METEOROLOGICAL

OBSERVATIONS

AND

ESSAYS.

BY

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AT THE NEW COLLEGE, MANCHESTER.

EST QUODDAM PRODIRE TENUS, SI NON DATUR ULTRA.

HORACE.

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PREFACE.

WHEN I firſt adopted the reſolution to offer the public, in this manner, the reſult of my meteorological obſervations, which was about twelve months ago, my principal deſign was, to explain the nature of the different inſtruments uſed in meteorology, particularly the barometer and thermometer. As the number of theſe is increaſing daily, many of them muſt fall into hands that are much unacquainted with their principles, and may therefore not profit by them in ſo great a degree as otherwiſe; for which reaſon, a ſhort and clear explanation, with a ſeries of obſervations ſerving further to illuſtrate and exemplify the principles, and a few practical rules for judging of the weather, deduced from experience, ſeemed to me to promiſe utility; whilſt the obſervations themſelves would be an addition to the ſtock already before the public, and might perhaps be found ſubſervient to the improvement of the ſcience.

Soon after this, having diſcovered the relation of the *aurora borealis* to magnetiſm, in the manner deſcribed in the introduction to that eſſay,

 I found,

I found, that in order to eſtabliſh the diſcovery, a pretty large diſſertation would be required, which muſt, of courſe, be addreſſed more peculiarly to philoſophers; this neceſſarily enlarged the work, and became a primary conſideration, though the original deſign was ſtill kept in view; I concluded afterwards, that the work ſhould conſiſt of two parts, the firſt of which was to contain the ſubſtance of the original deſign, namely, a brief explanation of the nature of the inſtruments, and a digeſt of all the obſervations I had made, as matters of fact; the ſecond was to contain the eſſay or diſſertation on the *aurora borealis*, together with ſhort theoretic remarks on the different phenomena of meteorology, which I intended to ſelect chiefly from the beſt accounts I could procure; however, not having by me all the books I could have deſired, I was neceſſarily, and perhaps luckily, forced to contemplate a good deal on the different ſubjects, and to try ſuch experiments as were within my reach. The reſult was, that ſeveral things occurred to me which were new, at leaſt to myſelf, and which throw light on the different branches of natural philoſophy, and of meteorology in particular. Theſe I have thrown into the form of Eſſays, in which are alſo given, ſuch uſeful diſcoveries and obſervations of others as ſeemed neceſſary to be known, in order to form a proper idea of the preſent ſtate of the ſcience, and of the improvements that are yet to be made in it.

In

In the firſt part I have given not only the obſervations made at *Kendal* by myſelf, but alſo, with his leave, thoſe made at *Keſwick* by Mr. *Croſthwaite*, keeper of the muſeum at that place, together with obſervations on the barometer and rain, made at *London*, for three years, taken from the Philoſophical Tranſactions. The reſults of the ſeveral obſervations I have arranged and digeſted to the beſt of my judgment. The obſervations on the height of the clouds, and on the *aurora borealis*, particularly the ſupplemental ones, are new, and, I ſuppoſe, in ſome reſpects, original, having never ſeen any other of a ſimilar nature publiſhed.

In the ſecond part, the firſt eſſay, though it contains little or nothing new, will be found a proper introduction to the ſubſequent ones.

The ſecond eſſay, containing the theory of the trade-winds, was, as I conceived when it was printed off, original; but I find ſince, that they are explained on the very ſame principles, and in the ſame manner, in the Philoſophical Tranſfor 1735, by *George Hadley*, Eſq. F. R. S.—See Martyn's Abridgment, Vol. 8, part 2, page 500.

The third eſſay, on the variation of the barometer, I ſhould ſuppoſe will be conſidered as having ſome merit; it is new to myſelf, but as I am not well read in the modern productions on the

the atmofphere, I cannot fay it will be found entirely fo to others. It may be proper to obferve, that I had not adopted the theory of vapour which is maintained in the fixth effay, when the third was printed; but I know of no material alteration I would have made in this effay, had it been otherwife.

The fourth and fifth effays are chiefly felected from the publications of others, except that in the latter I have offered fome new thoughts on the effect of the fituation of countries upon their temperature.

In the fixth effay, amongft other things I have advanced a theory of the ftate of vapour in the atmofphere, which, as far as I can difcover, is entirely new, and will be found, I believe, to folve all the phenomena of vapour we are acquainted with; I have attempted to folve feveral, particularly in the appendix.

In the feventh effay the relation betwixt the barometer and rain is inveftigated, from the obfervations in the firft part; fome conclufions are thence obtained in fupport of theory, and from which feveral ufeful and practical obfervations may be deduced.

The eighth effay is the large one on the *aurora borealis*, which I have divided into fix fections;

ſections; this will no doubt attract the attention of philoſophers. The reader will perceive all along, that I have ſpoken of the diſcovery therein contained as an original one; when I wrote the note at page 158, I had not ſeen the Abridgment of the Philoſophical Tranſactions of the Royal Society; but I find from it that the learned and ingenious Dr. *Halley* formed an hypotheſis to account for the *aurora borealis* by magnetiſm; in the Abridgment by *Jones*, Vol. 4, part 2, we find, that the Doctor, after enumerating particulars of ſeveral appearances, conjectures that they are occaſioned by the earth's magnetiſm; and he endeavours to illuſtrate the hypotheſis by placing a *terella*, or ſpherical magnet, with one of its poles upon an horizontal plane ſtrewed with ſteel filings, which being done, the filings form various ſtraight lined and curvilinear figures, according as they are ſituate near to or diſtant from the magnetic pole; theſe he thinks are analogous to the beams of the *aurora borealis*. The *light* of the *aurora* he is pretty much at a loſs to account for, as electricity was then but imperfectly known.——If theſe hints of his had been purſued by others, the fact would undoubtedly before this have been eſtabliſhed, *that the beams of the* aurora borealis *are governed by the earth's magnetiſm;* but inſtead of this, philoſophers have amuſed themſelves and others with forming various other theories to account for the phenomena, moſt of which are extravagant,

gant, not to ſay ridiculous, M. *Mairan*'s *zodiacal light* not excepted. Notwithſtanding what the learned Doctor has ſuggeſted, I preſume it will be allowed, that the above mentioned fact has not hitherto been aſcertained, unleſs it be done in the following work.

Whilſt I am blaming others for framing fanciful theories, perhaps the cenſure may be retorted upon myſelf.—The fourth ſection of the eſſay in queſtion, entitled the 'theory of the *aurora borealis*,' will perhaps be regarded by many as wild and chimerical; but the *facts* which I have endeavoured to aſcertain, reſpecting the *aurora*, will excuſe me for a momentary indulgence of the ideas of a viſionary theoriſt, if they be conſidered as ſuch.

The appendix contains the reſult of barometrical and other obſervations to determine the height of *Kendal* and *Keſwick* above the ſea, more exactly than is ſtated in the preliminary remarks to the obſervations on the barometer; alſo, an account of the heights of ſome mountains in the neighbourhood of *Keſwick*; it concludes with a further illuſtration of the doctrine of vapour, and an explanation of ſome facts relating thereto, particularly thoſe obſerved in working the air-pump.

It will be ſufficiently evident that I have not had a ſuperabundant aſſiſtance from books, in providing

providing and digesting the matter contained in the following pages; by an attentive consideration of facts I have drawn conclusions in some instances which had formerly been done, though unknown to me at the time; these, however, are such as would have been inserted had it been otherwise, and therefore the design of the work is not in any manner frustrated by the circumstance*. At the same time I acknowledge, with particular satisfaction, the friendly aid and assistance of one or two individuals, in the article of books; to one person more particularly I am peculiarly indebted, not only in this respect, but in many others; indeed, if there be any thing new, and of importance to science, contained in this work, it is owing, in great part, to my having had the advantage of his instruction and example in philosophical investigation.

I CANNOT help observing here, that the following fact appears to be one of the most remark-

* Since writing the above, I have met with an account of Mr. *De Luc*'s elaborate work on the modifications of the atmosphere, (vid. the Appendixes to the 49th and 50th vols. of the Monthly Review) from which it appears he maintains nearly the same principles in explaining the variations of the barometer as I have done; his idea of *vapour* too seems not unlike mine.—It is a favourable circumstance to any theory, when it is deduced from a consideration of facts by two persons independently of each other.

able that the hiſtory of the progreſs of natural philoſophy could furniſh.——Dr. *Halley* publiſhed in the Philoſophical Tranſactions, a theory of the trade-winds, which was quite inadequate, and immechanical, as will be ſhewn, and yet the ſame has been almoſt univerſally adopted; at leaſt I could name ſeveral modern productions of great repute in which it is found, and do not know of one that contains any other. The ſame gentleman publiſhed, through the ſame channel, his thoughts on the cauſe of the *aurora borealis*, as mentioned above, which muſt then have appeared the moſt rational of any that could be ſuggeſted, and yet I do not find that any body has afterwards noticed it, except *Amanuenſis* (ſee page 159). On the other hand, *G. Hadley*, Eſq. publiſhed in a ſubſequent volume of the ſaid Tranſactions, a rational and ſatisfactory explanation of the trade-winds; but where elſe ſhall we find it?

Mancheſter, Sep. 21, 1793.

ERRATA.

IN Part firſt, the ſections after the eighth are numbered wrong; they ſhould be corrected as follows:

Page 52, for Section *tenth*, read Section *ninth*.
Page 54, for Section *eleventh*, read Section *tenth*.
Page 61, for Section *twelfth*, read Section *eleventh*.

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METEOROLOGICAL

OBSERVATIONS AND ESSAYS.

PART FIRST,

OBSERVATIONS.

SECTION FIRST,

Of the Barometer.

THE barometer, or common weather-glaſs, conſiſts of a ſtraight glaſs tube, above 31 inches long, and open at one end, that has been filled with quickſilver, and afterwards inverted into a baſon of the ſame fluid, by applying a finger to the open end, ſo as to exclude all air from entering the tube; in this caſe, the finger being withdrawn, and the tube erected, the quickſilver leaves the top of it, and ſinks ſo as to ſtand at the height of about 29 or 30 inches

above

above the ſurface of that in the baſon; it is then applied to a frame, with a ſcale graduated ſo as to mark at all times the height of the column, in inches and tenths, &c. The inſtrument thus completed is called a barometer.—It is uſual now to blow a pretty capacious bulb at the open end of the tube, and then bend the tube a little above the bulb, ſo that the bulb may ſtand upright, leaving a little orifice in it to admit the quickſilver; then the tube being filled as before, upon being inverted, the column of quickſilver in the tube ſtands at the height of 29 or 30 inches above the ſurface of that in the bulb, as in the former caſe.

The reaſon of the fact may be explained thus: every body that ſupports another, bears all its *weight;* therefore when the ſurface of any non-elaſtic fluid is expoſed to the air, it bears the weight of a column of air whoſe baſe is equal to the ſaid ſurface, and its height that of the atmoſphere, ſuppoſed to be 40 or 50 miles; now though air be a very light ſubſtance, being in its uſual ſtate, at the earth's ſurface, about $\frac{1}{800}$th part of the weight of an equal quantity of water, yet ſo prodigious a column of it as that above mentioned, has a very conſiderable weight; moreover, it is a fundamental principle in hydroſtatics, that the preſſure upon the ſurface of a fluid muſt be the ſame on each part, or the fluid will not reſt till that is the caſe; if, therefore,

fore, the preſſure be removed from any place of the ſurface, either wholly or in part, the fluid will yield in that place, and aſcend till the weight of the column of fluid above the ſurface, together with the preſſure upon the column, if any, are equal to the general preſſure upon the fluid in every other part.—In the caſe of the barometer, there is a *vacuum* at the top of the column, and conſequently no preſſure upon its ſurface, ſo that the weight of the column alone balances the preſſure of the atmoſphere without, upon the ſurface of the fluid in the baſon. This *equilibrium*, between the mercurial column and column of air, is very clearly illuſtrated and confirmed by means of the air-pump; for, when a barometer is incloſed in a receiver, as the air is exhauſted, and its preſſure of conſequence decreaſed, the mercurial column deſcends proportionally. It appears then, that the weight of the air ſupports the mercury in the barometer, and that the weight of the mercurial column is equal to the weight of a like column of air extending to the top of the atmoſphere.——When the tube is bent at the bottom, and turned up, the ſame reaſoning, joined to the principle that fluids in bent tubes always riſe to the ſame height in each leg, when they are both open to the atmoſphere, will explain the fact in this caſe.

From theſe conſiderations, the weight of the whole atmoſphere may be readily found; for, it is equal to the weight of a quantity of quick-silver sufficient to cover the whole surface of the globe to the height of 30 inches nearly.

The great weight and pressure above ascribed to the atmosphere, and their effects, are assented to by philosophers of the present age, without scruple; but people not much versant in philosophical enquiries admit them with reluctance; they apprehend, that if bodies were pressed with the force above mentioned, which amounts to about 15lbs. avoirdupoise upon each square inch of surface, the effect should be obvious; whereas it is found that bodies of the slightest texture are unhurt by the atmosphere,—and the great facility with which bodies are moved in the atmosphere, they conceive as another objection.—Perhaps it may be some help to these to observe, that the atmosphere presses equally upon bodies in every direction, and has therefore no tendency to separate their parts; and, as for the resistance which bodies meet with in moving through the atmosphere, it is not proportionate to the *pressure* of the atmosphere, but to its *density*, which being very little, as has been observed above, the resistance is small.

The barometer was invented in 1643, by Torricelli, at Florence, in Italy.—The phenomenon soon attracted the notice of philosophers of that age, and the more so, as it seemed to prove

prove the exiſtence of a *vacuum*, when the opinion of its non-exiſtence was general, and the maxim that *nature abhors a vacuum*, was almoſt unqueſtioned. Had the quickſilver ſtill continued to fill the tube when erected, the fact would have been accounted for on this imaginary principle, and have paſſed without further notice. As it was, however, thoſe who ſtill adhered to the maxim were reduced to great difficulties, and forced to have recourſe to various unmeaning ſubtilties, to get rid of the *vacuum*; whilſt many began to queſtion the truth of the maxim itſelf. At length it was clearly proved, from the inſtance in queſtion, and from other phenomena, that the maxim was contradictory to the laws of nature; the ſuſpenſion of the mercury in the barometer was attributed to its true cauſe, the weight of the air; and the ſpace at the top of the tube was aſcertained to be nearly a perfect *vacuum*, or ſpace void of matter. This diſcovery, as it led to that of the air-pump, and other important ones, is juſtly regarded as one of the greateſt in the laſt century.

Torricelli, the inventer of the barometer, obſerved, that if it was ſuffered to ſtand for a length of time, the height of the mercury in the tube was perpetually varying, though its whole range did not exceed 2 inches at that place; it was further noticed, that this variation ſeemed to have ſome affinity to the weather, the quickſilver being

being generally low in windy and rainy weather, and high in ferene and fettled weather, which circumftance procured the inftrument the name of *weather-glafs*. This difcovery promifed to be of the utmoft importance to mankind, by enabling them to forefee thofe changes in the atmofphere, the knowledge of which was fo interefting to them, and the moft fanguine expectations were entertained on the fubject. The experience of a century and a half has now been obtained, from which the barometer does not feem to be that infallible guide that it was once expected to be, though it is certainly a very ufeful inftrument in this refpect, in the hands of a judicious and fkilful obferver.—But of this more hereafter.

SEVERAL ingenious contrivances have been ufed, by different perfons, to make barometers of a more ample range, in order to obferve minute alterations of weight in the atmofphere; but all thefe are liable to fuch objections as render the common upright one preferable.

Thofe who wifh to make barometrical obfervations, in order to compare them with others, fhould be well affured of the accuracy of their inftruments;—fuch as incline to make their inftruments themfelves would do well to attend to the following particulars,

That

That the tube be not lefs than one-eighth of an inch diameter within.

That the quickfilver be ftrained through a cloth, or rather through leather.

That the infide of the tube be perfectly dry, and the quickfilver dry when put into it.

If there be any moifture in the tube or quick-filver, it expands into an elaftic vapour when the preffure of the air is removed, and, afcending into the *vacuum*, depreffes the mercurial column fometimes to the amount of one-quarter of an inch, or more, below its proper ftation. The criterion to difcover moifture is to apply the warm hand to the *vacuum*, and the mercury will fink confiderably; but it will not be affected if clear of moifture.—Alfo, if upon a gentle agitation of the barometer in the dark, there appear a light in the *vacuum*, it is a fign there is little or no moifture. If, upon a gentle inclination, the quickfilver rife to the top of the tube, and completely fill it, leaving no fpeck, it is clear of air.

The fcale in ftrictnefs ought not to be full inches, but fomething lefs, owing to the rifing and falling of the furface of the refervoir. If the tube have a bulb, then the area of the furface at the top of the column, divided by the fum of the areas

areas of the top and reservoir, will give the part to be deducted; but if the tube be straight, then the whole area of the reservoir, lessened by the area of the glass annulus, made by a horizontal section of the erected tube, must be used as the denominator of the fraction; hence, if the fraction be $\frac{1}{60}$, then the scale of 3 inches must be diminished by half a tenth.

PREVIOUS to the detail of observations, it will be proper to describe the situation of the places of observation. ——The latitude of *London* is 51° 31′ N.—*Kendal* is situate in lat. 54° 17′ N. long. 2° 46′ W. There lies an extensive range of mountains from it in every direction, except to the south. Their height may be from 1 to 6 or 7 hundred yards*; some are near, but from the north to the east their distance is 3, 4, or 5 miles. *St. George's Channel* bears SW. and the high water at spring comes up the river to within 6 miles of the town, but low water is at a great distance. The town may be 25 yards above the level of the sea.—*Keswick* is situate in lat. 54° 33′ N. long. 3° 3′ W.; it is well known to be in the centre of a mountainous country, and some of the highest mountains in the north of England are in its neighbourhood. It is 16 miles from the *Channel*, and perhaps about 45 yards above its level †.

The

* *Benson-knot* is 310 yards above the level of the river; *Whinfel-beacon* is 500 yards above the same; and *Kendal-fell* from 1 to 2 hundred yards.

† It may not be amiss to remind the young Tyro here, that the higher any place is above the level of the sea, the lower will the mean state of the barometer be at that place.

The obſervations at *Kendal* were made by the author, three times each day, namely, betwixt 6 and 8 o'clock in the morning, at noon, and at 8 or 10 in the evening.—Thoſe at *Keſwick* were likewiſe made three times each day, the morning and noon obſervations about the ſame time as at *Kendal*, but the evening obſervations were made at 4 or 5 in the winter, and 6 in the ſummer; the obſerver was Mr. *Croſthwaite*, a gentleman, who, beſides his attention to meteorology, has been for ſeveral years paſt aſſiduouſly furniſhing a *muſeum*, for the entertainment of the touriſts, at preſent conſiſting of a great variety of natural and artificial productions from every quarter of the globe, foſſils, plants, &c. and he has alſo made accurate ſurveys of the lakes.

The obſervations at *London* are taken from the Philoſophical Tranſactions of the Royal Society, being thoſe made there by order of the preſident and council; they are made twice a day, namely at 7 A. M. (in December, January, and February at 8 A. M.) and at 2 P. M.

With reſpect to the barometers at *Kendal* and *Keſwick*, they were both clear of air and moiſture, and exhibited the electric light in the dark. The ſcales were both *full* inches, and therefore the variations were ſomewhat greater than the obſervations denote them.—About $\frac{1}{30}$ ſhould have been allowed upon them.

In the following account we have given the mean ſtate of the barometers, at the reſpective places, for each month of the year, and likewiſe for the whole year, together with the higheſt and loweſt obſervations each month, and the time they took place; as alſo the direction of the wind, and its ſtrength, at the time: the direction we have uſually referred to ſome one of 8 equidiſtant points of the compaſs, and the ſtrength is denoted by the figures 0, 1, 2, 3, and 4, reſpectively, the firſt marking a calm, or very gentle breeze, and the laſt a hurricane.

