, and whoſe ſenſuality *beguiled him to the very heart of loſs*; but the character of Antony was not uniform. Brave and generous by nature, but corrupted by his early education, *his faults in him ſeemed as the ſpots of heaven, more fiery by night's blackneſs*. When his paſſions, which habitual indulgence had rendered ungovernable, did not *o'er his ſpirit* exert their *full ſupremacy*, he was noble and humane. The pure patriotiſm of Brutus received from him its deſerved eulogium,

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and his generofity overpowered the mind of Enobarbus, who had deferted him in his laft diftrefs. *His taints and honours waged equal with him.* Such is the character of Antony, as Shakefpeare has taken it from the impartial account of Plutarch, and not from the exafperated eloquence of Cicero. In this foliloquy he pours forth the genuine fentiments of his heart. His affectionate attachment to Cæfar fuggefts to him an animated and ftrong conception of the calamities which fhould overwhelm his country; of that *domeftic fury and fierce civil ftrife* which fhould *cumber all the parts of Italy.* In the latter part of the fpeech there is indeed prefented to us a direct picture of revenge. It muft however be obferved that *Cæfar's fpirit, with Ate by his fide, come hot from hell,* does not exhibit to us human paffions. Thus reprefented we regard him as a fuperior being; and though the vengeance of a mortal could not give us an elevated idea of his character, we may bow with reverential awe before the terrors of a deftroying Angel. At the fame time I will alfo admit, with Doctor Stack, that amongft thofe *with whom revenge is virtue,* it is a direct object of fublime conception. Junius tells us that an infult *lowers the mind in its own opinion, and forces it to recover its level by revenge.* To thofe who think with the acrimony of that elegant writer, the fpirit of vengeance is an exaltation of the human character, and therefore, without any variation in the principle, muft, to their vitiated minds, give impreffions of moral fublimity.

THIS effay, on the Origin and Nature of our Idea of the Sublime, has been reduced, as nearly as poffible, to the ftrictnefs of philofophical reafoning. The opinions of different writers have been

been examined, and their inſufficiency pointed out by examples, which may be conſidered as what philoſophers call *experimenta crucis* ; experiments of that deciſive kind whoſe reſult not only correſponds to the cauſe aſſigned, but proves that ſome other cauſe before aſſigned is not adequate to the explication of the effect. Each of theſe opinions however, though ſingly inſufficient, appears, from the inſtances alleged by its author, to have been founded in nature, and therefore, by a kind of *induction*, they have been collected into one ſyſtem, which has, in the laſt place, been applied to the ſolution of more doubtful *phænomena* of taſte. Scientific demonſtration cannot be applied, but advantage may ariſe from the regularity of ſcientific method.

ESSAY *on the following Subject, proposed by the* ACADEMY, *viz.* "*On* STYLE *in* WRITING, *considered with respect to* "*Thoughts and Sentiments as well as Words, and indicating the* "*Writer's peculiar and characteristic Disposition, Habits and Powers* "*of Mind.*" *By the Rev.* ROBERT BURROWES, *D. D.* *F. T. C. D. and Secretary to the Royal Irish Academy.*

DOCTOR BLAIR says the best definition he can give of style is "the peculiar manner in which a man expresses his "conceptions by means of language." This definition however he saw would leave style merely verbal, and therefore he proceeds to amend it by observing "that it is different from "mere language or words—that it has always reference to an "author's manner of thinking—and that to separate the style "from the sentiment is extremely difficult. No wonder," says he, "that these two should be so intimately connected, as "the style is nothing else than that sort of expression which our thoughts most readily assume." Hence he remarks that different countries have been noted for peculiarities of style suited to their different temper and genius; a remark which he afterwards

Read May 11, 1793.

wards on ſome occaſions applies to individuals. But in what manner this variety in thinking produces its effects on the clothing of the thought, and what are the peculiarities of ſtyle which are ſuited thus to the ſeveral diverſities in temper and genius—theſe are points into which, though directly connected with his explication of ſtyle, he has not ſyſtematically enquired: much leſs has he gone into an examination of thoſe diſpoſitions and habits which give to individuals their peculiar caſt of thought, and account for the different mode in which different authors treat the ſame ſubject. In ſhort he has omitted the conſideration of that quality which, from its obvious analogy to the difference of ſtyle in language, the words of the queſtion propoſed by the Academy have properly termed Style in thought. This view of the ſubject being peculiarly intereſting, and in a great meaſure new, the deſign of the following pages is to point out its importance, and to give ſome ſlight ſpecimens of its utility: the author with great deference ſubmits to the Academy what may perhaps ſerve to furniſh ſome hints as to the mode in which it may be advantageouſly treated of by ſuch as have more leiſure and ſuperior talents to purſue the inveſtigation.

Those who have written on Style have uſually conſidered it as taking its character from the varieties of the ſubject, and the ſpecies of compoſition in which it was employed. Thus the diſtinct ſtyles of hiſtory, of oratory and philoſophy, of epic, lyric and dramatic poetry, have been diffuſively treated of by numerous critics of the antient and modern world. But an an author's peculiar habits of thinking predominate over the general laws of critics. There is no ſtyle ſo directly appropriate to any one ſpecies of writing as to exclude the operation of the various habits and diſpoſitions of different writers, while the writings of the ſame author, though in different ſpecies of compoſition, have a certain degree of ſimilarity in their ſtyle which at once points him out to the intelligent reader. The Hiſtory of Livy is very different from that of Tacitus, and the ſtyle of Virgil's epic poetry very unlike to that of Homer: while Cicero appears the ſame in his letters, his orations and his philoſophy; and Doctor Johnſon never fails to diſcover himſelf in his debates, his biography and his morals, in his compoſitions and his converſation.

THAT ſuch peculiarities in ſtyle of thought ſhould be found amongſt authors is not at all ſurprizing; for what is there in which men are alike? Their geſture, their voice, their gait, their hand-writing, their countenance, are all peculiar and appropriate to each individual; why then ſhall we ſuppoſe that their minds are not various? Various habits of thinking and diſpoſitions of mind do in fact preſent themſelves to us at every moment and in every ſituation. The different impreſſions which the ſame object makes on different individuals, the different reception which the ſame compoſition meets with from different readers, the different teſtimonies given of the ſame fact by different witneſſes poſſeſſing equal opportunities of obſervation are all ſo many evident proofs of this. In thoſe works which are peculiarly the works of minds inventing, combining, and arranging, theſe characteriſtic varieties are more conſpicuous, and thoſe who have made ſuch works their ſtudy ſeldom fail to appropriate them to their reſpective authors. The ſkilful muſician can readily diſcover the compoſer by his ſtyle, or the performer by his manner; and the connoiſſeur in painting can readily diſtinguiſh the pictures of one ſchool from thoſe of another, and even diſcern the hand of each maſter in pictures of the ſame age, and country, and ſubject. Literary works may be found to exhibit equal or greater variety, proceeding from the different habits of thinking in their reſpective authors. In the works of writers whoſe modes of life were very different, and characters oppoſite in the extreme, theſe varieties are obvious to the leaſt obſervant reader, and a more accurate acquaintance with ſtyle and knowledge of character will enable the more judicious critic to diſcover diſtinguiſhing marks in the writings even of authors who lived much together, and applied to the ſame ſorts of compoſition. There is no man who will not perceive the different minds of Mr. Sterne and Doctor Johnſon in a ſingle page of their works; and there is no reader poſſeſſing any claim to acuteneſs or critical ſagacity who will not in the papers of the Spectator find internal evidence ſufficient to diſcriminate the eſſays of Mr. Addiſon from thoſe of Sir Richard Steele.

Corporeal diverſities have a manifeſt and important uſe: they are marks which ſerve to the purpoſes of diſtinguiſhing each individual from every other, and thus prevent infinite confuſion

confusion and mistake. Different habits of thinking in like manner distinguish different authors from each other, prevent the possibility of issuing literary forgeries, or by borrowed names gaining credit with the world. The dignity attached to the profession of an author will not suffer him to travel incognito. Varieties in the dispositions of mind give to society all its charms, and recommend its duties. They ensure an attentive reception to the stranger who stands in need of it; for they introduce him to us as a new character, and they send us from the flattery and the indolence of domestic endearment to more extended benevolence, and an active intercourse with a chequered world, where the varieties of disposition relieve varieties of want, and receive reciprocal gratifications. From these varieties, as peculiarly shewn in literary works, some important advantages will be found to arise. There is no dull uniformity to disgust and fatigue him who wishes to acquire extensive and various information: every thing worth being diffused through the world, or transmitted to posterity, finds some person whose habits lead him to take notice of and qualify him for recording it; and every man of whatever disposition will meet some author or other whose powers of mind and style of thought will interest his attention, and seduce him to information.

Two observations of acknowledged truth in criticism establish beyond all doubt the powerful influence of peculiar disposition of mind in each individual author. The first is, that the same person is rarely found to excel in more than one species of

 composition.

compofition. He excels in that fpecies to which his habits of thinking are adapted; and in others the degree of his failure is always proportioned to the degree of their diftance from this. The cafe of literary mimickry is no exception, very few perfons having ever fucceeded in varieties of imitation; and of fuch as have practifed it with fuccefs it has been obferved, that few of them have had marked peculiarities in their own manner, or have given proofs of original genius. The fecond obfervation is, that thofe authors who by the peculiar fpecies of compofition they are engaged in are compelled to introduce different perfons fpeaking in their proper characters, have not often fucceeded in their efforts to give them their appropriate ftyle of thought and fentiment. In dramatic writing this circumftance conftitutes an acknowledged difficulty, diminifhed however by the characters originating often in the author's mind, without any external ftandard to which they are referred, and being known only by that dialogue which the author has given: diminifhed alfo by the hurry of action, the brifk interruptions of different perfonages, and the fhortnefs of each feparate fpeech. The difficulty is more evident in periodical effays where introduced characters write letters of confiderable length; and in hiftories, where fpeeches are given at large, as fuppofed to have been fpoken by the orators in perfon. In the orations recorded by Thucidydes there is much good fenfe, information and argument, but in not more than one or two of them is there any nice difcrimination of character.

SIMILAR habits of thinking, and fimilar difpofitions of mind will more or lefs prevail among inhabitants of the fame country, and thus lay the foundation of a national ftyle of thought and fentiment. The different idioms of different languages prevent clofe tranflation. The variety in minds and habits of different countries caufe an equal difficulty in imitating an author of a different nation. But a fimilarity of individual mind will overcome the difficulty, and enable a writer of whatever country to imitate or tranflate with fuccefs. From this caufe is derived the excellence of Rowe's tranflation of Lucan : and to the fame caufe we may afcribe the fuperiority of Swift's imitations of Horace to thofe of the other wits of his age. The journey to Brundufium fhews us what circumftances made impreffion on the mind of Horace, and traditional ftories of Swift's habits fhew that many of them would with him have met a fimilar reception. Swift had Horace's knowledge of common life, his fondnefs for familiar incident, and his turn of eafy and natural expreffion. Milton, according to his own tafte, has imitated one of Horace's odes, by giving an Englifh verfion with all the Latin conftructions ; and Pope has followed his own eftablifhed habits by imitating fome of the fatires in ornamented phrafeology and harmonious verfification.

IF the proper object of mankind be man, an enquiry into the varieties of the human mind, a difcovery of them in their natural effects, in the ftyle of thought, traced out through the medium of literary productions and ftyle of language, could not fail of being highly ufeful. Critics, who have confined their obfervations

obfervations on ftyle to expreffion and language, have omitted the moft dignified and important confideration of their fubject. They have begun at the wrong end, and applied themfelves folely to examine the effect, in the hope of being able to correct its faults, without any attention to that which is their caufe. The confequence muft be extremely injurious to literature: authors neglect the cultivation of their minds for the polifhing of fentences, and never having formed a true eftimate of their powers rafhly engage in works ill fuited to their habits, and derogatory to their fame. Criticifm becomes verbal inftead of rational; and men begin to write and to publifh, who have never once employed themfelves in learning to think.

Besides the critical ufes which may be derived from fpeculations of the fort here pointed out, fuch fpeculations may be yet farther recommended by the general pleafure with which they would be received by every defcription of readers. The developing of character is an univerfal and favourite employment: every perfon conceives himfelf an adept in the art, and thinks he poffeffes a knowledge of criticifm which give peculiar certainty to his conjectures. Lavater obferves, in commendation of his art, that every man is in fome degree a phyfiognomift: and I believe very few perfons ever read a book, at leaft a book of fancy, without forming fome ideas of the author's character. If this be fo univerfally done, it is defirable that fome affiftance be given by which it may be done with judgment; by which it may be regulated to greater certainty, and directed to fome advantage.

To the want of ſufficient information in the art the abſurd conjectures which are often formed reſpecting authors are to be aſcribed. The lady who from Thomſon's poems found reaſon to perſuade herſelf that he was much addicted to ſwimming attempted a ſpecies of mental phyſiognomy for which ſhe was not qualified. It is not every deſcription, made neceſſary to an author by his ſubject, which is to be conſidered as giving certain information of his habits and propenſities: a man who has choſen for his topic the pleaſures of the country, may be ſaid to have a general fondneſs for rural life or rural ſituation, but he will be obliged ſometimes to depict ſcenes of which he has not felt the pleaſure, and ſometimes to deſcribe ſports of which he has not partaken. The indolence and the benevolence of Thomſon appear in many parts of his writings; but unleſs he had gone out of his way to treat of ſwimming, or had treated of it more frequently or more fully than was proportioned to its importance towards his general theme, there was no reaſon for ſuppoſing it an amuſement in which he took particular delight.

An accurate and complete treatiſe on ſtyle in writing, conſidered with reſpect to thoughts and ſentiments as well as words, and as indicating the writer's peculiar and characteriſtic diſpoſitions, habits and powers of mind, would, it muſt be confeſſed, be a work of great difficulty: it would require a perfect knowledge of the human mind in all its varieties, and an acquaintance with the works of authors who wrote

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in various languages, at diſtant ages and in different ſpecies of compoſition: it would require alſo a perfect inſight into character, national and individual: a ſagacity which could not be impoſed on by aſſumed diſguiſe in the writings it would examine, and a reſolute underſtanding, which could ſet aſide all deception of internal prejudice, and repreſs the forwardneſs of its own vanity in forming its judgments. To aid and direct ſuch qualities in making the enquiry, an accurate biographical account of various authors would be eſſentially neceſſary; for general obſervations on the ſubject could reſt their veracity only on an induction of many particulars confirmed by actual fact. As to the antient authors, ſo little can at this diſtance of time be known of their perſonal habits and private characters, that any critic who would found his theories on them could at beſt entertain us with probable conjecture. Nearly the ſame objection holds with reſpect to foreign writers. It muſt therefore be from works of a later date only, and chiefly from the works of our own countrymen, that any ſuch theories are to ſeek ſtability: and we know how much the pre-diſpoſitions and paſſions of more modern biographers tend to prejudice the mind of the reader, and miſrepreſent the character of him whoſe life they write: we know how difficult it is to develope the truth from the contradictory reports of authors under impreſſions of oppoſite affections, and to form a a juſt notion of the features of the original from the pictures drawn by enemies or by admirers.

NATIONAL character is much more eaſily diſtinguiſhable in writings than the individual character of the reſpective authors, as being the effect of cauſes operating with more ſteadineſs and on a great number of writers: it is therefore better underſtood and more readily perceived: and hence we find thoſe dramatic authors, who have little knowledge of manners and little acquaintance with the modes of individual character, find a never-failing reſource in the introduction of ſome Iriſh or Scotch or Frenchman, by a difference in his language and dreſs to make himſelf known at once to the vulgar part of the audience, and to keep alive and flatter their prejudices. National character is ſometimes ſo ſtrongly marked as to deſtroy the perception of ſingular differences, as provincial pronunciations are loſt to a foreigner in the peculiarity of the general accent. The ſtyle of French poetry in general is ſo different from that of other nations, that a perſon of a different country does not ſoon arrive at the art of diſtinguiſhing the ſtyle of one French poet from that of another.

THE peculiar ſpecies of compoſition alſo will ſometimes leave very little information to be collected as to the peculiar and characteriſtic habits of the mind of an author. All writers of paſtoral poetry are from the modes of life they would repreſent obliged to ſeparate themſelves as much as poſſible from their own habit and character. Hence we find this ſpecies of writing has been rarely cultivated but by juvenile poets, who not having yet acquired a diſcriminate character could more eaſily adopt any which might come recommended to them.

Dramatic works, by the ſtrength with which they put forward a variety of characters, uſually keep that of their author unperceived. Thoſe writings in which the author gives his detail in perſon, and particularly oratory and lyric poetry, where he ſpeaks from the fulneſs and force of his own mind, muſt bear the ſtrongeſt marks of his peculiar habits of thinking.

One author, it is true, often imitates another, and thus preſents the peculiarities of another's mind inſtead of his own. When the imitations are general, when authors of one deſcription imitate authors of another, in the ſame ſenſe in which the moderns are generally ſaid to imitate the antients, a falſe colouring is undoubtedly laid on which diſguiſes the truth, and traditional ſentiments are conveyed, which not being the genuine offspring of the author's mind bear little impreſſion of its peculiarities. The works of authors however cannot be wholly made up of ſuch fictitious materials, and even among theſe it may be obſerved that the ſelection of ſome particular authors from among the whole claſs, the preference given to ſome parts of their works above others, may give information as to the individual mind of the writer who borrows from them. When the imitation is particularly confined to one favourite author, ſome degree of ſimilarity in turn of thought or diſpoſition may in all caſes be concluded on. If this has not led to the imitation it will naturally follow it. The ſame habits of thinking, the ſame modes of conſidering a ſubject, will be inſenſibly contracted. The taſte will be formed on the favourite model, and opinions delivered in a ſtyle of which we

commend

commend the force and beauty, or from authority which we admire and refpect, cannot fail of becoming our own, the principles of our reafoning, and probably the rules of our conduct.

FROM thefe obftructions which the ftrength of national character, the peculiar fpecies of compofition, or the fondnefs for imitation interpofe, it is evident that individual character cannot in all cafes be difcovered to the fame degree of clearnefs and certainty, or with the fame facility. But greater minds (and thefe are beft worth our attention) will overleap thefe obftacles and fhew themfelves to the difcerning; and though there may be many parts of every author's works which do not tranfmit the peculiarities of his mind, it is always fufficient if there are fome which do. It happens much to our advantage in fpeculations of this fort, that thefe parts of an author's works are ufually more attractive, and always the beft executed.

THOSE parts of an author's works in which we are to look for the cleareft indications of his habits and difpofitions of mind, are the parts which are not abfolutely effential to his narrative, but which are introduced and ornamental; and hence in thofe works where fuch prevail his habits and difpofitions are moft apparent. Thofe parts which are brought in to pleafe the reader are ufually fuch as have pleafed the writer. When a man quits the direct path, it is always to go by fome way which he likes better; when he ftops for any time on his road, it is becaufe he has met with fomething in which he finds delight. The digreſſions of an author are, in like manner, indications of what is agreeable to his diſpoſitions, for he cannot expatiate on what he diſlikes. Metaphors and ſimiles he will ſeek in thoſe ſources which his prior occupations have made familiar and his habits have endeared to his taſte. Thus Pope is found to have been a lover of the arts, and Dryden of the ſciences. Every alluſion in the writings of Cowley and the other metaphyſical poets is taken from remote learning and abſtruſe philoſophy; and Mr. Addiſon's fondneſs for claſſical literature has made that the principal ſource from whence moſt of his illuſtrations are derived.

In general, where an author has written much and has written well, his works will always ſhew what degree of antecedent labour has been expended in furniſhing his ſtore-houſe with literary treaſures, what accuſtomed employments have given given facility to his exertions, and what modes of life have been familiarized to him by ordinary habits. In Milton's works we ſee proofs of a life ſpent in ſtudy, of every ſource of information ſearched out with the moſt perſevering diligence. In Shakeſpeare we ſee ſuch an extenſive knowledge of human nature as could only have been acquired by much time ſpent in actual obſervation. In the writings of Swift we perceive habits of familiar converſation with ordinary perſons; in thoſe of Dr. Johnſon we readily diſcover that his habit was reaſoning, and his ſpeech was diſſertation.

LORD

Lord Bacon has from Plato's allufion confidered the underftanding of every individual man as a cavern which makes the appearances of things vary much from the reality. From the diverfity of appearances of the fame object in different caverns the different nature of the caverns themfelves may be difcovered. Thus it is we talk of the various lights in which the fame fubjects appear to different writers, and from their different modes of treating them the characteriftic differences of their own underftandings obvioufly appear. When you fee a writer always confidering each particular fubject as a part of fomething more extenfive, dealing out general aphorifms and fearching after univerfal certainties, you have an evident mark of a fpirit towering above and looking down upon his fubject, imperious and commanding. When you fee a writer collecting every thing within individual bounds, taking up the fubject no higher than itfelf, and careful not to digrefs or go beyond it, you have a mark of a mind humble, minute and timid. When you find no affertion without a quotation to enforce it, you may afcertain of the author that his intellect is fhackled to authority, and that he probably fees little merit but in learning. When you find thoughts perpetually digreffing from each other by trivial and irrelevant affociations, you may pronounce of the writer that his habits are mean, his judgments flender, and his underftanding incapable of reafoning and argument. By thefe criteria we would form this decifion on his underftanding from his converfation, and by the fame we may equally form it from his writings.

Though

THOUGH it muſt be admitted that it is not always ſafe to infer a man's moral character from his expreſſed ſentiments, yet perhaps from the writings of an author ſome inferences as to his moral as well as his intellectual qualities may with caution be drawn. We may be ſatisfied of the exiſtence of thoſe faults which his utmoſt induſtry could not conceal, though we may not always give him credit for thoſe virtues which he may poſſeſs. No man from their writings can heſitate to pronounce generally that Addiſon was a man of virtue and religion, and Horace voluptuous and a debauchee. Such information is notorious—*votiva veluti in tabella vita patet.* Sometimes however the deduction is more ſubtle and the proofs leſs obvious, in proportion to the knowledge which the author may have of his own defects, and the addreſs he can employ in concealing them; yet ſometimes the difficulty of knowing himſelf, ſometimes his contempt of his reader's ſagacity in making the the diſcovery, ſometimes his aukwardneſs, and frequently his vanity, betray a character which he himſelf does not know, or which perhaps, with all its faults, he contemplates with pleaſure. An author, as well as all other men, though he be not perfectly ſatisfied with all parts of his own character, finds conſolation in contemplating ſome features of it for his diſguſt at others: this favourite part of the author's character he labours for occaſions of introducing, praiſes thoſe who poſſeſs it, and magnifies its excellence. His vanity would not ſuffer him to debate on a moral or intellectual quality which he knew he did not poſſeſs, nor could he be comfortable in holding out perpetually to public deteſtation what he was conſcious was his

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own indulged habit or private defect. Pope dwells on the poverty of his rivals, becauſe it was his prudence and his pride to have acquired a competence; while moſt other poets diſcloſe and commend their poverty by inveighing againſt the ignorance of the great who do not reward their talents, or by frequent, and vehement declamation againſt a love of that wealth they never have poſſeſſed.