The obſervations at *London* are only for 3 years, becauſe the later ones could not be procured; thoſe at *Kendal* and *Keſwick* for 5 years, from 1788 to 1792, incluſive. To the end of theſe is added the mean monthly ſtate of the barometer, found from the means of the 5 years, as alſo the mean upon the extremes, the former of which is corrected, on account of the variations of heat in the different months, by which the quickſilver in the barometer is contracted or dilated, though retaining the ſame weight.—We have alſo ſummed up the ſpaces deſcribed by the quickſilver each month, noted the number of changes from aſcent to deſcent, and the contrary, and found their amount for the year.

By the *mean ſtate*, applied to obſervations, is to be underſtood the ſum of all the particular obſervations divided by their number,

The upper part of the following tables, having no abbreviations, is ſufficiently explicit; and in the under part, which contains the days in the ſeveral months on which the higheſt and loweſt obſervations were taken, and the winds at thoſe times, we have uſed H, for higheſt, L, for loweſt, m, for morning, n, for noon, and nt, for night.

N. B. *Kendal* bears N. 30° W. from *London*, diſtant nearly 226 Engliſh miles, meaſured on a great circle of the earth; *Keſwick* bears N. 35° W. from *Kendal*, diſtant 22 Engliſh miles, meaſured on a great circle.

1788.

1788.

	LONDON.			KENDAL.			KESWICK.		
	Mean	highest	lowest	Mean	highest	lowest	Mean	highest	lowest
Jan.	29.97	30.70	28.89	29.87	30.56	28.38	29.82	30.56	28.35
Feb.	29.68	30 21	28.65	29.47	30.22	28.65	29.42	30.17	28.61
March	29.68	30.08	29.32	29.56	30.09	29.15	29.51	30.07	29.12
April	30.07	30.48	29.50	29.95	30.41	28.97	29.89	30.36	28.92
May	30.04	30.34	29.58	30.02	30.41	29.47	29.94	30.32	29.37
June	29.94	30.28	29.49	29.94	30.31	29.50	29.89	30.26	29 46
July	29.99	30.22	29.73	29.82	30.12	29.47	29.76	30.12	29.40
Aug.	29.95	30 45	29.22	29 83	30.37	29.19	29.77	30.37	29.14
Sep.	29.86	30.25	29.37	29.74	30.16	29.28	29.67	30.09	29.20
Oct.	30.32	30.55	29.64	30.07	30.62	29.50	30.02	30.63	29.43
Nov.	30.11	30 50	29 61	29.98	30.34	29.22	29.92	30.32	29.20
Dec.	29.92	30.33	29.50	29.92	30.28	29.53	29.90	30.23	29.50
Inches	29.96 annual mean			29.85 annual mean			29.79 annual mean		

		London	Kendal	Keswick
Jan.	H	16 n WNW 1	16 n & nt W 1	16 n & nt W 2
	L	3 n S 2	3 n S 2	4 m W 1
Feb.	H	7 m & n NE 1 (*a*)	7 n SE 0	7 n calm
	L	21 n SSE 1	21 n & nt NE 2 (*b*)	21 nt SE 1
Mar.	H	3 n E 2 (*c*)	3 n & nt NE 1	3 n & nt W 0
	L	23 m N 2	1 m SE 1	1 m SW 0
Apr.	H	9 m WNW 1	9 n calm	9 n S 2
	L	3 n W 2	3 n NW 3	3 n W 3
May	H	3 m ENE 2	3 all day NE 2	3 m & n E 2
	L	29 m SW 1	9 n SW 3	9 n SW 3
June	H	5 n W 1	9 all day NE 1	9 n & nt NE 1
	L	27 n S 1	26 nt & 27 m NW 0	27 m NW 0
July	H	21 n NW 1 (*d*)	21 n NW 0	21 n W 1
	L	5 n SSW 1	16 m SW 1	16 m SW 1
Aug.	H	2 m N 1	2 m NW 1 (*e*)	2 n NW 1
	L	14 m SW 1	14 m W 0	14 m W 1
Sep.	H	12 m ESE 1	11 nt NW 0	11 nt NW 0
	L	21 m calm	29 n W 2	29 n W 2
Oct.	H	8 m SEbS 2 (*f*)	8 n & nt NE 0	8 n & nt E 0
	L	16 n W 1	16 m & n SW 0	16 m & n W 1
Nov.	H	1 m E 1	1 m SW 0	1 m SE 0 (*g*)
	L	4 m SW 2	3 nt SW 2	3 nt SW 3
Dec.	H	30 m E 1	28 nt N 0	28 nt NE 0
	L	14 n NE 1	31 nt calm	31 nt calm

(*a*) And 12 m. SW 1. (*b*) And 22 m. NE 2. (*c*) And 11 n. ENE 1.
(*d*) And 31 m. SW 1. (*e*) And 2 n. SE. 1. (*f*) And 8 n. NE 2.
(*g*) And 16 m. NE 0.

1789.

	LONDON.			KENDAL.			KESWICK.		
	Mean	highest	lowest	Mean	highest	lowest	Mean	highest	lowest
Jan.	29.72	30.75	28.58	29.59	30.75	28.12	29.51	30.72	28.09
Feb.	29.70	30.34	28.65	29.52	30.19	28 50	29.44	30.20	28.43
March	29.72	30.13	28.94	29.71	30.12	28.87	29.59	30.13	28.71
April	29.77	30.18	29.10	29.64	30.09	28.94	39.51	29 98	28.77
May	29.88	30.27	29.57	29.77	30.19	29.37	29.66	30.12	29 23
June	29.84	30.23	29 40	29.77	30.20	29.25	29.66	30.12	29.14
July	29 85	30.09	29.54	29.74	30.00	29.50	29 63	29.88	29.40
Aug.	30 06	30 33	29.70	29 99	30.32	29 62	29.88	30.23	29.46
Sept.	29 88	30.38	29.30	29.75	30.25	29 25	29 64	30 15	29.17
Oct.	29.52	30.29	29.00	29.56	30.30	28.59	29 46	30.20	28 48
Nov.	29.70	30.46	28.72	29 60	30.34	28.69	29 48	30.27	28.60
Dec	29 86	30.56	28.88	29.63	30.41	28.72	29 48	30.32	28,57
Inches	29.79 annual mean			29 69 annual mean			29.58 annual mean		

		London		Kendal		Keswick	
Jan.	H	5 n	ENE 1	5 nt	N 0	5 n & nt	E 0
	L	18 n	S 2	18 n	S 3	18 n	SW 2
Feb.	H	17 m	SW 1	16 nt	calm	16 nt	NW 1
	L	25 n	SW 2	25 n & nt	SW 1	25 n & nt	NE 1 N 2
Mar.	H	3 m	NNE 1	6 m & n	NE 1	6 m & n	NE 1 N 1
	L	13 n	W 1	13 m & n	NE 2 E 3	13 n	SE 1
Apr.	H	21 m	WSW 1	9 nt	SW 0	9 all day	E 0
	L	3 m	SW 1	26 n	SW 3 (*a*)	26 m & n	W 1 (*b*)
May	H	19 n	W 2	11 n & nt	SW 0	11 nt	W 1
	L	31 m	SW 1	17 nt	SW 2	17 nt	S 2
June	H	13 m & n	ENE 1	13 n & nt	N 0	13 n & nt	S 1
	L	4 m	WSW 2 (*c*)	22 m	SW 0	22 m	E 0 (*d*)
July	H	1 m	W 1	28 & 30 nt	calm	28 & 30 nt	W 1 S 0
	L	13 m & n	ESE 1	17 n	W 1	17 n	S 0
Aug.	H	18 m	NNW 1	17 m	NE 0	17 all day	NE 0
	L	21 n	W 1 (*e*)	22 & 31 nt	calm	31 nt	calm
Sept.	H	12 n	WNW 1	12 n	SW 1	12 n	NW 1
	L	19 n	WNW 1	19 n	N 0	19 nt	NW 0
Oct.	H	27 n	NNW 1	27 nt	NE 1	27 nt	N 1 (*f*)
	L	6 m	W 2	1 n	SW 3	1 m	S 2
Nov.	H	27 m	W 1	27 all day	N 0	27 all day	NW 0
	L	7 m	N 1	6 n & nt	NE 2	6 nt	N 3
Dec.	H	9 m & n	W 1	9 all day	SW 0	9 nt	W 1
	L	15 m	W 2	15 n	W 1	15 n	SW 2

(*a*) And 27 m. SE 1. (*b*) And 27 m. SE 0. (*c*) And 4 n. also 22 n. S 2.
(*d*) And 4 m & n. W 1. (*e*) And 22 m. W 1. also 31 m & n. SSE 1.
(*f*) And 28 m. NW 0.

1790.

	LONDON.			KENDAL.			KESWICK.		
	Mean	highest	lowest	Mean	highest	lowest	Mean	highest	lowest
Jan.	30.07	30.47	29.27	29.91	30.34	28.65	29.89	30.36	28.64
Feb.	30.25	30.62	29.88	30.06	30.41	29.47	30.02	30.41	29.40
March	30.26	30.65	29.83	30.18	30.59	29.57	30.15	30.59	29.48
April	29.86	30.30	29.38	29.85	30.28	29.28	29.81	30.28	29.19
May	29.90	30.14	29.50	29.85	30.28	29.25	29.82	30.28	29.19
June	30.03	30.35	29.49	29.89	30.25	29.31	29.87	30.28	29.22
July	29.84	30.20	29.29	29.72	30.09	29.34	29.68	30.15	29.28
Aug.	29.97	30.16	29.64	29.81	30.03	29.47	29.77	30.05	29.42
Sept.	30.00	33.42	29.31	29.87	30.34	29.25	29.84	30.34	29.14
Oct.	29.89	30.40	29.62	29.80	30.28	29.22	29.75	33.26	29.11
Nov.	29.81	30.40	29.02	29.75	30.34	28.97	29.70	30.28	28.90
Dec.	29.88	30.38	28.80	29.70	30.34	28.84	29.67	30.31	28.74
Inches	29.98 annual mean			29.87 annual mean			29.83 annual mean		

Jan.	H	7 n	WNW 1	7 n & nt	calm	7 n & nt	E 1, S 0
	L	28 n	SW 2	28 nt	SW 2	28 nt	W 0
Feb.	H	4 n	W 1	4 n & nt	W 1	6 n	W 1
	L	26 n	SW 2	26 n	SW 2	26 m	SW 4
Mar.	H	16 m	NNE 1	15 m & n	NE 0	15 n	SE 0
	L	24 m	E 1	10 m	W 2	10 m & n	SW 4 W 3
Apr.	H	3 m	N 2	2 all day	NE 1 (*a*)	2 n	SE 1 (*b*)
	L	11 m	NE 2	30 m	S 2	30 m	S 1
May	H	13 m	NE 2	12 n & nt	NE 1 (*c*)	12 all day	NE 2 (*d*)
	L	2 m	SSW 2	2 m	SW 1	2 m	NE 0
June	H	21 m & n	WSW 1	14 nt	NE 0 (*e*)	14 nt 15 m	W 0 E 0
	L	9 n	SSW 2	9 nt	SW 0	9 nt	calm
July	H	7 n	NW 1 (*f*)	7 all day	calm	7 n & nt	S 1 NW 1
	L	5 n	W 1	13 n & nt	W 1 (*g*)	20 m & n	SW 3 W 2
Aug.	H	18 m	WNW 1	11 nt & 12 m	W 0	11 all d.	NW 0 (*h*)
	L	26 m	———	3 n	SW 0	3 m & n	SW 1
Sep.	H	26 n	N 1 (*i*)	26 n	N 0	26 n	NW 0
	L	3 m	W 2	3 m	W 0	20 nt	SW 3
Oct.	H	16 n	E 1	16 n & nt	W 0	16 n & nt	W 0 SE 0
	L	23 n	E 1	12 nt	W 2	12 n	S 3
Nov.	H	28 m	N 1	14 m	NE 1 (*k*)	14, 15 all day	(*l*)
	L	21 m	SW 2	19 nt	N 0	19 all day	S 0 E 0
Dec.	H	6 n	NW 1	6 n & nt	NE 0	6 n	calm
	L	18 m	W 2	15 m	SW 4	15 m	WSW 4

(*a*) And 3 all day E 1. (*b*) And 3 all day NW 1. (*c*) And 13 m. NE 1. (*d*) And 13 n. NE 2. (*e*) And 15 m & n. NE 0. (*f*) And 17 m & n. NW 1. also 26 m. WSW 1. (*g*) And 20 m & n. SW 2. (*h*) And 12 all day NW 0. also 14 n. & 18 m. SW 0. (*i*) And 26 m. WNW 1. (*k*) And 13 nt & 27 nt. NE 1. (*l*) E 2. SE 0.

1791.

	KENDAL.			KESWICK.		
	Mean	higheſt	loweſt	Mean	higheſt	loweſt
Jan.	29.33	30 22	28.40	29.23	30.17	28.31
Feb.	29.83	30.47	29.00	29.77	30.42	28.85
March	30.06	30.59	28.88	30.01	30.51	28.82
April	29.72	30.12	28.97	29.66	30.11	28.91
May	29.94	30.37	29.22	29.90	30.37	29.08
June	29.89	30.19	29.50	29.86	30.17	29.40
July	29.76	30.22	29.22	29.71	30.19	29.11
Auguſt	29.96	30.47	29.47	29.91	30.48	29.29
Sept.	30.04	30.34	29.31	30 01	30.31	29.19
Octo.	29.62	30.47	28.56	29.55	30.46	28.45
Nov.	29.58	30.15	28.66	29.51	30 11	28.56
Dec.	29.51	30.28	28.84	29.44	30.19	28.68
Inches	29.77 annual mean			29 71 annual mean		

Jan.	H	24 m	W 2	24 m and n	WSW 3&4
	L	20 m	E 1	20 m	E 2
Feb.	H	4 nt	calm	4 nt	calm
	L	18 n	NW 1	18 m	W 2
Mar.	H	8 m and n	SW 1, NE 0	8 m and n	E 1
	L	20 nt, 21 m	SW 2	20 nt, 21 m	SW & W 3
Apr.	H	29 nt, 30 m	NE 1	29 nt, 30 m	NW & NE 2
	L	23 m and n	SW 1	23 m and n	NW & N 1
May	H	28 nt	NE 0	28 n	E 1
	L	19 m	W 3	19 m	WSW 4
June	H	7 m	NE 0	7 m	W 1
	L	30 all day	SW 1	30 n and nt	SE 1, SW 0
July	H	15 m and n	NE 0	15 nt	calm
	L	4 n	SW 3	4 m and n	WSW 3
Aug.	H	20 m	NE 0	19 nt, 20 m	calm
	L	28 m	W 1	28 m	W 0
Sep.	H	26 m	NE 0	26 m	NE 0
	L	4 m	SW 0	4 m	SW 1
Oct.	H	29 all day	NE 1	29 m and n	NE 2
	L	21 m	SW 1	21 m	SE 1
Nov.	H	26 nt	S 0	26 nt	SW 2
	L	16 m	SW 1	16 m	SW 1
Dec.	H	17 m and n	NE 0	17 n	NE 0
	L	13 m and n	SW 1	13 m	WSW 3

1792.

	KENDAL. Mean	highest	lowest	KESWICK. Mean	highest	lowest
Jan.	29.60	30.37	28.87	29.53	30.34	28.65
Feb.	29.87	30.47	29.34	29.82	30.43	29.25
Mar.	29.60	30.41	29.00	29.51	30.40	28.91
April	29.80	30.28	29.16	29.73	30.23	29.02
May	29.88	30.34	29.03	29.82	30.28	28.79
June	29.86	30.37	29.37	29.82	30.39	29.28
July	29.80	30.09	29.47	29.74	30.06	29.37
August	29.86	30.22	29.12	29.81	30.22	29.02
Sept.	29.65	30.22	29.06	29.59	30.17	28.91
Octo.	29.73	30.47	29.09	29.67	30.45	28.94
Nov.	29.90	30.37	29.09	29.82	30.31	28.96
Dec.	29.71	30.28	28.90	29.62	30.20	28.71
Inches	29.77 annual mean			29.71 annual mean		

Jan. H	5 all day	calm	5 all day	calm
Jan. L	16 m	SW 0	15 nt	S 1
Feb. H	16 nt, 17 m	NE 1	17 m and n	NE 0
Feb. L	1 m	SW 1	1 m	SW 0
Mar. H	12 m and n	NE 1	12 m	NE 0
Mar. L	4 n	SW 0	4 n	W 0
Apr. H	29 m	SW 0	29 m and n	calm
Apr. L	4 nt	SW 1	4 nt	calm
May H	5 nt, 6m	NE 1	5 nt, 6m	N 2, NE 1
May L	29 m and n	SE 3	29 m	SE 4
June H	3 all day	N 1	3 n and nt	NE 1, N 0
June L	11 m and n	SW 1	11 m	calm
July H	15 n, 31 nt	W 0	15 n, 31 nt	SW 1, SE 1
July L	27 m and n	SW 1	27 m	calm
Aug. H	1m&n 29 all d.	NE0&1	1 n, 29 n	NE 0
Aug. L	23 m	SW 1	23 m	SW 1
Sep. H	16 m	SW 0	15 nt	SW 0
Sep. L	22 m	SW 0	21 nt	SW 0
Oct. H	24 nt	NE 0	24 nt	N 1
Oct. L	14 n, 15 m	SW 1	14 n and nt	S 1
Nov. H	24 nt	NE 0	24 nt	calm
Nov. L	14 n and nt	SE 2	14 n and nt	SE 1
Dec. H	2 nt	calm	2 nt	calm
Dec. L	6 nt	SW 3	6 nt	SW 4

GENERAL OBSERVATION.

It will be ſeen from the above accounts, that the barometer is generally higheſt and loweſt about the ſame time at all the three places; and if the obſervations had been all taken at the ſame hour, it would have been more generally the caſe.—Whenever the barometer happens to be at the monthly extreme at one place, and not at another, I find it is always near it at the other; the greateſt differences in this reſpect ſeem to take place about the lower extreme, and to be occaſioned by rain,—thus, when it happens to be exceſſively heavy rain at one place, and not at another, the barometer is relatively loweſt where the rain falls.

Mean ſtate of the barometer at Kendal *and* Keſwick, *for the whole 5 years, for each particular month of the year; together with the means upon the extremes of high and low, and the mean monthly range.*

	KENDAL.				KESWICK.			
	Mean*	higheſt	loweſt	range	Mean*	higheſt	loweſt	range
January	29.68	30.45	28.49	1.96	29.62	30.43	28.41	2.02
February	29.77	30.35	28.99	1.36	29.71	30.33	28.91	1.42
March	29.84	30.36	29.09	1.27	29.77	30.34	29.01	1.33
April	29.79	30.23	29.06	1.17	29.72	30.19	28.96	1.23
May	29.88	30.32	29.27	1.05	29.82	30.27	29.13	1.14
June	29.85	30.26	29.38	.88	29.80	30.24	29.30	.94
July	29.74	30.10	29.40	.70	29.68	30.08	29.31	.77
Auguſt	29.86	30.28	29.37	.91	29.80	30.27	29.27	1.00
September	29.80	30.26	29.23	1.03	29.74	30.21	29.12	1.09
October	29.76	30.43	28.99	1.44	29.69	30.40	28.88	1.52
November	29.78	30.31	28.23	1.38	29.70	30.26	28.85	1.41
December	29.72	30.32	28.97	1.35	29.65	30.25	28.84	1.41
Inches	29.79	30.31	29.20	1.21	29.72	30.27	29.00	1.27

The

* The means in this column are corrected, on account of the expanſion of the mercury, by heat; the correction is made by increaſing the height in the colder months, and leſſening it in the warmer months, proportionally to the defect or exceſs of temperature, relative to the mean; it never exceeds .03 of an inch.

The mean monthly range at *London*, upon an average of the 3 years we have given, is, Jan. 1.73 inches, Feb 1.33, March 0.96, April 0.99, May 0.70, June 0.83, July 0.65, Aug. 0.79, Sept. 1.02, Oct. 0.99, Nov. 1.34, Dec. 1.36. Mean range 1.06 inches.

A Table of the mean spaces described by the mercury each month, determined by summing up the several small spaces ascended and descended; also the mean number of changes from ascent to descent, and the contrary, each month, it being reckoned a change when the space described is upwards of .03 *of an inch.—The means are for* 5 *years, at* Kendal *and* Keswick.

	KENDAL.		KESWICK.	
	Mean spaces described by the mercury, in inches.	Mean number of changes, &c.	Mean spaces described by the mercury, in inches.	Mean number of changes, &c.
January	9.97	23	10.15	20
February	7.57	21	7.90	20
March	6.64	19	7.30	21
April	6.06	17	6.15	20
May	5.47	19	5.65	19
June	3.89	16	4.25	16
July	4.98	21	5.20	22
August	4.32	18	4.93	19
September	5.87	19	6.59	20
October	6.30	18	6.24	20
November	7.36	18	7.69	20
December	10.08	22	9.95	24
Ann. space	78.51	231	82.00	241

SECTION SECOND.

Of the Thermometer.

THE next important inftrument in meteorology is the thermometer: by which the temperature, or degree of heat, of the air and other bodies, is determined. An inftrument under this character was invented prior to the barometer, but never brought to a tolerable degree of perfection till the prefent century.

Philofophers are generally perfuaded, that the fenfations of *heat* and *cold* are occafioned by the prefence or abfence, in degree, of a certain principle or quality denominated *fire*, or *heat*;—thus, when any fubftance feels cold, it is concluded the principle of heat is not fo abundant in that fubftance as in the hand; and if it feel hot, then more abundant. It is moft probable, that all fubftances whatever contain more or lefs of this principle. Refpecting the nature of the principle, however, there is a diverfity of fentiment: fome fuppofing it a *fubftance*, others a *quality*, or property of fubftance. *Boerhaave*, followed by moft of the moderns, is of the former opinion; *Newton*, with fome others, are of the latter; thefe conceive heat to confift in an internal vibratory motion of the particles of bodies.

Whatever

Whatever doubts may be entertained respecting the *cause* of heat, many of its effects are clearly ascertained: in treating of those effects it is expedient to adapt our language to one or other of the suppositions respecting the nature of their cause; and as nothing has yet appeared to render the common mode of expression unphilosophical, we shall therefore speak of fire as a *substance*, under the denomination of fire, or heat.

One universal effect of fire is its expanding or enlarging those bodies into which it enters; which bodies subside again when the fire is withdrawn. *Solids* are least expanded by it; *inelastic fluids*, as water, spirits, &c. are more expanded; and *elastic fluids*, as air, most of all. Hence, if a glass tube of very small bore, and a large bulb at the end, be filled with any liquid so as it may rise into the stem, and heat be applied to the bulb, the liquor will rise in the tube, and it is obvious to infer, that the larger the bulb and the smaller the bore of the tube, all other circumstances being the same, the greater will be the ascent for a given variation of heat: such an instrument, when applied to a frame properly graduated, is called a *thermometer*. Different fluids have been occasionally used for thermometers, but none is found to answer so well, in all respects, as quicksilver.

Boiling water is of a conftant and uniform temperature at all times and places, provided the barometer be at a certain height; and a mixture of pounded ice, or fnow, and water is likewife of an uniform temperature. Hence, we are favoured with the means of finding two fufficiently diftant points upon the thermometric fcale, without the neceffity of another thermometer; thefe are called the *boiling* and *freezing points*, and are marked with 212° and 32° refpectively, upon the common fcale, or that of *Fahrenheit*, the boiling point being found when the barometer ftands at 30 inches. The fcale is divided into equal parts, and extended above and below thefe points, *ad libitum;* when the degrees go below 0, they are counted from it, and termed *negative*, merely for diftinction*. At 55° the word *temperate* is ufually placed upon the fcale, and *fummer heat* at 75°; 98° denotes the ufual heat of the human blood; 112° the heat of the blood fometimes in an inflammatory fever; and at 175° fpirits boil; quickfilver itfelf boils at about 600°.

Reaumur's fcale, ufed by fome philofophers on the continent, marks the freezing point with 0°, and the boiling point with 80°.

THE

* The Tyro will pleafe to obferve, that the term 0°, does not imply a total deprivation of fire; it is a mere arbitrary term, and there would have been no lefs propriety in calling it 100°, or 1000°, than 0°.

THE following is the refult of obfervations on the thermometer, taken three times a day, at *Kendal* and *Kefwick*, from 1788 to 1792, inclufive. The morning obfervations were taken between 6 and 8 o'clock; the mid-day obfervations about 12 or 1; the night obfervations at *Kendal* about 9 or 10, but at *Kefwick* at 6 in fummer, and 4 in winter; this circumftance makes the mean temperature of *Kefwick* too high, when compared with that of *Kendal*, which ought to be noticed in the comparifon.

The fituation of the thermometers too, is another particular that fhould be adverted to;—that at *Kendal* was without, in a garden, under the fhade of a pretty large goofeberry tree, facing the north: the garden is open to the country in the north, and has houfes at the diftance of 8 or 10 yards to the fouth. The thermometer at *Kefwick* is fituate near, but not in contact with, the wall and window of a houfe facing the north, which is open to the country; it is about 6 yards above the ground; the fun never fhines on it in winter, and only a few weeks in fummer, and that early in the morning, long before the obfervation is taken.

From thefe accounts it is obvious to infer, that the thermometer at *Kefwick* will not be liable to the *extremes* of heat and cold, owing to the influence of the adjoining wall; whereas that at *Kendal* is perhaps liable to too great an extreme of heat, occafionally, owing to the reflection from the ground, though the fun never fhines upon the frame for an hour at leaft before any obfervation is taken.

The following tables, it is prefumed, will be fufficiently explicit; we have given a table each year, containing the days on which the extremes of heat and cold happened at each place, as with the barometer.

AT

AT KENDAL, 1788.

	Morning.			Noon.			Night.			month.
	Mean	high.	low.	Mean	high.	low.	Mean	high.	low.	means.
	°	°	°	°	°	°	°	°	°	°
Jan.	37.7	46	20	41	47	31	38.3	46	24	39
Feb.	36.3	44	28	41	47	31	37.7	46	28	38.3
March	33.9	46	18	40.3	50	31	36.3	50	21	36.8
April	43.8	49	32	49.5	69	39	45.8	55	34	46.3
May	48.7	61	39	61.8	80	43	48.6	61	38	53.0
June	55.0	60	47	66.4	80	57	52.5	60	45	57.3
July	55.0	62	49	61.0	68	53	54.4	62	47	56.8
Aug.	53.5	58	47	63.7	74	57	54.2	60	49	57.1
Sep.	49.5	60	35	59.4	70	50	51.8	62	43	53.6
Oct.	41.5	55	28	52.6	58	47	43.1	57	30	45.7
Nov.	38.3	50	27	44.5	52	33	39.3	52	28	40.7
Dec.	26	40	10	33.5	46	23	27.6	40	18	29
an. m.	43.1			51.2			44.1			46.1

AT KESWICK, 1788.

(*The observations on the thermometer at* Keswick *this year were not complete till May.*)

	Morning.			Noon.			Night.			month.
	Mean	high.	low.	Mean	high.	low	Mean	high.	low.	means.
	°	°	°	°	°	°	°	°	°	°
May	54.8	71	41	61	77	41	58.9	72	39	58.2
June	57.6	67	52	62.3	77	56	60.5	75	51	60.5
July	58.6	68	52	61.7	70	58	60.3	64	56	60.2
Aug.	58.5	62	52	63.2	75	56	61.6	72	53	61.1
Sep.	54.6	64	46	58.3	68	48	55.9	66	48	56.3
Oct.	44.4	57	28	50.5	59	41	46.8	57	34	47.2
Nov.	42.1	52	28	44.2	52	32	42.5	54	28	42.9
Dec.	26.2	43	8	30.2	44	18	28.5	41	17	28.3

1788.

1788.

		Morning.		Noon.		Night.	
		Kendal.	*Keswick.*	*Kendal.*	*Keswick.*	*Kendal.*	*Keswick.*
Jan.	H	24th day		24th & 27th days		26th day	
	L	15		15		24	
Feb.	H	15		15		14	
	L	2		2		10	
Mar.	H	30		22, 30		30	
	L	7, 11		13		7, 8	
Apr.	H	11, 12		30		30	
	L	4. 5		4		4	
May	H	28	26	26	26, 27	27	25, 27
	L	6	29	29	29	10	29
June	H	18	16, 17	21	17	17, 18, 20	17
	L	1, 8	12, 20, 28	19	27, 28, 30	9	19
July	H	12	13	12	13	11	12, 13, 30
	L	28	5	10	7, 8, 22	8	25
Aug.	H	1,13	3, 10, 13, 14	4	4	12	3, 4
	L	18	27	23	26	19	24
Sep.	H	5	8	4	4, 8	4	4
	L	15	21	29	21	14	21
Oct.	H	2	2	2, 5, 22	2, 22	2	22
	L	19	18	18, 19, 20	18	18	18
Nov.	H	11, 12	11	3, 11	2, 3, 11, 12	12	3, 11
	L	16	15	16	15	15	26
Dec.	H	24	25	24	24	24	24
	L	16	8	16	15, 16	15, 17, 28	17, 28

AT KENDAL, 1789.

	Morning.			Noon.			Night.			month, means.
	Mean	high.	low.	Mean	high.	low.	Mean	high.	low.	
	°	°	°	°	°	°	°	°	°	°
Jan.	30.8	47	4	36	50	19	32	47	5	33
Feb.	37.7	46	28	42.2	47	32	37.7	46	30	39.2
March	30.5	37	22	41.4	48	31	32	37	25	34 6
April	39 4	47	25	49.8	60	36	40.5	49	31	43.2
May	48.8	55	41	60	70	51	49.4	57	38	52.7
June	51 5	60	38	63.6	79	53	51.6	60	45	55.6
July	54	62	41	64.8	74	54	54.4	62	49	57.7
August	53.7	62	42	69.4	79	60	56	62	50	59.7
Sept.	49.8	57	32	58.4	67	50	49.2	58	37	52.5
Octo.	42.4	55	27	51	57	39	43.4	55	29	45.6
Nov.	35	44	20	42.5	51	31	35.7	45	23	37.7
Dec.	40.6	48	25	43.4	49	35	40.9	49	29	41.6
ann. ms.	42.8			51.9			43.6			46.1

AT KESWICK, 1789.

	Morning.			Noon.			Night.			month.
	Mean	high.	low.	Mean	high.	low.	Mean	high.	low.	means.
	°	°	°	°	°	°	°	°	°	°
Jan.	33	49	7	34.9	50	20	33.8	50	16	33.9
Feb.	37.7	46	29	40.4	48	34	38.7	47	30	38.9
Mar.	29.7	39	23	36.4	46	29	33.2	40	28	33.1
April	41.6	49	28	45.9	56	33	44.1	51	29	43.9
May	50.9	60	41	56.7	68	45	54	68	41	53.9
June	55.3	69	44	59	73	49	55.8	69	47	56.7
July	58.2	66	49	62.4	71	51	60.2	70	50	60.3
August	59.6	67	51	64.9	74	67	62.9	71	51	62.5
Sept.	52.7	61	41	56.4	65	46	54.8	64	46	54.6
Octo.	44.2	52	26	48.9	56	36	46.9	57	32	46.7
Nov.	37	45	23	41.2	48	31	39.1	48	28	39.1
Dec.	42.5	49	31	43.4	49	34	42.2	52	34	42.7
ann. ms.	45.2			49.2			47.1			47.2

1789.

		Morning.		Noon.		Night.	
		Kendal.	*Keswick.*	*Kendal.*	*Keswick.*	*Kendal.*	*Keswick*
Jan.	H	30	30	31	30	26	30
	L	12	12	5, 12	7	5	11, 12
Feb.	H	15	1, 15	1, 15, 18, 24	15	14	1
	L	12	27	4	11, 12, 16	7	11
Mar.	H	20, 21	13	21	21	2, 28	2
	L	24	8, 17	13	7	11	7, 10, 13
Apr.	H	21	20, 21	30	30	16	16, 30
	L	12	4	7	3	4	3
May	H	24, 25, 28	26, 28	13	13	13	13
	L	1, 11	2, 4	5	18	3	3
June	H	18	18	17	17	17	13, 16
	L	2	26	6, 26	6, 28	7, 25	27
July	H	4	3	4	4	4	3
	L	24	23	22	22	23	22
Aug.	H	5	4	13	18	4	18
	L	23	1	7, 22	6	22	30
Sep.	H	1, 4, 9	1	5	6	3, 10	1
	L	17	17	15, 19	19	18	16
Oct.	H	21	21	23	23	20	20
	L	26	31	31	31	31	31
Nov.	H	14	2, 14	3	2	1	10
	L	27	26	27	26	26, 27, 28	26
Dec.	H	6	22	6, 27	6, 7	23, 27	7
	L	1	16	16	16	25	17

AT KENDAL, 1790.

	Morning.			Noon.			Night.			month.
	Mean	high.	low.	Mean	high.	low.	Mean	high.	low.	means.
	°	°	°	°	°	°	°	°	°	°
Jan.	36.3	47	22	40	49	31	37.2	47	24	37.8
Feb.	39.8	47	25	46.2	54	39	41.2	47	28	42.4
March	35.5	45	24	50.7	58	39	38.1	46	29	41.4
April	37.5	46	23	50.1	58	40	37.5	47	28	41.7
May	48.5	55	38	59.8	71	51	48	58	43	52.1
June	52.4	62	43	61.7	76	53	53.4	62	46	55.8
July	51.6	58	41	60.9	67	54	53.2	59	47	55.2
Aug.	52.5	60	38	61.4	71	56	55	63	48	56.3
Sep.	47	56	33	56.7	68	51	49.1	52	42	50.9
Oct.	43.3	55	28	54.3	65	47	45.4	58	33	47.6
Nov.	37.8	50	24	44.1	53	34	37.6	48	21	39.8
Dec.	34.3	46	6	39	49	22	34.9	47	22	36.1
an. m.	43.0			52.1			44.2			46.4

AT KESWICK, 1790.

	Morning.			Noon.			Night.			month.
	Mean	high.	low.	Mean	high.	low.	Mean	high.	low.	means.
	°	°	°	°	°	°	°	°	°	°
Jan.	36.4	48	29	39.7	50	30	38.7	50	32	38.6
Feb.	43.3	49	32	45.4	51	39	43.8	49	35	44.2
March	39.3	49	31	47.7	52	38	44.3	50	38	43.8
April	39	49	31	44.8	54	38	41.2	49	32	41.7
May	50.8	58	45	55.5	64	46	52.9	63	45	53.1
June	55	68	47	58.7	71	49	57.6	70	49	57.1
July	53.9	59	48	59.5	65	52	56.5	63	51	56.6
Aug.	54.8	61	51	60.5	69	55	57.5	64	51	57.6
Sep.	49.2	56	41	53.7	62	43	50.8	62	43	51.2
Oct.	46.5	59	35	52.1	62	44	48.8	59	37	49.1
Nov.	37.6	49	25	41.6	50	30	39.9	50	26	39.7
Dec.	35.7	47	18	37.9	49	27	36.8	47	28	36.8
an. m.	45.1			49.8			47.4			47.4

1790.

		Morning.		Noon.		Night.	
		Kendal.	*Keswick.*	*Kendal.*	*Keswick.*	*Kendal.*	*Keswick*
Jan.	H	3, 12	14	3	12	12	12
	L	21	9, 15, 21, 30	15, 17	15	20	20, 25
Feb.	H	25	26	28	22	24	22, 25
	L	21	1	1	10	20	10
Mar.	H	2, 12	2	28, 30	20, 22, 30	11	18
	L	17	4, 15, 17, 18	5, 10	10	15, 16	5, 15
Apr.	H	23, 27	29	19	23	22	28
	L	17	11, 13	16	13	14	12, 13, 14
May	H	30, 31	30	29, 31	31	29	16
	L	14	19, 21	4, 27	2	3	5
June	H	22	16	15	22	15, 16	22
	L	13, 28	5, 10	11	11	11	6, 7
July	H	26	4, 26	27	17, 25	17	24
	L	30	30	29	31	21	14
Aug.	H	16	21	12	15	7	16
	L	27	1, 3, 4, 26, 27	4, 9	3, 23	26	29
Sep.	H	12	19	19	19	12	19
	L	8	15	21	14	7	14
Oct.	H	4, 5, 21	22	4	6	21	22
	L	10	25	27	30	9, 30	30
Nov.	H	6	6	6	6	25	6
	L	29	28	18, 29	27	30	30
Dec.	H	10, 13	13	13	13	9	7
	L	20	20	20	1, 20	28	28

AT KENDAL, 1791.

	Morning.			Noon.			Night.			month,
	Mean	high.	low.	Mean	high.	low.	Mean	high.	low.	means.
	°	°	°	°	°	°	°	°	°	°
Jan.	38.3	48	23	40.4	48	32	38.8	48	30	39.2
Feb.	36.5	46	26	41.9	50	36	36.5	46	28	38.3
March	38.3	47	23	47.2	55	39	40.5	48	25	42
April	43.7	53	36	52.8	67	42	44.1	55	37	46.9
May	45	55	34	56.5	73	44	45.1	61	33	48.9
June	51.1	59	38	63.8	81	48	52.2	62	40	55.7
July	54.3	67	48	63.5	78	51	54	66	48	57.3
August	54.8	66	45	64.3	74	48	53.8	62	46	57.6
Sept.	50.3	60	38	63.4	79	52	51.5	60	42	55.4
Octo.	43	57	24	51.8	60	42	43.9	57	24	46.2
Nov.	39.4	49	22	45.2	53	39	39.3	50	28	41.3
Dec.	29	40	10	33	42	20	30.2	46	-10*	30.7
ann. ms.	43.6			52			44.2			46.6

* Thermometer at 8½ P. M. —6°; at 9¼, —10°, and till 10 P. M. —10°.

AT KESWICK, 1791.

	Morning.			Noon.			Night.			month.
	Mean	high.	low.	Mean	high.	low.	Mean	high.	low.	means.
	°	°	°	°	°	°	°	°	°	°
Jan.	37.9	45	24	39.3	49	31	39.2	49	31	38.8
Feb.	35.8	47	25	38.4	47	30	36.7	47	30	37
Mar.	40.4	47	22	43.6	54	34	41.8	48	29	41.9
April	44.4	54	36	49.6	65	41	46.1	59	37	46.7
May	47	64	37	52.7	70	40	48.9	66	40	49.5
June	54.2	70	41	59.3	76	45	56.3	73	41	56.6
July	56.4	70	50	59.8	73	51	57.6	71	50	57.9
Auguft	55.6	64	48	60.9	68	47	58.2	67	45	58.2
Sept.	53.2	66	41	59.1	73	49	56.6	69	46	56.3
Octo.	43.5	55	27	47.7	61	36	46.4	58	32	45.9
Nov.	39.1	47	22	42.3	51	33	40.6	51	32	40.7
Dec.	29.9	39	13	33.2	40	22	32.1	41	15*	31.7
ann. ms.	44.8			48.8			46.7			46.8

1791.

		Morning.		Noon.		Night.	
		Kendal.	*Kefwick.*	*Kendal.*	*Kefwick.*	*Kendal.*	*Kefwick*
Jan.	H	31	25	16, 31	16	16, 30	10
	L	28	28	5	28	3	28
Feb.	H	14	14	7	14	10	14
	L	4	4	4, 23	4	16, 28	2, 3
Mar.	H	15	29, 30	28, 30	30	22	29
	L	2	1	3	21	1	1
Apr.	H	17	15	16	16	16	16
	L	1	11	6	5	25	6, 10
May	H	28	31	30	30	30	30
	L	4, 8	6	1	1	6	3, 6, 23
June	H	4	4, 5	6	4	3, 5	3
	L	14	11, 12	21	12	13, 21	12
July	H	18	17	17	16, 17	17	17
	L	5, 14	5, 6	4	4	10	4
Aug.	H	15	23, 24	15	12, 20, 23	14	23
	L	19	30, 31	31	31	31	31
Sep.	H	10	11	11	11	10	11
	L	30	19	22	19	1	18
Oct.	H	4	4	3, 4, 5	3	3	3, 4
	L	24	23	26	22	23	23
Nov.	H	11	11	13	11	11	11
	L	6	6	6, 18, 30	6	6	18
Dec.	H	2, 31	23, 31	2, 31	1, 27, 31	1	1
	L	15	11	11	11	11	11

* Thermometer at 4 P. M. 15°; at 10 P. M. 8°; at 1 A. M. 6o.