Authors ſometimes make their works direct channels for the conveyance of their character and hiſtory to the public. Thus Milton tells us of his blindneſs; Virgil puts a narrative of his own fortunes into the mouth of his ſhepherd Tityrus; Swift, in his Cadenus and Vaneſſa, is known to have intended a juſtification of himſelf againſt a miſrepreſented ſtory; and Savage celebrates the talents and apologizes for the profligacy of the baſtard. I am ſometimes inclined to ſuſpect authors of preſenting directly their own pictures to the reader. Smollet certainly did this in his character of Bramble, making at the ſame time ſome of the facts recorded in his travels the incidents in his novel. Dr. Johnſon has given us at full length the portrait of a Mr. Johnſon, an imaginary member of a literary club, as drawn by Blackmore in the firſt eſſay of an unſucceſsful periodical work. I ſuſpect this extraordinary quotation has been made, that the reader may be ſurpriſed into a compariſon of the great qualities of the biographer himſelf with thoſe which Blackmore, as if by a ſort of prophetic ſecond ſight, had beſtowed on his gigantic Johnſon.

Thus

Thus it ſeems that ſome information, both with reſpect to an author's intellectual and moral character, is always to be collected from his writings. In ſome caſes it may be more difficult to collect it than it may in others. In ſome works the inference may more nearly approach to certainty, in others the degrees of probability may be ſlight, but in all ſome information will reward the reſearch, and the reſearch itſelf is above all other employments of the mind intereſting and inſtructive. To diſcover character, to trace out the cauſes of literary excellence and defects, to explain the efficacy and operation of habits, to exhibit the influence of the morals on the underſtanding, will afford a dignified exerciſe to the critic, an uſeful one to the metaphyſician, and an agreeable one to the moraliſt.

The firſt object of every author's attention is the choice of his ſubject. The choice of this is an act directed by the habits and diſpoſitions of the author, and therefore indicative of theſe. From the infinite variety of ſubjects that one is ſelected, which either is moſt pleaſing to the fancy of the author, or in which he thinks he is moſt likely to excel; in either caſe it is that which beſt ſuits his habits, diſpoſitions and powers of mind. Achilles was known at the court of Lycomedes by his preferring the armour to all the toys brought by Ulyſſes; and, from the ſubjects they choſe for writing on, we may certainly infer that Virgil loved peace, and that Milton had an high reſpect for religion. The Engliſh Garden, is the work of a poet viewing ſcenes of external nature with the eye of a painter; the Botanic Garden, of a poet ſtudying her internal operations with the abſtraction of a philoſopher. The latter

could

could only have been written by an author whose habits had cherished a fondness for philosophic speculation, and whose situation had given him opportunities of becoming acquainted with its modern experimental progress: the former might naturally be expected from the friend of Sir Joshua Reynolds and his associate in an edition of Du Fresnoy.

This remark, however, must be received with some limitation. An author writes on many and various subjects. His choice is not always left free to the influence of his characteristic dispositions. On several occasions these are made sacrifices to his convenience, his necessities or his ambition. He often writes on subjects occasionally recommended, on the story that is popular, on the event that is recent, at the suggestions of his own vanity or the command of his patron. Professed authors are not more disinterested than other men: and a name in the literary world is of such value that a bookseller often pays an high price for prefixing it to a work, which not being suitable to the author's disposition only derogates from his reputation. Almost all occasional writings, prose as well as poetry, pamphlets and odes, contain within themselves the elements of speedy dissolution. We should say then that it is only the choice of a subject in which the author has excelled, which may be considered as giving some intimation as to his habits and dispositions.

The nature of the subject selected in a great measure ascertains the species of composition in which it is to be treated. Where this is left a matter of doubt the habits and mind of the author must decide. Whether the same catastrophe shall be the subject of an elegy or of a tragedy depends wholly on the writer's fondneſs for contemplating the emotions of his own mind, or viewing external and viſible effects of their operations on the character of others, on the penſive or obſervant turn of the author. Whether the ſame ludicrous incident ſhall give occaſion to a comedy or to a mock heroic depends on the author's acquaintance with the living or the learned world, with men or with books.

OTHER matters relative to the nature of the work are in like manner aſcertained from the characteriſtic habits and diſpoſitions of the writer. A profeſſed admirer of the ancients will divide his ode into ſtrophe and antiſtrophe: Mr. Harris, from his fondneſs for the Platonic ſchool, has given us his philoſophy in dialogue, and the gentleman who afterwards tranſlated the letters of Cicero and Pliny might naturally be expected to publiſh his eſſays in the epiſtolary form. A man of extended and diſcurſive views will not confine himſelf within the bounds of rhyme, but will compoſe his epic or didactic poem in blank verſe. Perhaps an enthuſiaſm in the general cauſe of political liberty, or a horror of licentiouſneſs, with a fondneſs for regulation, are often in the minds of poets and critics connected with the principles which decide them in the compariſon of blank verſe with rhyme. Whimſical as this opinion may ſeem it is confirmed by ſeveral inſtances. Pope always wrote in rhyme, and Doctor Johnſon is its great advocate; while in all their more important works, Cowper, Thomſon, Milton and Akenſide employed blank verſe.

WHEN the ſubject has been choſen, and the ſpecies and mode of compoſition aſcertained, the thoughts and ſentiments of an author

author come next under conſideration. Various views of his ſubject will preſent themſelves, various trains of aſſociated thought will ſucceſſively ariſe in his mind. But aſſociations of that particular ſort to which his habits have been formed will occur moſt readily, and be received with the cordiality of intimate acquaintance. Man has been ſaid to be a bundle of habits: habit then will account for the frequent recurrence of a kindred train of thinking in the mind of the ſame perſon, and the prediſpoſition for that to which it has been accuſtomed will ſecure to it a preference.

SHOULD the ſame range of thought preſent itſelf to the mind of authors different in their habits and diſpoſitions, what has been ſaid may ſerve to ſhew that it would not with all meet an equally friendly reception. It is not, however, at all probable that the ſame range of thought ſhould occur. No man, it has been obſerved, forgets his original trade. The rights of nations, ſays Doctor Johnſon, ſink into queſtions of grammar when grammarians diſcuſs them. A mathematician conſidering a ſubject not mathematical will from habit employ himſelf in an analytic inveſtigation of its properties and cauſes. A lawyer will apply to ſolving objections and ſcrutinizing diſtinctions. Profeſſional men of every deſcription will recur to thoſe ideas and trains of thought to which they have been accuſtomed. Dramatic writers, who underſtand character, conſtantly mark out each profeſſion, by a peculiar train of thought as well as a technical language.

Every literary work muſt contain narratives of ſome events, deſcriptions of ſome objects, expreſſion of emotions and enforcements of opinion. It does not ſeem extraordinary that opinions ſhould be enforced by arguments drawn from topics which are congenial to an author's diſpoſitions, and which therefore have proved themſelves to him the moſt powerful inſtruments of conviction. It will readily be admitted that the ſame emotion will ſhew itſelf differently in different minds and tempers, and that of courſe the modes of expreſſing ſuch emotions will vary conſiderably. With reſpect to narratives of events and deſcriptions of objects this is equally certain, though not equally obvious. Each event is attended by a great number of circumſtances relating to perſons, motives, places, inſtruments: each object has a variety of particular adjuncts accompanying it in its actual exiſtence. To enumerate all theſe, if it were poſſible, would be unneceſſary and diſguſting. A ſelection is therefore, in all caſes, to be made, and the varieties of ſuch ſelections naturally proceed from the variety in the views and habits of the authors who relate the events, or deſcribe the objects. If anecdotes related in private converſation partake of the character of the ſtory-teller, the ſame muſt be preſumed of the biographer, who undertakes his taſk through the impulſe of ſome affection, which of neceſſity gains ſtrength in the progreſs of his work. If no two eye-witneſſes of the ſame fact agree exactly in their reports, a greater agreement cannot be expected in the records of hiſtorians viewing various communications of events, and equally under the influence of variety of temper, and underſtanding. Travellers,

deſcribing

defcribing the fame identical fcene in nature have been obferved often to make a different felection of its circumftances. When the object then to be defcribed is general, of an intellectual nature, or of extended influence, poetic fancy in various minds muft be expected to vary the defcription. The Allegro and Il Penferofo of our great poet are beautiful illuftrations of the variety of felections made from the great ftore-houfe of nature by men under the influence of different habits and difpofitions.

After the fentiments the language naturally comes to be confidered; and if the former indicate the author's powers of mind, the latter, directly connected with them, muft give correfponding information. *Verbaque provifam rem haud invita fequuntur.* A writer's language may fometimes be had from imitation, but, as has been mentioned, it muft be either fome predifpofition in favour of a particular author's habits of thinking, which induces the imitation of his ftyle of words, or fome ftriking peculiarities in his language, which by a natural affociation would infinuate alfo and imprefs his ftyle of thought; fo that the author's language is the offspring of antecedent difpofitions of mind directing him to models fuitable, or by reflex influence of words on the underftanding it generates kindred habits of thinking, of which it is therefore indicative. Every writer's vocabulary is made up of the words he has learned in converfation or in reading; converfing with thofe who have regulated the mode of his thinking, or reading the works of thofe authors who are his favourites. Collocation, arrangement and connection he learns in the very fame

manner.

manner. His ſtyle in language is thus congenial to his ſtyle of thinking.

If it ſhall be a matter for his option what words he ſhall prefer, or what arrangement he ſhall give them, I do not ſee what there is to regulate that choice but the habits and powers of his mind, directing a language congenial to the train and modes of his thought, and exciting ſimilar ſenſations. The propriety and beauty of language is this analogy to the train of thought to be expreſſed by it; and accordingly we find that all the terms which are applied to denote diverſities of ſtyle do in ſtrictneſs of primitive acceptation belong to thinking and its modes.

The habits, diſpoſitions and powers of mind ſometimes exert a direct influence over the words and language. Accuracy of thought will naturally demand preciſe expreſſion, and obſcurity in ſtyle will be the conſequence of dull conceptions. Licentious phraſes and ſtrained figures of ſpeech will follow the unreſtrained indulgence of wayward imagination, and foreign words always aſſume a place in the works of an author who has been in habits of intercourſe with foreign learning, or is guided by a foppiſh affectation of polite ſociety. Obſolete idioms mark pedantic habits, and technical language is the neceſſary reſult of profeſſional employment. Redundance of copulatives and particles acknowledge a difficulty in perceiving any connection but what cannot poſſibly be overlooked; circumſtances ill arranged betray habitual

negligence

negligence or forgetfulneſs, and the repetition of tautologous ſounds can only proceed from the emptineſs of the underſtanding.

FROM the ſtyle of words joined with the ſtyle of thought and ſentiment a full portrait of the writer's intellectual habits and powers may be drawn, as far at leaſt as is neceſſary for underſtanding his works, or uſeful for admonition from his example. We may form a proper eſtimate of the value of his authority from the diſcovery we may thus make of his means of information and capacities of judging, and we may learn what in his habits was conducive to his improvement, and what gave riſe to his faults. Such uſeful knowledge confirmed from facts in the known hiſtory of ſome writers may furniſh matter for analogical reaſoning as to others, concerning whom we have no authentic biographical accounts; but it may more eſpecially ſupply uſeful documents to young proficients in literature, and valuable leſſons of prudence, of diligence, and of morals to all.

THUS from the writings of Milton we may ſee the value of ſtudious habits, even under the greateſt diſadvantages, and we are taught the folly of thoſe who would encourage imagination by repreſſing learning. From the works of Shakeſpeare, a man, from whom birth and circumſtances have withheld all direct communication with ancient authors, may find that "with ſmall Latin and "leſs Greek" a poet may, through a diligent examination of the human heart and an acute obſervation of human life, riſe to the higheſt

higheſt pinnacle of celebrity. From Pope we learn the value of prudential habits in life and literature; from the paucity and poverty of charcter in Virgil's Æneid we ſee that to great works ſomething more is required than the labours of the ſtudy; and from every conſiderable defect of a great author we learn the injuries of a vain or imperious temper, which will not ſubmit to eſtabliſhed regulation, nor ſtoop to conſult ſuch friends as have capacity to judge and honeſty to cenſure.

ESSAY, No. II. *on the ſame Subject as the preceding.*

By the ſame Author.

IF in the preceding eſſay it has been eſtabliſhed that there is a ſtyle in thought depending on the varieties of the intellectual character, and therefore indicative of theſe, it will follow that from the ſame ſource ſome information reſpecting the moral character may alſo be derived. Diſpoſitions are generated and habits confirmed by the approbation of the mind, over which they in turn exert a reciprocal influence. When the moral qualities do not obey the controuling direction of the underſtanding, what has depraved the morals will uſually be found to warp and bias the judgment; ſince external circumſtances, which produce forcible effects on one part of man's conſtitution, do more or leſs affect every other. The varieties

Read April 19, 1794.

then of moral difpofitions and peculiar habits may be traced out through that variety in the intellectual character with which they are fometimes affociated as caufe and effect, and fometimes as common effects of the fame caufe diffufing a general operation over the whole fyftem. The ftyle of thought therefore which flows from the one muft in fome degree indicate the other.

That an author mixes much of himfelf with his fubject, however ridiculoufly and extravagantly Sterne in his life of Mr. Shandy may have caricatured the fyftem, is undoubtedly true. That an author's difpofitions may thence be inveftigated we have teftimony of much weight and antiquity. *Laudibus arguitur vini vinofus Homerus* is the affertion of Horace, and the diflike of Euripides to the fair fex has been long fince collected from the unfavourable pictures of them he has always drawn. Longinus tells of internal difpofitions neceffary for producing the fublime, and Quintilian gives a catalogue of the moral qualities which an orator fhould poffefs. But on this queftion every man bears teftimony for himfelf; for does not every man think that he can in fome degree anticipate the mode in which thofe with whofe minds and habits he is acquainted will act on any particular occafion, or will treat of a given fubject? I do not mean to fay that he will be able to write a treatife in the ftyle of each author of his acquaintance. There is a divifion of literary as well as natural labour which makes the beft ufe of the productive capital by confining each writer to one particular fpecies of employment.

And

And if Milton has been thought by critics to have ſhewn ſome melancholy in his mirth, even in poems of the ſame ſtructure, and in which he had intended to contraſt them, verſatility in ſtyle of thought ſhould be deemed not ſo much an aſſemblage of many qualities, as a peculiar natural quality in itſelf, not to be attained by effort, and not neceſſary to excellence.

Through the ſtyle in words theſe characteriſtic differences may eaſily be diſcovered. There are few words in any language which can in ſtrictneſs be termed ſynonimous. Many of them may expreſs the ſame primitive idea, but each expreſſes it in a different degree, in various circumſtances and relations, and under different impreſſions of the writer's mind. Every quality intellectual or moral has many names by any of which it may be expreſſed, according to the different reception which it meets from him who deſcribes it; which depreſs or heighten its power, according as it is to be commended or condemned. When the ſame man is ſpoken of by one author as liberal and by another as prodigal, the ſame country by one traveller as bleak and by another as romantic, the ſame theory by one critic as ingenious and by another as extravagant, the moral character and habits of the writers can alone account for this diverſity, and through the medium of their language this diverſity may be pointed out. Even the ſame fact will be related in various words according to the intent and diſpoſitions of him by whom it is related. When it is ſaid that Brutus *killed* Cæſar, the fact ſimply is ſtated; and when we ſay that he *ſtabbed* Cæſar, the fact is related circumſtantially; an addition is made of the mode in which the act was accompliſhed. But when we ſay that Brutus *murdered* Cæſar, our aſſertion goes beyond the fact, and we pronounce an opinion on the criminality of Brutus. Words of the firſt and ſecond claſs form the proper language of hiſtorical narrative; words of the laſt by their reflex or ſecondary ſignifications are the language of the writer's character. Hence the greater the ſimplicity of ſtyle, the more proper for an hiſtorian; and, on the other hand, the more vain the hiſtorian, the greater his fondneſs for diſplaying himſelf and putting forward his own opinions, the more faulty is his ſtyle. On this ground it is that the ſtyle of Mr. Gibbon as an hiſtorian is extremely unfit for imitation: his work is much more an hiſtory of his own mind and opinions than of the decline and fall of the Roman empire.

Were it poſſible for the human mind to diveſt itſelf in an inſtant of the paſſion to which it had immediately before been ſubject, and to view every thing which comes before it as wholly new and perfectly ſingular in its nature, ſtill an author would have ſome ſtyle of thought ariſing from a predilection for certain modes of conſidering his ſubjects, founded on the peculiarity of his natural underſtanding, his education and intellectual habits. But as many of the objects are not new, what at the preſent time occurs will coincide with or be judged of by what has formerly been thought on the ſame ſubjects, or on ſuch others as are by ſome of their nume-

rous

rous analogies connected with them. What therefore is at any one time said has probably been often before thought, and is part of a system of opinions which have long had an influence on the understanding and on the practice. The dispositions of mind too are more permanent, the force of habits too stubborn to give place at whatever moment an author chooses for writing; what is written in conformity to the reigning disposition will be written with spirit, and appear to the author in a high degree true, natural and forcible. If a man be dissatisfied with himself he will on very slight occasion quarrel with any person who comes in his way: if an author is peevish or choleric his writings will shew his discontent; they will exhibit gloomy prospects of nature and melancholy views of life. In the manners of foreigners we observe many national prejudices, and in the conversation of every individual we see the singularities of his mind; an author then, who must be supposed in like manner affected with his national and individual prejudices, will betray them to such as can view his character from a distance, and examine his writings under different impressions.

THAT these indications of character are in most writers sufficiently strong appears from this, that even in those who are under the influence of immediate inspiration they are perceivable. The Deity makes use of the natural man as the instrument of his communications, and the several pages of the sacred volume shew the distinct habits and dispositions of their respective authors. Thus the character of St. Paul is

fully

fully delineated in his epiſtles: the dignity of his ſpirit and the energy of his mind appear in his words as well as his acts: his learning and his profeſſional habits ſhew themſelves in his alluſions to paſſages in claſſic authors, and in the ſources of his metaphors. St. Peter's natural vehemence is exhibited by the rapidity of tranſition in his thought, and the boldneſs of grammatical conſtruction in his ſentences. And St. John, the diſciple whom Jeſus loved, pours forth the grateful return of his heart in dwelling particularly on his maſter's diſcourſes, and ſhews the general mildneſs of his nature by frequent and earneſt exhortations to benevolence and love.

EVERY particular relating to the moral character and habits of an author is of much importance to his readers. Without ſome acquaintance with theſe we ſhould in many caſes fail of comprehending his meaning, and in no caſe ſhould we be able rightly to appreciate his judgments. Many of his words are relative while they are deemed poſitive, denoting compariſons made by his own mind according to ſtandards indirectly and imperfectly repreſented. Many of his opinions are conveyed by ſtealth in his writings, left to produce their effect on the reader by the collective force of many minute atoms of miſrepreſentation. Many of his deciſions reſt more on his authority than his arguments; and to learn the value of his authority, to enquire into the means of his information, and to examine the probable ſources of his prejudices, is neceſſary to enable the reader to aſcertain by all due allowances the actual

actual and limited truth. The enquiries which are thus useful to assist a reader's comprehension may be in a much higher degree useful to an author. To make the detection of vice in literary characters more easy would in all probability have effect on the morals of authors, and through them on the world. The critic would perhaps learn to overcome his resentments did he know that it was impossible to conceal them from the public; and the traveller would learn to venerate truth, when he found that the vanity which prompted him to exaggerate must betray itself in his writings, and bring universal discredit on his testimony.

The general modes in which such enquiries are to be conducted, and the exact degrees of probable evidence which will support particular conclusions, it is not easy on this first view of the subject to determine. Something of a nature analogous to this has in particular instances been done, where from proofs furnished by the works themselves the precise time at which they were written is detected, and the author, his age, his rank or his country ascertained. Some valuable treatises of literary controversy proving certain supposed ancient writings genuine or otherwise, some judicious observations of modern historians and critics separating what in very remote periods is fabulous from what was fact, and all that occurs any where relating to internal evidence of the truth of narratives and the credibility of witnesses, will be found to throw light on this subject. The remaining part of this essay will contain some specimens of this theory applied to discover the indications of

habits,

habits, moral and intellectual, of difpofitions and external circumftances in the writings of known authors, and in fome inftances to trace out their operation.

The lights in which the fame fubject appears to different authors are indeed fo very different that it is not poffible to read a page of the copious index to the edition of the Englifh Poets, or even the quotations under the fame word in a Dictionary, without finding fomething characteriftic of the habits or difpofition of the author. Thus wine is by Congreve after Ovid fpoken of as in alliance with love, and by Gay as putting time and care to flight; Swift pronounces that

Wine, powerful wine, can thaw the frozen cit,
And fafhion him to humour and to wit,

after which he employs a page in fatirically defcribing its effects on feveral of the public characters of the day; but Milton, whofe difpofition was religious, and whofe habit was ftrict temperance, fpeaks of the *fweet poifon of mifufed wine*, and introduces it as a topic to be commended by the crew of Comus, and condemned by the chorus of Samfon Agoniftes. Thomfon in his beautiful defcription of night has given its vifible marks with minute diftinctnefs; he talks of the glow-worm *twinkling with its moving radiance*, and tells that

. . . . a faint

. . . . a faint erroneous ray,
Glanc'd from th' imperfect ſurfaces of things,
Flings half an image on the ſtraining eye.

Milton, who had for many years loſt the advantages of the viſual ray, and had not viſible images ſo freſh and accurate in his fancy, has deſcribed night by its effect on the animal creation, by the ſilence which accompanies it, and the fanciful and claſſic imagery of Heſperus and the moon. Night with Young is virtue's immemorial friend, and loud calls on devotion; to Waller it only gives an opportunity of diſcovering the charms of Mira's mind, by concealing the dazzling ſplendor of her perſonal graces.

Atterbury and Clarke have both written ſermons on this text: "If they hear not Moſes and the prophets, neither will "they be perſuaded though one roſe from the dead." Each of them begins by explaining the occaſion on which thoſe words were ſpoken: but Atterbury in the courſe of his explication ſhews us the fitneſs of the rich man's making his requeſt particularly to Abraham, and deſcribes with pointed irony the voluptuaries of his own day under the character of the ſenſualiſts of the evangelical times; while Clarke in his introduction exactly aſcertains how far the rich's man's reaſonings were juſt, and wherein lay his miſtake. Each then proceeds to the main body of his diſcourſe, and here Atterbury conſidering the poſition in the text as a truth rather ſurprizing, and one not likely to meet ready acceptance on the firſt propoſal, employs himſelf to limit its extent ſo as to ſecure to it a more favourable reception; while Clarke prefaces his main argument by proving, from the deſign of religion and the faculties of man, that perfect and irreſiſtible evidence on theſe points is not to be expected.

ATTERBURY on his firſt head of proof eſtabliſhes that ſuch a meſſage as that in the text ſent to a wicked man would not be complied with—that he would doubt of its reality, and find out natural modes of accounting for it—that he would ſuppoſe it ſome dream of a melancholy fancy, or ſome trick of his unbelieving acquaintance—and that even if he ſhould receive it at firſt as a revelation, the progreſs of time would take away his horror, and the raillery of his companions laugh him out of his perſuaſion. On his ſecond head of proof he then argues that the evidence ſpecified is in reality a leſs probable or powerful means of conviction than the actual evidence of the goſpel—becauſe the goſpel evidence contains reſurrections from the dead, with many other proofs—becauſe the evidence required exerts all its force on the firſt impreſſion, after which it is ever afterwards in a declining ſtate, whereas that which is given gains ground by degrees, and the more it is conſidered the more it is approved—and laſtly, becauſe the force of the motive in the one caſe is particular and confined within a ſingle breaſt, whereas the other is an univerſal ſtanding proof, tried and approved by men of all deſcriptions,

descriptions, and falling in with the general fenfe and perfuafion of thofe with whom we converfe. Clarke proves firft that God has given all the intrinfic evidence from the nature of the thing itfelf that it is poffible to be conceived, with all the external proof from unqueftionable teftimony that was ever given to any matter of fact in the world—and fecondly he proves that fuch as will not be perfuaded by that evidence would not, by reafon of the wickednefs of their hearts, be perfuaded by any other evidence which their own fancy could fuggeft.