AT KENDAL, 1792.

	Morning.			Noon.			Night.			month.
	Mean	high.	low.	Mean	high.	low.	Mean	high.	low.	means.
	°	°	°	°	°	°	°	°	°	°
Jan.	32.3	46	11	37.5	48	26	32.5	49	12	34.1
Feb.	37	49	18	44	55	35	37.6	46	27	39.5
March	38.2	48	20	46.3	54	32	39.3	48	22	41.2
April	45	52	36	54.4	72	43	43.9	50	29	47.8
May	45.3	52	35	54.8	62	47	45.4	52	36	48.5
June	52.1	61	48	61.4	75	52	50.2	58	44	54.5
July	56.1	62	51	64.3	72	56	54.9	62	50	58.4
Aug.	55.6	66	48	69	83	58	56.1	66	48	60.2
Sept.	47.6	59	34	56.8	69	46	48.5	59	36	51
Oct.	43.4	58	28	51.4	63	46	44	56	35	46.3
Nov.	41.6	50	24	47.1	58	34	42	51	30	43.6
Dec.	36.9	50	21	40.3	51	29	37.7	52	24	38.3
an. ms.	44.3			52.3			44.3			47

AT KESWICK, 1792.

	Morning.			Noon.			Night.			month.
	Mean	high.	low.	Mean	high.	low.	Mean	high.	low.	means.
	°	°	°	°	°	°	°	°	°	°
Jan.	32.1	45	12	35	47	20	33.4	47	17	33.5
Feb.	36.5	47	19	41.2	53	30	39.7	50	30	39.1
March	37.3	47	17	42.3	51	25	39.6	47	21	39.7
April	44.8	53	35	51.2	67	41	46.9	65	35	47.6
May	46.5	53	36	51.8	62	40	48.2	56	39	48.8
June	53.4	63	46	57.1	66	48	54	63	46	54.8
July	57.3	63	49	61.4	69	50	57.9	65	48	58.9
Aug.	60	71	47	65.2	75	54	59.9	70	48	61.7
Sept.	48.9	59	38	53.9	64	43	51.2	60	42	51.3
Oct.	44.9	57	32	48.8	64	41	46.6	57	37	46.7
Nov.	42.2	55	23	45.7	60	32	44.2	57	33	44
Dec,	36.4	51	20	38.7	51	23	38	49	24	37.7
an. ms.	45.0			49.4			46.6			47

1792.

		Morning.		Noon.		Night.	
		Kendal.	*Keswick.*	*Kendal.*	*Keswick.*	*Kendal.*	*Keswick.*
Jan.	H	31	31	30, 31	31	30	30
	L	13	11	11	11, 12	11	11
Feb.	H	2	2	26	12	6, 27, 29	26
	L	21	21	18, 19, 24	18	20, 21	18, 20, 21
Mar.	H	29	1	17	1, 2	17, 24, 28	2, 17, 30
	L	11	9	13	8	9	8
Apr.	H	13, 14, 29	12, 13	10	11	11, 13	11
	L	7	5, 20	5	5	20	19
May	H	19	15	27	12	24, 27	24
	L	11	1	1, 3	1, 2	2	1, 10
June	H	5	16	29	16	16	4
	L	2, 10, 20	8, 10	12	19	2	19
July	H	16	9, 10, 15, 24	29	29	15	31
	L	30	1	5, 11	11	28	11
Aug.	H	1	3	1	3	2	3
	L	28, 29	28	20	28	19, 28	28
Sep.	H	13	2, 7	2	2	7	4
	L	16, 22	22	21	22	15	27
Oct.	H	1	1	1	1	1	1
	L	12	24	12	3, 4	11	24
Nov.	H	3, 5, 8, 11	5	5	5	3	5
	L	17	17	20	17	19	16, 17, 19
Dec.	H	10, 18, 20	18	18	18	18	18
	L	25, 31	24	23	24	7	23

The monthly and annual means of the thermometer, upon 5 years, are as under.

	Jan.	Feb.	Mar.	April	May	June	July	Aug.
At Kendal	36.6°	39.5°	39.2°	45.2°	51°	55.8°	57.1°	58.2°
At Keswick	36.8	39.5	39	45.3	52.7	57.1	58.8	60.2

	Sept.	Oct.	Nov.	Dec.	annual mean.
At Kendal	52.7°	46.3°	40.6°	35.1°	46.4°
At Keswick	53.9	47.1	41.3	35.4	47.3

The

The annual mean at *Keſwick* may perhaps be ſtated more accurately at 46°, as the evening obſervations were taken too ſoon to give the true mean temperature. It may however be proper to obſerve here, that the time or times of the day at which the obſervations ought to be made, in order to determine the true mean, has not, that I know of, been aſcertained.

I made the following obſervations on the temperature of a pump well, the ſurface of which is uſually from 3 to 6 feet below that of the ground; at the end of January its heat was 45°; February, 45°; March, 46°; April, 46°.5; May, 48°; June, 50°; July, 51°; Auguſt, 52°; September, 50°; October, 48°.5; November, 47°.5; December, 45°.——These obſervations give an annual mean of 47°.8.

About the middle of June, 1793, I found the temperature of ſeveral wells in *Kendal*, after having pumped a few gallons of water from each; ſix of the deepeſt, being from 5 to 10 yards below the ſurface of the ground, were juſt 48° each; three other deep ones were 47°.5; one not quite ſo deep was 46°; and three that were only 2 or 3 yards below the ſurface of the ground were 49° each. The deep ones, I believe, in general are ſubject to very little variation in temperature all the year round.

From theſe obſervations on the temperature of wells, I am inclined to think, the heat of the earth at 10 or more yards depth is not the ſame, at *Kendal*, as the mean heat of the air, but ſomething greater. Perhaps this is a general fact; the temperature of the cave of the obſervatory at *Paris*, which is 30 yards below the pavement, is 53°.5; whereas the mean heat of the air there, is only 52°.—However this may be, I cannot believe the mean heat of the air at this place is ſo great as that of the pump water.

SECTION

SECTION THIRD.

Of the Hygrometer.

THE hygrometer is an inſtrument meant to ſhew the diſpoſition of the air for attracting water, or for depoſiting the water it has in ſolution with it.

Some of the greateſt philoſophers of the preſent age have been endeavouring to improve thoſe inſtruments of this deſcription we have already, and to invent others leſs objectionable; but I preſume the object is not yet fully attained. —To aſcertain the exact quantity of water in a given quantity of air, and alſo the diſpoſition of the air for imbibing or depoſiting it, is an object indeed, highly important to the ſcience of meteorology, and to philoſophy in general.

It does not ſuit our intended brevity to enter into a detail of the different inſtruments lately propoſed, with their reſpective merits and demerits; we ſhall only obſerve, that moſt ſubſtances are affected more or leſs with the dryneſs and moiſture of the air, particularly animal and vegetable fibres, which become turgid, and contract by being expoſed to moiſt air. Spunge, paper, &c. imbibe moiſture, and become alternately lighter and heavier by being expofed to the air. Strings, whether made of animal or vegetable fibres, twift and untwift by the moifture and drynefs of the air, and confequently are fhortened and lengthened alternately.—— The force with which a cord contracts is amazingly great. Mr. *Boyle*, who feems to have been the firft that made a feries of experiments of this fort, ufed to fufpend a weight of 50 or 100lbs. to the end of a rope, which was alternately raifed and lowered by the moifture and drynefs of the air, as a fmall weight would have been.

Obfervations on the Hygrometer.

THE only hygrometrical inftrument I have ufed, is a piece of whip-cord, about 6 yards long, faftened to a nail at one end, and thrown over a fmall pulley; in this manner it has been kept ftretched, by a weight of 2 or 3 ounces, fince September, 1787. It is in a room without a fire, and where the air has a moderate circulation; the fcale is divided into tenths of an inch, and begins at no determined point; the greater the number of the fcale, the longer is the ftring, and the drier the air. This ftring has varied in length above 13 inches, or $\frac{1}{16}$ of its whole length. The obfervations were taken three times a day the two firft years, and once a day after, namely, at noon.———The refult follows.

Mean

Mean ſtate of the Hygrometer, at Kendal.

	1788	1789.	1790.	1791.	1792.	Mean of the whole.
January	40.3	83.4	85	85	102.6	79.3
February	54.7	81	92.8	97	100.5	85.2
March	81	106	109	105.7	112	102.7
April	85	112	131.7	116	125	113.9
May	116	123	129	123	128	123.8
June	127	127	129	135	137	131
July	104	126	126	131	134	124.2
Auguſt	113	132	121	129.6	138.5	126.8
September	108.5	114	117	129	120.7	117.2
October	102	104	109	119	123.3	111.5
November	87.6	99	104	113	106.4	102
December	100	85	92	107.7	102.6	97.5
An. means	93.3	107.7	112.1	115.9	119.2	
Drieſt	138	140	141	144	150	
Moiſteſt	15	63	71	65	83	

It is obvious, from the means of the ſeveral years, and likewiſe from the extremes, that the cord has been increaſing in length each year, ſo that, in ſimilar ſtates of the air, the index pointed at greater numbers each year ſucceſſively; this increaſe too appears to have been nearly in arithmetical progreſſion after the firſt year.—In conſequence of this increaſe in the length of the cord, ſome allowance ought to be made in comparing the mean ſtate of the hygrometer in the different months of the year; thus, if the months of June or July be taken for a ſtandard of compariſon, then the means of the preceding months muſt be increaſed, and thoſe of the following diminiſhed, in ſuch proportion as the annual increaſe ſhall require.

The above mentioned inſtrument ſerves to ſhew a variation in the dryneſs or moiſture of the air; but it is very inadequate to the purpoſe for which a hygrometer is deſired.

SECTION FOURTH.

Of Rain-gauges, and an account of the quantity of rain that fell at Kendal *and* Keſwick, *in the years* 1788, 1789, 1790, 1791, *and* 1792, *together with the quantity at* London *in the three firſt of theſe years.*

THE *rain-gauge* is a veſſel placed to receive the falling rain, with a view to aſcertain the exact quantity that falls upon a given horizontal ſurface at the place. A ſtrong funnel, made of ſheet iron, tinned and painted, with a perpendicular rim two or three inches high, fixed horizontally in a convenient frame with a bottle under it to receive the rain, is all the inſtrument required.

In order to determine the depth of water that falls in the open field, with this apparatus, we muſt have given, 1ſt, the weight of the water caught in the bottle; 2d, the area of the aperture of the funnel; and, 3d, the weight of a cubic foot of water, which has been found equal to $62\frac{1}{2}$lbs. avoirdupoiſe. Then, if $a =$ the area of the aperture, in inches, $W = 62\frac{1}{2}$lbs. and w $=$ the weight of the water caught, in pounds, we ſhall have this theorem, per menſuration,

20736

1728

$\frac{20736w}{aW}$ = the depth of water, in inches, that falls upon any horizontal ſurface at the time and place, as required.

By inverting this theorem, one may eaſily find the weight of water correſponding to any given depth; which being once found, it is moſt expeditious, and ſufficiently accurate, when the funnel has 8 or 10 inches diameter, not to *weigh* the water each day, but to *meaſure* it, by means of phials, &c. ſuitable for the purpoſe.

IN the following account, we have given the amount of the rain each month, at *Kendal* and *Keſwick*, for 5 years, except for 3 months at the laſt place; and alſo at *London*, for 3 years: the laſt is taken from the Philoſophical Tranſactions. The rain at the two before mentioned places was taken each evening at 8 or 10 o'clock.——To the account we have added, the number of *wet days* each month, or thoſe on which the rain amounted at leaſt to .001 of an inch.

N. B. My rain-gauge at *Kendal* is 10 inches diameter; and Mr. *Croſthwaite*'s at *Keſwick* about 8: they were both ſufficiently diſtant from trees, houſes, &c.

1788.

	At Kendal.		At Keſwick		At London.
	Inches of rain.	wet days	Inches of rain.	wet days	Inches of rain.
Jan.	5.6160	20			0.439
Feb	3.3064	23			1.461
March	2.8183	16			0.336
April	2.9047	16	3.9204	22	0.607
May	1.1872	10	2.0840	9	0.497
June	2.3137	7	3.6876	9	3.275
July	7.0323	28	6.3757	28	1.620
Aug.	3.0883	18	5.0771	19	2.699
Sept.	4.6756	19	7.1382	23	3.345
Oct.	2.1220	11	1.7537	13	0.103
Nov.	3.0460	18	3.2841	17	0.510
Dec.	1.1470	7	0.9849	12	———
Total	39.2575	193	34.3057	152	14.892
from Mar.	27.5168	134			

1789.

	At Kendal.		At Keſwick.		At London.
	Inches of rain.	wet days	Inches of rain.	wet days	Inches of rain.
Jan.	7.343	22	8.5435	26	1.345
Feb.	8.924	24	9.0442	27	1.605
March	1.347	15	1.3245	21	1.549
April	4.778	19	4.2383	21	0.957
May	5.388	20	3.6611	25	1.103
June	4.311	18	7.0637	19	3.244
July	6.389	25	5.2770	26	2.467
Aug.	1.556	12	3.4569	14	1.864
Sept.	5.436	24	7.2709	24	2.155
Oct.	6.864	21	8.0907	25	3.253
Nov.	5.451	16	6.0965	21	1.244
Dec.	12 048	28	8.1776	27	1.190
Total	69.835	244	72.2449	276	21.976

1790.

	At Kendal.		At Keswick.		At London.
	Inches of rain.	wet days	Inches of rain.	wet days	Inches of rain.
Jan.	6 567	18	5.9377	19	0,967
Feb.	3 662	15	4.0124	17	0 115
March	1.606	10	1.3228	10	0.122
April	1.960	11	2.3198	17	1.470
May	2,645	14	3.4588	18	2.898
June	4.114	17	5 1077	21	0.708
July	7,894	25	6.2509	24	1.700
Aug.	6.200	26	5,8524	26	1.991
Sep.	6.682	16	8.3950	20	0.368
Oct.	5.382	15	6.1304	16	1.108
Nov.	5.345	12	5.0550	13	2.512
Dec.	10 306	24	10.9010	24	2.093
Total	62.363	203	64.7439	225	16.052

1791. 1792.

	1791. At Kendal.		1791. At Keswick.		1792. At Kendal.		1792. At Keswick.	
	Inches of rain.	wet days.	Inches of rain.	wet days.	Inches of rain.	wet days.	Inches of rain.	wet days
Jan.	8.369	28	11.3574	28	4.120	13	4.5041	15
Feb.	6.641	16	9.2244	21	5.820	14	4.9375	20
March	3.641	17	3.1231	17	6.684	23	9.6261	26
April	4.810	17	3.3190	21	10.091	16	11.6460	17
May	3.983	18	3,9963	18	5.922	19	6.5167	21
June	3.493	13	2.0133	20	3.514	16	2.7110	20
July	6.344	18	8.2060	20	5.926	21	3.8643	20
Aug.	5.165	17	5.8852	16	7.398	18	5.9704	16
Sep.	3.409	10	2.7715	11	11.229	28	10.6179	25
Oct.	5.505	22	7.1272	23	6.028	20	6.7357	21
Nov.	6.465	21	8.7238	23	6.030	18	5.8350	14
Dec.	8,375	22	7.8050	23	12.122	27	11.6404	23
Total	66 200	219	73.5522	241	84.884	233	84.6051	238

Mean

journal, every morning, noon, and evening, the height of the clouds, in yards, above the level of the faid lake, when their height did not exceed that of *Skiddaw;* and when it did, he has marked it as fuch.

The refult of 5 years obfervations is contained in the following table. All the obfervations when the clouds were between 0 and 100 yards high are placed in one column, and thofe when they were between 100 and 200 yards high in the next column, &c.—In order to determine what effect the feafons of the year have upon the clouds, in this refpect, we have kept the obfervations in the feveral months diftinct.—It is to be noted, that the column containing the number of obfervations when the clouds were above *Skiddaw*, includes thofe obfervations when there were no clouds vifible; but Mr. *Crofthwaite* has noted this laft circumftance alfo, in the journal, and it appears, that about 1 obfervation in 30, of thofe in that column, fhould be deducted on that account.

Clouds

	Clouds from 0 to 100 yards high.	From 100 to 200 yards high.	From 200 to 300 yards high.	From 300 to 400 yards high.	From 400 to 500 yards high.	From 500 to 600 yards high.	From 600 to 700 yards high.	From 700 to 800 yards high.	From 800 to 900 yards high.	From 900 to 1000 yards high.	From 1000 to 1050 yards high.	Above 1050 yards high.	Number of obfervations.
Jan.	0	9	12	28	53	39	37	32	30	39	36	116	431
Feb.	5	10	5	15	41	45	45	27	43	38	29	94	397
Mar.	2	1	6	11	22	40	32	36	24	32	44	184	434
Apr.	0	4	5	18	24	34	37	26	23	38	35	206	450
May	0	1	4	8	13	31	22	25	30	34	27	270	465
June	0	2	2	6	24	24	29	21	34	41	34	233	450
July	0	2	2	18	35	36	35	25	35	48	38	191	465
Aug.	0	4	5	13	27	39	35	26	25	45	30	215	464
Sep.	0	1	7	13	38	38	32	30	27	51	27	186	450
Oct.	2	0	5	13	26	49	31	31	46	61	37	164	465
Nov.	0	0	3	13	30	58	42	38	46	45	47	128	450
Dec.	1	8	6	23	41	53	39	50	47	45	35	111	460
Total	10	42	62	179	374	486	416	367	410	518	419	2098	5381

It may be proper to obferve, that the fuppofition of the clouds rifing or falling with the barometer, or as the denfity of the air increafes or diminifhes, is not at all countenanced by thefe obfervations.—Alfo, that in very heavy and continued rains, the clouds are moftly below the fummit of the mountain; but it frequently rains when they are entirely above it.

SECTION SIXTH.

Account of Thunder-ſtorms and Hail-ſhowers.

WE ſhall arrange the dates and accounts of theſe, in the order of their ſucceſſion. When the diſtance of the thunder is mentioned, it is calculated by obſerving the number of ſeconds between ſeeing the lightning and hearing the thunder, and allowing 1142 feet of diſtance for every ſecond of time.

Thunder-ſtorms at Kendal *and* Keſwick.

1788.

May 26. Several loud peals of thunder a little before 7, and again before 9, P. M. the laſt very near, at Kendal. The ſame at Keſwick, at 7 P. M. with a few drops of rain. —The ſtorm from the SE.

July 3. From 6 to 7 P. M. much thunder, and very heavy ſhowers at both places. It came from the S.

Auguſt 15. From 7 to 8 P. M. thunder and heavy rain, from the NW. at Keſwick.

Auguſt 16. At 7½ P. M. a tremendous ſtorm paſſed on the SE. of Kendal, 8 or 10 miles diſtant; 20 or 30 flaſhes and reports ſucceeded each other in about half an hour.

September 26. Diſtant thunder in the night, at Kendal. At 7½ P. M. 2 claps at Keſwick, with much rain.

1789.

April 27. At $3\frac{1}{2}$ P. M. ſome loud peals of thunder, at Kendal.

May 13. From $6\frac{1}{2}$ to 7 P. M. ſeveral loud claps of thunder, diſtant, at Kendal.—Between 7 and 8 P. M. much thunder heard at Keſwick, from the SW.

May 17. A little before 3 P. M. one clap of thunder heard at Kendal.