ATTERBURY concludes with feveral inferences directly pointed againft practical errors or received prejudices—againft the unreafonablenefs of expecting miracles on occafions of little importance—againft the belief of fuch frivolous miracles—againft pretended ftipulated appearances from the dead—againft our objecting to the degree of evidence vouchfafed to us becaufe others have had fuch as we deem irrefiftible—and he concludes his inferences (which take up a third part of his whole difcourfe) with an exhortation to magnify the divine wifdom, which hath fo ordered the firft proofs of our faith that they will be equally fatisfactory to the end of time, his conduct in the moral world being fimilar to that in the natural, and reafonable motives being preferable as inftruments of conviction to aftonifhing by immediate miracles. Clarke's inference is in one page—that if we free ourfelves from thofe unreafonable prejudices with which careleffnefs, and want of con-

 fideration,

ſideration, and unrighteous practice are uſed to blind us, we ſhall be fully convinced by the evidence vouchſafed us of the truth of chriſtianity.

I HAVE given minutely the ſchemes of theſe two ſermons, becauſe perhaps there is not any where to be found a more complete contraſt of habits and diſpoſitions exemplified in two compoſitions of the ſame ſort and on the ſame ſubject. The Biſhop of Rocheſter, a man of elegant literature, of much knowledge of the world, and of political habits and aſſociations, conſiders his ſubject with refined ingenuity and practical addreſs, diſplaying an extenſive acquaintance with human manners, and a perfect inſight into the prejudices of the heart. Clarke, whoſe habits were originally formed to academic ſtudies, and who through his life continued a man of ſcientific reſearch, ſteadily purſues his train of important demonſtration, without any endeavour to find out novel topics, or any deference to preconceived notions, with little light from experience, and little attention to practice. It is not unpleaſant to obſerve Clarke glancing with a careleſs and haſty view at ſome of the principal topics on which Atterbury ſo largely dilates. Suppoſing the meſſage in the text conveyed to the wicked, " as ſoon as " the preſent terrible apprehenſions were ceaſed," ſays Clarke, " it is extremely probable they would find ſome way or " other to aſcribe it all to the deluſion of fancy and imagi- " nation, and that their old vitious habits and deſires and be- " loved ſins would again by degrees prevail over them." Theſe

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collateral points however he will not go out of his way to difcufs, fatisfied that if he can by one undeniable chain of reafoning eftablifh the pofition in the text, what may occur on probable grounds againft it is not worth confideration. Atterbury, who knew how ill the truth is received which oppofes a prejudice, how much attention is always paid to him who fhews an accurate knowledge of the thoughts of his hearers, and how eafy it is to convince after you have filenced an objection, confiders all thefe practical topics at full length. On the whole Clarke looks for what will prove, and Atterbury for what will perfuade: Atterbury would affect his audience, and Clarke will convince his readers.

Even in tranflations of the fame paffage, through their common likenefs to the original, the characteriftic difference of the tranflator's habits will break out; as feveral portraits of the fame perfon will to a judicious eye difcover the painter as well as him who fat for the picture. The following are tranflations by Pope and by Cowper of the beautiful paffage in the fixth book of the Iliad, where Hector takes his infant fon Aftyanax into his arms:

Thus

Thus having ſpoke, th' illuſtrious chief of Troy
Stretch'd his fond arms to claſp the lovely boy.
The babe clung crying to his nurſe's breaſt,
Scar'd at the dazzling helm and nodding creſt:
With ſecret pleaſure each fond parent ſmil'd,
And Hector haſted to relieve his child.
The glittering terrors from his brows unbound,
And plac'd the beaming helmet on the ground.
Then kiſs'd the child, and lifting high in air,
Thus to the Gods preferr'd a father's prayer. POPE.

So ſaying, illuſtrious Hector ſtretch'd his arms
Forth to his ſon, but with a ſcream the child
Fell back into the boſom of his nurſe,
His father's aſpect dreading, whoſe bright arms
He had attentive mark'd, and ſhaggy creſt
Playing tremendous o'er his helmet's heighth.
His father and his gentle mother laughed,
And noble Hector lifting from his head
His dazzling helmet, placed it on the ground:
Then kiſs'd the boy, and dandled him, and thus
In earneſt prayer the heavenly powers implor'd*. COWPER.

MR.

* The paſſage in the original ſtands thus, vide Clarke's Homer, Il. vi. verſ. 466 to end of 475:

Ὣς εἰπὼν, οὗ παιδὸς ὀρέξατο φαίδιμος Ἕκτωρ.
Ἂψ δ' ὁ πάϊς πρὸς κόλπον ἐϋζώνοιο τιθήνης
Ἐκλίνθη ἰάχων, πατρὸς φίλη ὄψιν ἀτυχθεὶς,
Ταρβήσας χαλκόν τε, ἰδὲ λόφον ἱππιοχαίτην,
Δεινὸν ἀπ' ἀκροτάτης κόρυθος νεύοντα νοήσας·
Ἐκ δ' ἐγέλασσε πατήρ τε φίλος, καὶ πότνια μήτηρ,
Αὐτίκ' ἀπὸ κρατὸς κόρυθ' εἵλετο φαίδιμος Ἕκτωρ,
Καὶ τὴν μὲν κατέθηκεν ἐπὶ χθονὶ παμφανόωσαν·
Αὐτὰρ ὅγ' ὃν φίλον υἱὸν ἔπει κύσε, πῆλέ τε χερσὶν,
Εἶπεν ἐπευξάμενος Διί τ', ἄλλοισίν τε θεοῖσι·

Mr. Pope had formed his eſtabliſhed ſtyle of elegant poetry before he engaged in tranſlating Homer, a poet whoſe ſtyle was extremely different. To this it is to be aſcribed that we have here ſo many prettyneſſes which are not to be found in the original—*fond arms—lovely boy—with ſecret pleaſure—glittering terrors—lifting high in air—father's prayer.* I think I can alſo perceive in this paſſage the effect of habits of tranſlating even on Mr. Pope. *Dazzling helms* and *nodding creſts* were phraſes which had become by tranſlating the battle ſcenes of Homer ſo familiar to his ear, that though in general more verboſe than his author, he could not here dilate the expreſſions beyond the dimenſions in which they had uſed to appear: he has therefore contracted into one line the ſubſtance of two in the original. Had this been the only paſſage of the Iliad which Mr. Pope tranſlated, I am confident we ſhould have found it, if not more like Homer, yet certainly more vigorous and affecting.

Mr. Cowper has been led by his fondneſs for the ſimplicity of Homer to too cloſe a literal adherence to the words of the original, in prejudice of the ſentiment and the ſenſe. Thus becauſe the word νοήσας, uſually ſignifies an act of attention voluntary and protracted, Cowper has rendered it in this paſſage, *he had attentive marked*, an expreſſion utterly inapplicable here, as unſuited to the age of the infant and the terror he ſhewed. The word ſhould be taken here in its ſecondary ſignification, for the bare intellectual act of perception. The word ἐγελάσσε in like manner Mr. Cowper has rendered *laughed*, though

though its meaning in this paſſage is by the Scholiaſt in his note (which Mr. Cowper gives) pronounced to be ſomewhat different in degree from its ordinary one which alone our Engliſh term *laughed* expreſſes. There is not any one Engliſh word perhaps which can render πῆλε, but ſurely it would have been better to have uſed a periphraſis than to have tranſlated it by the mean and vulgar term *dandled.*

On comparing theſe two tranſlations with the original it does not appear that either of theſe gentlemen, however great their merits, ſeems to have rightly felt the beauty of this paſſage. The mode of motion denoted by ἐκλίνθη is not at all expreſſed either by *fell back* or *clung:* the one is too ſudden and violent, the other deſcribes what might perhaps have been the ſtate after the movement had taken place. Mr. Pope was never married: he was not a man of domeſtic endearment, or family obſervation: and without knowing any thing of the private life of Mr. Cowper (which from many paſſages in his works I am convinced is perfectly amiable) I think we might venture to aſſert that he did not receive Homer's image in the nurſery. The paſſage was too natural and ſimple for Mr. Pope, and Mr. Cowper has left it mean and proſaic *.

The

* ὃ παιδὸς is falſely rendered by Pope *lovely* boy. It was not admiration of the infant's beauty, but affection for his child, with which Hector was ſtruck. The delicate epithet εὐζώνοιο, a word of peculiarly ſoft ſound, is not attempted in either verſion. Ὄψιν Cowper renders *aſpect,* which more uſually denotes the look a perſon aſſumes than the appearance he exhibits. The fourth line of the original ſeems to amplify the terror by a full enumeration of the ſeveral circumſtances immediately crowded on each other—Χαλκόντε ἰδὲ λόφον. Mr. Cowper has deſtroyed the effect

The enquiry into contrasted character might be carried on in a comparison of plays founded on the same story, criticisms on the same work, letters written on similar subjects, and poems on the same occasions. But to pursue it at full length in this way would exceed the limits usually assigned to essays of this sort. It may be useful to shew that where no comparison of one author with another takes place, still some insight into his character, either in an absolute state, or compared with itself, may to a certain degree be had. The letters of Swift to Stella form one of the most complete pictures of mind which can be exhibited: probably not so studied as confessions which he might have published, but more true and equally discoverable. He left Ireland full of his own importance, with high expectations of cabinet intercourse and political ascendency. On his arrival at London every object is interesting, every circumstance is made to conspire with the predispositions of his mind; his thoughts are active, his letters exhibit a perpetual flow of vivacity and animation. After some time the aspect of the political horizon begins to change: he finds that he is treated with ceremony where he looked for confidence, and that however

effect by separating them in his fourth and fifth lines. Cowper's sixth line, except that the word *playing* is ill associated with *tremendous*, is the best in this whole passage. Pope's *And Hector hasted to relieve his child*, has no foundation whatever in the original. Homer relates the simple facts—the motive is obvious. Παμφανόωσαν situated where it is in the original excites in my mind the idea of a radiant light thrown by the helmet every way about it as it stood on the ground. The translators have in the general words *beaming* and *dazzling* lost this image. φίλον in the last line but one has been entirely passed over. So many minute imperfections would not have occurred, or greater beauties would have prevented our taking notice of them, had this been felt as a favourite passage by the translators.

ufeful a fubordinate inftrument may be to a ftatefman, it ftill muft be fubordinate; however valuable the fecret advice of an humble friend, his merits muft remain in fecrecy, and his ftation ftill be humble. Thofe even who wifhed for Swift's affiftance were afraid to afk it, and thofe who admired his talents dreaded his feverity. His hopes at laft appear delufive; he is difcontented with himfelf for having formed them, and with others for their difappointment. His pride is mortified, his vivacity is loft, and peevifh complaints and gloomy reflections fill up the latter part of his correfpondence. The whole of it is much to be prized for the vivid picture it exhibits of diftinct and progreffive variations of mind, and much more for the ufeful leffon it inculcates on literary men, to reprefs the fuggeftions of their own vanity, and not to prefume too much on the flattery of friends, or the condefcending civility of a patron.

The Night Thoughts or complaint of Edward Young prefent another very remarkable picture of mind. Young is himfelf the conftant complainant. Every view of general mifery leads him to the confideration of his own ftate, and the defcription of his individual misfortunes. The death of Philander —his own ficknefs—Narciffa—the peculiar rancour of death to him—the perils which await Lorenzo—recur by every affociation to his thoughts. The mention of friendfhip reminds him of the lofs of friends, and the counterfeit friendfhips of the great: the addrefs to fleep with which the poem begins felects for a topic its forfaking the wretched, ferves as an occafion for inveigning veighing againſt the ingratitude of the world, and for introducing the miſery of the author:

> Sleep, like the world, his ready viſit pays
> Where fortune ſmiles; the wretched he forſakes.

Yet Young at the age at which he wrote his Night Thoughts was the ſame man in temper and intellectual habits as when ſo many years before he had publiſhed his Satires: exaſperated ſomewhat at the world, which had not rewarded him exactly in the mode and the degree which the author had apportioned to his own merits. He has ſtill that high reſpect for birth and rank which lead him to accumulate on himſelf all poſſible patronage by a ſeparate dedication of each of his Night Thoughts: he gives up the dignified ſeriouſneſs of his work to flatter, and almoſt to invoke, a Dutcheſs who had appeared at a maſquerade in the character of Night: he conſiders himſelf ſtill as a profeſſed author, and enumerates glory as one of his inducements to write. The ſame wit, the ſame imagination, the ſame antitheſis and epigrammatic point, appear in both theſe great works; and no other change ſeems to have taken place in his diſpoſition, than the natural effect of time on a temper, which ſhewed its diſcontent in his early life by ſarcaſtic animadverſion, and in age by melancholy complaint.

Dr. Goldſmith was a man the ſingularities of whoſe life are well known; and though they may not perhaps be diſcovered on a ſuperficial view, the traces of them are laid ſufficiently deep in his writings. Some of them being ſuch as he could not but know to be his faults, diſcloſe themſelves by his efforts to palliate and defend them; others are ſeen either through his ignorance of their exiſtence, or his ignorance of any mode by which they might be concealed. For that even Goldſmith made ſome attempts at concealing his ſingularities is I think evident from his ſtriking out of the Vicar of Wakefield the following, deemed by Johnſon a fine paſſage, which originally was in it. "When I was a young man, being " anxious to diſtinguiſh myſelf, I was perpetually ſtarting new " propoſitions. But I ſoon gave this over, for I found that " generally what was new was falſe." The only reaſon to be conjectured for his ſuppreſſing this was a conſciouſneſs that the fault ſpecified was the fault of his youth, and that the reaſoning which condemned it was not in his advanced age ſtrong enough to oppoſe his anxiety to diſtinguiſh himſelf, or to prevent its betraying itſelf in his converſation by dogmatical, ridiculous and paradoxical aſſertions.

Goldsmith has drawn all his principal perſonages aukwardly ignorant of the world, as if he had wiſhed to inſinuate that this quality is generally an aſſociate of virtue, and a neceſſary component part of an amiable character. His Good-natured Man, Young Marlow, and Vicar of Wakefield agree in this particular with each other, becauſe in this particular they all agree with the author himſelf. Goldſmith's plots and ſtories ſhew the very ſame quality: they uſually turn on incidents which an author who knew the world could never for a moment

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ſuppoſe would meet credit. Managers, who had more experience of the ill effects of violating dramatic probability, rejected his plays, and it is a ſure criterion of merit in a very high degree that, utterly incredible as his incidents are, his plays and his novels are ſuch favourites with the public. Goldſmith was envious: but he was envious through vanity, not through malignity. Indications of a benevolent heart appear every where in his writings: he rarely indeed praiſes any other author, but he ſhews no malice againſt thoſe he might have conſidered as his rivals. Whatever he may have borrowed he ſeldom quotes. Sometimes indeed he quotes himſelf, a circumſtance not ſo much to be aſcribed to a poverty of intellectual ſupply, as to a vanity which thought nothing better could be ſaid on the ſubject than what he had before given. Of this vanity he has left many other proofs; he diſapproves judging in literary matters by popular opinion: in his own caſe he will not ſubmit to it, and will force on the public in his printed play the ſcene which could not be tolerated in the repreſentation. Goldſmith did not ſtudy the powers of his mind, for the purpoſe of employing them with ſteadineſs on ſuch taſks as he could have executed with credit, becauſe he had ſo high an opinion of thoſe powers that he conſidered himſelf equally qualified for every taſk which might preſent itſelf. And it was natural for him who projected a journey to Aleppo to learn the Oriental arts, when he did not know any thing of the European ones, to write a poem with a profeſſed intent of deprecating evils, of whoſe exiſtence he in his preface expreſſes himſelf with much doubt. That *nullum ſcribendi genus non tetigit*, was the joint effect of

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his poverty, his vigour and his vanity: that *nullum quod tetigit non ornavit*, is the panegyric of a friend writing a terſe inſcription on his tomb.

By an induction of many particular remarks of the ſort I have here ſuggeſted ſome general obſervations might be drawn, as to the parts of an author's writing which may be ſuppoſed indicative of character, and as to the indications which they afford. When of many particular inſtances, all equally appoſite, one is ſpecially ſelected, that one will uſually be found to afford ſome indication of the author's habits and circumſtances, diſpoſitions and powers of mind. "Whoever," ſays Profeſſor Reid, "would "infer the inutility of logic from finding that men of good "ſenſe reaſon juſtly without rules, might as well infer, that "becauſe a man may go from Edinburgh to London by way of "Paris, therefore any other road is uſeleſs." This ſentence muſt appear to every reader deciſive as to the country of the author. When of ſeveral ſubjects, all equally important, one is more largely inſiſted on than the reſt, it muſt be becauſe that one is in ſome eſpecial manner accommodated to the prediſpoſitions of the author's mind, peculiarly congenial to his habits, or connected with his fortunes. If the exploits of Julius and Auguſtus Cæſar, as exhibited on the ſhield of Æneas, engroſs nearly one-half of Virgil's deſcription, we can have little doubt of the age in which Virgil flouriſhed, and the protection he courted or enjoyed.

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THE frequent recurrence of any one topic gives information of the ſame ſort. Milton often celebrates the muſic of the nightingale, for many of his nights were ſpent in ſolitary ſtudy, and he wooed *the nightly viſitations of his muſe.* Terence, who was himſelf a ſlave, has always produced on his ſtage ſome ſlave of eminent talents and addreſs to be the principal perſonage in his drama. Smollet, who was a ſurgeon in the navy, has generally preſented to us ſome naval incidents or naval characters, and makes a ſhip of war the frequent ſcene of his novels; while Farquhar, who had been in the land ſervice, has generally introduced a military man into his plays. It is natural for every man to ſuppoſe that thoſe circumſtances and ſituations will appear moſt intereſting to others in which he has found himſelf peculiarly intereſted; and an author judges wiſely when he prefers for his ſubjects thoſe modes of life with which he is beſt acquainted. When a critic, not very laviſh of his commendation, gives ſupereminent praiſes to particular paſſages, I have always, on examination, found ſomething in them which met his prejudices, his habits, or his temper. Johnſon, in his life of Congreve, ſays, that were he called on to point out the moſt beautiful paſſage in all Engliſh poetry, he knows not what he would ſelect in preference to the deſcription of the temple in the Mourning Bride. In his life of Dryden he tells us, that the deſcription of the different modes in which the Engliſh and the Dutch are, in the Annus Mirabilis, recorded to have paſſed the night after the engagement, is one of the faireſt flowers of Engliſh poetry. It is ſomewhat ſingular that theſe two paſſages expreſs nearly the ſame mental affection:—horror; dread of that melancholy which reſults from our own thoughts under ſtrong impreſſions of internal diſtreſs wrought upon by external circumſtances, and eagerneſs to eſcape from their oppreſſion or to remove them by ſociety. "Oh! ſpeak to me and let me hear thy voice, "my own affrights me with its echos," is the language of Almeria.

In dreams they frightful precipices tread,
Or ſhipwreck'd labour to ſome diſtant ſhore,
Or in dark churches walk among the dead,
They wake with horror, and dare ſleep no more

is the deſcription of the ſenſations of the Dutch. Any one who is acquainted with the character of Doctor Johnſon cannot be at a loſs for the circumſtance which imprinted the beauty of theſe paſſages ſo very ſtrongly on his imagination.

The comparative view of thoſe works of an author in which he has ſucceeded with thoſe in which he has failed would furniſh ſome information as to his diſpoſitions and habits. If Waller's verſes on the Protector excelled thoſe on the King, it is not ſufficiently accounted for by his remark that poets ſucceed beſt in fiction. If Dryden's plays are ſo much inferior to his other works, it muſt either have been becauſe he was ignorant of the nature of dramatic compoſition, or becauſe his neceſſitous circumſtances drove him to a taſk which he performed negligently. The preciſe modes of his failure may ſhew to which of theſe it

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is to be imputed. When he makes the Emperor of Barbary acquainted with Roman fables and allufions; when he introduces Cleomenes fpeaking of the Copernican fyftem, two thoufand years before its invention, thefe are evidently the faults of negligence. Dryden's neceffitous life is therefore fufficiently eftablifhed.

The country and time of an author ufually leave very fignificant marks in his writings. Mr. Wood has, with much ingenuity, afcertained from the direction in which certain winds in Homer's poems are faid to blow, and from the Ionian views he gives of the relative fituations of the Grecian iflands, that he was of a country eaftward of Greece: and works, which falfely pretend to be of great antiquity, feldom fail to betray themfelves by anachronifms. Thus Bentley urges againft the Epiftles of Phalaris, that they fpeak of *tragedies*, before tragedy had exiftence, or the name its modern acceptation; and Mr. Warton looks on it as decifive againft the poems publifhed by Chatterton, that they fpeak of *Stone-henge* as a *druidical temple*, a difcovery made by the laborious difcuffion of modern antiquaries, againft the affertions of antient chroniclers; and that they recommend, inftead of the abfurdity and impropriety of religious dramas, *fome great ftory of human manners*, an idea which muft appear to be the refult of tafte and difcrimination belonging only to advanced periods of fociety. The time and place when a particular work was compofed may fometimes be difcovered. From internal evidence the dates of Horace's Odes are, to a confiderable degree of precifion, fettled by his commentators. The original definition of *penfion* in the Englifh dictionary fhews that the work was compofed before the author had received that honourable mark of royal munificence; and the mention of fome cries peculiar to London, with fome other characteriftic circumftances, fhews that when Swift wrote his City Morning he was not refident in Dublin.

The favourite opinions of an author will, in fome way or other, force themfelves into his works. It is hard to fay into what fpecies of writing a deiftical writer will not be able to inftil the poifon of his prejudices; and it is unfortunate for the caufe of religion that its fupporters have not fhewn equal addrefs in infinuating and propagating the truth. Political opinions take fo ftrong an hold on the minds of Englifh authors that they almoft always bring themfelves into notice. Gray has, in his Elegy, fhewn us that Hampden, Milton, and Cromwell were, in his mind, the greateft perfonages in Englifh hiftory, and Mr. Horne Tooke makes, in his Ἔπεα πτερόεντα, frequent recurrence to thofe political fituations of his life to which we are indebted for this admirable grammatical treatife. The rank in fociety which an author holds is ufually difcoverable in his writings. Otway ufually makes poverty one of the ingredients of the diftrefs of his drama. Fielding defcribes with great fidelity the manners of the lower clafs, but fails whenever his ftories make it neceffary for him to bring his readers into thofe fcenes in which he had never walked himfelf; and perhaps to the want of authors of a

higher

higher rank it is owing, that we have ſo few juſt repreſentations of their manners exhibited on the ſtage.

The age of the author at which his ſeveral works were compoſed is generally diſtinguiſhable by the works themſelves. In juvenile compoſitions we have common-place remark, poetic mythology, extravagant ſentiment, and improbable ſtory. Scholaſtic information is all which their authors, at that period of their lives, have attained; and vivacity and fancy are their only excellencies. Hence it is that the juvenile works of all our poets have ſo great a ſimilitude to each other; for to a certain age the knowledge of all men differs only in degree, and not until after that does it differ in kind. Age and dignified experience ſupply information and mature the judgment. The paſtorals even of Pope fall far ſhort of the excellence of his other poems. To the praiſe of Swift's early good ſenſe let it be obſerved, that his firſt compoſitions are free from the uſual faults of immaturity, and almoſt entirely treat of topics connected with human life. Yet even in theſe poems we have an evidence that they were juvenile performances; for what but the licentiouſneſs of a juvenile mind, the propenſity to imitate without ſelection whatever has been admired, and to be taken with what is moſt dazzling, could have induced Swift to undertake the diſcurſive views, vehement tranſitions and florid diction of the Pindaric odes.

But it is now time to conclude theſe eſſays. I have done my duty to the Academy in enquiring into the ſubject they had

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recommended. It is of fuch importance and extent that I feel I have but touched lightly on a few of its principal topics. On this flight confideration I have found it fo interefting that I do violence to my inclinations in not purfuing it farther. I conclude it thus abruptly, for were I to profecute the enquiry to the extent of my own wifhes or the fubject's importance, I fhould offend againft the indulgence of the Academy and the patience of the Public.

ANTIQUITIES.

ANTIQUITIES.

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Some CONSIDERATIONS *on a* CONTROVERTED PASSAGE *of* HERODOTUS. *By the Right Honourable the Earl of* CHARLEMONT, *President of the Royal Irish Academy and F. R. S.*

HOW far the prevailing mode of philofophic fcepticifm may or may not have benefited mankind I will not pretend to determine, it being fufficient for my purpofe that its prevalence be allowed. Neither does this fafhionable wifdom content itfelf with the higher ranges of philofophical enquiry: it defcends even to criticifm and hiftorical refearches; and the modern wife man, deeming it below his dignity to follow thofe ancient guides by whom our forefathers have, perhaps too implicitly, been led, and prefuming on his own fagacity, fets up his bold guefs againft the relations of authors almoft contemporary with the facts they have afferted, and delights in proving, or endeavouring to prove, that he is more profoundly fkilled in the knowledge of antiquity than the ancients themfelves. There is perhaps no author who has fuffered more from this critical pre- Read July 7, 1790.

ſumption than that beſt and earlieſt of profane hiſtorians, Herodotus. This elegant and inſtructive writer, " *qui princeps*," as Tully ſays, " *genus hoc ornavit*," has of late years been the principal but of conceited criticiſm; his opinions have been controverted, and decried as abſurd; his aſſertions have been peremptorily contradicted; and this luminary, which had for ages been ſuppoſed to have thrown the moſt certain light on the dark hiſtoric times, has been diſcovered to be at beſt an ignis fatuus, while in its ſtead the bright ſun of modern erudition has been ſet up as ſufficiently luminous to enlighten the moſt remote and obſcure ages, by caſting its rays backwards into the depths of time. Whether I may not be too partial to an author who, during my Eaſtern voyage, was my conſtant and beloved companion, I will not pretend to ſay; but this I can ſafely aſſert, that though perhaps in thoſe circumſtances and opinions which he relates or adopts on the authority of others he may be often erroneous, wherever he ſpeaks from his own knowledge I have always found him a faithful guide; and in many inſtances, with ſome of which I may perhaps hereafter trouble the Academy, I have clearly diſcovered that the errors which have been imputed to him have proceeded, not from his fault, but from our ignorance of his true meaning; one of which miſconceptions, (for ſuch at leaſt it appears to me) ſhall be the ſubject of the preſent eſſay.

ROBINSON in his Diſſertation prefixed to Heſiod, and Mr. Muſgrave in a poſthumous work entitled " Two Diſſertations on Grecian Mythology," and many others, have bitterly inveighed againſt

againſt Herodotus for the opinion ſuppoſed to be conveyed in the following words, Lib. II. Cap. 53. page 129. Edit. Weſſel: Ἡσίοδον γὰρ καὶ Ὅμηρον ἡλικίην τετρακοσίοισι ἔτεσι δοκεω μευ πρεσβυτέρους γενεσθαι, και ου πλεοσι. οὗτοι δὲ εισι οἱ ποιήσαντες Θεογονίην Ἕλλησι, και τοισι Θεοῖσι τας ἐπωνυμίας δοντες, καὶ τίμας τε και τεχνας διέλοντες, καὶ εἴδεα αὐτῶν σημηναντες—“ For Heſiod and Homer, whom I believe to “ have exiſted four hundred years before me and no more, “ were they who formed a theogony for the Greeks, gave “ ſirnames to the Gods, diſtinguiſhed their honours and their “ functions, and inveſted them with their ſeveral forms.”

In the common acceptation of this paſſage, which theſe gentlemen ſeem to have adopted, nothing is more certain than that Herodotus is miſtaken. That religion, and Gods, together with their reſpective names, were known to the Greeks long before the times of Heſiod and Homer has been proved by ſundry irrefragable arguments; but, if no other proofs were to be had, the manner in which theſe poets ſpeak of the Gods, as of beings long ſince known, and worſhipped by the anceſtors of the generation then exiſting, would alone be ſufficient evidence to this point; and more eſpecially Homer, who clearly ſuppoſes every theological circumſtance of which he treats to have been commonly known at the time of the Trojan war, many years before he was born; and ſurely it would have been a ſtrange, abſurd and unaccountable anachroniſm in this great bard, if he ſhould have made his heroes invoke by name deities whoſe worſhip did not exiſt in their time, and whoſe names he himſelf had invented, little leſs, by the ſhorteſt calculation, than

a century

a century afterwards; yet, as I do not like to ſuppoſe an author, circumſtanced like Herodotus both in antiquity and in character, guilty of an opinion abſurdly erroneous, and conſequently do not like to diſſent from him in a matter, of which he muſt have been a much more competent judge than the moſt learned antiquarian of the preſent age, I ſhould wiſh, if poſſible, to diſcover ſome ſuch ſenſe of his words as might reconcile his opinion with what is evidently the truth, and might clear him from the ſuſpicion of abſurdity, an imputation which, from the general tenor of his writings, he ſo little deſerves. In order to this I ſhall endeavour to ſhew that, by the words of our author, it is not neceſſary he ſhould be underſtood to mean that Heſiod and Homer were the inventors, or even the firſt importers, of Grecian theology; but only that before their time, and previous to their writings, the Greeks poſſeſſed no regular ſyſtem of that ſcience, which was by them regulated, amplified and improved in all its ſeveral branches.

AND firſt, we are told by Herodotus that theſe poets *formed a theogony for the Greeks.* The word ποιησαντες may perhaps be conſtrued to mean *, as in ſome inſtances it does, not that a theogony was originally framed by them, but that they were the firſt who *poetized* upon this ſubject, or who gave to the Greeks a ſyſtem of theogony in verſe. But, to take the word in its more obvious acceptation, the aſſertion can mean no more

* Vide Stephani Theſaur. Art. ποιεω.

more than that they firſt traced and diſtinguiſhed the families of the Gods, or, in other words, gave a compleat and perfect ſyſtem of the divine genealogy. Neither does this opinion of our author appear by any means ill-founded, or even contrary to the ideas at this day adopted by many learned men. For though Muſæus is ſaid to have written upon the ſubject before the time of Heſiod, yet is this fact problematical; and the poetic treatiſe of the latter *, which has come down perfect even to our times, is, at the leaſt, a ſtrong preſumption in favour of Herodotus; eſpecially when we conſider that, whether right or wrong I will not preſume to determine, that primeval antiquarian is, as we ſhall preſently ſee, decidedly of opinion that all thoſe poets who are ſaid to have exiſted before Heſiod and Homer were in fact poſterior to their time.

The ſecond point aſſerted is, that theſe poets gave *ſirnames* to the Gods. And here I muſt premiſe that the ſenſe in which this paſſage is uſually underſtood, namely, that Heſiod and Homer

* I am well aware that though the Theogony of Heſiod be generally admitted to be genuine, ſome few critics, both ancient and modern, have ventured to ſuſpect that the poem which has come down to us is not the work of that moſt venerable bard. Among theſe, Pauſanias ſeems to doubt, when in his Bœoticks, Cap. xxvii. page 762, he ſays, " we know alſo that Heſiod, *or whoever in his name has written the Theogony*." And expreſsly declares his opinion, Arcadica, Cap. xviii. page 635, " that having " accurately read the Theogony of Heſiod, and certain verſes attributed to Quinus, " he doth not think either of them genuine." The authenticity of the poem is however generally confeſſed, and this paſſage of Herodotus ſeems to me a ſtrong proof in its favour, as it from hence appears more than probable that, at the leaſt, Heſiod had written a Theogony.

Homer were the firſt who aſſigned names to the Gods, muſt neceſſarily involve Herodotus in an abſurdity of which no author, even the meaneſt, can be ſuppoſed capable, by making him contradict what he himſelf has aſſerted a few lines before, where he tells us that *the Pelaſgians received the names of the other Gods, and laſtly that of Bacchus from Egypt, and the Greeks from them.* Nothing ſurely leſs than abſolute neceſſity ſhould induce us to ſuppoſe our author capable of ſo manifeſt a contradiction, and I hope to ſhew that no ſuch neceſſity exiſts. The word Επωνυμιας muſt be interpreted not *nomina* but *cognomina;* ſuch is undoubtedly its plain ſignification, and indeed our author himſelf, not many lines previous to the paſſage in queſtion, ſpeaking of the Pelaſgians, and of their early theology, clearly infers an eſſential difference between the Ουνομα and the Ἔπωνυμια—Ἔπωνυμίηνδ' ὲδ' Ουνομα εποιευντο ὀυδενι αυτεων. We may alſo obſerve that Ουνοματα is invariably uſed to expreſs the names received by the Pelaſgi from Egypt, and by the Greeks from them, while the appellations ſaid to be given to the Gods by Heſiod and Homer are alone diſtinguiſhed by the word Επωνυμιαι.

As the entire paſſage is curious, and may help to elucidate the ſubject of this eſſay, I will give its ſenſe at large, tranſlating as literally as I am able.

" Theſe Pelaſgi, before this time, as I learned at Dodona,
" praying to the Gods, ſacrificed all things to all in common,
" but

" but affixed to none of them either name or firname, for they " had no where been informed in this particular; but they " called them Gods from this cause, as aptly disposing of " all events and of all regions. But, after a long space of " time, they received the names (τα ουνοματα) of the other Gods " from Egypt, but that of Bacchus a long while after. And " some time afterward they consulted the oracle at Dodona " concerning these names (περι των Ουνοματων) for this oracle is " esteemed the most ancient among the Greeks, and was the " only one existing at that period. These Pelasgi then, con- " sulting at Dodona whether they should make use of those " names (τα Ουνοματα) which they had from the barbarians, the " Oracle answered that they should make use of them. And " so from that time they worshipped (or sacrificed—Εθυον) giving " those names (τοισι Ουνομασι) to the Gods; and afterward the " Greeks received them from the Pelasgi. But from whence " each of the Gods had his existence, or whether they have all " of them been from eternity, or under what forms, are matters " unknown until yesterday, as I may say. For Hesiod and " Homer, whom I believe to have lived four hundred years " before me, and no more, were they who formed a Theogony " for the Greeks, gave firnames (τας Επωνυμιας) to the Gods, " distinguished their honours and their functions, and assigned " to them their several forms. To these men the poets, who " are reported to have been prior, were, in my opinion, poste- " rior; and the first things which I have related the priestesses of " Dodona told me, but the latter, respecting Hesiod and Homer, " I myself assert."

 Thus

THUS we find, and muſt evidently infer from the context, that, though Herodotus may be perfectly right in ſaying that Heſiod and Homer were the firſt who gave to the Gods certain appellations which he terms Επωνυμιαι, it does not follow from thence that they were not before diſtinguiſhed by ſpecific names, Ουνοματα; neither can our hiſtorian, who poſitively aſſerts the contrary, be charged with any ſuch abſurd and falſe aſſertion. What theſe Επωνυμιαι were it is very difficult to explain, and I fear even to hint a wild conjecture that they might poſſibly have been the epithets * which Homer uſually annexes to the names of the Gods, and which ſeem to be ſtrictly appropriated; ſuch as the cloud-compelling Jupiter, the ox-eyed Juno, the far-darting Apollo; epithets which are ſometimes formed into names, and uſed as ſuch, as in the inſtance of Αργυροτοξος, bearer of the ſilver bow———

Κλῦθι μευ Αργυροτοξ, ὅς Χρυσην ἀμφιβεβηκας.

Or rather perhaps they may have been thoſe ſirnames given to the Gods, either from the place in which they were principally venerated, and worſhipped as tutelar deities, or from ſome peculiar and diſtinctive attribute; as Ζευς Λυκαῖος—Απολλων ὁ Επικουριος, the helper, &c. &c. And this conjecture is in ſome degree fortified by a paſſage of Pauſanias,—Arcadica, Cap. xxxviii. Page 679, where ſpeaking of ſeveral temples on Mount Lyceus, he

* Heſiod's Poem is alſo full of theſe epithets. For examples, vide the firſt twenty lines of the Θεογονια.

he mentions one of Apollo in these words:—Ἔστιν δὲ ἐν τοῖς πρὸς ἀνατολας του ὄρους Απολλωνος ἱερον, ἐπίκλησιν Παῤῥασιον—"There is in "the part of the mountain that looks eastward a temple of "Apollo, *sirnamed Parrhasius.*" And again, speaking of a temple of Eurynome in the country of the Phigalenses, he thus expresses himself. Arcadica, Cap. xli. page 684: την δε Ευρυνομην ὁ μεν των φιγαλεων Δημος Επικλησιν ειναι πεπίστευκεν Αρτεμιδος—"The "people of the Phigalenses believe that Eurynome is a *sirname* "of Diana." Now that Επίκλησις and Επωνυμία are words perfectly synonimous we know from the authority of Stephanus, who explains the former by the latter. Perhaps also I may be allowed to hazard another conjecture, which appears to me not entirely without foundation: The appellations given to the Gods by Homer and Hesiod may possibly have been no other than translations into the Greek language from the original names received from Egypt by the Pelasgi, and may have been considered as additional or sirnames, the old Egyptian appellations being still esteemed the real Ονοματα of the several divinities. Most of the names given by the Greeks to their deities have certain etymological meanings, which mark either the origin, or some essential and peculiar attribute of the Gods who are distinguished by them. Thus Αφροδιτη, Venus, is derived from Αφρος, spuma *, because she was supposed to have arisen

 from

* Of this derivation we are informed by Hesiod, Θεογονια, page 16, verse 194. The whole of this passage is so poetical, that I will endeavour to give a literal though very inadequate translation of it.

. . Beauteous

from the froth or foam of the ſea, and may poſſibly have been tranſlated from an Egyptian word of the ſame ſenſe and etymology. That the Greeks were accuſtomed to ſubſtitute tranſlations for Egyptian names we know to a certainty. Thus Chemmis or Chemmo *, which meant the city of Pan, was by them named Panopolis, and ON, the city of the ſun, was tranſlated

. . . . Beauteous and revered
Went forth the dame divine. Around, the graſs
Beneath her ſoft feet ſprang. Her, Aphrodite,
A Goddeſs Foam-begot, and Cytharea
With garlands crown'd, both men and gods have named,
For that from foam her nouriſhment ſhe drew,
And that Cythera firſt of lands received her;
And Cyprogene, as born within the bounds
Of billowy Cyprus, and Philomeda,
Partial to that from whence ſhe claims her birth.
New-born ſhe ſeeks the aſſembly of the Gods,
Uſher'd by Love, while fair Deſire attends;
Ev'n from the firſt this honour ſhe poſſeſs'd,
With men and Gods immortal doom'd to rule
O'er virgin converſe, ſmiles and wanton wiles,
Dulcet delight, friendſhip, and blandiſhment!

ὅτι γένλο πολυκλύσlω ἐνὶ Κυπρω muſt mean, " becauſe ſhe was born in the Cyprian Sea," and I have therefore tranſlated, *within the bounds of billowy Cyprus*. We have already been informed that the Goddeſs was born at ſea, and may therefore conclude that ſhe firſt aroſe ſome where near the coaſt of Cyprus, from whence the winds drove her to Cythera, where ſhe firſt landed. The appellations of Venus recorded in theſe lines may all of them have been Επωνυμιαι.

* τȣlο γαρ τους εγχωριους ȣ μονον αγαλμαlα πεποιηκεναι καlα πᾶν ἱερὸν, αλλα και πολιν επωνυμον καlα την Θηβαΐδα, καλȣμενην μεν υπο των εγχωριων Χεμμω, μεθερμηνευομενην δε Πανος Πολιν. Diod. Sic. Lib. i. page 21.

For ON, Heliopolis, vide Theophil. ad Autol. III. Vide alſo Cellarii Geog. Tom. II. Africa antiqua. page 35—6.

OM is a myſtical word in the Sanſcrit or ſacred Indian language. Vide Aſiatic Reſearches, page 242—Sir William Jones's Diſſertation on the Gods of Greece, Italy and India. " The lated Heliopolis. Neither was this cuſtom, which probably aroſe from their extreme delicacy of ear, and from a well-founded, though ſometimes perhaps ſaucy predilection for their own melodious idiom, confined to the Egyptian tongue. All other Eaſtern

" The Viſhnu, Siva and Brahma are expreſſed by the letters A.U.M, which coaleſce and " form the myſtical word OM. Whether the Egyptian ON, which is commonly ſuppoſed " to mean the ſun, be the ſanſcrit monoſyllable, I leave others to determine." And again, page 262, " I am inclined to believe that not only Criſhna or Viſhnu, but even Brahma and " Siva, when united, and expreſſed by the myſtical word OM, were deſigned by the firſt " idolaters to repreſent the ſolar fire." And afterwards, page 272, " The three powers, " creative, preſervative and deſtructive, which the Hindus expreſs by the triliteral word " O'M, were groſsly aſcribed by the firſt idolaters to the heat, light and flame of their " miſtaken divinity, the Sun; and their wiſer ſucceſſors in the Eaſt, who perceived that the " Sun was only a created thing, applied thoſe powers to its Creator."

Dioſpolis, Hermopolis, Heracleopolis, *Aphroditopolis*, and all the many other Greek names of Egyptian cities ſo formed, were probably tranſlations from the Egyptian. The firſt of theſe, Dioſpolis, is evidently tranſlated from the Egyptian and Hebrew name of this metropolis, AMON NO, or NO AMON, the city of Jupiter, which was indeed its only Eaſtern name, the appellation Thebes, ancient as it is, having been given to it by the Greeks, as we are informed by Diodorus Siculas, Lib. i. page 54, who, ſpeaking of its foundation by Buſiris, has theſe explicit words—Κτίσαι τὴν ὑπο μὲν Αἰγυπτίων καλυμένην Δίος πολιν την μέγαλην, ὑπο δὲ τῶν Ελληνων Θηβας. Neither can I avoid taking notice of the ſingularity of this circumſtance, from which it appears that even at a period ſo early as previous to the time of Homer, who mentions the Egyptian metropolis by the name Thebes, it was cuſtomary with the Greeks to give names of their own to foreign cities, and even to entire countries, ſince Herodotus informs us, Lib. ii. that anciently all Egypt was called Thebæ—παλαι αι Θηβαι Αιγυπτος εκαλεετο. In the time of the Ptolomies, when, from the wideſpread conqueſts of Alexander the Greek language was become univerſal, when that faſtidious people had every reaſon to look down upon all mankind as their inferiors, and when the ſovereigns of Aſia, and particularly of Egypt, were Greeks, ſuch tranſlations as we have already mentioned, from languages by them accounted barbarous, might naturally have been expected; but that at a period ſo early as that of the Trojan war, when Greece was yet in her infancy, and when the Greeks were far leſs poliſhed than the nations of the Eaſt, they ſhould have taken this impertinent liberty, appears to me ſurprizing, and even unaccountable; a liberty which has undoubtedly been miſchievous to poſterity, by ſuperadding confuſion to the natural and inevitable obſcurity of remote hiſtory.

Eastern languages were treated by them in the same manner; and the fact which we have asserted may be farther exemplified by their translation of Baalbec, the city of Baal, which in the Phœnician signified Lord, and was used as an appellation of the Sun, as the supreme Lord of that people, into the same more musical word, Heliopolis *. Such translations may possibly have been the Επωνυμιαι in question, and under this idea we may suppose that Homer in the Iliad may have made his heroes invoke the divinities by these translated names, as better adapted to

* A difficulty, however, which occurs respecting this last conjecture, must not be concealed. In the beginning of this book, Euterpe, page 105, Herodotus informs us that he journeyed to Heliopolis and Thebes in order to discover if the priests of these cities concurred in sentiment with those of Memphis, from whom he had hitherto principally received his information: "For," says he, "the Heliopolitans are esteemed the wisest among the Egyptians." He then proceeds to relate what he heard from them, excepting only that mysterious knowledge concerning the divine nature into which probably he had been initiated, and which consequently he was not at liberty to reveal. And here, among other Egyptian pretensions, he tells us that, *according to their report*, the Egyptians first made use of the *sirnames* of twelve Gods, which the Greeks derived from them—δυωδεκα τε θεων Επωνυμιας ελεγον πρωτους Αιγυπτιους νομισαι, και Ἑλληνας παρα σφεων αναλαβειν.—From whence we may perceive that the priests of Egypt, in their zeal to be accounted inventors and founders of all mythological science, arrogated to themselves the original use (or rather sanction, for νομιζω properly signifies *lege sancio)* not only of the *names* but of the *sirnames* also of the Gods. That Herodotus however does not give credit to this claim we may infer, as well from his afterwards ascribing the invention to Hesiod and Homer, as from the concluding words of the paragraph in question, where he expressly says that the *greater part only* of the Heliopolitan claims they demonstrate to be well founded—και τουτεων μεν τα πλεω εργω εδηλων ουτω γενομενα—which two passages taken together would induce us to suppose that the original use, or institution, of *sirnames* for the Gods, and their pretension that the Greeks had received such sirnames from them, was precisely that part of their claims for which he did not think they had any good foundation. What these Επωνυμιαι were, of which the Heliopolitan priests arrogated to their country the first use, it is impossible even to guess: but as they might *possibly* have been of the same nature with those of which our historian ascribes the invention to the Grecian bards, and cannot well be supposed to have been *translated names*, candour will not allow me to conceal a circumstance which might perhaps seem in some degree to militate against my last conjecture.

to the melody of his metre, and to the faſtidious ears of his countrymen, without incurring the cenſure of anachroniſm, though none but the original Egyptian appellations had been uſed or even known at the time of the Trojan war. Virgil may ſeem, at the firſt glance, to have been guilty of a ſimilar error, if ſuch it ſhould be deemed; his heroes ſpeak of the Gods by names which could not poſſibly be known to the Trojans, the Greeks, or probably to the ancient inhabitants of Italy. But then his heroes ſpeak a language alſo of which in their times they muſt neceſſarily have been ignorant, and conſequently the names of that adopted language are ſubſtituted for thoſe by which they had in reality invoked their deities, as the only appellations which could be intelligible to ſuch as poſſeſſed no other tongue but that in which he wrote. His Trojans ſpeak Latin, and therefore neceſſarily call upon their Gods by Latin names. A ſimilar apology cannot, however, be made for Homer; his language was probably the ſame in fact, allowing for ſuch alterations and improvements as would naturally be made in the time which elapſed between the Trojan war and his day, with that which was ſpoken by the Greeks, and, as ſome ſuppoſe, by the Trojans alſo, at the ſiege of Troy. But yet he may be ſurely allowed, without being liable to any great degree of cenſure, to make uſe of names for the divinities tranſlated into the vernacular idiom, both of his own time and of the period which he celebrates, from thoſe Egyptian appellations which were uſed in the age of his heroes; but had he made his heroes invoke by name Gods, who before his time were nameleſs, and to whom he himſelf had firſt given names, the

anachroniſm

anachronifm would then indeed be palpable, and without excufe.