June 12. Diſtant thunder in the evening at Kendal.

—— 19. Diſtant thunder P. M. at Kendal.

—— 20. At 1 P. M. ſeveral claps at Kendal;—the ſtorm returned at 4 P. M. and there were 35 peals in $\frac{3}{4}$ of an hour, many of them uncommonly loud, and near; there was rain in the mean time, but not heavy.

N. B. A woman was killed by lightning, in a houſe at *Sedbergh*, about 11 miles from Kendal.

June 27. Diſtant thunder in the evening, at Kendal.

July 4. Diſtant thunder at 2 P. M. at Kendal.—Loud thunder, and heavy ſhowers, P. M. at Keſwick.

July 6. After 2 P. M. diſtant thunder, at Kendal.

——10. At 3 P. M. a diſtant thunder clap, at Kendal.

——19. At $2\frac{1}{2}$ P. M. diſtant thunder, at Kendal.

——21. Paſt 5 P. M. 3 loud peals, at Kendal; and diſtant thunder, at Keſwick.

Auguſt 29. P. M. ſome thunder, with heavy rain, at Keſwick.

September 29. After 9 P. M. much diſtant thunder, with ſhowers, NW. at Kendal.—At $8\frac{1}{2}$ P. M. one long and loud peal, at Keſwick.

September 30. Diſtant thunder in the night, at Kendal.

1790.

April 26. At 1 P. M. ſome peals of thunder, at Kendal.

May 16. At 9 P. M. one loud crack, from tho E. at Keſwick.

1790.

May 17. At $11\frac{1}{2}$ A. M. one loud crack, and a heavy ſhower, at Keſwick.

June 9. From 6 to 10 P. M. much thunder, with little rain, at Kendal.

June 16. Diſtant thunder in the evening, at Kendal*.—At $8\frac{1}{2}$ P. M. ſeveral loud claps, at Keſwick.

June 22. From 6 to paſt 8 A. M. much loud thunder, with rain, at both places.

Auguſt 27. Some thunder P. M. at Kendal.

September 3. P. M. a little thunder, at Keſwick.

1791.

January 5. Loud thunder in the night, with hail, at Keſwick.

May 21. At 6 P. M. diſtant thunder, and hail ſhowers, at Kendal.

June 4. Betwixt 1 and 2 P. M. ſeveral peals of thunder, at Kendal. The laſt of them was the moſt remarkable one ever remembered at this place;—inſtantaneouſly after the flaſh, was heard a very loud and tremendous crack, exactly ſimilar, but incomparably more loud, than the report of a muſket; every houſe in the town was ſenſibly ſhaken by it, and univerſal terror produced by the loudneſs and ſingularity of the report; but providentially no harm was done.—The rain, mixed with hail, exceeded in quantity what has ever been produced here on a ſimilar occaſion, for 6 years at leaſt; there fell upwards of *one inch and a half* in the ſpace of *three quarters of an hour*, though a conſiderable part of that interval was moderate rain.

N. B. It is remarkable that the barometer was ſtationary all that day, and ſo high as 30.06.

1791.

* There was, this evening, about Preſton-hall, 6 miles from Kendal, one of the moſt extraordinary torrents of hail and rain, attended with thunder, that is upon record.

1791.

June 12. At 4 P. M. a crack of thunder, with hail and rain, at Kendal.

July 17. At 10 P. M. loud claps to the NW. at Kefwick.

——18. After 2 P. M. feveral claps, at Kendal; one of which not unlike that of the 4th of June. At Kefwick, 2 claps A. M. and 3 P. M. with exceffively heavy fhowers.

Auguft 15. Between 8 and 9 P. M. there was the moft lightning I ever remember to have feen at one time, at Kendal; fome thunder was heard, but it was diftant, E.

Auguft 16. From $5\frac{1}{2}$ to paft 7 A. M. much thunder at both places, and heavy rain at Kefwick.

October 20. At $8\frac{1}{2}$ A. M. one loud clap of thunder, at Kefwick, and much lightning from 7 to 10 P. M.—heavy rain all day.

December 25. Much thunder from 5 to 7 P. M. at Kefwick.

1792.

April 13. At 3 P. M. much diftant thunder, at Kendal.

May 27. Between 3 and 4 P. M. fome thunder and rain, at Kendal.

July 9. At 7 P. M. diftant thunder, at Kendal.

——16. P. M. much thunder, at Kendal. Between 6 and 8 P. M. loud thunder, at Kefwick.

July 18. At 8 A. M. thunder, at Kendal.

——25. After 6 P. M. thunder, at Kendal.

Auguft 26. At 3 P. M. fome thunder at Kendal.

October 14. In the evening, lightning; and at 10, diftant thunder, at Kendal. From 6 to 11 the fame evening, lightning, at Kefwick; and at the later hour, one long and loud crack of thunder,

Days

Days on which HAIL *has been noted in the journals at* Kendal *and* Keſwick.

Hail at Kendal.	Hail at Keſwick.
1788. January 18.	1788. April 4. Nov. 4. Dec. 26 & 31.
1789. Jan. 18. Mar. 9. April 26 Oct. 1. Nov. 14. Dec. 15 & 16.	1789. Jan. 15. Feb. 11. April 1, 11, & 24. June 27. Sep. 14 & 30. Oct. 1 & 30. Nov. 13. Dec. 15, 16, & 31.
1790. Jan. 27. April 25. Aug. 3. Dec. 11 & 15.	1790. Feb. 16. April 11 & 14. July 31. Sep. 2 & 3. Dec. 11 & 13.
1791. Jan. 5, 7, 11 & 13. Feb. 1 & 11. Mar. 21. May 21, 22 & 23. June 4 & 12.	1791. Jan. 2, 4 & 5. Feb. 11, 15 & 18. Mar. 21. May 18, 20, 22, 23, & 25. June 12 & 21. July 5. Oct. 8 & 24. Nov. 5, 16 & 28.
1792. Mar. 19. May 22. Oct. 17. Dec. 6 & 22.	1792. Mar. 7. May 1 & 2. June 30. Sep. 20 & 21. Oct. 18. Nov. 15.

N. B. The winds that bring hail-ſhowers are always SW, W, or NW, in theſe places; and the barometer is generally low.

In order to diſcover what particular months or ſeaſon of the year, is moſt liable to thunder-ſtorms and hail-ſhowers, we have collected the ſeveral obſervations, at both places, in each month of the year, into one ſum, and placed them below.

	Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sep.	Oct.	Nov.	Dec.
Thunder	1	0	0	3	7	5	12	7	4	2	0	1
Hail	11	7	5	8	11	6	2	1	6	7	7	13

SECTION

SECTION SEVENTH.

Obſervations on the Winds.

I Have before obſerved, that my obſervations on the winds refer them all to 8 equidiſtant points of the compaſs, and to 5 degrees of ſtrength, marked 0, 1, 2, 3, and 4, reſpectively. Mr. *Croſthwaite* has referred them to 32, or the whole number of points, and to 12 degrees of ſtrength; but I have reduced his obſervations to agree with my own, in order to prepare the following table of compariſon.

The obſervations at both places were made three times each day, namely, morning, noon, and evening.

It may be obſerved, that the high winds do not in general differ materially, either in ſtrength or direction, at *Kendal* and *Keſwick*, as might be expected from the proximity of the places; but when the wind is moderate, there is often a difference in direction; probably the mountainous ſituations of the places may have ſome influence in this laſt caſe.

Here follows Tables containing the number of obſervations on the winds each year, in all the different directions, at both places.

WINDS

WINDS AT KENDAL.

Years.	N.	NE.	E.	SE.	S.	SW.	W.	NW.	Numb. of Obfervati.
1788	131	139	40	79	91	186	84	87	837
1789	94	118	38	49	94	309	76	46	824
1790	100	195	17	21	25	329	137	47	871
1791	62	259	16	33	19	440	138	50	1017
1792	51	294	33	24	35	472	92	33	1034
Total	438	1005	144	206	264	1736	527	263	4583

WINDS AT KESWICK.

Years.	N.	NE.	E.	SE.	S.	SW.	W.	NW.	Numb. of Obfervat.
1788	46	50	158	98	137	105	238	113	945
1789	53	47	150	120	180	146	211	119	1026
1790	32	62	143	105	134	174	237	89	976
1791	44	73	133	66	117	225	257	67	982
1792	49	84	139	88	164	213	219	49	1005
Total	224	316	723	477	732	863	1162	437	4934

To thefe tables we fhall fubjoin an account of thofe days on which the higheft winds prevailed, at one or both places.

Higheft winds, marked 4, *at* Kendal *and* Kefwick.

1788.

Jan. 19. March 16. April 1 and 3. Dec. 26 and 27.

1789.

Jan. 13. Feb. 2, 3, 4, 11, 15, and 24. Oct. 1. Nov. 13. Dec. 15, 18, 19, 20, 24, 25, and 30.

1790.

Jan. 11. Feb. 12 and 26. March 10. June 19. July 5, 20, and 21. Oct. 12. Dec, 15 and 23.

1791.

1791.

Jan. 4, 5, 7, 8, 11, 12, 13, 15, 17, 18, 19, 24, 25, 29, and 30.—N. B. Theſe winds were all W. or SW. except on the 18th and 19th, SE. Feb. 1, 10, 11, 12, 15, 18, 19, and 22. March 4, 13, 19, 21, and 23. May 17 and 19. June 16. Oct. 20. Nov. 9, 11, 12, 19, 26, and 27. Dec. 1, 13, and 25.

1792.

Feb. 2. March 18. April 2, 15, 22, and 23. Sept. 10. Oct. 1, 2, 3, and 31. Nov. 18, 19, 20, and 21. Dec. 4, 5, 6, 9, 10, 11, 18, 20, 22, and 23.

In order to determine what months of the year are moſt liable to high winds, we have found the amount of the number of days in the ſeveral months, on which the higheſt winds were obſerved, according to the above account.

Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sep.	Oct.	Nov.	Dec.
18	17	8	6	2	2	3	0	1	7	12	24.

SECTION EIGHTH.

Account of the firſt and laſt appearance of Snow, each winter; the Froſt, Snow, ſeverity of the Cold, &c.

MOST people know that ſnow firſt appears in general upon the mountains; and the higher theſe are, all other circumſtances being the ſame, the ſooner their ſummits are covered with ſnow; if they exceed a certain height (which varies with the latitude) ſnow continues upon them all the year round, or is perpetual; but this is not the caſe with any mountains in *England*.

The higheſt mountains ſeen from *Kendal* are to the NW. and do not exceed 6 or 7 hundred yards in height, as has been obſerved; it is theſe of courſe that are firſt topped with ſnow. The mountains in the neighbourhood of *Keſwick* are much higher.

The firſt appearance of *hoar froſt*, each autumn, has been pretty carefully noted, but the laſt appearance of it, in the ſpring, has not, it being inconvenient at that ſeaſon to make obſervations previous to the riſing of the ſun.

The dates of the different appearances follow for each year, together with the mean times; or, thoſe times before or after which, upon an equality of chance, the events may be expected in future.

	Laſt ſnow ſeen on the mountains, in the ſpring.		The ſummits of the mountains covered with ſnow.		The firſt hoar froſt on the graſs.	
	Kendal.	*Keſwick.*	*Kendal.*	*Keſwick.*	*Kendal.*	*Keſwick.*
1788	May 30	June 6	Nov. 15	Nov. 13	Sep. 15	Sep. 15
1789	May 14	June 30	Oct. 13	Oct. 29	Sep. 17	Sep. 17
1790	April 25	April 27	Nov. 22	Oct. 31	Sep. 8	Sep. 4
1791	June 12	June 12	Oct. 22	Oct. 22	Oct. 13	Oct. 13
1792	May 1	Mar. 13	Nov. 15	Oct. 9	Sep. 16	Sep. 15
Mean	May 16	May 17	Nov. 8	Oct. 27	Sep. 20	Sep. 19

1788.

1788.

IN the beginning of this year there was very little froſt or ſnow; the moſt ſnow was on the 7th of March, being above 2 inches deep, both at *Kendal* and *Keſwick.*

In the beginning of December the froſt ſet in, and continued for 5 weeks; the mean ſtate of the thermometer for which time was 28°; and at the end of it the froſt had penetrated 16 or 18 inches into the ground.—Above 3 inches of ſnow fell on the 31ſt.

1789.

Not much ſevere froſt after the middle of January.—Snow on the 14th and 21ſt of the ſame. Much ſnow from the 9th to the 14th of March; about 6 inches deep, at an average, both at *Kendal* and *Keſwick.*

Froſt in November; very little in December.

1790.

Little either froſt or ſnow, in the beginning of the year.

On the 17th, 18th, and 19th of December, much ſnow, 4 inches deep, at an average, at both places.

1791.

But little froſt or ſnow in the beginning of the year.

On the 8th, 9th, and 10th of December, a very great quantity of ſnow; the average depth, at *Kendal*, was 11 inches, which was the greateſt obſerved there for 24 years paſt; the average depth at *Keſwick* was about 8 inches.

It was on the evening of the ſucceeding day, the 11th, that the extreme of cold took place; the air was clear, and the wind from the N. but very moderate; the barometer was 29.75; it was riſing before this event, and it fell afterwards. At *Kendal*, the thermometer at 8½ P. M. was —6°, upon the ſnow; afterwards it fell to —10°; in the morning of that day it was 15°, and 20° at noon.—During the extreme cold, a prodigiously dense mist was carried from the river into the town, in which the thermometer fell no lower than 3°, whilst it was —10° to the N. of the river, and the air quite clear. The next morning the thermometer was at 18°, and the day windy, with showers of snow, hail, and rain.

Probably the cold at *Keswick* was as extreme as at *Kendal.* Mr. *Crosthwaite*'s lowest observation was 6°; but the proximity of his thermometer to the house, might be a means of keeping up the temperature in such an extremity as this.

1792.

Strong frost the second week in January.
Little frost or snow in November and December.

SECTION TENTH.

Account of Bottom winds *on Derwent lake.*

DERWENT lake is one of those few which are agitated at certain times, during a calm season, by some unknown cause. The phenomenon is called a *bottom wind.*

Mr.

Mr. *Crofthwaite* has been pretty affiduous in procuring intelligence refpecting thefe phenomena, and in obferving any circumftance that might lead to a difcovery of their caufe; but nothing has occurred yet that promifes to throw light on the fubject.

N. B. The *lake* is near *Kefwick*.

The following is an account of the times and circumftances of the feveral obfervations.

1789.

April 30. From 8 A. M. till noon, the lake pretty much agitated.

Auguft 9. At 8 A. M. the lake in very great agitation; white breakers upon large waves, &c. without wind.

Auguft 27. At 9 A. M. a fmall bottom wind.

1790.

June 20. At 8 P. M. a bottom wind on the lake.

October 11. At 8 P. M. a bottom wind on the lake.

December 1. At 9 A. M. a ftrong bottom wind on the lake.

1791.

The phenomena that took place this year, if any, were not noticed.

1792.

October 28. At 1 P. M. a bottom wind; the water much agitated.

SECTION

SECTION ELEVENTH.

Account of the Auroræ Boreales *seen at* Kendal *and* Keswick.

THE *aurora borealis*, or that phenomenon which in *England* is called the *Northern lights*, or *streamers*, has appeared frequently to all the northern parts of *Europe* since the year 1716, though it seems to have been a rare phenomenon before that time.

Sometimes the appearance is that of a large, still, luminous arch, or zone, resting upon the northern horizon, with a fog at the bottom; at other times, flashes, or coruscations, are seen over a great part of the hemisphere.—We shall describe the general phenomena more at large in the essay on the subject, in the second part of this book; and particular observations will be given at large in the addenda to this section.

Explanation of the following List.

IN the first column we have given the month and day on which the *aurora* was seen; in the second, the hour P. M.; when no hour is mentioned, it is to be understood to have happened between the end of the twilight and 10 o'clock. The third column contains the moon's age at the time, or the number of complete days betwixt the *change* and

and the *aurora;* the fourth contains the days in like manner betwixt the *full* and the *aurora;* the reasons for these two columns will appear in the Essay. In the fifth column we have characterized the *aurora,* by one or more words: *still,* denotes the northern horizontal arch; and *active,* denotes those appearances when distinct flashes and coruscations were seen: but this distinction was not always attended to, and if it had, the *aurora* often exhibits both appearances at the same time; *grand,* denotes a large display of streamers over great part of the hemisphere; *high,* denotes near the zenith, and *low,* near the horizon, apparently.

N. B. The dates of those observations not characterized, I received from a friend; they may be depended on as authentic.

A List of the Auroræ Boreales *observed at* Kendal *and* Keswick, *for 7 years, namely from May 1786 to May 1793, together with the moon's age at the respective times of observation.*

N. B. For distinction's sake we have marked all those that were observed at both places with 2, and those observed at *Keswick* only, with 1; the rest were observed at *Kendal* only.—Those marked D, were doubtful observations, from twilight, or other causes.

1786.	Hour P.M.	☽'s age.	☽ past full.	Character.	1786.	Hour P.M.	☽'s age.	☽ past full.	Character.
May 1		3			Sep. 21		29	14	
——11		13			——26		4		
——22		24	9		——29		7		
July 15		20	4		Oct. 13		21	6	
Aug 11		17	2		——25		3		
——17		23	8		Nov. 14		23	8	
Sep. 8		16	1		Dec. 25		5		active, low.
——19		27	12						Number 16.
——20		28	13						

1787.

1787.	Hour P.M.	☽'s age.	☽ past full.	Character.
Jan. 12		23	9	
——24		5		
——25		6		
Feb. 22		4		
Mar. 21	8	2		active, high
——24	8	5		active, high
Apr. 19	9	1		high. D
——20		2		high. D
——26		8		active, low
May 12		24	10	faint. D
——16	9	28	14	high. D
——17	9	0		active
——18	11	1		active
June 7	11	21	7	active
Aug. 7	9	24	9	active
——19	10	6		active
Sep. 19	9	8		bright, ftill
Oct. 4	10	22	7	ftill
—— 6	10	24	9	active, faint
—— 7	10	25	10	active, large
——17	11	6		active (*a*)
——19	9	8		ftill
Nov. 4	9	24	9	large, bright
—— 8	8	28	13	large, bright (*b*)
——28	9	19	3	ftill, fmall
——29	9	20	4	ftill, fmall
——30	9	21	5	ftill, fmall
				Number 27.
1788.				
Jan. 9	6	1		ftill, low
——10	8	2		ftill, large 2
——11	8	3		ftill, faint

1788.	Hour P.M.	☽'s age.	☽ past full.	Character.
Jan. 13	8	5		tranfient
——14	8	6		large, ftill
——15	9	7		large, ftill
Feb. 4	9	27	12	ftill
—— 6		29	14	active, high 1
—— 7		0		fmall 1
—— 8		1		faint, ftill
——12		5		active, fmall 1
Mar. 7	11	0		active, fmall 2
—— 8		1		active, fmall
——28	10	21	6	bright, large
Apr. 1	10	25	10	
—— 3	10	27	12	large, grand
—— 7	10	1		a glance, clouds
——14	12	8		
——27	10	21	7	ftill, low
——28	10	22	8	active, high 2
——29	10	23	9	large, active
——30	10	24	10	ftill
May 1	10	25	11	a glance, clouds
—— 4	10	28	14	tranfient
——10	10	4		high. D
——11	10	5		large, ftill
——24	10	18	4	very grand (*c*) 2
——25	11	19	5	grand
——27	10	21	7	active
June 3		29	14	large. D
July 30		27	12	active
Aug. 1	10	0		active (*d*)
—— 2	10	1		active
—— 3	10	2		fmall
——19	10	18	3	large, ftill 2

1788.

(*a*) A heavy fhower, with thunder, juft before.

(*b*) Several flafhes of lightning with it, after a very wet day.

(*c*) From 10 to 11 P. M. uncommonly brilliant, active ftreamers over moft of the hemifphere: they were faid to be heard.---Not much inferior the next night.

(*d*) Splendid ftreamers, extent from NE. to W.; no fog beneath.