Such are the conjectures that have occurred to me upon this very obfcure fubject, which however I only mention as fuch. In matters of antiquity fo very remote every poffible guefs may be allowed, and the antiquarian fcience feems exclufively to be entitled to the delightful privilege of building caftles in the air.

The third affertion of Herodotus is that Hefiod and Homer *diftinguifhed* the honours and functions of the Gods; for fo I interpret the word διελοντες. That is to fay, that whereas before the time of thefe bards no fpecific mode of worfhip, or fpecies of facrifice was allotted to each of the feveral divinities, and their tutelary powers were mixed and confounded, thefe poets regulated the tutelage and the functions of every feveral God, and affigned to each his particular mode of worfhip and of facrifice. And upon this part of our author's opinion I certainly need not dwell, as it is by no means abfurd to fuppofe that fuch was really the fact. Nothing can be more probable than that in the very early ages fuch confufion exifted in religious worfhip, and no perfons were more likely, both from their influence and fuperior knowledge, to inculcate and to fettle thefe regulations than the bards in queftion.

In the laft place our author afferts that the peculiar forms under which the Gods were pictured and adored were invented

by

by theſe poets. And this may certainly be true though the Gods had been acknowledged, named and worſhipped long before their time: neither is it improbable that the figures, by which the ſeveral divinities are known and diſtinguiſhed, may not have been in uſe before the period aſſigned to them by Herodotus. In the rude ages, when ſculpture, if known, was rarely and imperfectly practiſed, a ſtone unhewn, or at beſt but roughly cut or hammered, received the name of a god, and was worſhipped; and ſuch repreſentations are known to have deſcended even to the moſt poliſhed and enlightened ages of heatheniſm, being, as I ſuppoſe, reverenced as the firſt and original ideas under which the deity had been repreſented. Venus Urania, for example, is ſtill to be ſeen under the figure of a pyramidical ſtone on the reverſe of a Grecian coin of Caracalla, quoted by Triſtan, tom. II. page 220. Neither is this medal ſingular, as many others exiſt of different ages bearing the ſame impreſs. Pauſanias alſo informs us, Attica Cap. xix. page 44, that in his time the ſame goddeſs was worſhipped at Athens under a form nearly ſimilar, ταυ]ης γαρ σχημα, &c.; and the figure of this goddeſs, who was the ſame with the Paphian Venus, is accurately deſcribed by Tacitus, where he mentions the viſit paid to her temple by Titus Veſpatianus, Hiſtor. Lib. ii. page 198—"Simulachrum De non effigie "humana, continuus orbis latiore initio tenuem in ambitum, "metæ modo, exſurgens, et ratio in obſcuro." We are likewiſe told by Pauſanias, Bœotica Cap. xxvii. page 761, that the Theſpians from the beginning honoured Cupid principally among the Gods, and that their moſt ancient figure of him was *a white ſtone*—Θεῶν δε ὁι Θεσπιεῖς τιμῶσιν Ἔρωτα μαλιστα ἐξ αρχῆς, και σφισιν ἀγαλμα παλαιοτατον αργος λιθος.

If then we may be allowed to ſuppoſe that in the dark and rude ages of Greece thoſe elegant and peculiarly adapted figures of the Gods which have deſcended even to our times were not uſed or even known, there ſurely can be no abſurdity in ſuppoſing that ſuch figures, which from the beauty and harmony of their compoſition I may almoſt ſtile poetical, were the invention of theſe great and ancient bards, theſe bright luminaries by whom mankind was in all reſpects enlightened and influenced, and who, if they be allowed to have firſt diſtinguiſhed the *functions* of the Gods, may not improbably be ſuppoſed to have invented thoſe ſymbols by which ſuch appropriated functions were indicated; or, at the leaſt, that their writings may have afforded ſufficient hints for the compoſition and formation of the ſymbolical images which were afterward adopted. And the probability of this laſt ſuppoſition will be conſiderably increaſed, when we reflect that many of thoſe peculiar ſymbols and even features, by which the ſtatues and pictures of the Gods are marked and adorned, are particularly mentioned and deſcribed by Homer—ſuch as the thunderbolt and black brows of Jupiter, the large eyes of Juno, the bow and lyre of Apollo, the trident of Neptune, the œgis and blue eyes of Minerva, &c. Neither can it by any means be accounted improbable that ſtatuaries and painters may have framed their images and portraits upon the ideas of Homer, ſince we are informed that, in an age long after the exiſtence of that inſpiring bard, from his

his ſublime deſcription Phidias * caught the noble idea, which enabled him to form his great maſter-piece, the Olympian Jove. Perhaps alſo I may be allowed to mention, as in ſome ſort favourable to this conjecture, a paſſage of Strabo, who, Lib. viii. page 593, cites the following ancient ſaying—Κομψῶς δ' εἴρηται και το, ὁ τὰς τῶν Θεῶν εικονας, ἤ μονος ιδών, ἤ μονος δειξας—" It is wittily " ſaid of Homer that he alone ſaw the forms of the Gods, or he " alone ſhewed them." Indeed, though from the deſcriptive manner in which Homer every where ſpeaks of the Gods we might naturally be induced to ſuppoſe that he deſcribes them from images ſuch as now exiſt, and which were frequent in his time, yet as there is ſome reaſon to ſuſpect, not only from the

 authority

* For the verification of this ancient tradition ſee Valerius Maximus, Lib. iii. page 314; Macrobius, Lib. v. Saturnalium, Cap. xiii.; but above all Strabo, Lib. viii. page 543.—Απομνημονευουσι δε του Φειδιου, &c.—" It is recorded that Phidias, being aſked by Pandænus (or rather Panænus) what archetype he had choſen to imitate in expreſſing the image of Jupiter, anſwered, that which is propoſed in theſe verſes of Homer, Iliad i. verſe 528:

Ἠ, και κυάνεησιν ἐπ' ὀφρυσι νεῦσε Κρονίων,
Αμβρόσιαι δ' ἄρα χαῖται ἐπεῤῥώσαντο ἄνακτος,
Κρατὸς ἀπ' ἀθανάτοιο, μεγαν δ' ἐλέλιξεν Ολυμπον.

Euſtathius alſo, in his note on this inſpiring paſſage, informs us that not only Phidias formed his Jupiter upon this pattern, but that Euphranor alſo copied the ſame idea in his famous picture of Jove. In refutation of this ſentiment of all antiquity the redoubted Scaliger, Virgil's obſtinate champion, at once cuts ſhort all authority by the following acute obſervation: " Aut ludunt Phidiam, aut nos ludit Phidias; etiam ſine Homero puto illum ſciſſe Jovem " non carere ſuperciliis et cæſarie." Matchleſs aſſurance! A modern hypercritic, with his inconcluſive, flat and vapid witticiſm againſt all antiquity! But ſuch is the uſual triumph of modern ſagacity!

For an accurate deſcription of this ſtatue, which was counted among the wonders of the world, vide Pauſ. Eliac. Prior. Cap. xi. page 400.

authority of Herodotus, but from other concurrent circumſtances, that previous to his writing no ſuch repreſentations exiſted, the probable alternative will be that his poetical portraitures were in effect the archetypes from whence, either in his age or in that immediately ſucceeding, the images of the Gods were compoſed and formed. Indeed there is ſcarcely a divinity in all the hoſt of the Grecian heavens whoſe ſymbolic form, ſuch as it has been handed down to us by ſtatues, we may not diſtinctly figure to ourſelves by an attentive conſideration of his expreſſive epithets and pictureſque deſcriptions.

And here I will, with the utmoſt diffidence, take the liberty of ſuggeſting a circumſtance which, if it were founded in fact, would ſtrongly operate in favour of this laſt-mentioned opinion of Herodotus. My recollection does not enable me poſitively to aſſert, yet I do not believe that Homer in any part of his writings actually deſcribes as an idol any ſtatue of the Gods. The deities themſelves he frequently paints to our imagination in the moſt lively colours, but no where, that I recollect, enters into any detailed deſcription of their repreſentations. If this be the fact, the preſumption will undoubtedly be ſtrong that no idol worthy of being deſcribed exiſted previous to his time, or at the leaſt during the period of which he treats, ſince aſſuredly innumerable opportunities muſt have offered themſelves in the courſe of his poems for diverſifying and enriching them by ſuch deſcription. The only inſtance that I recollect either in the Iliad or Odyſſey where any mention is made of an an idol is in the fixth book of the former, verfe 237, &c. where the Trojans having been hard preffed by the valour of Diomed, Hector is enjoined by Helenus, as high prieft, to repair to Troy, and there to direct the Queen that a folemn offering of gifts and vows be made to Minerva, in order to propitiate the angry goddefs. Hector obeys, and Hecuba, as inftructed by him, chufes from her wardrobe the richeft and moft beautiful veil, which fhe carries, with a fplendid proceffion of matrons, to the fane of Minerva, and, through the means of the prieftefs Theano, lays it at the knees of the Goddefs—

> Ἡ δ' ἄρα πεπλον ἑλȣσα Θεανω καλλιπαρηος
> Θηκεν Ἀθηναιης επι γȣνασίν ἠϋκομοιο.

Here we have an idol, which, however wholly undefcribed either with regard to its form or material, we may fuppofe, from the mention of its knees, to have been a human figure; and this idol was probably no other than the fatal and celebrated palladium, as may be inferred as well from the part of the city in which its fhrine was placed—ἐν πόλει ἄκρῃ—in the citadel, as from the epithet ἐρυσιπτολι, guardian of the city.

> Πότνι' Ἀθηναίη, ἐρυσίπτολι,* δῖα Θεάων.

"O! guardian

* The beauty of this expreffion, which, in my opinion, means *Goddefs among Goddeffes*, feems totally loft in the diffufe tranflation of Pope:

> "Oh awful Goddefs, ever dreadful maid,
> "Troy's ftrong defence, unconquer'd Pallas, aid!"

O! guardian of theſe walls, Pallas revered,
Divine of Goddeſſes!

Now the palladium, if we may credit the deſcription of Apollodorus, Lib. iii. Cap. xi. was an idol little reſembling thoſe ſtatues of Minerva, which may be ſuppoſed to have been afterwards formed on the plan given by Homer, being a figure apparently in the Egyptian ſtyle, with its feet joined, for ſo the words τοῖς δὲ ποσι συμβεβηκὸς are underſtood by the beſt commentators, and indeed little more than a block of ſtone, or rather wood, cut out into ſomething like the human form; a ſpecies of ſculpture, which, conſidering the age, when there was probably no other taſte to be imitated but that of Egypt, was very likely to have prevailed. That it was a ſitting figure ſeems to be marked by the veil having been laid either at or upon its knees, for επι is capable of both ſignifications; and it muſt have been made of ſome light material, ſince Diomed could ſteal and carry it away, which circumſtance induces me to ſuppoſe that it was of wood, as Apollodorus gives it the height of three cubits, a proportion which in ſtone would render ſuch carriage impoſſible. This miraculous image is ſaid to have come down from Heaven, a fable which would ſeem to indicate that it was not the manufacture of Troy; and perhaps the idea of its heavenly origin may have been derived from its having been brought from Egypt, the great ſource of religion in remote ages, by ſome one of the early coloniſts * who ſettled

* I would here wiſh to be underſtood as alluding to the ſecond colonization from the Eaſt, which took place ſoon after the extinction of the family and empire of the Titans, when Cecrops

ſettled in Greece and the neighbouring countries of Aſia Minor, and began to introduce into thoſe ſavage regions ſomething like religion and manners.

In the deſcription of the ſhield of Achilles there is alſo an inſtance where figures of the Gods are mentioned, and that in a manner ſomewhat nearer approaching to our idea of divine repreſentations. In the compartment relating to a beſieged city a ſally is made headed by Mars and Pallas, which deities are perſonified and deſcribed in the following beautiful lines:

. . . . ἦρχε δ᾽ ἄρα σφιν Ἄρης καὶ Παλλας Ἀθηνη,
Ἄμφω χρυσείω, χρυσεία δὲ εἵματα ἕσθην,
Καλώ καὶ μεγαλώ συν τευχεσιν, ὥς τε θεω περ,
Ἀμφίς αριζηλώ. Λαοι δ᾽ ὑπολιζονες ησαν.

Here however we are to obſerve that, as the poet ſpeaks of a work of divine fabrication, and conſequently ideal, he may be allowed to give free ſcope to his fancy, and, without incurring the cenſure of anachroniſm, to paint his Gods in a ſtyle which did not exiſt in the age of his hero; neither does he mention theſe two magnificent figures as idols, but complying with the received opinion that the martial deities frequently aſſiſted their

Cecrops and Danaus from Egypt, and Cadmus from Phœnicia, ſettled in Greece, and introduced the uſeful arts, together with the worſhip of thoſe deities who in their reſpective countries preſided over them. Thus Cecrops introduced into Attica the cultivation of the olive and the worſhip of Minerva, who was adored in his native city, Sais, as the donor and patroneſs of that uſeful tree.

their friends in combat, and perhaps allegorically intimating that the party was conducted by Strength and by Prudence, he boldly perſonifies the patron and patroneſs of war according to his own ſublime conception of the ſuperior beauty and ſtature of Gods; an idea which, among many others, may be ſuppoſed to have given riſe to thoſe divine repreſentations which were afterward framed.

An inſtance alſo, where mention is made by Homer * of ſtatues, though not of Gods, occurs in the Odyſſey, Lib. vii. verſe 90, &c. where in the deſcription of the palace of Alcinous we find not only dogs of gold and ſilver, but golden boys holding torches. The dogs, however, which were endowed not only with life, but with immortality and perpetual youth, were the workmanſhip of Vulcan, and conſequently may be deſcribed as approaching nearer to the life, for that is the meaning of the miraculous endowment above-mentioned, than could have been expected from any mortal ſculptor of the age. The boys alſo, though nothing is ſaid of their origin, may poſſibly be ſuppoſed to have come from the ſame ſhop; and, if they were equal in elegance to the torches they bore, which at that time were probably

* It is ſomewhat ſingular that in all the writings of Homer there ſhould not be, that I can recollect, any word expreſſive of *ſtatue.* Αγαλμα indeed frequently occurs, but this word had not as yet obtained that ſignification, being only uſed to mean *ornamentum*, *oblectamentum*, vide Steph. Theſ. And that even in ages very far poſterior it did not neceſſarily convey the idea of a ſtatue, but, like our word *idol*, might mean any repreſention of a God, however diſtant from the human form, is evident from the paſſage of Pauſanias already quoted, where, ſpeaking of the Theſpian Cupid, he ſays, *και σφισιν αγαλμα παλαιοτατον αργος λιθος.*

probably branches of pine, or ſome other reſinous tree, I do not conceive that the divine workman had much reaſon to be proud of his work. But be that as it may, we are here to take notice that Phœacia, the ſeat of theſe miracles of art, was, as the learned and ingenious Mr. De Goguet * has well proved, an iſland of Aſia, where undoubtedly the arts were arrived at a greater degree of perfection than was known in European Greece.

THESE are the very few inſtances where, as far as I can recollect, Homer ſpeaks of ſtatues, which I ſhould therefore ſuppoſe were, at the time of the Trojan war †, extremely rare

 in

* Liv. ii. cap. i. page 84, in the note. Phœacia is uſually ſuppoſed to be the iſland of Corfu, where certainly all this magnificence could ſcarcely have been expected. And yet, even though we were to adhere to the common opinion, it may be ſaid that Homer in this inſtance ſeems to have indulged his imagination more perhaps than in any other part of his writings, and to have accumulated on this favourite ſpot every idea of ſplendour which his extenſive travels had enabled him to collect throughout the more refined and ſumptuous regions of Aſia.

† There is yet another reaſon which would induce me to ſuppoſe that, in the times of which we treat, ſtatues were in effect extremely rare in Greece, and that, if any were really wrought there, they muſt have been of the rudeſt form and workmanſhip, namely, the want of proper tools. We are told, it is true, that Dædalus and his nephew Talus, names which however appear apocryphal, invented the plane, the ſaw, the gimlet, the ſquare, the levelling plummet and the compaſs, yet from the ſilence of Homer, who is apt to tell all he knows reſpecting many of theſe inſtruments, there is ſome reaſon to ſuſpect that they did not all of them exiſt even in his time. When Calypſo, whoſe divine power might certainly have furniſhed her lover with the beſt implements then in uſe, provides tools for Ulyſſes, to enable him to build his ſhip, ſhe gives him nothing but a two-edged hatchet, a plane, a gimlet and a rule or ſtraight edge; and if the joiner or ſhipwright was ſo ill provided, in how much worſe a ſituation muſt the ſculptor have been, whoſe work is ſo much more delicate and difficult of execution? Such tools as theſe are indeed ſo inadequate to the forming a human figure out of any material, that I ſhould

in Greece; and, as one of theſe only is mentioned as an idol, I ſhould, if I may be allowed to hazard a conjecture, which I deſire may be received merely as ſuch, be inclined to think that the worſhip of any thing in the human form was yet novel * and unuſual in that half-civilized region. It may indeed

I ſhould be tempted to ſuppoſe that whatever idols of this kind exiſted in Greece were of foreign workmanſhip, and had been brought thither from Aſia, and particularly from Egypt. That in the earlier ages, long antecedent to the Trojan war, ſculpture was unknown in Greece, has already been made probable, from the ſort of idols then in uſe, and we ſhall find in the next note the Egyptian Danaus, who probably had neglected to bring with him any idol from his native country, compelled to content himſelf with conſecrating, as a ſymbol of Minerva, in her temple at Mindus, a rough block of wood.

* I muſt not however conceal that there are ancient authorities, not only for the exiſtence of ſtatues during the age of the Trojan war and even previous to it, but for ſuch ſtatues having been uſed as idols. Pauſanias, Achaica, p. 531. cites a tradition, which however he only mentions as ſuch, that the temple of Juno at Samos was built by the Argonauts, and that the ſtatue, τό αγαλμα, of that goddeſs was by them tranſported thither from Argos. His own opinion however is, that this temple muſt have been extremely ancient, as the image therein contained is the work of Smilis of Egina, who was contemporary with Dædalus, though leſs illuſtrious—He then proceeds to ſpeak of Dædalus the Athenian, generally accounted the moſt ancient of ſtatuaries, who executed, ſays he, Αγαλμάτα, which here muſt mean *carved works*, for Minos and for his daughters, *as Homer informs us in the Iliad.* The paſſage of Homer here alluded to is in the deſcription of the laſt compartment of the ſhield of Achilles, where the dance is repreſented, Lib. xviii. V. 590. The lines are as follow:

Ἐν δὲ χορὸν ποίκιλλε περικλυτὸς Ἀμφιγυήεις
Τῷ ἴκελον, οἷον ποτ' ἐνὶ Κνωσσῷ εὐρείῃ
Δαίδαλος ἤσκησεν καλλιπλοκάμῳ Ἀριάδνῃ.

This Dædalus is ſuppoſed to have flouriſhed in the time of Hercules and Theſeus, forty years before the Trojan war; but modern ſagacity has diſcovered that the ancients were erroneous in aſcribing ſtatues to him, an error into which they have been led by confounding this very ancient perſonage with Dædalus of Sicyon, who was indeed a ſtatuary, but who lived many ages after his nameſake of Athens. This aſſertion of the moderns againſt the ancients, and particularly againſt Pauſanias backed by Homer, whoſe meaning he *probably* underſtood, may poſſibly be true, indeed perhaps be matter of doubt whether, in the earlier ſtages of ſociety, the human form would not be the laſt of all others to be worſhipped. Independency of man on man is the conſtant and peculiar attribute of the ſavage ſtate, and men would not be apt to love, ſtill leſs to venerate and worſhip, thoſe fellow-creatures with whom they deemed themſelves on a perfect equality, and from whom they were in continual dread of hoſtility. Nor, on the other hand, would they chuſe to confeſs that they feared them; and, upon theſe principles, they would no doubt wiſh to annex the ideas of ſuperiority, love, awe and worſhip, to any thing rather than to one of their own ſpecies. Neither even in the ſecond ſtage of ſociety, when the hordes of ſavages had deemed it neceſſary in ſome degree to depart from their native rights by chuſing from among themſelves a commander, would ſuch precarious

 and

true, but is much too bold for me to rely on in corroboration of my idea. It may alſo poſſibly be ſaid that the word ἤσκησεν may ſignify that Dædalus had *compoſed* the dance for Ariadne, rather than that he had executed a repreſentation of it in carving, and conſequently that he was rather a maitre de ballet than a carver; but as Pauſanias, a tolerable judge, has evidently taken the word in the latter ſenſe, I muſt confeſs myſelf decided by his opinion, and admit that Dædalus the Athenian made carved works.

With regard, however, to the Juno of Samos, the poet Callimachus, as quoted by Euſebius in his Evangelical Preparation, ſays that it was the work of Celmis,' one of the Idæi Dactyli, who firſt taught the uſe of iron, and adds, that before his time the art of ſtatuary was unknown, and that Juno had been previouſly repreſented by a rough plank or piece of wood, Σανις, as alſo was the Minerva conſecrated by Danaus, in the city of Mindus.—This laſt circumſtance I mention as it ſerves to corroborate what I have ſaid in the text concerning the very ancient manner of repreſenting the Gods. I have there endeavoured to ſhew, that the earlier idols were no other than ſtones roughly hewn, and here we find the divinities ſtill farther debaſed when repreſented by planks or blocks of wood.

and limited chiefs * as yet obtain any confiderable fhare of refpect or reverence; until, at length,—the power of thefe chieftains gradually increafing by the natural effects of continued command, by fuccefsful wars, and a confequent acceffion of fuch fubjects as, from having been conquered by them, would be more immediately their vaffals, prompt to obey every arbitrary order, efpecially againft thofe new fellow-fubjects who had helped to vanquifh them,—they would become real, independent and abfolute monarchs; and then, but not until then, would begin to be confidered by thofe over whom they ruled as fomething more than human, and of a fpecies far fuperior to themfelves; from which ftate of fociety would naturally arife the worfhip of man, and confequently of the human figure.

There is yet another confideration which might perhaps cooperate to incline men, in their early ftate, to prefer even the worfhip of animals to that of each other. However fuperior the faculties of man, though uncultivated and wholly neglected, may be to thofe of the brute creation, fuch endowments, being rendered habitual to us by poffeffion and ufe, would be in a far lefs degree objects of our admiration than thofe inferior powers, which nature, through the means of inftinct, has allotted to brutes. It is only by reflection and philofophic enquiry that we come to appreciate our own fuperiority, and, to

* That at the time of the fiege of Troy the regal power, both among the Greeks and Trojans, was extremely limited, has been fully proved by the ingenious Monfieur de Goguet, Origine des Loix, Seconde Partie, Article vii.—and in many other parts of his excellent work.

to a race of men incapable of either, the ſagacity of a dog would appear more ſurprizing than the effects and efforts of their own untutored reaſon. Neither can this preference, however whimſical, be deemed unnaturally abſurd by us, who, even at this day, in the pride of our wiſdom, are more apt to admire the tricks of a monkey than what we are in the hourly uſe of ſeeing performed by our own ſpecies. The uſefulneſs alſo of animals would be contraſted with the miſchievous and inimical qualities of man to man. The cow would be worſhipped for her milk; and, in a nation addicted to hunting, the primitive occupation of mankind, the dog would be adored as the inſtrument of their favourite ſport; and the ſure means of providing food. That ſagacity of ſmell, by which he is enabled to purſue his unſeen game, would be deemed more uſeful, and far more admirable, than the faculty of reaſoning to which men were habituated, and which, for want of improvement and exertion, would be, in effect, little ſuperior to inſtinct, and far leſs certain in its operations.