1788.	Hour P.M.	☽'s age.	☽ paſt full.	Character.
Aug 23		22	7	very grand(*a*) 2
—29	10	28	13	active
Sep. 2		2		ſtill
— 6		6		a glance, clouds
—10		10		faint, ſtill
Oct. 12	4M	12		ſtill
—21	10	22	6	ſtill
—24		25	9	ſtill
—27		28	12	ſtill 1
—30	9	1		ſtill
—31	9	2		large, ſtill
Nov. 1		3		ſtill
—19	9	21	6	ſtill
—27		0		faint, ſtill
—28		1		bright, ſtill
—30	7	3		ſtill
Dec. 21		24	8	ſtill 2
—24		27	11	active
				Number 53.
1789.				
Jan. 11		15	0	ſtill
Feb. 15		20	5	active, ſmall 1
—23	11	28	13	active
—26	10	1		large, active
—28		3		large, bright(*b*)
Mar. 14		17	3	very grand(*c*) 2
—16		19	5	ſtill, ſmall

1789.	Hour P.M.	☽'s age.	☽ paſt full.	Character.
—29		3		a glance, clouds
—30		4		a glance, clouds
Apr. 12		17	2	ſtill
—13		18	3	ſtill 2
—30		5		ſtill
June 12	10	19	5	active
Aug 13		22	8	active, high 1
—14	10	23	9	active, high
—15	9	24	10	ſtill
—16	10	25	11	ſtill
—17	10	26	12	ſtill
—18	10	27	13	active
—19	10	28	14	active, high
—20	10	0		active
—25		5		active 1
Sep. 14	10	24	10	ſtill (*d*)
—15	10	25	11	ſtill
—20	10	1		fine, active 2
—23		4		fine, clouds
—26		7		grand(*e*) 2
Oct. 18		0		active
—19	11	1		active
—20		2		grand(*f*)
—23		5		large, ſtill 2
—25		7		ſtill
—27		9		ſtill
—31	8	13		ſtill

(*a*) Soon after 8 P. M. a broad arch was obſerved, extending quite acroſs the heavens, through the zenith, from E. to W. nearly; but its eaſtern extremity inclined to the north, and its weſtern to the ſouth: afterwards an uncommonly grand diſplay of ſtreamers over two-thirds of the hemiſphere.

(*b*) At $9\frac{1}{2}$ P. M. there was a large bow, like that of the 23d of Auguſt laſt.

(*c*) It began S. of the prime vertical, and afterwards ſpread northward.

(*d*) Flaſhes of lightning, both this evening and the ſucceeding.

(*e*) Moſt of the hemiſphere finely illuminated with ſtreamers.

(*f*) From 8 to 10 a grand diſplay of ſtreamers over great part of the hemiſphere.

1789.	Hour P.M.	☽'s age.	☽ paſt full.	Character.
Nov. 4		17	1	ſtill
—10	11	23	7	ſtill
—14		27	11	bright (*a*) 2
—19		2		large, ſtill 2
—21		4		active 2
—22		5		fine, active
—24	11	7		ſtill
—25	10	8		ſtill
—26		9		ſtill
—27	10	10		active
Dec. 14		27	12	ſtill, clouds
				Number 45.
1790.				
Jan. 14	7	29	13	ſtill
Feb. 3		19	4	ſtill
—9		25	10	ſtill
Mar. 10		24	9	ſtill, faint
—16		1		ſtill
—17		2		ſtill, faint
—18		3		ſtill
—19		4		ſtill, bright
—20		5		ſtill, bright
Apr. 3		19	4	ſtill, low
—4		20	5	active, low
—5		21	6	ſtill, faint
—6		22	7	ſtill
—7	11	23	8	ſtill
—9		25	10	ſtill
—16	11	2		active
—17	10	3		large, ſtill
May 12		28	13	active
—14		0		ſtill
—16		2		ſtill
—18		4		ſtill
Sep. 7	9½	28	14	ſtill
Oct. 9		1		ſtill, faint
—18	7½	10		ſtill 1

1790.	Hour P.M.	☽'s age.	☽ paſt full.	Character.
Oct. 31	8½	23	8	ſtill
Nov. 7	10	1		ſtill
—8	9	2		ſtill
—9		3		ſtill
—10	10	4		ſtill
—12	10	6		ſtill
—16	10	10		ſtill
—27	8	21	6	fine, active 2
—28	8	22	7	ſtill, faint
—30	10	24	9	ſtill, faint
Dec 25		19	4	ſtill, faint
—28		22	7	ſtill, faint
				Number 36.
1791.				
Jan. 6	9	2		very grand 2
Feb. 25		22	7	bright, ſtill
Mar. 3		28	13	ſtill
—5		1		ſtill
—7		3		ſtill 2
—26		22	6	large, ſtill
—29		25	9	ſtill, low
Apr. 3		0		ſtill, ſmall
—20		17	2	ſtill
—23		20	5	ſtill
—25		22	7	ſtill, ſmall
May 12	10	9		active
—20	11	17	2	active
June 10		9		active
Sep 5		7		active
—8	10	10		ſtill, ſmall
—11	9	13		ſtill
—13		15	1	ſtill
—27		0		ſtill
—28		1		ſtill
Oct. 15		18	3	ſtill
—19		22	7	ſtill
—20		23	8	active (*b*)

(*a*) Lightning afterwards.

(*b*) Thunder at a diſtance this evening.

	Hour P.M.	☽'s age.	☽ paſt full.	Character.
1791.				
Oct. 22		25	10	ſtill, bright 2
—23		26	11	ſtill
—29		2		large, bright
—31		4		active
Nov. 3		7		large, active 2
— 4		8		ſtill, faint
— 5		9		ſtill, faint
—11		15	1	ſtill
—14		18	4	ſtill
—17		21	7	ſtill
—18		22	8	ſtill
Dec. 13	6½	18	3	fine, active
—19		24	9	ſtill
—26		1		ſtill
				Number 37.
1792.				
Jan. 9	10	15	0	large, ſtill
—17		23	8	ſtill, faint
—18		24	9	ſtill, faint
Feb. 9		17	1	ſtill
—17		25	9	ſtill
Mar. 2		9		ſtill
—15		22	7	large, bright
Apr. 10		19	3	very grand 2
—11		20	4	grand 2
—16		25	9	ſtill, bright
May 6		15	0	active

	Hour P.M.	☽'s age.	☽ paſt full.	Character.
1792				
June 30	1 M	10		active
Aug. 4	10	16	2	active, ſmall
—23	10	6		active
Sep. 22	10	6		bright, ſtill
Oct. 12	10	26	12	ſmall, ſtill
—* 13		27	13	very grand 2
—14		28	14	active 2
—18	10	3		ſmall
—23		8		ſtill
—31	8½	16	2	active, low
Nov. 19	9	4		ſtill 2
Dec. 7		23		ſtill 1
				Number 23.
1793.				
Jan. 11	10	29	14	ſtill, ſmall
—12		0		active
—13		1		ſtill
Feb. 8		27	12	ſtill
—12		2		grand 2
—15		5		an arch 2
Mar. 5		23	8	ſtill
— 6		24	9	ſtill
—13		1		ſtill 2
—30		18	3	fine, high 2
Apr. 5		24	9	ſtill
— 9		28	13	active 2
—14	12	5		ſtill 1

General obſervations on the Auroræ *before October* 13, 1792.

IN making obſervations upon any phenomenon in nature, with a view to aſcertain its cauſe, every particular circumſtance ſhould be attended to; for, though many may be

I 2 found

* A more particular account of the ſucceeding ones will be given hereafter.

found afterwards to be trivial, and of little or no moment in leading us towards the discovery, yet some one or other of them generally happens to be of importance. It will be seen hereafter, that the exact bearing and extent of the large, still, horizontal arch of the *aurora*, and the point in the heavens to which the coruscations tend, are amongst the circumstances of much importance in the investigation of its cause. These circumstances, it must be confessed, were not accurately noticed, either at *Kendal* or *Keswick*, previous to the middle of October, 1792.

As for myself, the only minute I usually made upon the *still aurora* was, that it was situate in the NW. by which I meant that its centre was between the N. and the W. without once attempting to ascertain the exact bearing of the centre; and the *corona*, when there was one, is often mentioned in my notes, as being south of the zenith, but the number of degrees was not ascertained.

Mr. *Crosthwaite*, however, has been rather more particular at times with respect to the bearings, extent, &c. The centre of that on January 10, 1788, he observes bore NNW.; that of the 28th of April, NW. b N.; the centres of all the rest are said to have been between the North and West, or else North; not one was observed to have its centre to the East of the meridian.

N. B. The additional observations on the Auroræ, beginning with that on the 13th of October, 1792, will be given after the next Section.

SECTION.

SECTION TWELFTH.

On Magnetiſm, and the variation of the Needle.

IN order to underſtand the additional obſervations, and the ſubſequent Eſſay on the *aurora borealis*, a competent knowledge of magnetiſm is requiſite; and as the principal facts relating to that ſubject are few and ſimple, we have thought it would not be amiſs to ſtate them here, for the ſake of ſuch as may not be previouſly acquainted therewith.

The *Loadſtone*, or *natural Magnet*, is a mineral production, found in the bowels of the earth, amongſt rich iron ores, of which it is one itſelf; its diſtinguiſhing property is that of attracting iron and ſteel. This property, which is called magnetiſm, is communicable to *ſteel* only, ſo as to be permanent; and to *iron* when within the influence of a magnet, but as ſoon as the magnet is withdrawn, the magnetiſm of iron ceaſes.

Every magnet has two oppoſite points or extremities, called its *poles;* the one is denominated its *north pole*, and the other its *ſouth pole;* and the attraction of the magnet is ſtrongeſt at its poles.

If

If an oblong bar of tempered ſteel (it will anſwer well if 5 inches long, half an inch broad, and a quarter of an inch thick) be rubbed over from one end to the other, always the ſame way, by either pole of a magnet, it will be converted into a magnet itſelf; and that end to which the pole was firſt applied, will be a pole of the new magnet, of the ſame name as the generating pole. By rubbing the new magnet the contrary way, with the ſame pole, its magnetiſm will be firſt deſtroyed, and then freſh magnetiſm will be communicated; but the poles of the new magnet will be of contrary names to what they were before.

Either pole of a magnet attracts iron, or ſteel not magnetic; but the pole of one magnet, *repels* the pole of another magnet, of the ſame name, and *attracts* the pole of a contrary name; the repulſion in the former caſe ſeems to be equal to the attraction in the latter.

Magnetiſm is ſometimes communicated, deſtroyed, or inverted, by lightning, or by an electric ſhock, &c.

If a magnetic bar, or needle, be ſuffered to move freely in an horizontal plane, it will only reſt in one poſition, when the north pole points northward, and the ſouth pole ſouthward.— Hence the common needle and compaſs, which

was

was invented about the beginning of the 14th century.

If a plane perpendicular to the horizon be conceived to be drawn through the horizontal needle, when at reſt, it is called the plane of the magnetic meridian; and the angle made by this plane, with the plane of the true meridian, is called the *variation of the needle.*

If a magnetic needle be nicely poiſed on an axis paſſing through its centre of gravity, or middle, and ſuffered to move freely both horizontally and perpendicularly, it will reſt only in one poſition, namely, when in the plane of the magnetic meridian, and having its north pole pointing towards the ground; the angle of deflection from the horizontal plane, is called the *dip* of the needle, and the needle itſelf in this caſe a *dipping-needle;* its poſition is the proper and natural one of every magnet that is ſuffered to be guided ſolely by the magnetic influence. From this phenomenon, and others of the ſame nature, it is inferred, that the earth itſelf is a magnet; whether its magnetiſm reſults from the united influences of the natural magnets it contains, or whether its magnetiſm may be in its atmoſphere, is not certain; and as poles of unlike denominations attract each other, the ſouth pole of the earth's magnetiſm muſt be in the northern hemiſphere, becauſe it attracts the north pole of the needle.

The

The variation of the needle is very different at different places of the globe, and even at the ſame place at different times; in theſe parts it is at preſent weſterly, and is increaſing every year; the variation at *London* in 1580 was 11° 15′ E. in 1657 it was 0° 0′; at preſent, 1793, it is about 22°¼ W. and increaſes nearly 10′ each year. From the reſult of ſeveral obſervations I find it to be 25° W. at this time, at *Kendal.*

The dip of the needle too is very different at different places, and probably at the ſame place at different times; but, for various reaſons, the obſervations on this head are neither ſo numerous nor ſo accurate as thoſe of the variation. It ſeems at preſent to be about 72° at *London,* according to Mr. *Cavallo;* and there is reaſon to ſuppoſe, it is not many degrees different in any part of *England;* for want of proper inſtruments I have not been able to aſcertain it at this place.

Beſides the annual change in the variation of the needle, there is a daily change, or variation of the variations. According to Mr. *Canton,* who made a ſeries of obſervations on the daily variation for a long time, the north pole of the needle moves gradually weſtward till 2 or 3 P. M. and then returns gradually to its former ſtation; the mean daily variation in winter is about 7′, and in ſummer about 13′¼. He moreover obſerved, that the needle was diſturbed when an *Aurora borealis* was in the atmoſphere.

I have

I have myfelf made a like feries of obfervations for fome months, and find them in general to agree with his; but as it is not neceffary for my purpofe to relate the refult of them, any further than what is contained in the fubfequent pages, I fhall not detain the reader longer on the fubject.

Addenda *to the Obfervations on the Auroræ Boreales.*

1792.

OCTOBER 13. At *Kendal*, A. M. frequent gleams. P. M. hazy; from 4 till 8 rainy, at which time the clouds to the *fouth* were remarkably red, and afforded fufficient light to read with, though there was no moon, nor light in the north. The unufual appearance raifed my curiofity, and I waited with impatience to fee the clouds carried off to the SE. (for the wind was W. or NW. and pretty frefh). In the mean time, having by me a very good *theodolite*, made by *Dollond*, I took it out to make obfervations on the bearing, altitude, &c. of any remarkable appearance.

From 9½ to 10 P. M. there was a large, luminous, horizontal arch to the fouthward, almoft exactly like thofe we fee in the north; and there was one or more faint, concentric arches northward,—It was particularly noticed, that all the arches feemed exactly bifected by the plane of the magnetic meridian. At half paft 10 o'clock, ftreamers appeared very low in the SE. running to and fro from W. to E, they increafed in number, and began to approach the

 zenith,

zenith, apparently with an accelerated velocity; when, all on a ſudden, the whole hemiſphere was covered with them, and exhibited ſuch an appearance as ſurpaſſes all deſcription. —The intenſity of the light, the prodigious number and volatility of the beams, the grand intermixture of all the priſmatic colours in their utmoſt ſplendor, variegating the glowing canopy with the moſt luxuriant and enchanting ſcenery, afforded an awful, but at the ſame time, the moſt pleaſing and ſublime ſpectacle in nature. Every body gazed with aſtoniſhment; but the uncommon grandeur of the ſcene only laſted about one minute; the variety of colours diſappeared, and the beams loſt their lateral motion, and were converted, as uſual, into the flaſhing radiations; but even then it ſurpaſſed all other appearances of the *aurora*, in that the *whole* hemiſphere was covered with it.

Notwithſtanding the ſuddenneſs of the effulgence at the breaking out of the *aurora*, there was a remarkable regularity obſervable in the manner.—Apparently a ball of fire ran along from E. to W. and the contrary, with a velocity ſo great as to be but barely diſtinguiſhable from one continued train, which kindled up the ſeveral rows of beams one after another; theſe rows were ſituate one before another with the exacteſt order, ſo that the baſes of each row formed a circle croſſing the magnetic meridian at right angles; and the ſeveral circles roſe one above another in ſuch ſort that thoſe near the zenith appeared more diſtant from each other than thoſe towards the horizon, a certain indication that the real diſtances of the rows were either nearly or exactly the ſame. And it was further obſervable, that during the rapid lateral motion of the beams, their direction in every two neareſt rows was alternate, ſo that whilſt the motion in one row was from E. to W. that in the next row was from W. to E.

The point to which all the beams and flaſhes of light uniformly tended, was in the magnetic meridian, and, as near as could be determined, between 15 and 20° ſouth of the zenith.—The *aurora* continued, though diminiſhing in ſplendor, for ſeveral hours. There were ſeveral meteors (falling

ſtars)

ſtars) ſeen at the time; they ſeemed below the *aurora*, and unconnected therewith.——It was ſeen at *Keſwick*, *Leeds*, &c. with much the ſame circumſtances; but how far it extended I have not learned.

The variation of the needle during the *aurora*, was not noticed.

October 14. I did not notice the *aurora* myſelf this evening; there was thunder and lightning, both here and at *Keſwick*, at the time of the *aurora*.

October 18. At Kendal. The *aurora* this night was an oblong, luminous cloud, about 15 or 20° long, and 4 or 5° broad, bearing about SE. by E. and 10 or 20° above the horizon; its ſouthern extremity was higher than its northern, and it evidently lay in the tract of a great circle from E. to W.—It diſappeared ſeveral times, and reappeared again almoſt inſtantly; and ſeveral times waxed and waned without vaniſhing; no radiations ſhot from it.

October 23. At Kendal. The *aurora* this evening appeared as an arch in the north-weſt quarter, from which proceeded ſeveral beams; they converged to a point on the magnetic meridian, about 18° beyond the zenith.

October 31. At Kendal. A few beams were ſeen to run to and fro from E. to W. low, or near the horizon: the moon ſhone bright at the time, and the clouds coming on ſoon after, the whole was obſcured.

November 19. At Kendal, the particulars of the obſervation were miſlaid; at Keſwick, the *aurora* roſe to about 18° above the horizon, and was ſituate in the uſual quarter.

December 7. At Keſwick, a faint appearance; about 5° high.

1793.

January 11. At Kendal, a ſmall arch in the horizon; it roſe to 5 or 10° altitude, and was biſected by the magnetic meridian.

January 12. At Kendal, from 6 to 9 P. M. a horizontal, luminous arch, 20° altitude, and biſected by the magnetic meridian.

meridian. After 9, fine ftreamers ftruck out, and ran to and fro a while acrofs the faid meridian, and then were converted into flafhes, as ufual; fome rofe up to the zenith. The point of convergency, and every other particular, were, to all appearances, the fame as have been defcribed before.

The needle was confiderably agitated at the time.

January 13. At Kendal, very bright in the northern horizon, but clouded above.—The variation of the needle at 6 P. M. 25° W.; at 9 P. M. 24° 34′; at 10 P. M. 24° 54′; next morning 25° 4′.

February 8. At Kendal, bright northward at 8½ P. M. at 10, the luminous arch was 16° altitude.—The other circumftances relating to it follow, fuppofing the variation of the needle at the noon of that day 25° W.

H. M.	Variation of the needle.	
10 — P. M.	25° 0′ W.	the arch rifing.
10 10 ——	24 54 —	bright ftreamers, low, with clouds.
10 30 ——	24 42 —	ftreamers rifen; fine, weftward*.
10 35 ——	24 37 —	a ftill light; clouded above.
10 45 ——	24 57 —	bright, eaftward; clouds above.
10 55 ——	25 7 —	light equal, eaft and weft.
11 5 ——	25 7 —	bright, low; clouded above.
11 15 ——	24 57 —	clouded, but bright eaftward.

It was related to the magnetic meridian as the former ones.

February 12. At Kendal, the *aurora* appeared foon after 6 P. M. flaming over two-thirds of the hemifphere. The beams all converged to a point in the magnetic meridian, about 15 or 20° to the fouth of the zenith, as was found from frequent trials.—The other particulars follow.

H. M.	Variation.	
5 — P. M.	25° 5′ W.	
6 35 ——	24 49 —	altitude of the clear fpace fouth 35°.
6 42 ——	24 55 —	alt. of do. 20°; ftreamers bright, eaft.
6 50 ——	25 — —	ftreamers bright and active all over the illuminated part.
7 2 ——	25 28 —	(same)
7 5 ——	25 12 —	(same)

H.