ANOTHER cauſe may yet be added which might not have been wholly inoperative:—Even in an age of the groſſeſt ignorance ſome men muſt have exiſted ſuperior in intellect to their fellows. Theſe comparative philoſophers, on whom the regulation of national religion would naturally devolve, either from ſome remaining trace of tradition, or from the ſuperior ſtrength of their own underſtanding, would have been apt to frame idols to be adored rather as typical repreſentations of thoſe divine qualities, which even ſavages could not fail to attribute to the ſupreme being, than as real Gods to be perſonally worſhipped;

and

and for this purpofe they would probably have preferred animals to men, not only as lefs adverfe to the prejudices of their countrymen, but as better adapted to exprefs thofe attributes of which they were meant to be emblems. Such beafts as were peculiarly poffeffed of certain qualities to the exclufion of others would naturally be chofen in preference to man, in whom, though in a lefs ftriking degree, all thofe qualities were united. Thus the wifdom of the deity would be figured by the fox or the ferpent, his omnipotent might by the lion or tiger, and his beneficence by the cow or the fheep. And indeed we may perceive how inadequate the human figure was ultimately found to reprefent the attributes of the divine nature by the neceffity under which the barbarous nations laboured, and ftill labour, of making monfters of their human idols; nay, even in the moft enlightened times of idolatry, and among the moft ingenious and polifhed people, it was found neceffary to affociate and connect animals with the figures of their Gods in order to make out their fymbolical meaning *: And thus we have the eagle of Jupiter, the peacock of Juno, the owl of Minerva, and the wolf of Mars.

Such are the caufes which may be fuppofed to have been favourable to the precedency of animal worfhip; while, on the other hand, thofe flavifh ideas, on which the adoration of our fellow

* Since in matters fo profoundly obfcure as thofe of which we now treat every poffible guefs is allowable, perhaps I may be permitted to hazard a conjecture that thefe affociated animals might have been the original forms under which the powers of the refpective Gods, with whom they are invariably connected, had been worfhipped previoufly to their having been endued with the human figure.

fellow creatures was originally founded, and which would render the tranſition eaſy of worſhipping that being when dead, to whom, while alive, men had been accuſtomed to proſtrate themſelves, could not, as we have already mentioned, exiſt in the earlier ſtage even of monarchy; neither would gratitude, the ſecond probable inducement, as yet operate, ſince the principal objects of that gratitude, the invention of uſeful arts, and even the inſtitution of beneficial laws, muſt neceſſarily have been the reſult of time and experience, and cannot be ſuppoſed to have taken place until ſome conſiderable time after communities had been formed. The ſcale then of idolatry would probably be thus graduated:—When the traces of original revelation had been confuſed and well nigh obliterated, nothing remaining but the univerſal traditionary belief in ſomething ſupreme to which homage was due, and mankind, ceaſing to adore one inviſible God, had begun to ſeek for deities among his creatures, the firſt objects of adoration would undoubtedly be the great and glorious phenomena of nature, and firſt of all the Sun,

——— that with ſurpaſſing glory crown'd
Looks from his ſole dominion, like the God
Of this new world.

His dazzling light, beyond the capacity of the human organ, would be admired with aſtoniſhment, his genial heat would be felt with grateful acknowledgment, and his benign influence on the vegetable world would ſpeedily be underſtood and acknowledged even by ſavages. The awful majeſty of the

the Moon, by whofe mild fplendour nature is relieved from the comfortlefs and unferviceable gloom of night, would next attract the admiration of man, which would be gradually extended through the reft of the heavenly orbs; neither can we be furprifed that thefe high placed objects, feated, as it would feem, to be adored, within our ken indeed, but far beyond the reach of our infpection, fhould firft attract the admiration and confequent worfhip of the infant world, when we reflect how fhort a time has elapfed fince mankind have ceafed univerfally to concur in allowing them a fuperior and controlling influence over human affairs. *Animal worfhip, for the reafons already affigned, and becaufe the inconftancy of our nature, *unfteadied* by revelation, would prompt us to wifh for change even in our Gods, would probably be the next in fucceffion, and laft of all man would bow down to man.

THAT fuch has been the actual progrefs of idolatry we have reafon to believe from the lights, faint and uncertain as they are, which hiftory throws upon this obfcure fubject. That the fun and moon and ftars were the original objects of adoration in the moft

* Sanconiatho feems to fuppofe that plants, for the deification of which the Egyptians long afterwards became infamous, were objects of worfhip in the earlieft age. "But thefe firft men, "(fays he) confecrated the plants fhooting out of the earth, and judged them Gods, and "worfhipped them, upon whom they themfelves lived, and all their pofterity, and all before "them; to thefe they made meat and drink offerings." *Tranflation by Cumberland*, page 7.

By *thefe firft men* he means men in the earlieft times, probably during his ten firft generations. If there ever was a time when men lived entirely upon vegetables, thefe, as their only aliment, might, on account of their utility, have been deified; and, if fuch cuftom ever exifted, it muft probably have been in the world's infancy.

moſt remote ages among the very ancient Chaldeans and Egyptians appears to be a fact well ſupported by hiſtorical proof; and how very early this primitive ſuperſtition gave place to the worſhip of animals among the laſt mentioned people, we know from the moſt uncontrovertible evidence, being informed, by ſacred authority, that the Iſraelites, in imitation undoubtedly of thoſe maſters from whom they had been lately emancipated, erected in the wilderneſs, not a repreſentation of the ſun, nor yet a human idol, but a golden calf.

EGYPT indeed, where, as Cicero tells us, "* Omne ferè genus "beſtiarum conſecraverunt," ſeems to have been the original and copious ſource of this ſpecies of idolatry, which ſtrange propenſity, in a people excluſively celebrated for their wiſdom, has been, both by ancient and modern writers, generally, though perhaps unjuſtly, aſcribed to the pre-eminent and boundleſs ſuperſtition of the Egyptians. I ſay, *perhaps unjuſtly*, ſince the ſuppoſition appears to me by no means improbable that it may have taken its riſe rather in the peculiar genius

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* De Natura Deorum, lib. iii. cap. xv. tom. 2. Edit. Oliv.
VIRG. ÆNN. viii. Omnigenumque Deum monſtra, et latrator Anubis.

JUVENAL, SAT. XV. Quis neſcit, Voluſi Bithynice, qualia demens
Ægyptus portenta colat? Crocodilon adorat
Pars hæc, illa pavet ſaturam ſerpentibus Ibin.

Ib. O Sanctas Gentes! quibus hæc naſcuntur in hortis
Numina——

of that people, extremely addicted, as we well know, to emblem and ſymbolical repreſentation, than, as is commonly thought, in the prepoſterous and unaccountable deſire of multiplying their gods under the moſt humiliating and degrading forms; which conjecture, if it ſhould be allowed any weight, will, in ſome meaſure, free us from the difficulty under which we labour of being compelled to ſuppoſe that the moſt enlightened nation of all antiquity was alſo the moſt abſurdly ſuperſtitious. The concealment of truth under appoſite emblems was a favourite and faſhionable wiſdom of the remote ages, and from a marked ſuperiority in this ſcience Egypt had perhaps principally obtained the univerſal character of wiſe, ſo that poſſibly the very practice, which appears to us the reſult of folly, may in effect have been derived from what, in thoſe times, was denominated ſuperior wiſdom.

At what preciſe period the human form began to be worſhipped is no where, that I know of, aſcertained; but I cannot avoid thinking that this ſpecies of idol, though of high antiquity, is of later date than animal repreſentations*. Many Egyptian deities, it is true, have come down to us in the human

* Idolatry appears indeed to have adopted the human figure by degrees, ſince in very early times, and among ſome of the earlieſt nations, we find idols compounded of man and animal. Thus Dagon, a ſupreme goddeſs among the Philiſtines, is ſuppoſed to have been formed like our idea of mermaids, half woman, half fiſh. The Egyptian Sphinx is alſo of this kind, woman and lion. But the moſt whimſical compoſition is that of the Canopus, woman and jug.

human ſhape; but, as the ſame deities have alſo been repreſented under the figure of animals, I ſhould be apt to give theſe the priority, as well for the reaſons already mentioned, as becauſe, in effect, the idols of this kind uſually bear evident marks of ſuperior antiquity. Thus Oſiris and Iſis are ſometimes repreſented as a man and woman, but they are alſo repreſented as two ſnakes intertwiſted *, which latter I ſhould

* Such at leaſt is the interpretation I give to a very ancient and curious baſſo relievo in my poſſeſſion. It is of green porphyry, and was brought by me from Egypt. There are on the heads of the ſnakes, ornaments which differ from each other, and are proper to the divinities they repreſent. That in times early indeed the ſupreme God among the Egyptians was repreſented by a ſerpent we learn from Sanchoniatho, who tells us, page 14 of Cumberland's tranſlation, "that the God called by the Phœnicians the good Dæmon, Ἀγαθοδαίμων, is named by "the Egyptians Kneph, and they draw him as a dragon or ſerpent, but put on him a hawk's "head." Oſiris is alſo often found in a human figure with the head of a hawk, whoſe ſharpſightedneſs and rapid flight were meant to indicate the ſun, which was undoubtedly repreſented by this god, as the moon was by his wife and ſiſter Iſis. Probably alſo the hawk's head might have been given to him as ſupreme among the Gods, in imitation of the Kneph above mentioned, and for the ſame reaſon he might have been figured by the ſerpent. Theſe two principal deities among the Egyptians may poſſibly ſerve in ſome degree to illuſtrate that graduation of idolatry which I have ſuppoſed. Oſiris and Iſis, though in proceſs of time they came to be taken for almoſt all the divinities, were originally no other than the ſun and the moon, which luminaries were probably firſt worſhipped in their real ſubſtances, until by degrees they began to be repreſented by animals ſignificant of their qualities. Half-human monſters next took place; and laſt of all human figures, decorated, or rather explained, by various emblems.

Apis, which was probably meant for the ſymbol of cultivation, ſtill retained his animal ſhape, though he alſo was, in more modern times, modelled according to the novel faſhion, being in ſome inſtances figured with a human head.

Anubis alſo, who was probably at firſt repreſented by a dog, in alluſion to the dog-ſtar, the propitious precurſor of the Nile's increaſe, gradually grew into the form of a man, with the head of that animal.

The baſſo relievo, mentioned in the beginning of this note, will, I think, ſerve to explain a very be tempted to ſuppoſe the original idols, while the former were probably the fruit of Grecian conqueſt, and conſequently not more ancient than the time of the Ptolemies, under whoſe empire Egypt is known to have received a tincture of Grecian manners and taſte, which however ſhe mixed with her own; and indeed I have never yet ſeen any image of this ſort in which, through the Egyptian ſtyle, the Grecian ſculpture was not eaſy to be diſtinguiſhed.

But I ought to aſk pardon of my reader for this long digreſſion of conjectural argument, which I have been induced to hazard from the hope of rendering more probable the aſſertion of Herodotus, that the figures of the Gods, as worſhipped in his time, were firſt invented by Heſiod and Homer.

If my interpretation be allowed any weight, this moſt ancient and venerable of prophane hiſtorians may be reſcued from the imputation of falſe opinion and abſurdity, his ſenſe being no more than that Heſiod and Homer were the firſt among the Grecians who reduced the genealogy of the Gods to a complete and regular ſyſtem, who gave to them certain ſirnames which they did not poſſeſs before the time of theſe poets, who diſtinguiſhed their tutelary functions,

very obſcure, and very doubtfully explained marble, given us by Montfaucon, tom. ii. part. ii. page 70.

tions, and appropriated to each of them a peculiar mode of worſhip and of ſacrifice, and who invented or gave riſe to the particular forms under which they have ever ſince been repreſented. And indeed Herodotus appears to have purpoſely explained himſelf, reſpecting thoſe opinions which he wiſhes to be conſidered as his own, at the concluſion of the paſſage now under conſideration, where, without controverting the relations of the prieſts, (whoſe ſacred authority in matters of remote antiquity where religion is concerned, he ſeems by his ſilence implicitly to admit,) he ſeparates and diſtinguiſhes his own ſentiments from their traditions:—"The firſt things (ſays he) "the prieſts of Dodona told me, but the latter, reſpecting "Heſiod and Homer, I myſelf aſſert." That is, the prieſts of Dodona are they who gave me the account of this very early ſtate of religion, and informed me that the names firſt given to the Gods were received by the Pelaſgi from Egypt, and by the Greeks from them; but reſpecting what I have ſaid of the very imperfect and ſcanty knowledge of the ancients in theology, and with regard to my aſſertion that Heſiod and Homer gave ſirnames to the Gods, and were the authors and founders of our preſent improved ſyſtem, that I declare, as my own opinion, which I think myſelf capable of forming, and authorized to give, as the time of thoſe bards, in compariſon with the remote ages, is not very far diſtant from my own.

It would ſeem alſo, from the words of Herodotus, that the hiſtorian aſcribed ſomewhat more, and of much greater importance, to Heſiod and Homer than the inventions expreſsly detailed

tailed in the paſſage relating to them. "But from whence "(ſays he) each of the Gods had his exiſtence, or whether "they have all been from eternity, or under what forms, "are matters unknown until yeſterday, as I may ſay; for "Heſiod and Homer, who lived four hundred years before me, "and no more, &c." From this we may not unreaſonably infer that Herodotus attributes to the bards in queſtion, not only the mere theogony, ceremonial worſhip, ſirnames, functions and figures of the Gods, but the inveſtigation and elucidation of that great and eſſential point in divinity, whether their exiſtence had been from all eternity; a queſtion indeed of the higheſt importance, eſpecially in a religion where the received opinion limited the exiſtence of the Gods, by aſſigning to each of them fathers and mothers, but which I do not recollect to have been diſcuſſed or elucidated in any writings of theſe bards that have come down to us, though I doubt not that ſuch elucidation may by inference be drawn from ſundry paſſages in theſe poems. May we however allow ourſelves to ſuppoſe that our hiſtorian had ſeen ſome philoſophical poems of Heſiod or Homer wherein this great ſubject was treated, but which are now buried in oblivion? The fact, though unlikely, is by no means impoſſible, as many of their works are known to have been loſt, and the bare poſſibility that ſuch treatiſes may have exiſted is a matter of much curioſity.

These imperfect and looſe hints I have thrown out merely to ſhew that the opinion of Herodotus may be reconciled to the

the truth; and ſurely, if that ſhould be poſſible, it is far better to endeavour ſuch reconcilement, than boldly to controvert, or peremptorily to contradict, the aſſertions of this moſt reſpectable and moſt ancient hiſtorian, or to pretend more knowledge of Grecian antiquity in the preſent age than was poſſeſſed by a learned Greek, who wrote four hundred years only after Homer, and whoſe antiquity is ſo remote that he ventures to account the æra of this bard but of yeſterday—μὲχρι ὅυ πρῶην τὲ και χθες.

And now having drawn to a concluſion the more immediate ſubject of this eſſay, I ſhall take leave to advert to a relative point, from the diſcuſſion of which I have hitherto abſtained, leſt the thread of my diſcourſe ſhould have been thereby interrupted and confuſed. The opinion of Herodotus, that all thoſe poets who were ſaid to have exiſted before Heſiod and Homer, were in effect poſterior to their time, has brought down upon him a torrent of abuſive contradiction. Certain it is that there are great authorities againſt him; but then it is as certain that, ſince in a queſtion of this ſort ſuperior antiquity may be ſuppoſed to include ſuperior knowlenge, none of thoſe authors, upon whoſe authority he is contradicted, can in this reſpect be put in competition with him. Pauſanias, in many parts of his work, mentions the names of ſeveral poets who lived before Heſiod and Homer, one of whom he ſuppoſes to have been prior even to Orpheus—Cap. xvii. page 762, Λυκιος δε Ωλην, &c.—“ The Lycian Olen, “ who compoſed among the Greeks the moſt ancient hymns.—

“ But

" But after Olen, Pamphus and Orpheus made verſes." This laſt famous poet, Orpheus *, is ſaid to have written in the time of Hercules, and conſequently forty years, at leaſt, before the Trojan war. Diogenes Laertius, in his Procemium, page 3, has theſe words—ἰδού γοῦν παρα μὲν Ἀθηναιοις γεγὸνε Μουσαῖος, παρα δὲ Θηβαίοις Λίνος, &c. " Muſæus was conſpicuous among the " Athenians, Linus among the Thebans, and the former, ſon " of Eumolpus, is ſaid to have firſt treated in poetry the " genealogy of the Gods, and of the ſphere. He is reported " alſo to have ſaid that all things ſprang from one, and into " that one would be reſolved. He is thought to have died in " the time of Phalaris."

Innumerable other ſuch authorities might be produced, which are certainly of conſiderable weight, though not abſolutely concluſive againſt the opinion of Herodotus, who, from the

* Some moderns, relying on the authority of Ariſtotle as quoted by Cicero, have gone far beyond Herodotus reſpecting Orpheus, poſitively denying, not, like our hiſtorian, that he was prior to Homer, but that any ſuch man ever exiſted. The words of Cicero, De Natura Deorum, Lib. i. Cap. 38, page 429, are " Orpheum poetam docet Ariſtoteles nunquam fuiſſe, et " hoc Orphicum carmen Pythagorei ferunt cujuſdam fuiſſe Cercopis." The treatiſe of Ariſtotle here alluded to is loſt, but that philoſopher probably meant no more than that the Orphic verſes attributed to this ancient ſage were not written by him, or, perhaps, that Orpheus never was a poet, in either of which ſenſes Ariſtotle ſeems to coincide with the opinion of Herodotus. The collection which has come down to our times is certainly of very high antiquity, and, excepting ſome interpolations inſerted by the pious zeal of the early Chriſtians, probably exiſted in the time of Herodotus, whoſe judgment, reſpecting the priority of Homer, may be ſuppoſed to have been founded on a critical examination of theſe very poems. Indeed it ſeems to be the generally received opinion that, however ancient the Orphic collection may be, it is, in effect, a very ancient forgery.——For a full and learned account of Orpheus, vide Cudworth's Intellectual Syſtem, page 294, &c.

the very early age in which he lived, may be ſuppoſed to have been better able to detect the forgery of the works attributed to theſe ſuppoſed ancient poets, or, allowing the compoſitions to be genuine, more accurately to aſcertain their preciſe degree of antiquity, than thoſe writers who lived long after his time.

But, however it may operate againſt me and my favourite writer, I muſt not conceal a proof, ſeemingly of a much more deciſive nature, which is produced by the ingenious author of the Enquiry into the Life and Writings of Homer, page 100, where Herodotus is brought forward to diſprove his own aſſertion, and flatly to contradict himſelf. The hiſtorian, in that very book, Euterpe, page 113, where he gives his opinion concerning the priority of Heſiod and Homer to all ſuch bards as were ſaid to have lived before them, ſpeaking of the word *Ocean*, thus expreſſes himſelf, Ὁμηρον δε, ἠ τινα των προτερον γενομενων Ποιητων, δοκεω το ουνομα ευροντα ες την Ποιησιν εσενεικασθαι; which words Blackwall thus tranſlates:—" Homer, I believe, *or ſome* " *of the poets who lived before him*, having invented the word, " inſerted it into their poetry." The paſſage however may be otherwiſe tranſlated, as it indeed is in moſt of the verſions I have ſeen; and the words, ἠ τινα των προτερον γενομενων Ποιητων, may mean no more than, *or ſome one of the ancient poets;* or, as we commonly expreſs ourſelves, *ſome one of the more ancient poets.* And ſurely, where a paſſage is capable of two meanings, that which may tend to involve the author in a ſeeming contradiction ought ſeduloufly to be avoided. But, not to inſiſt upon

upon this, and ſuppoſing the interpretation of Blackwall to be the true one, may it not be ſaid that Herodotus here gives no opinion of his own, but merely ſpeaks according to that which was generally received, and which, even where he afterward contradicts it, he allows to have been the generally adopted idea. An author may, without inconſiſtency, mention a popular ſtory or belief, which may poſſibly be true, but to which he gives no great credit, without combating it at the time; and yet, when the courſe of his argument leads him to a ſerious inveſtigation of the fact, he may give his opinion in contradiction to ſuch popular belief, the ſubſequent paſſage thus ſtanding in ſome ſort as an illuſtration of the former. Homer, ſays he, or ſome prior poet, invented the word *ocean*—but, in my opinion, there were no poets prior to Homer, therefore Homer muſt have invented it. Beſides we may obſerve that Herodotus never poſitively aſſerts that there were no anterior poets, but only tells us that *ſuch is his opinion*—ὕστερον, εμοι γε δοκεειν, εγενοντο τουτων; and therefore may, without incurring the cenſure of inconſiſtency, previouſly to his declaring his ſentiment on the point, mention an idea, which may *poſſibly* be well founded, though, according to his judgment, it be erroneous.

There is yet another authority produced, which, if it were clear of objection, would indeed put the matter out of all diſpute—no leſs than that of Homer himſelf, who, as interpreted by ſome, in his deſcription of the ſhield of Achilles, *ſeems* to make his young muſician ſing of Linus. Doctor Gillies,

who,

who, in his Hiſtory of Greece, has warmly adopted the faſhionable opinion concerning Herodotus, and whoſe learned work will ſerve throughout to exemplify what I have taken the liberty to ſuggeſt, reſpecting the ſuperiority of modern adepts in the knowledge of antiquity over the ancients themſelves, ſeems however rather too peremptory in his aſſertion, page 184, note 4, " that the *ignorance* of Herodotus, and of " his contemporaries, concerning the hiſtory of their ancient " bards, is clearly proved from the paſſage of Homer above " mentioned, and from another paſſage, which he quotes from " the Odyſſey, reſpecting Melampus." The lines, which are ſuppoſed to allude to *Linus*, are as follow:

> Τοῖσιν δ' εν μέσσοισι Παῖς φόρμιγγι λιγείῃ
> Ιμέιρον κιθάριζε· λίνον δ' ὑπο καλὸν ἄειδε
> Λεπ]αλέῃ φωνῇ.

Lib. xviii. page 193, verſe 570.

But in the meaning of this paſſage commentators eſſentially differ, ſome tranſlating that the boy ſung the ſong of Linus, while others, no leſs names than Didymus, Euſtathius, Madame Dacier, Boivin and Clarke, take Λίνον to ſignify the ſtrings of the inſtrument, which, ſay the old commentators, were at that time made of flax, thoſe of gut having been diſpleaſing to the Gods. So that, according to this interpretation, which ſeems by far the more natural, the lines will mean no more than that, *in the midſt of theſe a boy played ſweetly on a ſhrill harp, and ſung* TO THE FAIR STRING *with a tender voice.*

 It

It muſt not however be concealed that Pauſanias, a great authority, favours the former interpretation, vide Bœotica, page 766. But the authority of Herodotus, who, as he *probably* had read Homer, would ſurely never have hazarded his aſſertion had he ſuppoſed that the bard himſelf had mentioned a poet previous to his time, is, in a diſputed paſſage, ſtill greater than that of Pauſanias. I wonder the Doctor did not chuſe to quote Herodotus againſt himſelf, who, in his ſecond book, page 140, mentions a ſong ſung by the Egyptians, which, though they term it *Maneros*, is in effect the ſame with that which the Greeks call *Linus*. But the ſong of Linus,* which was *probably* no other than a lamentation for the death of that perſonage, may have exiſted and been ſung in the days of Herodotus both in Egypt and in Greece, and yet Linus, the ſubject of that ſong, may not have lived before the time of Homer.