* That is, relative to the magnetic meridian, here and elfewhere.

H. M.	Variation.	
7 10 P. M.	24°40′ W.	diſappeared in the weſt; active, eaſt.
7 15 ——	24 40 —	
7 20 ——	24 35 —	active about the zenith; light faint.
7 25 ——	24 45 —	light faint.
7 35 ——	24 45 —	light faint.
8 — ——	24 45 —	ſtrong light northward.
8 10 ——	24 45 —	a large, uniform, ſtill light, covering half the hemiſphere, with flaſhes now and then.
8 35 ——	24 47 —	
9 15 ——	24 43 —	ſtreamers NW. bright, eaſt; clouds.
9 20 ——	24 43 —	the *aurora* burſting out afreſh.
9 30 ——	24 50 —	as fine and large a diſplay of ſtreamers as has appeared this evening.
10 — ——	24 55 —	
10 15 ——	24 57 —	the light growing fainter and fainter.
10 35 ——	24 40 —	
8 — A. M.	24 57	

N. B. The arch bounding the *aurora* to the ſouth, was always at right angles to the magnetic meridian, when perfect.

At Keſwick, the ſame evening; 7 P. M. ſtreamers from ENE. to WSW. and 28° paſt the zenith; perpendicular beam bore N. 17° W.—At 9h 25m very fine; they converged to a point 15° ſouth of the zenith, bearing SSE.—Altitude of clear ſpace 30°. The perpendicular beam N. 35° W.; extent on the horizon from ENE to WSW.—At 10h 30m they were ſettled in the northern quarter into an arch of 13° altitude, whence ſtreamers ſhot up towards the zenith.

February 15. The *aurora* of this evening was ſeen both at Kendal and Keſwick, and, as far as the eye could judge, the appearances ſeem to have been the ſame at both places.—It was a luminous arch, the centre of which bore SSE.; and it was extended in the oppoſite directions of ENE. and WSW.: on the weſt ſide its extremity ſeemed to touch the mountains at both places, at the altitude of 6°; and on the eaſt ſide it extended about half way to the horizon. The eaſtern end was rather ovaliform, about 8 or 10° broad, and where

where it joined to the reſt, was narroweſt of all, being but 2 or 3° broad, and bearing SE.; after which its breadth increaſed towards the weſt, being in ſome places 6 or 8°. —The ſky was clear, and there was no appearance of an *aurora* in the north, except two or three ſmall ſtreamers at one time, quite in the horizon. The eaſtern end of the arch waxed and waned frequently, and ſometimes entirely vaniſhed, and then reappeared again, in the ſpace of a few ſeconds. About a quarter paſt 10 it grew faint, and finally diſappeared. It did not ſenſibly vary in poſition during its appearance, and juſt before it vaniſhed, its ſituation amongſt the ſtars, as ſeen from Kendal, was as follows:—the ſouth edge of the arch ſeemed to touch pretty exactly the ſtar *lucida colli*, or *gamma Leonis*, to paſs 4 or 5° above *Procyon*, thence through the middle of the conſtellation *Orion*, leaving his bright foot, *Rigel*, 2 or 3° to the ſouth.

From theſe obſervations it reſults, that the greateſt altitude of the edge, at Kendal, muſt have been about 53°. Mr. *Croſthwaite* found the greateſt altitude of the ſaid edge, at Keſwick, to be 48°. The diſtance of the two places, as has been obſerved, is about 22 Engliſh miles, and it fortunately happens, that they lie very nearly in the direction of a plane at right angles to the arch; hence, we have the requiſite *data* to determine the height of the arch, which, by trigonometry, comes out 150 Engliſh miles.

The parallactic angle being ſo ſmall, an error of 1 or 2° in the altitudes, is of great conſequence.—Mr. *Croſthwaite* thinks the error in his obſervations could not exceed $1°\frac{1}{2}$, as the light was ſteady at the place where the altitude was taken.—Admitting the errors amounted to 2° at each place, which exceeds the bounds of probability, and that they were contrary; we ſhall then find the height 83 miles in the one caſe, and 750 in the other, which may, I think, be ſafely conſidered as boundaries, betwixt which the true height was; and hence it may be inferred, that the arch would be viſible to all *Great-Britain* and *Ireland;* that it is much to be wiſhed, ſome perſons in more diſtant places, may have made

ſimilar

fimilar obfervations upon the phenomenon, by which its height may be determined with more precifion.—In the mean time we fhall confider it as 150 Englifh miles.

March 5. The *aurora* at Kendal was feen at 8 P. M.; it was a bright ftill light a while, but foon clouded.——The needle was not attended to.

March 6. At Kendal, a few fine ftreamers at 9 P. M. altitude 15°, and extent along the horizon 70°; exactly bifected by the magnetic meridian. It foon dwindled into a faint light. At 9h 35m brighteft on the northern fide.—— The needle was 25° at 9h 4m,—24° 58′ at 9h 14m,—24° 50′ at 9h 35m,—24° 55′ at 10h 30m,—24° 52′ at 8 the next morning.

March 13. At Kendal the needle was at 8h 30m 24° 30′, —at 10h 30m 25° 4′,—and at 8 next morning 25° 4′.— There was a brightnefs northward at 10 P. M. but pretty much clouded; this circumftance, with that of the needle, rendered it probable an *aurora* was in the atmofphere.—— It was confirmed by the following account.

At Kefwick, the fame evening, at 8h 18m a horizontal arch, extent from NW. by W. to NNE. with faint ftreamers; the arch 20°, and ftreamers 25° altitude; the vertical ftreamers bore NNW. At 10, an arch from WSW. to ENE. its greateft altitude 30°: no ftreamers.

March 30. At Kendal, at 8 P. M. there appeared fome faint concentric arches of an *aurora*; it was not further noticed till,

H. M.	Variation.	
8 35 P. M.	25° 5′ W.	a grand horizontal arch, altitude 6°.
8 40 ——	25 25 —	ftreamers to 30° paft the zenith.
8 48 ——	25 5 —	bright eaftward.
8 55 ——	25 5 —	ftreamers faint.
9 ——	25 5 —	denfe light north; rare above.
9 5 ——	25 10 —	ditto
9 10 ——	24 55 —	bright weftward.
9 15 ——	24 55 —	a fine, perfect, horizontal arch.
9 20 ——	24 55 —	altitude of its upper edge 30°.

H.

H. M.	Variation.	
9 30 P. M.	24° 30′ W.	streamers up to the zenith.
9 35 ——	24 35	— dispersed, and not so high.
9 45 ——	24 58	— faint light; brightest eastward.
9 52 ——	24 45	— dull light.
10 — ——	24 42	— dull light; haze below.
10 10 ——	24 42	— haze risen; light fainter.
10 15 ——	——-	— clouds risen; light almost vanished.
10 30 ——	24 45	— clouds more risen.
11 15 ——	24 45	— several small clouds cover the hemisphere.
8 —A. M.	24 54	

There were several fine, perfect, concentric arches northward, during most of the time.—At 8h 48m one fine arch, the altitude of its under edge 10°. At 8h 55m two perfect arches, altitude of the higher 12°, with a fine edge. At 9h several concentric arches, one with a fine edge, altitude 11°. At 9h 5m one of the upper arches with a very bright edge, its altitude 13°; the bases of the streamers composing it of very dense light, and rare above. At 9h 10m its altitude 13 or 14°.—At 9h 15m the upper edge of the large horizontal light seems now as well defined as that of a rainbow, its altitude 47°, and that of the under edge 10°. At 9h 20m altitude of upper edge 30°.

The arches were all at right angles to the magnetic meridian, and the beams had their usual convergency.—At one time several small streamers formed a *corona* upon the magnetic meridian, the centre of which was determined by a good observation to be 72° from the south.

The sky was free from clouds till the last.

At Keswick, the same evening, at 8h 20m there were bright streamers WNW.—At 8h 28m they had spread from WSW. to ENE.; altitude of the arch 14°; vertical streamers bore NW. by N. At 8h 35m streamers 43° past the zenith*: previous to this there were at one time three concentric

* By the observations at *Kendal*, the *aurora* was 30° past the zenith at 8h 40m, and the clocks being corrected at both places, so as to be near the

centric arches northward, set with bright streamers, which had a very quick lateral motion; the under edge of the highest was not more than 14° high. At 9h 6m the altitude of the said arch was 13°½, bearing NW. ½ N.; streamers short, being only 5° higher than the under edge; horizontal extent of the arch from W. by N. to NE.

April 5. At Kendal, a small blushing of light, exactly in the magnetic north, at 9 P. M.; it soon faded away.

The disturbance of the needle was imperceptible.

April 9. The *aurora* was first seen at Kendal, at 9h 30m P. M. being a small blushing of light in the magnetic north. At 10h the arch risen to 6 or 8° of altitude, with streamers from 3 or 4 to 10° altitude, and a mist below; the rest of the sky was extremely clear; the light was dense at the under edge of the arch. At 10h 25m bright and active westward; the mist below.—Soon after, uncommonly active streamers, very low; the light seen dense through the mist. At 10h 35m the mist vanished; the *aurora* rather larger, and duller. At 11h a larger arch, altitude 10°, with mist below; no streamers, the light being still and uniform. At 11h 10m streamers very active; their progress seemed down, or northward.

The needle was not sensibly disturbed all the while.

At Keswick, the same evening, a faint light at 9h 45m.—It was 7° high at 9h 54m, and the highest part bore N. by W. ½ W.; one minute after, bright streamers from NE. to WNW. the greatest altitude of their base 5°⅓, the bearing of the same NW. by N. ¼ N.—From this to 10h 30m, bright streamers at intervals, low in the NW. quarter.—After 10h 30m grown faint; horizontal extent from WNW. to NE. by N.

April 14. At Keswick, about midnight, or soon after, a still, horizontal light, altitude of the under edge 5°, of the upper edge 9°½; bearing of the centre NW. ½ N.

L GENERAL

the true solar time, it is presumed this observation would be almost cotemporary with that at *Kendal*.——Now, supposing this to be the case, the height of the *aurora*, or of the lower extremity of the beams, will be found equal to 62 English miles.

GENERAL OBSERVATIONS.

IN order to determine the bearings of the middle or higheſt part of the arches of the *aurora*, I placed myſelf in a ſtation where I had a diſtant object before me, in the direction of the magnetic meridian, and I always found the higheſt part in the ſame direction as this object;—a deviation of 2 or 3° would in moſt caſes have been very ſenſible.—Sometimes, to confirm the obſervation, equal altitudes of the arches were taken on each ſide of the magnetic meridian, with the theodolite, and the horizontal angle divided into two equal parts, which gave the ſame bearing of the centre as the other method.—It does not, however, always happen that the horizontal arch, eſpecially when high, is perfect and complete.

The ſtreamers, or flaſhes, which pointed up, or perpendicular to the horizon, were only thoſe in the magnetic meridian, as well ſouth as north of the zenith.

The altitude of the centre of the *corona*, when there was one formed, was taken with a quadrant and plummet, with as much exactneſs as the thing ſeemed to admit of.

With regard to the needle of the theodolite, which was uſed to make the obſervations with, it is $3\frac{1}{2}$ inches long, and ſeems to move very freely upon its centre; I have often tried the effect of friction, by drawing it from its ſtation, and then ſuffering it to vibrate till it ſettled, when it uſually ſettled in the ſame ſtation within one or two minutes, but I have ſometimes obſerved it *five* minutes of a degree altered in ſuch a caſe.

I have never obſerved any conſiderable fluctuation of the needle in any evening but when there was an *aurora* viſible, except once; this was on the 13th of February, 1793, the evening of which was very wet and ſtormy; the needle varied

ried as follows:—the variation was 24° 57′ at noon; 24° 35′ at 5¾ P. M.; 24° 35′ at 5h 50m; 24° 20′ at 5h 58; 24° 20′ at 6h; 24° 48′ at 6h 20m; 24° 45′ at 6h 45m; 24° 35′ at 8h; 24° 47′ at 8h 30m; 24° 49′ at 10h 30m; 24° 53′ at 8 A. M. next day.

N. B. There had been an *aurora* the preceding evening.

It ſhould alſo be noticed, that whilſt making theſe obſervations upon the diſturbance of the needle, during an *aurora*, I did not always know the *abſolute* variation at the time; and therefore no inferences ſhould be made relative to the change in the abſolute variation, in the interval from one *aurora* to another, from the obſervations I have given.

END OF THE FIRST PART.

METEOROLOGICAL

OBSERVATIONS AND ESSAYS.

PART SECOND.

ESSAYS.

ESSAY FIRST.

On the Atmoſphere; its Conſtitution, Figure, Height, &c.

THE atmoſphere is that inviſible, elaſtic fluid which every where ſurrounds the earth, to a great height above its ſurface.—It was formerly ſuppoſed, that common air, or any portion of the atmoſphere, when cleared of vapours and exhalations, was a pure, ſimple, elementary fluid; but modern philoſophy has demonſtrated the contrary, and it now appears that the pureſt air we breathe at any time, conſiſts of an intimate mixture

mixture of various elaſtic fluids, or *gaſſes*, in different proportions. Thoſe properties of the atmoſphere, called its ſalubrity and inſalubrity, depend principally upon the greater or leſs quantity of one of its conſtituent principles, *vital* or *dephlogiſticated* air.—Whether the ſuperior regions of the atmoſphere conſiſt in like manner of various elaſtic fluids, or whether the fluids are the ſame or different from theſe below, cannot, from the nature of the caſe, be determined experimentally.

The figure of the exterior ſurface of the atmoſphere would, from the principles of gravitation, be ſimilar to that of the earth, or of an oblate ſpheroid; or, its height and quantity of matter about the equator, would be ſomething greater than at the poles, to preſerve an equilibrium every where, owing to the centrifugal force, which is greateſt at the equator. The denſity of the atmoſphere, ſuppoſing it of an uniform temperature, and alike conſtituted every where, would decreaſe in aſcending, in a geometrical progreſſion: thus, if the denſity at one mile high was 1, and that at four miles high $\frac{1}{2}$; then that at ſeven miles high would be $\frac{1}{4}$, at ten miles high $\frac{1}{8}$, &c.——I ſay theſe circumſtances *would be*, were it not for the ſun, or the principle of heat which it ſeems to produce; but by means of the unequal diffuſion of this principle, the circumſtances are very materially different.

The

The mean annual temperature of the air, at the earth's ſurface, decreaſes in going from the equator to the poles. Mr. *Kirwan* * ſtates the mean annual heat at the equator at 84°, and that at the pole at 31°. Moreover, the temperature of the air over any place, in clear, ſerene weather, decreaſes in aſcending above the earth's ſurface, nearly in an arithmetical progreſſion, and at the rate of 1° for every hundred yards. Experience proves this, as far as to the ſummits of the higheſt mountains, which is about 3 miles; and hence it may be inferred to be ſo above that height.

The great heat in the torrid zone rarefies the air, by increaſing its elaſticity; conſequently the equilibrium of the atmoſphere is diſturbed. The rarefied air aſcends into the higher regions, where, meeting with little reſiſtance, it muſt flow northward and ſouthward; the preſſure upon the northern and ſouthern regions is thus increaſed, and a current muſt ſet in below, towards the equator, to reſtore the equilibrium.—Hence, the higher temperature within the torrid zone, ſwells the atmoſphere there, and raiſes it, or at leaſt the groſs parts of it, to a much greater height than elſewhere; whilſt in the frigid zone it is contracted by cold.—This is the effect of the different temperatures at the earth's ſurface: but the increaſe of cold in aſcending deſtroys the

* *Eſtimate of the temperature of different Latitudes.*

the law of decreafe in denfity above mentioned, and greatly contracts the height of the atmofphere, as deduced from fuch law; though this circumftance has perhaps no effect upon the figure of the atmofphere.

Philofophers have attempted to find the height of the atmofphere by two methods; namely, by the duration of twilight, and by experiments upon the defcent of the barometer on high mountains. The former determines the height about 45 miles, as follows:—the twilight difappears when the fun is 18° below the horizon; hence it is argued, that a ray of light emitted from the fun, fo as to be a tangent to the earth's furface, after paffing through the atmofphere, is reflected from its external furface fo as to be a tangent to the earth's furface again, at 18° diftance from the former place of contact. This argument being admitted, affords *data* to find the height of the atmofphere, a proper allowance for refraction being firft made.—Several objections to this conclufion however may be ftated; amongft others, it may be faid, we do not know whether the light, which comes to us at the dawn or departure of day, has been once or twice reflected; it may, and probably does, proceed from the zone of the earth illuminated by the twilight itfelf; in this cafe, therefore, we can determine no more from the twilight, than that the height of the atmofphere, or of that region

region of it which is dense enough to reflect light, is not so much as 45 miles.

Barometrical experiments afford a much surer approximation to the height of the atmosphere, or rather perhaps of the more gross and heavy parts of it. From these we are assured, that a *stratum* of air reaching from the earth's surface to the height of 4 English miles, at all times contains above *one half* of the quantity of matter in the whole atmosphere; and by extending the laws thence resulting, we infer, that a *stratum* 12 or 13 miles high, contains $\frac{29}{30}$ths of the whole: or, if a barometer, standing at 30 inches, was elevated to that height, the mercury would fall 29 inches.

The following table and theorem, extracted from Sir *George Shuckburgh*'s letter to Col. *Roy*, (Philosophical Transactions, Vol. 68.) will serve to give my readers an idea in what manner the barometer is made subservient to the purpose; and also how the height of mountains, &c. may be ascertained by means of the barometer.——In order to understand the use of the table, it should be observed, that two persons are to take cotemporary observations, upon two barometers and thermometers, one person having one of each at the bottom of the mountain, and the other at the top.

The

The Table.

Thermometer.	Feet.
32°	86.85
35	87.49
40	88.54
45	89.60
50	90.66
55	91.72
60	92.77
65	93.82
70	94.88
75	95.93
80	96.99

EXPLANATION.

This table gives the number of feet in a column of the atmosphere, equivalent in weight to a like column of quickfilver $\frac{1}{10}$th of an inch high, when the barometer ftands at 30 inches, for every 5° of temperature from 32 to 80*.—For any other height of the barometer it will be in the inverfe ratio of that height to 30.——Let A = the mean height of the two barometers, in inches; a = the difference of the two, in tenths of an inch; b = the number of feet, per table, correfponding to the mean height of the two thermometers; x = the height of the mountain, in feet: then, we fhall have this theorem, $\frac{30ab}{A} = x$, the height required.

EXAMPLE.

Suppofe the barometer at the bottom to be 29.72 inches, thermometer 64°; the barometer at the top 27.46, thermometer 58°; required the height of the mountain?

Here the mean height of the two barometers, or A = 28.59 inches; their difference in tenths

M of

* From the table it appears, that, in round numbers, every 30 yards of elevation reduces the height of the mercury in the barometer $\frac{1}{10}$ of an inch, near the earth's furface.

of an inch, or $a = 22.6$; the mean heat of the two thermometers $= 61°$; the proportional number may be found from the table $= 92.98$ feet $= b$; hence, $\frac{30 \times 22.6 \times 92.98}{28.59} = 2205$ feet, the height required.

From this theorem we can deduce another:— fuppofing the elevation of the upper barometer given, and the height of its mercurial column required; the other *data* as before.——Let H = the height of the barometer below, in inches; b = the number of feet, per table, as before*; p = the perpendicular elevation of the upper barometer, in feet; y = the height of its mercurial column, in inches: then, we obtain this theorem, $y = \frac{600b - p}{600b + p} \times H$.

Hence we may calculate the height of the mercurial column of the barometer at any given moderate elevation, and by repeating the procefs, for a larger alfo, fufficiently accurate for the purpofe of explaining the theory of the variation of the barometer; though we cannot from this fix the boundary of the atmofphere with precifion. To what height the very thin and rare medium in the higher regions rifes, we cannot afcertain; but there is fufficient reafon

* The height of the thermometer below being given, the height of that fuppofed above may be eftimated, by deducting 1° for every hundred yards of elevation.