But even though we ſhould follow Pauſanias in his interpretation, ſtill I muſt aſſert that the *ignorance of Herodotus* would not be thereby proved, ſince that elegant traveller moſt certainly mentions Linus as a muſician, and by no means as a poet; an evident proof, by the way, that, though the two vocations

* The λινος among the Greeks was a dirge or ſong of lamentation, but I do not think it at all clear that the death of Linus was therein commemorated and lamented. Perhaps this ſpecies of muſic was ſuppoſed to have been invented by Linus, and may have taken its name from him. This earlieſt of muſicians is ſaid by ſome to have been ſlain by his father Apollo for teaching the uſe of gut inſtead of flaxen ſtrings, while others report that his brains were knocked out with his own lyre by Hercules, the ruſticity of whoſe muſical performance he had derided.

vocations were usually united, they were notwithstanding sometimes separated even in the earliest times, and long before their formal separation at the re-establishment of the Pythian games in the year before Christ 590. Pausanias, in the passage alluded to, has these words—Ἔπη δὲ οὔτε ὁ Αμφιμάρου Λίνος, οὔτε ὁ τούτου γενομενος ὕστερον, ἐποίησαν· ἢ καὶ ποιηθέντα ἐς τοὺς ἔπειτα οὐκ ἦλθε—*But neither Linus, the son of Amphimarus, nor the latter, who lived afterwards, made verses; or, if they made any, none of them have come down to after ages.* And this is the conclusion of the same paragraph, where the interpretation of Homer's verses supposed to respect Linus is given. Indeed in this very chapter, not many lines before, the vocation of Linus as a musician, who had acquired his fame by his skill in that science, is expresly pointed out—μεγίστην δὲ τῶν ἐφ' ἑαυτου, καὶ ὅσοι πρότερον ἐγένοντο, λάβοι δοξαν επὶ Μουσικῇ—Bœotica, Cap. xix. page 766.

Hence it appears that Herodotus, where he mentions the lamentation for Linus as sung by the Egyptians, and Pausanias also, speak of him as a musician, and not as a poet; and consequently that neither Herodotus contradicts himself, nor does the testimony of Homer, supposing the interpretation of Pausanias to be the true one, in any degree combat the opinion of Herodotus that there were no *poets* among the Greeks more ancient than Homer. That Linus may in effect have possessed this disputed priority is a presumption supported by many strong circumstances; but neither Homer, nor Herodotus in contradiction to himself, nor Pausanias, can be brought to prove it.

With

With refpect to Melampus, he is indeed mentioned in the 15th book of the Odyffey, verfe 225; but I cannot fee how it appears from that paffage that he was a poet. The name of this very ancient perfonage is mentioned upon the following occafion:—Theoclymenus, having killed one of his own tribe, and being purfued as a murderer, conjures Telemachus to fave him from the impending danger by receiving him on board his fhip. This Theoclymenus was a foothfayer, Μαντις, and was, as the poet informs us, by a long genealogy, lineally defcended from Melampus; of whom, however, nothing is faid which can convey the flighteft hint refpecting his profeffion. The hiftory of this ancient fage is well known—Bayle, article Melampus, gives a full account of every thing that has been faid of him by ancient writers. He, as well as his defcendant, was a foothfayer or prophet, and a great phyfician, in which laft character he is principally illuftrious; but no writer of antiquity gives the moft remote hint of his having been a poet. The phyficians of the early ages were ufually foothfayers; their vocation was accounted holy; and religious ceremonies, or exorcifms, went hand in hand with the practice of medicine. Virgil, in his third Georgic, mentions this Melampus, but certainly not as a bard:

. . . . ceffere magiftri
Phylirides Chiron, Amathæontufque Melampus.

It feems indeed to be fuppofed by many that every profeffional man of remote antiquity, who is recorded with diftinction

tinction by the ancient authors, muſt of courſe have been a bard, upon this plauſible aſſumption, that, in the diſtant ages, whoever taught muſt neceſſarily have taught in verſe. That this, however, was not the opinion of Pauſanias is clearly evident from the paſſage already quoted relating to Linus, who, as he informs us, *wrote no verſes*; and though he adds that, *if he did write any they are not come down to us*, his bare ſuppoſition, *that he had written none*, ſufficiently proves that this ancient and judicious writer did not deem the characters even of muſician and bard by any means inſeparable; and, if any profeſſions could have been deemed neceſſarily ſo, it muſt certainly have been thoſe.

AND here I will conclude this long, and, I fear, tedious eſſay, with a repetition of my teſtimony in favour of Herodotus, namely, that through the whole courſe of my Eaſtern travels I have ever found him a faithful guide; a teſtimony which I am happy to find corroborated, and, in my opinion, confirmed, by the much more extenſive and certain experience of one, whom, in a point of this nature, I ſhould almoſt deem an infallible judge—my ever lamented friend, Robert Wood, whoſe ſagacity and erudition could only be equalled by his diligence and candour. In his Eſſay on the original Genius of Homer, page 184, he coincides with me in the following deciſive words:—" Not that I would encourage that diffidence in Hero-
" dotus, which has been already carried too far. Were I to
" give my opinion of him in this reſpect, having followed him
" through moſt of the countries which he has viſited, I

" would

"would ſay, that he is a writer of veracity in his deſcrip-
"tions of what he ſaw, but of credulity in his relations of
"what he heard.

Such is the judgment of the moſt competent of critics, whoſe comment upon his author was not the reſult of cloſet inveſtigation, but of ocular examination into the facts reported —Such is the judgment of a learned and diligent enquirer, who followed the ſteps of Herodotus through almoſt all his travels, and had every poſſible opportunity of detecting his errors, and contradicting his falſifications. Yet even in this character of our author, which good ſenſe, experience and candour have dictated, there is ſtill ſomething which may perhaps be allowed to bear rather too hard upon the venerable father of hiſtory. The credulity of Herodotus is a fault which his moſt ſanguine favourers have generally imputed to him; and yet even this may perhaps be palliated, when we candidly conſider the ſtate of the times in which he lived. Egypt was in thoſe days eſteemed the ſeat of poliſh, and the fountain of ſcience—Greece, not long ſince emerged from ignorance, had from thence received her philoſophy, her religion, her Gods; and conſequently the Egyptian prieſts, in whom excluſively reſided all the knowledge of that ſcientific region, would by the Grecians be held in the higheſt veneration; in religious matters eſpecially they would be thought to poſſeſs a patriarchal authority; their relations and opinions would obtain implicit credit, and almoſt be conſidered as articles of faith. Herodotus was a traveller for inſtruction, and had journeyed

into

into thoſe parts from whence alone, according to the received opinion, it could be gathered, and from whence his predeceſſors had imported into their native land all the knowledge it then poſſeſſed. Can any thing then be more natural, or indeed more neceſſary, than that, in reciting his hiſtory to his countrymen aſſembled at the Olympic games, he ſhould fully and even indiſcriminately inform them of all he had heard in a country by them deemed the mother of ſcience, and more eſpecially from that claſs of men whom they eſteemed as oracles?

YET even here he acted with caution. His own good ſenſe got the better even of the credulity of his age, and we accordingly find in many parts of his work hints thrown out, which ſufficiently evince that he himſelf did not thoroughly believe all thoſe facts and opinions which he thought himſelf bound to relate. In his ſecond book, page 161-2, after having related ſome wonderful ſtories concerning Rampſinitus, one of which he plainly tells us *he does not believe*—ἐμοι μέν οὐ πίστα, he concludes his narration with the following words:—Τοισι μεν νυν ὑπ᾽ Αἰγυπτιων λεγομενοισι χρασθω ὅτεω τα τοιαυτα πιθανα εστι· εμοι δε παρα παντα τον λογον ὑποκειται, ὅτι τα λεγομενα ὑπ᾽ εκαστων ακοῇ γραφω—"These things however which the Egyptians relate "let every man think credible according as he likes; for my "part through my whole diſcourſe I have determined to "write whatever has been told me." Nay in his ſeventh book, page 574, he goes ſtill farther, and makes the following proteſtation—'Εγω δε ὀφειλω λεγειν τα λεγομενα, πειθεσθαι γε μην ου πανταπασι ὀφειλω. και μοι τοῦτο τὸ ἔπος ἐχετω ες παντα τον λογον—"But it is my duty to relate the things which are told me, "though I am not bound to believe them all; and let what "I now ſay be eſtabliſhed through the whole courſe of my "hiſtory."

Thus far have I ventured to eſſay my weak endeavours towards the vindication of an author by whoſe guidance and inſtruction my travels have been rendered delightful and profitable, and by whom, I muſt again repeat it, I have never been deceived; neither let me be accuſed of preſumption or of arrogance in having thus attempted to controvert the opinions of thoſe, who, from their acknowledged ſuperiority both in erudition and capacity, are ſo much more competent judges than I can pretend to be, when I aſſure my brethren of the Academy, to whoſe partiality, rather than to their judgment, this ſlight eſſay is ſubmitted, that my juſt and too well founded timidity would have yielded to no motive leſs powerful than the ardent, and, I truſt, not unwarrantable deſire, of contributing towards that important and univerſal benefit, hiſtoric certainty, by endeavouring to redeem from ſuſpicion a writer upon whoſe credit our knowledge of antiquity, and of remote hiſtory, almoſt wholly depends, and without whoſe aid and information the darker ages would be plunged into tenfold obſcurity. The certainty of hiſtoric relation is of the higheſt importance to mankind—hiſtory is the ſchool of man-

ners.

ners. All bounteous Heaven, while it wifely denies us the knowledge of futurity, becaufe fuch knowledge would but tend to increafe and aggravate the miferies and dangers of our lives, has beneficently granted to us the recollection of things paft, a faculty, which the habit of poffeffion alone could prevent our acknowledging to be as wonderful as that of prefcience, and which is effentially neceffary to the regulation of all our actions, and confequently to our happinefs both now and hereafter. But as the fhortnefs of our abode in this brief and temporary manfion might render nugatory the benefits of this falutary gift, the fame all-bountiful providence has fent hiftory to our aid, by the intervention of which our experience is lengthened backwards into the moft remote ages, and even to the beginning of time! Let us then refpect as we ought this facred fource of all our wifdom, and, while we candidly examine into the probability of hiftoric narration, be cautious of prefuming too much upon our own fagacity; never, but with the utmoft circumfpection and humble diffidence, daring to contradict thofe ancient guides, by whom alone our fteps can be conducted through the mifty labyrinth of antiquity, and more efpecially the venerable parent of that fcience, which, by recording the obfervation of all ages, has put into our poffeffion the whole feries of progreffive improvement, and the accumulated wifdom of the world, even from its infancy, and without which our boafted noon of knowledge muft neceffarily have been but a dawn.

An ACCOUNT *of the* GAME *of* CHESS, *as played by the* CHINESE, *in a* LETTER *from* EYLES IRWIN, *Esq;* *to the Right Honourable the* EARL *of* CHARLEMONT, *President of the Royal Irish Academy.*

MY LORD,

I CONSIDER no apology neceſſary for this intruſion on the public ſituation in which your talents and reputation have placed you. Whatever tends to the acceſſion of knowledge, or the illuſtration of antiquity, cannot prove unacceptable to your Lordſhip, when adding a mite to the Tranſactions of the Academy which is diſtinguiſhed by your ſuperintendence. Read Nov. 16, 1793.

WHY I have addreſſed a ſubject of this nature to the Iriſh Academy, when there is a ſociety exiſting who ſeems to have a title a title to it from its name—or why the firſt offering of my reſearches ſhould proceed from the remote empire of China, are, I truſt, queſtions that are not neceſſary for me to reſolve. If a patriot wiſh to promote the ſpirit of inveſtigation in my country, by the exertion of my mean abilities, be not denied me, I am indifferent to cenſure or praiſe on this occaſion.

I MUST premiſe to your Lordſhip, that, during a long reſidence in the Eaſt Indies, where the game of Cheſs is generally ſuppoſed to have originated, I had often heard of its exiſtence in China, though on a different footing, as well in reſpect to the powers of the King, as to the aſpect of the field of battle. The Bramins, who excel in this game, and with whom I uſed frequently to play for improvement, had a tradition of this nature, which is a further argument in behalf of what I am about to advance. But, with all my enquiries from perſons who had been there, and from the publications relative to China, I could never obtain any confirmation of the game being even known in the country, except that Chambers, in his Dictionary, mentions it to be the favourite paſtime of the ladies, but quotes no authority for the aſſertion.

SOME unlooked-for circumſtances in the courſe of the laſt year at length brought me to the quarter, which I had once wiſhed, but never expected, to viſit. I need not ſay, that among

among other objects of curiofity, I was eager to afcertain the reality of the Bramins ftory. And if the difficulty of acquiring information here, not more from the want of interpreters, than the jealoufy of the government, were not well known in Europe, I fhould be afhamed to tell your Lordfhip that I defpaired of fuccefs for fome time. A young Mandarin, however, of the profeffion of arms, having an inquifitive turn, was my frequent vifitor; and, what no queftions could have drawn from him, the accidental fight of an Englifh chefs-board effected. He told me, that the Chinefe had a game of the fame nature; and, on his fpecifying a difference in the pieces and board, I perceived, with joy, that I had difcovered the defideratum of which I had been fo long in fearch. The very next day my Mandarin brought me the board and equipage; and I found, that the Bramins were neither miftaken touching the board, which has a river in the middle to divide the contending parties, nor in the powers of the King, who is entrenched in a fort, and moves only in that fpace, in every direction. But, what I did not before hear, nor do I believe is known out of this country, there are two pieces, whofe movements are diftinct from any in the Indian or European game. The Mandarin, which anfwers to our bifhop, in his ftation and fidelong courfe, cannot, through age, crofs the river; and a rocket-boy, ftill ufed in the Indian armies, who is ftationed between the lines of each party, acts literally with the motion of the rocket, by vaulting over a man, and taking his adverfary at the other end of the board. Except that the King has

his

his two ſons to ſupport him, inſtead of a Queen, the game, in other reſpects, is like ours; as will appear in the plan of the board and pieces I have the honour to encloſe, together with directions to place the men and play the game.

As the young man who had diſcovered this to me was of a communicative and obliging diſpoſition, and was at this time purſuing his ſtudies in the college of Canton, I requeſted the favour of him to conſult ſuch ancient books as might give ſome inſight into the period of the introduction of Cheſs into China; to confirm, if poſſible, the idea that ſtruck me of its having originated here. The acknowledged antiquity of this empire, the unchangeable ſtate of her cuſtoms and manners, beyond that of any other nation in the world; and more eſpecially the ſimplicity of the game itſelf, when compared to its compaſs and variety in other parts, appeared to give a colour to my belief. That I was not diſappointed in the event, I have no doubt will be allowed, on the peruſal of the tranſlation of a manuſcript extract, which my friend Tinqua brought me, in compliance with my deſire; and which, accompanied by the Chineſe manuſcript, goes under cover to your Lordſhip. As the Mandarin ſolemnly aſſured me that he took it from the work quoted, and the tranſlation has been as accurately made as poſſible, I have no heſitation to deliver the papers as authentic.

In the purſuit of one curioſity I flatter myſelf that I have ſtumbled by accident on another, and have gone ſome length to reſtore to the Chineſe the invention of gun-powder, ſo long diſputed with them by the Europeans; but which the evidence on their cheſs-board, in the action of the rocket, ſeems to eſtabliſh beyond a doubt. The inſtitution of the game is likewiſe diſcovered to form the principal æra in the Chineſe hiſtory; ſince, by the conqueſt of Shenſi, the kingdom was firſt connected in its preſent form, and the monarch aſſumed the title of Emperor, as may be ſeen in the extract which I have obtained from their annals.

From theſe premiſes I have therefore ventured to make the following inferences:—That the game of Cheſs is probably of Chineſe origin. That the confined ſituation and powers of the King, reſembling thoſe of a monarch in the earlier parts of the world, countenance this ſuppoſition; and that, as it travelled weſtward, and deſcended to later times, the ſovereign prerogative extended itſelf, until it became unlimited, as in our ſtate of the game. That the agency of the Princes, in lieu of the Queen, beſpeaks forcibly the nature of the Chineſe cuſtoms, which exclude females from all power or influence whatever; which Princes, in its paſſage through Perſia, were changed into a ſingle Vizier, or miniſter of ſtate, with the enlarged portion of delegated authority that exiſts there; inſtead of whom, the European nations, with their uſual gallantry, adopted a Queen on their board *. That the river between the parties is expreſſive of the general face of this country, where a battle could hardly be fought without encountering an interruption of this kind, which the ſoldier was here taught to overcome; but that, on the introduction of the game into Perſia, the board changed with the dry nature of the region, and the conteſt was decided on terra firma. And laſtly, that in no account of the origin of Cheſs, that I have read, has the tale been ſo characteriſtic or conſiſtent as that which I have the honour to offer to the Iriſh Academy. With the Indians, it was deſigned by a Bramin to cure the melancholy of the daughter of a Rajah. With the Perſians, my memory does not aſſiſt me to trace the fable; though, if it were more to the purpoſe, I think I ſhould have retained it. But, with the Chineſe, it was invented by an experienced ſoldier, on the principles of war. Not to diſpel love-ſick vapours, or inſtruct a female in a ſcience that could neither benefit nor inform her; but to quiet the murmurs of a diſcontented ſoldiery; to employ their vacant hours in leſſons on the military art, and to cheriſh the ſpirit of conqueſt in the boſom of winter quarters. Its age is traced by them on record

* That on the acquiſition of ſo ſtrong a piece as the Vizier, the Paö were ſuppreſſed, this as poſſeſſing powers unintelligible, at that time, to other nations; and three pawns added, in conſequence, to make up the number of men; and that as diſcipline improved, the lines, which are ſtraggling on the Chineſe board, might have been cloſed on ours.

record near two centuries before the Chriſtian æra; and among the numerous claims for this noble invention, that of the Chineſe, who call it, by way of diſtinction, Chong Kè, or The Royal Game, appears alone to be indiſputable.

I have the honour to remain,

MY LORD,

Your Lordſhip's obedient,

Humble ſervant,

EYLES IRWIN.

Canton,
14th March 1793.

Tranſlation of an Extract from the Coneum, or Chineſe Annals, reſpecting the Invention of the Game of Cheſs, delivered to me by Tinqua, a Soldier Mandarin of the Province of Fokien.

THREE hundred and ſeventy-nine years after the time of Confucius, or one thouſand nine hundred and ſixty-five years ago, Hung Cochu, King of Kiangnan, ſent an expedition into the Shenſi country, under the command of a Mandarin, called Hanſing, to conquer it. After one ſucceſsful campaign, the ſoldiers were put into winter quarters; where, finding the weather much colder than what they had been accuſtomed to, and being alſo deprived of their wives and families, the army, in general, became impatient of their ſituation, and clamorous to return home. Hanſing, upon this, revolved in his mind the bad conſequences of complying with their wiſhes. The neceſſity of ſoothing his troops, and reconciling them to their poſition, appeared urgent, in order to finiſh his operations in the enſuing year. He was a man of genius, as well as a good ſoldier; and having contemplated ſome time on the ſubject, he invented the game of Cheſs, as well for an amuſement to his men in their vacant hours, as to inflame their military ardour, the game being wholly founded on on the principles of war. The ſtratagem ſucceeded to his wiſh. The ſoldiery were delighted with the game; and forgot, in their daily conteſts for victory, the inconveniencies of their poſt. In the ſpring the general took the field again; and, in a few months, added the rich country of Shenſi to the kingdom of Kiangnan, by the defeat and capture of its King, Choupayuen *, a famous warrior among the Chineſe. On this conqueſt Hung Cochu aſſumed the title of Emperor, and Choupayuen put an end to his own life in deſpair.

* The ſame romantic tales are circulated of the prowefs of Choupayuen as of our celebrated Guy Earl of Warwick.

Explanation of the Poſition, Powers and Moves of the Pieces on the Chineſe Cheſs Board, or Chong Kè (Royal Game).

AS there are nine pieces inſtead of eight, to occupy the rear rank, they ſtand on the lines between, and not within, the ſquares. The game is conſequently played on the lines.

THE King, or Chong, ſtands in the middle line of this row. His moves reſemble thoſe of our King, but are confined to the fortreſs marked out for him.

THE

The two Princes, or Sou, ſtand on each ſide of him, and have equal powers and limits.

The Mandarins, or Tchong, anſwer to our Biſhops, and have the ſame moves, except that they cannot croſs the water or white ſpace in the middle of the board to annoy the enemy, but ſtand on the defenſive.

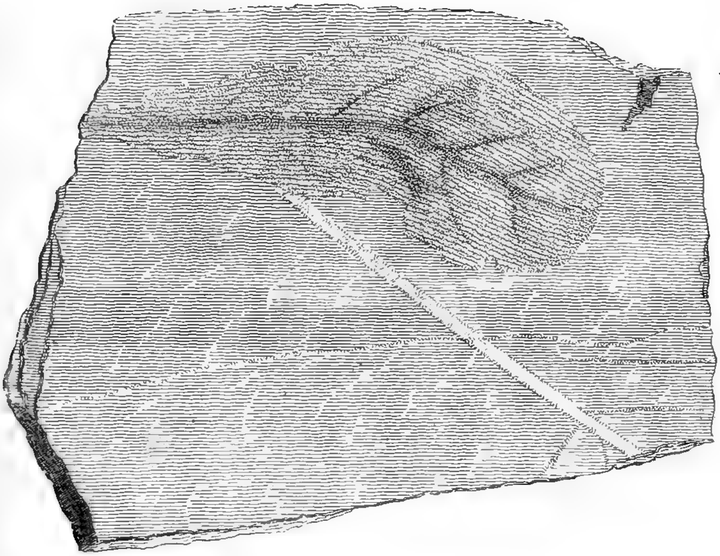
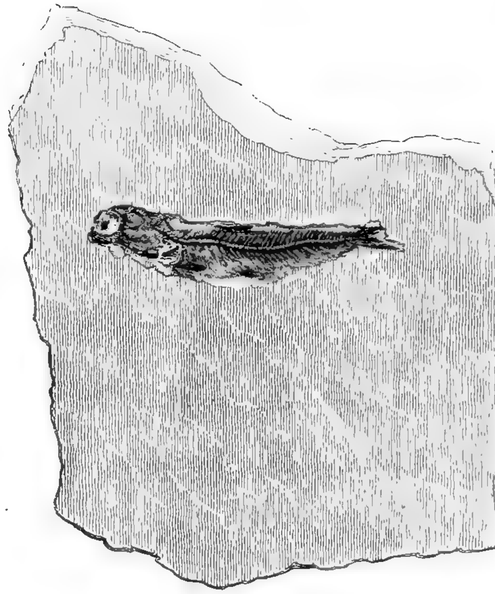
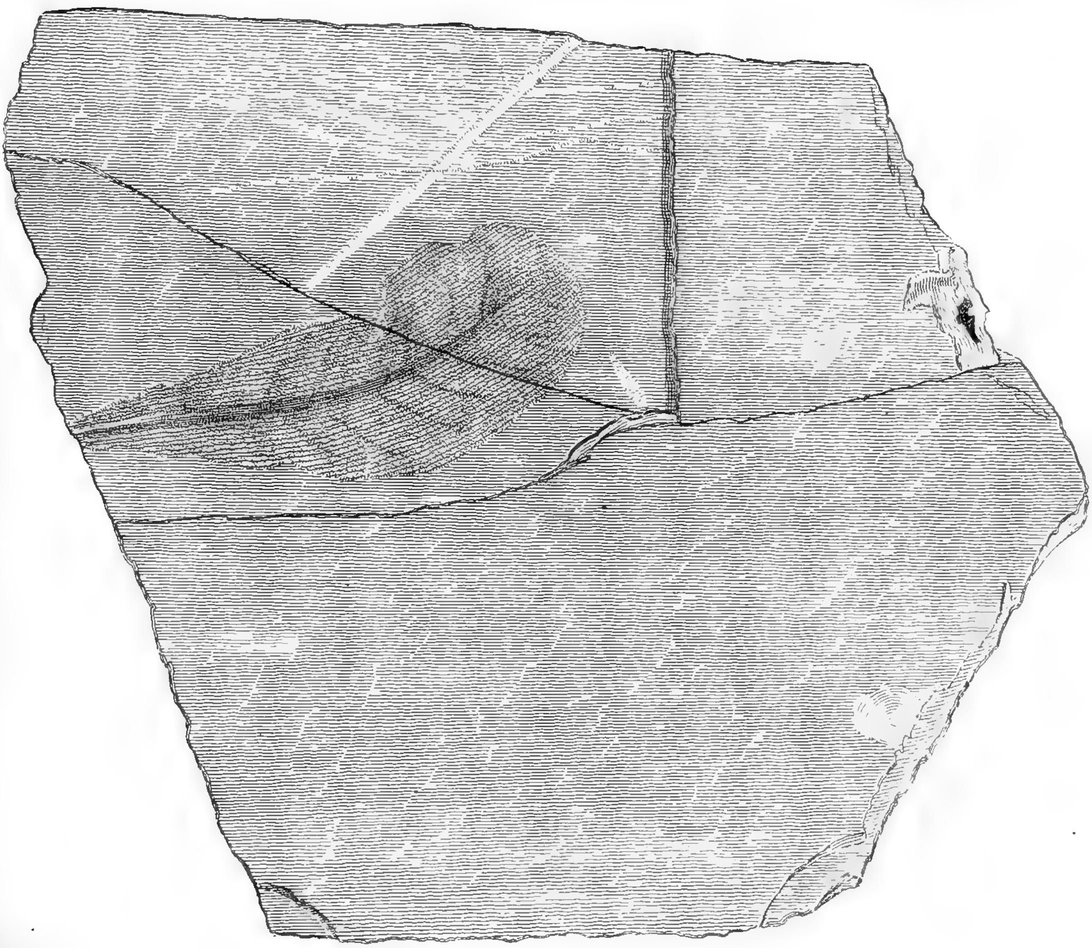
The Knights, or rather Horſes, called Mãã, ſtand and move like ours in every reſpect.

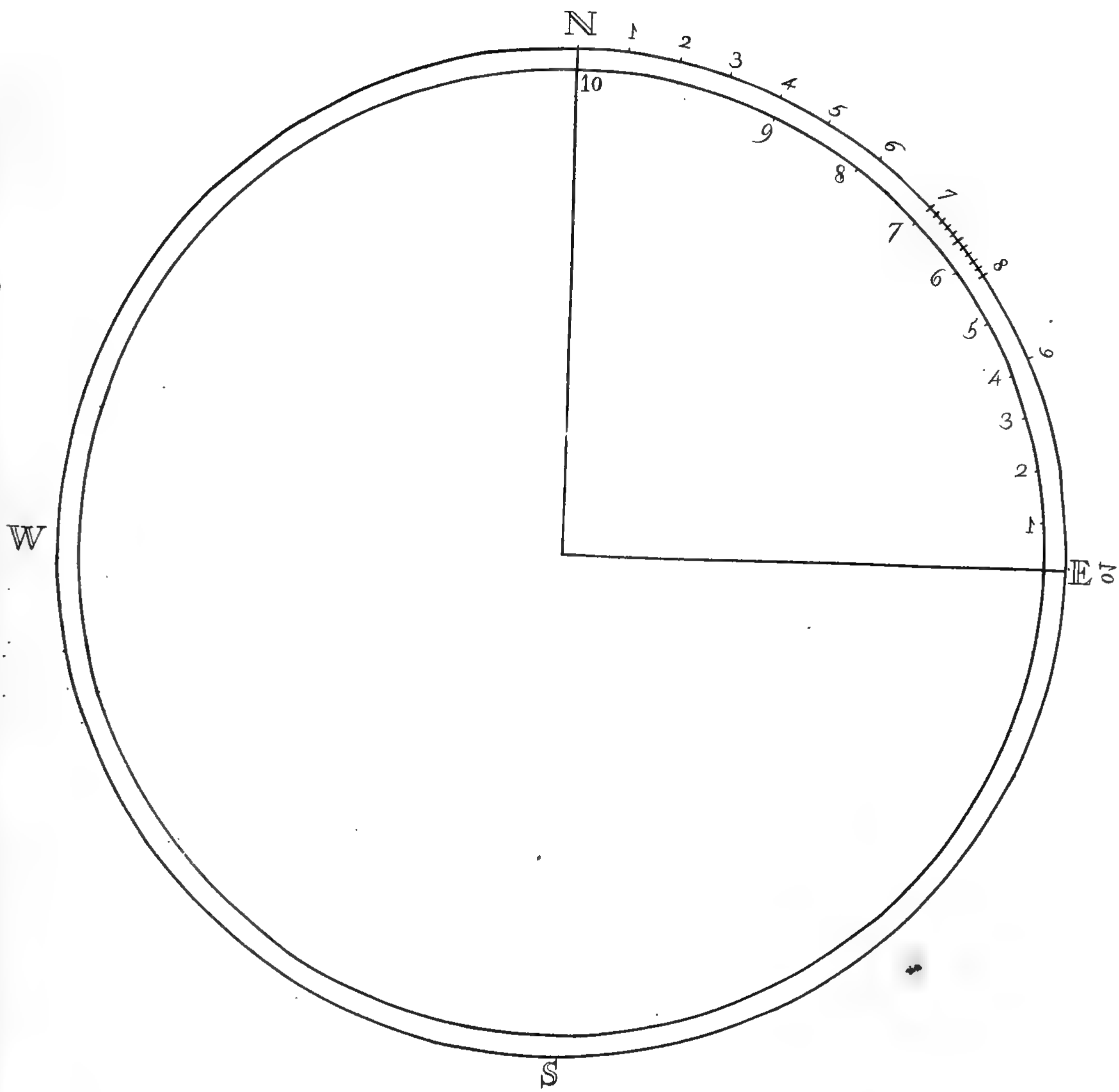
The War-chariots, or Tchè, reſemble our Rooks or Caſtles.

The Rocket-boys, or Paö, are pieces whoſe motions and powers were unknown to us. They act with the direction of a rocket, and can take none of their adverſary's men that have not a piece or pawn intervening. To defend your men from this attack it is neceſſary to open the line between, either to take off the check on the King, or to ſave a man from being captured by the Paö. Their operation is, otherwiſe, like that of the Rook. Their ſtations are marked between the Pieces and Pawns.

The five Pawns, or Ping, make up the number of the men equal to that of our board. Inſtead of taking ſideways, like ours, they have the Rook's motion, except that it is limited to one ſtep, and is not retrograde. Another important point, in in which the Ping differs from ours, is that they continue in ſtatu quo, after reaching their adverſary's head quarters. It will appear, however, that the Chineſe pieces far exceed the proportion of ours; which occaſions the whole force of the conteſt to fall on them, and thereby precludes the beauty and variety of our game, when reduced to a ſtruggle between the Pawns, who are capable of the higheſt promotion, and often change the fortune of the day. The poſts of the Ping are marked in front.

EYLES IRWIN.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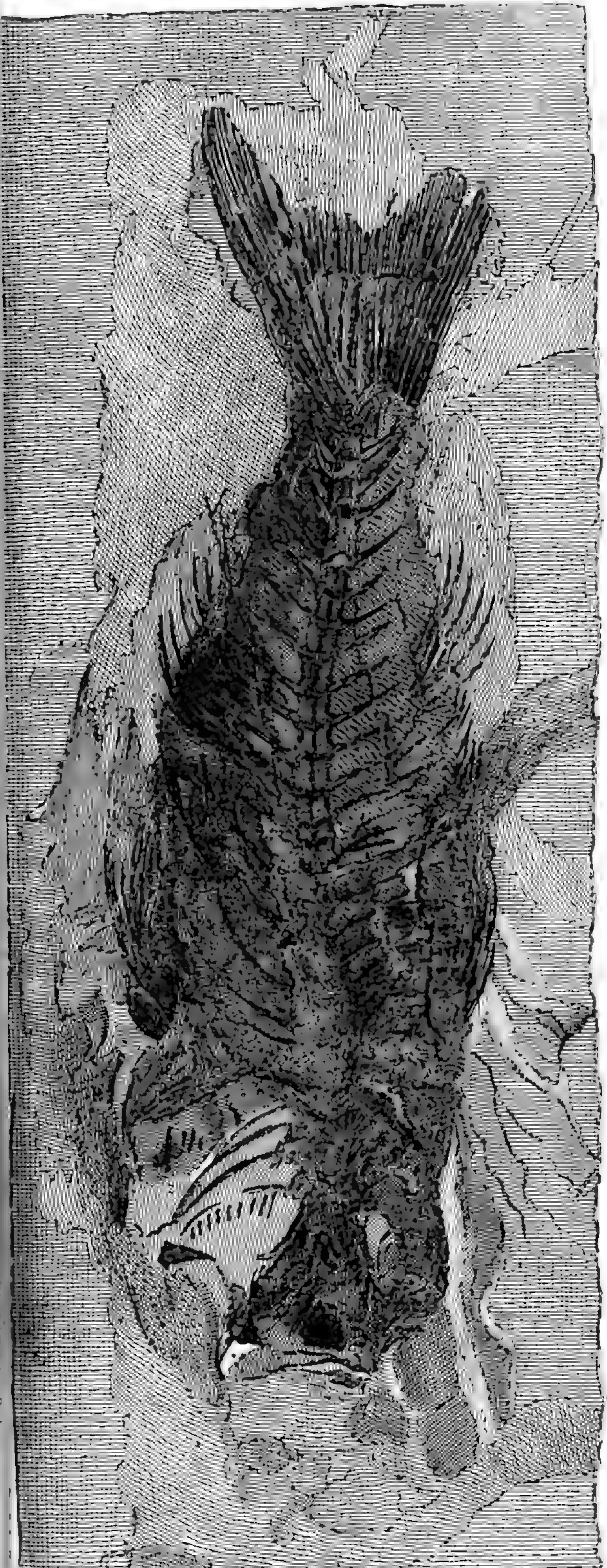

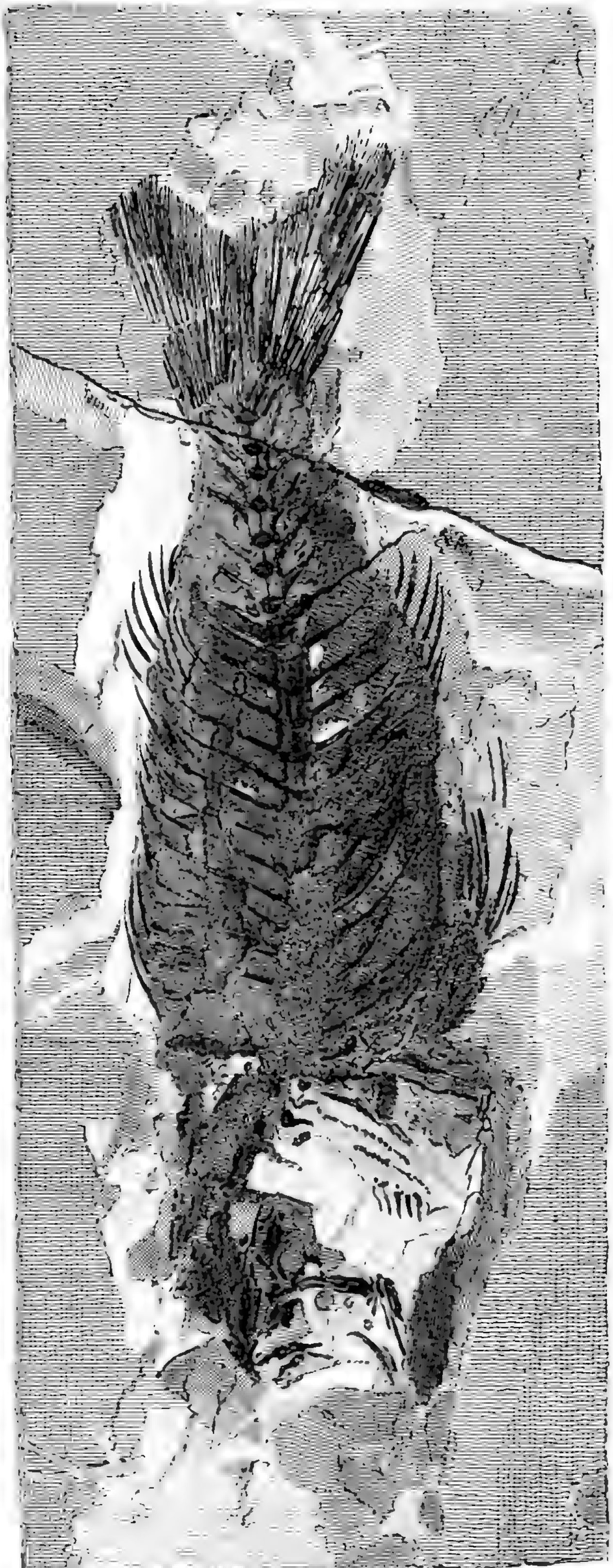

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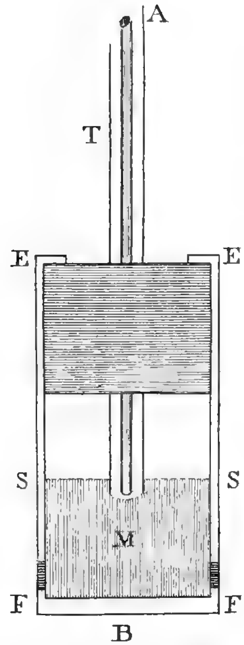
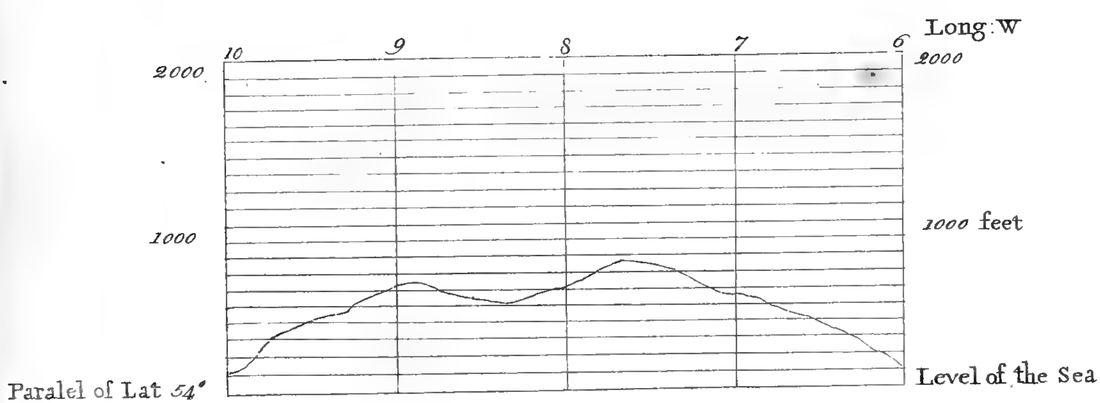
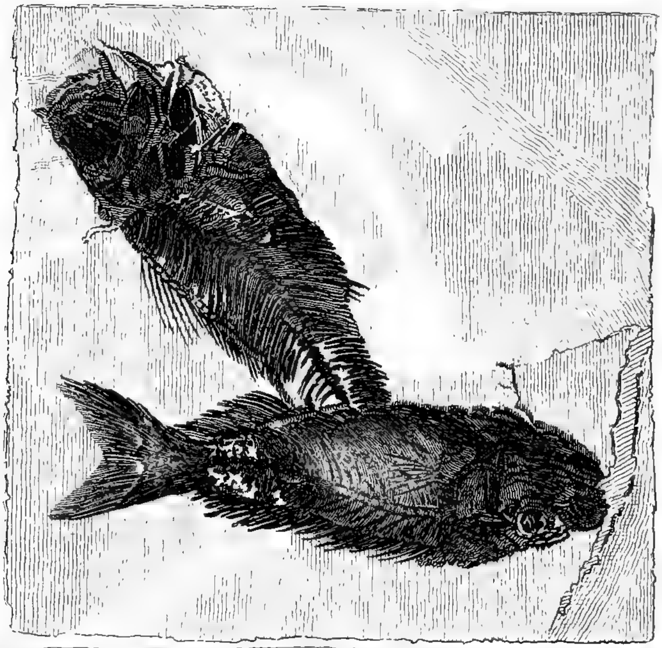
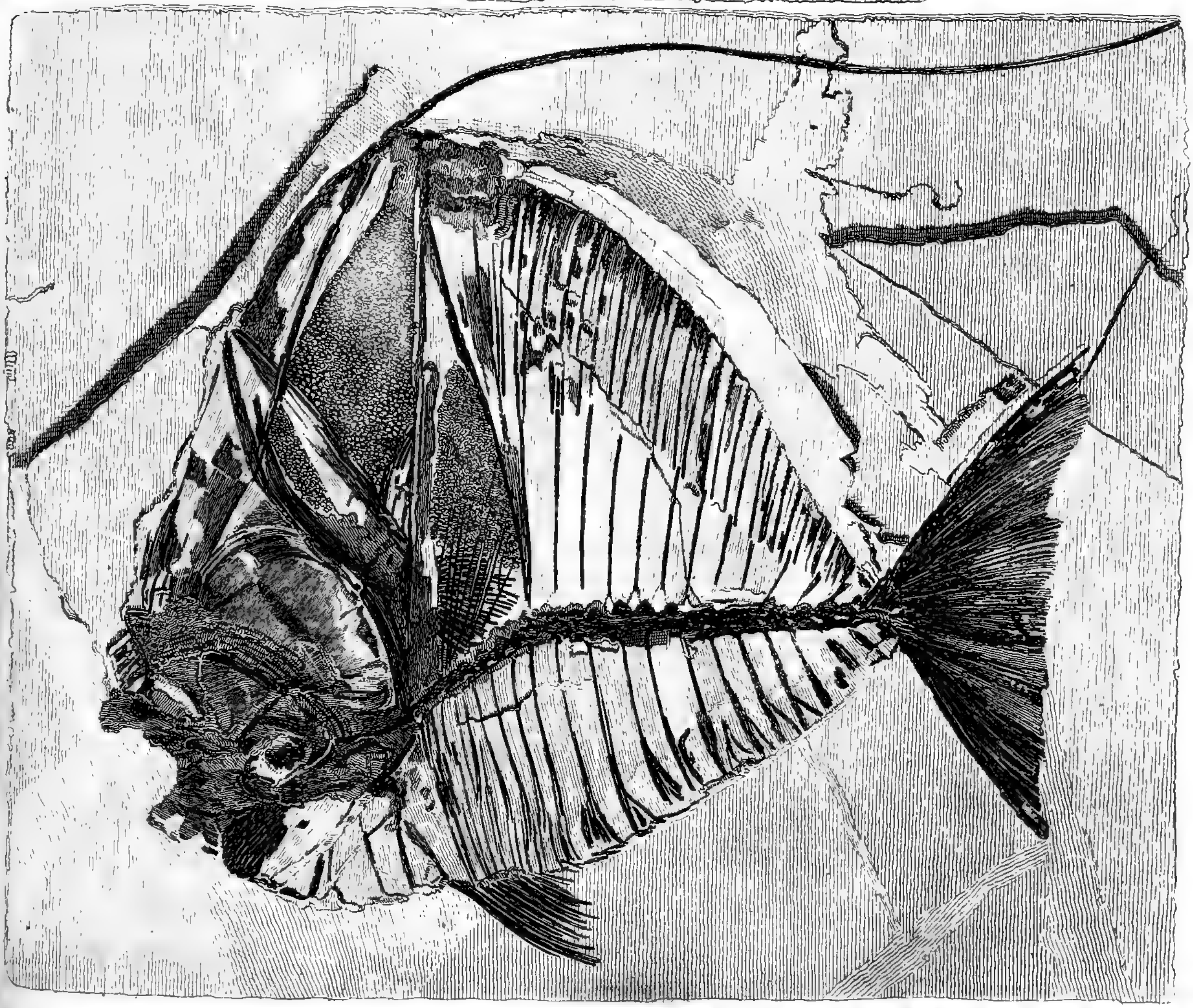




Barometrical Map of Four degrees Long:





孔夫子三百七十九年後之一事漢高祖姓劉名邦有一大元帥姓韓名信年三十六歲文武全才十分本事漢高祖遣他帶兵數萬人馬爭戰楚霸王未得平服及至冬天數萬人馬思想回家疾病鬱悶甚多韓信一聞此事甚然憂悶無計可施想出此象棋與兵人解悶賭暢不想回家

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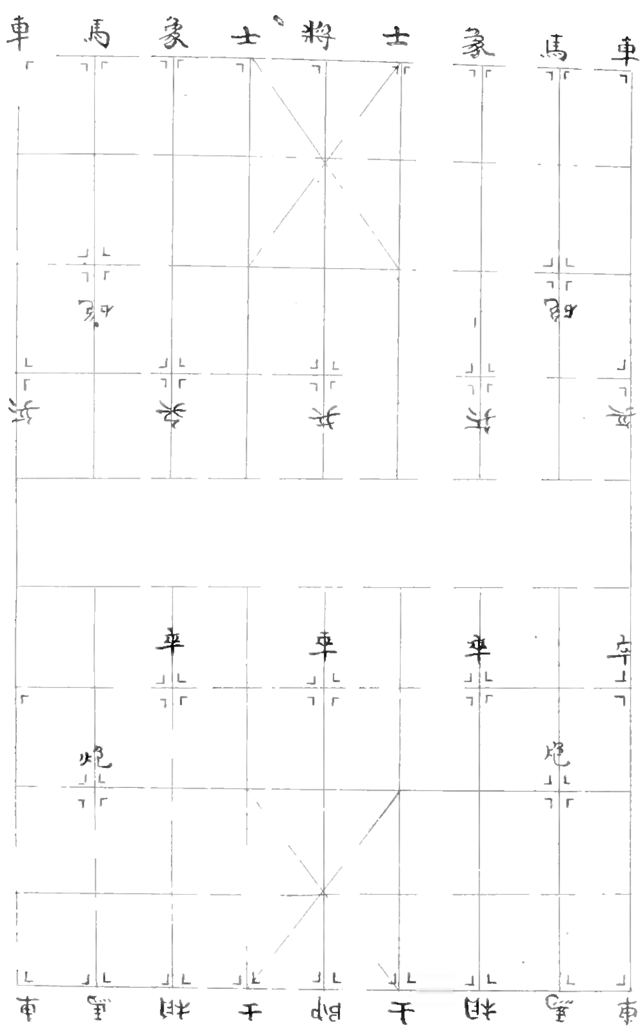

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