ſon to conclude, as will be ſeen in a ſubſequent Eſſay, that it extends to a much greater height than has commonly been ſuppoſed.

The following table contains the reſult of a calculation from the laſt mentioned theorem, of the height of the mercurial column, at certain elevations, above the equator, and likewiſe over the north of *England*, and the north pole. The mean heat at the earth's ſurface, under the equator, is ſuppoſed 84°; the mean heat in theſe parts, for the hotteſt month of ſummer, at 60°, and for the coldeſt month of winter at 35°; the mean annual temperature at the north pole being ſuppoſed 31°, the mean temperature for the coldeſt month of winter at that place may perhaps be ſtated at 2°.

Elevation of the barometer above the level of the ſea, in Engliſh miles.	Height of the mercurial column of the barometer, in inches.			
	Above the equator.	Above the North of England.		Above the north pole.
		In ſummer.	In winter.	In winter.
0	30.00	30.00	30.00	30.00
2	20.55	20.10	19.58	18.81
4	13.61	12.96	12.24	11.19
6	8.66	7.98	7.26	6.24
8	5.25	4.65	4.03	3.19
10	3.00	2.52	2.05	1.45
12	1.58	1.24	.93	.56

ESSAY SECOND.

On Winds.

WINDS have ever been confidered, with reafon, as having a principal fhare in producing changes of weather, and therefore they demand a particular regard in meteorology.

Moft people know that the winds are not every where fo changeable as in thefe parts. In the torrid zone, the winds are much more uniform in direction than they are either in the temperate or frigid zones: over the Atlantic and Pacific oceans, particularly between 30° of north and 30° of fouth latitude, the *trade winds*, as they are called, blow pretty uniformly from eaft to weft, all the year round, with a fmall variation in the different feafons.

The caufe of thefe conftant winds, within the tropics, the ingenious and learned Dr. *Halley* has endeavoured to explain, and his explication feems to have been univerfally adopted by others fince its publication.—The chief phyfical principle he ufes, is the undeniable and well known one, that the air is rarefied by heat; and, as the earth, in revolving from weft to eaft, expofes the torrid zone every day to the direct rays of the

the ſun, the earth, and conſequently the air, is there moſt heated; the *maximum* of heat follows the ſun, and therefore moves in a contrary direction, or from eaſt to weſt; the rarefaction occaſioned thereby diſturbs the equilibrium of the atmoſphere ſucceſſively; and he argues, that a current of air will conſtantly follow the extreme of heat, to reſtore the equilibrium,—and thus he accounts for the trade winds.

It appears to me, however, that this concluſion is premature, and not warranted by the laws of motion. For, to ſimplify the conception, let us ſuppoſe a ring with a number of beads arranged upon it at equal diſtances, and, abſtracting from the force of gravity, that each of them is endued with a repulſive power, in the ſame manner as are the particles of air. This ſuppoſition being made, let the principle of heat, or any other power, which acts ſimply by increaſing their elaſticity, act upon them in one part of the ring more than in another; this will of courſe ſeparate the particles in ſuch part farther than they were before, and condenſe the others; but it can never produce a rotary motion of the whole number of them round the ring, becauſe the action being mutual, the motion generated muſt be equal and contrary;—or, in other words, no momentum of the whole maſs of particles around the ring, can be produced by any forces, which they exert upon each other, agreeably to *New-*

ton's

ton's third law of motion.—We have here ſuppoſed the heat applied to one part of the ring only, but it is plain the ſame concluſion will obtain if it be applied to ſeveral parts at the ſame time, or ſucceſſively, or in any other manner; likewiſe if the *addition* of heat produce no momentum, the *abſtraction* of it will not.

Now to apply this to the matter in queſtion: let the ſun be upon the equator, and the air underneath be heated; then the air in the plane of the equator cannot recede from that plane, becauſe the lateral preſſure on each ſide will be equal; and the action of the particles in the ſaid plane upon each other, will be in the ſame circumſtance as that of the particles upon the ring, with reſpect to any horizontal motion that may be produced in the plane by the heat of the ſun. It appears then, that no rotary motion of the air round the earth can be produced by the action of the ſun upon the particles in that plane; and by a like method of reaſoning it may be proved, that no ſuch motion can be produced in any other parallel plane; conſequently, the cauſe we are ſpeaking of, or the ſucceſſive rarefaction of the air from eaſt to weſt, cannot produce the effect in queſtion, nor immediately contribute thereto.

It will be aſked, if the trade winds are not produced by the ſucceſſive rarefaction of the parts

parts of the atmofphere within the torrid zone, what are they produced by?—To this it may be replied, that they admit of an explanation upon mechanical principles without requiring any hypothetical reafoning, or any other phyfical principle than that Dr. *Halley* ufes; namely, that heat rarefies the air. The inequality of heat in the different climates and places, and the earth's rotation on its axis, appear to me the grand and chief caufes of all winds, both regular and irregular; in comparifon with which all the reft are trifling and infignificant. The trade winds in the torrid zone, and the variable winds every where elfe, feem to be the natural effects of thefe two caufes, and might have been deduced from them *a priori*, if the facts had never been afcertained by the navigation of the torrid zone. Notwithftanding, as we are in poffeffion of many facts relative to the winds, it may be proper firft to ftate them, and then to confider how they refult from the caufes above mentioned.

Facts relating to the Winds.

1. Over the Atlantic and Pacific oceans, as has been obferved, the trade-winds extend from 30° of north to 30° of fouth latitude.

2. When the fun is on the equator, the trade-winds, in failing northward, veer more and more from the eaft towards the north; fo that about their limit they become nearly NE.: and

and, *vice versa*, in failing fouthward, they become at laft almoft SE.

3. When the fun is near the tropic of cancer, the trade-winds north of the equator become more nearly eaft than at other times, and thofe fouth of the equator more nearly fouth: and, *vice versa*, when the fun is near the tropic of capricorn.

4. The trade-wind is not due eaft upon the equator, but about 4° to the north of it.

5. The winds in the northern temperate zone are variable, but the moft general are the SW. and W. and the NE. and E.——See page 48.

6. In the northern temperate and frigid zones, and doubtlefs in the fouthern alfo, the winds are more tempeftuous in winter than in fummer.——See page 49.

Now in order to perceive the reafon of thefe facts, it muft be remembered, that the heat is at all times greateft in the torrid zone, and decreafes in proceeding northward, or fouthward; alfo, that the poles may be confidered as the centres of cold at all times: hence it follows, that, abftracting from accidental circumftances, there muft be a conftant afcent of air over the torrid zone, as has been obferved, which afterwards falls northward and fouthward; whilft the colder air below is determined by a continual impulfe towards the equator. And, in general, wherever the heat is greateft, there the air will afcend,

afcend, and a fupply of colder air will be received from the neighbouring parts.——Thefe then are the effects of the inequality of heat.

The effects of the earth's rotation are as follow: the air over any part of the earth's furface, when apparently at reft or calm, will have the fame rotary velocity as that part, or its velocity will be as the co-fine of the latitude; but if a quantity of air in the northern hemifphere, receive an impulfe in the direction of the meridian, either northward or fouthward, its rotary velocity will be greater in the former cafe, and lefs in the latter, than that of the air into which it moves; confequently, if it move northward, it will have a greater velocity eaftward than the air, or furface of the earth over which it moves, and will therefore become a SW. wind, or a wind between the fouth and weft. And, *vice verfa*, if it move fouthward, it becomes a NE. wind. Likewife in the fouthern hemifphere, it will appear the winds upon fimilar fuppofitions will be NW. and SE. refpectively *.

The trade-winds therefore may be explained thus: the two general maffes of air proceeding

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* M. *De Luc* is the only perfon, as far as I know, who has fuggefted the idea of the earth's rotation altering the direction of the wind, which idea we have here purfued more at large.—Vid. "Lettres phyfiques, &c." Tom. 5. Part. 2. Let. cxlv.

from both hemispheres towards the equator, as they advance are constantly deflected more and more towards the east, on account of the earth's rotation; that from the northern hemisphere, originally a north wind, is made to veer more and more towards the east, and that from the southern hemisphere in like manner is made to veer from the south towards the east; these two masses meeting about the equator, or in the torrid zone, their velocities north and south destroy each other, and they proceed afterwards with their common velocity from east to west round the torrid zone, excepting the irregularities produced by the continents. Indeed the equator is not the centre or place of concourse, but the northern parallel of 4°; because the centre of heat is about that place, the sun being longer on the north side of the equator than on the south side. Moreover, when the sun is near one of the tropics, the centre of heat upon the earth's surface is then nearer that tropic than usual, and therefore the winds about the tropic are more nearly east at that time, and those about the other tropic more nearly north and south.

Were the whole globe covered with water, or the variations of the earth's surface in heat regular and constant, so that the heat was the same every where over the same parallel of latitude, the winds would be regular also: as it is, however, we find the irregularities of heat, arising from

from the interſperſion of ſea and land, are ſuch, that though all the parts of the atmoſphere in ſome ſort conſpire to produce regular winds round the torrid zone, yet the effect of the ſituation of land is ſuch, that ſtriking irregularities are produced: witneſs, the monſoons, ſea and land breezes, &c. which can be accounted for on no other principle than that of rarefaction; becauſe the rotary velocity of different parallels in the torrid zone is nearly alike.—For this reaſon we have omitted giving the facts, and their explanation, as having been done by others.

From what has been ſaid it might be ſuppoſed that the winds in the northern temperate zone ſhould be between the north and eaſt below, and between the ſouth and weſt above, almoſt as regularly as the trade-winds; but when we conſider the change of ſeaſons, the different capacities of land and water for heat, the interference and oppoſition of the two general currents, the one of which is verging towards a central point, and the other proceeding from it, we might conclude it next to impoſſible that the winds in the temperate and frigid zones ſhould exhibit any thing like regularity: notwithſtanding this, obſervations ſufficiently evince, that the winds in this our zone are, for the moſt part, in the direction of one of the general currents; that is, ſome where between the north and eaſt, or elſe between the ſouth and weſt; and that winds in other directions happen only as accidental varieties, chiefly in unsettled weather.

In winter, the heat decreases more rapidly in leaving the equator, and proceeding northward, than at any other season; consequently the currents of air to and from the equator, in the northern hemisphere, move with the greatest velocity, and occasion the most tempestous weather, in that season: and, *vice versa*, in summer.

The effect of the earth's rotation to produce, or rather to accelerate the relative velocity of winds, being as the difference betwixt the co-sines of any two latitudes, (or, to speak more strictly, the effect is as the fluxion of the co-sine of the latitude, the fluxion of the latitude being supposed constant) it will be small within the torrid zone, and increase in approaching the poles. The hourly rotary velocity of the equator is about 1040 English miles; if we suppose it 1000 miles it will be accurate enough for our purpose, and then, from a table of natural sines, the rotary velocity of any parallel may be had at once; the differences of these velocities, will serve to give us some idea of the comparative effect of the earth's rotation at different parallels; for which purpose we have subjoined a table, giving the rotary velocity of the parallels of latitude for every 10 degrees, together with their differences, agreeable to the above supposition.

Degrees

Degrees of latitude.	Hourly rotary velocity of the parallels, in English miles.	Differences of their velocities.
°		
0	1000	
10	984.8	15 2
20	939 7	45 1
30	866	73.7
40	766	100
50	642.8	123.2
60	500	142.8
70	342	158
80	173.6	168.4
90		173.6

From the table it appears, the effect of the earth's rotation, to accelerate the relative velocity of winds, is about ten times as great at the poles as at the equator;—by *relative velocity*, my readers will perceive I mean, all along, the velocity of the wind relative to the place of the earth's surface over which it blows; hence, the relative velocity and direction of the mass of air from the equator is at first altered very slowly, and afterwards more rapidly, by the earth's rotation; and, *vice versa*, with respect to that from the poles.

Had the trade-winds been produced by the daily rarefaction of the air from east to west alone, independent of the earth's rotation, they should have extended to 50° of north latitude when the sun is at the tropic of cancer, because the heat at that parallel is then as great as at 30° of south latitude, which is quite contrary to experience: in fact, they ought to have ex-

tended,

tended, in a greater or leſs degree, over the ocean, from the equator to the poles, and the ſummers have been more tempeſtuous than the winters, becauſe the daily variation in heat is then greateſt; neither of which we find conſiſtent with obſervation.

The relative velocity of winds may be beſt aſcertained by finding the relative velocity of the clouds, which, in all probability, is nearly the ſame as that of the winds; the velocity of a cloud is equal to that of its ſhadow upon the ground, which, in high winds, is ſometimes a mile in a minute, or 60 miles an hour; and a briſk gale will travel at the rate of 20 or 30 miles an hour. —It may be imagined, that the relative velocity of winds ſhould be continually upon the increaſe, by reaſon that their cauſes are conſtantly in action, and not for a moment only; but the reſiſtance which a current of air meets with from the atmoſphere itſelf, and from objects upon the earth's ſurface, muſt be very conſiderable; the increaſe or diminution of the relative velocity of a wind will therefore depend upon the proportion between the active cauſes and the reſiſtance.

The œconomy of winds, an illuſtration of which we have been here attempting, is admirably adapted to the various purpoſes of nature, and to the general intercourſe of mankind:—had the ſun revolved round the earth, and not the

the earth on its axis, the air over the torrid zone, and particularly about the equator, would have been in effect ftagnant; and in the other zones the winds would have had little variation either in ftrength or direction; navigation, in this cafe, would have been greatly impeded, and a communication between the two hemifpheres, by fea, rendered impracticable. On the prefent fyftem of things, however, the irregularity of winds is of the happieft confequence, by being fubfervient to navigation; and a general circulation of air conftantly takes place between the eaftern and weftern hemifpheres, as well as between the polar and equatorial regions; by reafon of which, that diffufion and intermixture of the different aerial fluids, fo neceffary for the life, health, and profperity of the animal and vegetable kingdoms, is accomplifhed:—fuch is the tranfcendent wifdom and providential care of the common FATHER OF ALL!

PROOF OF THE EARTH'S ROTATION.

The trade-winds being matter of fact, if the mechanical principles we have explained them upon be admitted, we may draw from hence a very fatisfactory, and indeed conclufive argument for the earth's rotation on its axis; for, the trade-winds blowing from eaft to weft, we muft conclude, *a pofteriori*, that the earth revolves the contrary way, or from weft to eaft.

ESSAY THIRD.

On the variation of the Barometer.

THE caufes of the variation of the barometer have never yet been difcovered, fo as to admit of demonftration; though feveral eminent philofophers have given the public the refult of their reafoning and experience on the fubject. We propofe to confider the principal of their allegations; but in the firft place it will be proper to lay down the chief *facts* refpecting the variation, which are the refult of obfervation, and not of any hypothefis.

Facts relating to the Barometer.

1. The barometer has very little variation within the tropics.

I believe the barometrical range has not been obferved much to exceed half an inch, in the torrid zone.

2. Within the northern temperate zone, and doubtlefs the fouthern alfo, the range of the barometer increafes in going from the equator.

The mean annual range* at *Paris*, in latitude 48° 50′ N. for 20 years, was $1\frac{1}{2}$ inch; the greateft range, or difference between the higheft and loweft obfervations, for the fame term, was 2 inches. (Vid. *Martyn's Abridgment of the Parifian*

* By *annual range*, I mean the difference between the higheft and loweft obfervations each year.

ſian Memoirs). At *Kendal*, in latitude 54° 17′ N. the mean range for 5 years was 2.13 inches; the greateſt range was 2.65 inches. A compariſon of the obſervations made at *London*, *Kendal*, and *Keſwick* likewiſe corroborates the ſame. —In *Sweden*, and *Ruſſia*, the range is ſtill greater.

3. In the temperate zones the range and fluctuation of the barometer is always greater in winter than in ſummer.

See the obſervations, particularly the tables, p. 16 and 17.

4. The riſe and fall of the barometer are not local, or confined to a ſmall diſtrict of country, but extend over a conſiderable part of the globe, a ſpace of two or three thouſand miles in circuit at leaſt.

See the general obſervation, page 16.

In the French Philoſophical Tranſactions for 1709, there is a compariſon of obſervations upon the barometer made at *Paris* and *Genoa*, for 3 years; the diſtance of the places is at leaſt 350 miles; notwithſtanding this, it was found to riſe and fall almoſt univerſally on the ſame day at both places, only the variation was leſs at *Genoa* than at *Paris*, becauſe its latitude is leſs; no difference in time was perceived, whether the fluctuations were ſudden or gradual, except in one inſtance, when the riſe was one day later at *Genoa* than at *Paris*.

The preciſe extent to which the fluctuations of the barometer reach, has not, that I know of, ever yet been aſcertained in any one inſtance, for want of cotemporary obſervations made at a great number of diſtant places.

5. The barometrical range is greater in *North America* than in *Europe*, in the ſame latitude.

From the American Philoſophical Tranſactions we find the range is as great in *New England* as in this country, though it is 10° nearer the equator. Alſo, at *Williamſburg*, in *Virginia*, latitude 37° 20′ N. the annual range is above 1 inch, which is the fame as at *Genoa*, latitude 44° 25′ N.

6. In the temperate zones the mean ftate of the barometer in the fummer months is nearly equidiftant from the extremes in that feafon; but in winter the mean is much nearer the higher extreme than the lower.

According to the obfervations at *Kendal* (fee page 16*) the mean height of the barometer in July is diftant from the higher extreme .33 of an inch, and from the lower extreme .37; in January the mean is diftant from the higher extreme .79, and from the lower 1.17: the ratio of the former diftances is as 11 to 12, and of the latter as 8 to 12, nearly.

Profeffor *Muffchenbroek*, in his Elements of Natural Philofophy, (tranflated by *Colfon*) publifhed about 50 years ago, has endeavoured to account for thofe changes of weight in the atmofphere; he has adverted to all or moft of the caufes that have ever been confidered as agents in producing the effects: he enumerates the following caufes, namely;—Firft, the oppofition of winds; fecond, the north wind blowing, which cools and condenfes the air; third, the winds blowing upward or downward; fourth, an increafe or diminution of heat, which rarefies or condenfes the air, in confequence of which the air's diftance from the earth's centre is increafed or diminifhed, and its weight, as well as centrifugal force, thereby affected; fifth, the air being

* The mean for July, uncorrected, is 29.77, and for January 29.66, which muft be ufed in this cafe, becaufe the extremes are not corrected.

being loaded with, or cleared of vapours and exhalations.

Professor *De Saussure*, of *Geneva*, thinks the causes of the changes of the barometer are heat, different winds, and unequal density of the contiguous *strata* of air; hence the little variation within the tropics. The principal cause is opposing winds. He does not deny that chymical changes in the air may affect the barometer; he however suspects that some unknown cause has the greatest effect *.——We shall now consider the causes above alleged severally.

The idea of opposite winds having the principal share in producing the changes in the barometer, has evidently been suggested by the uniformity of the trade-winds, and the small variation of the barometer where they blow; but it should be considered, that the land-winds within the tropics do not always blow with the general or trade-winds, and that sometimes they are in direct opposition; also, the monsoons, especially about their change, produce uncommon conflicts of winds, and tempestuous weather, notwithstanding which circumstances, the barometer never has those fluctuations that are experienced in the other zones. If, therefore,

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* These his sentiments are taken from the *Critical Review*, for 1787.—Without being possessed of his work, we cannot examine his arguments particularly.

the idea of oppofite winds, mechanically accumulating or difperfing the air, be inconfiftent with the firft fact, it will certainly fail of explaining the reft. Befides, it would not be difficult to prove, *a priori*, that the oppofition of winds, admitting the fact at the time, could not produce thofe great and long continued accumulations of air which we often experience.

The fecond caufe, or that of a cold north wind blowing, has doubtlefs an effect upon the barometer, though perhaps not altogether in the manner that has been conceived.—We fhall confider this in another point of view by and by.

The third caufe, fuppofing it to exift at any time, can only be local and tranfitory at moft; but the rife or fall of the barometer is general, and of confiderable duration: it cannot, therefore, produce the effect.

The fourth caufe is much too trifling to have any material influence.

With refpect to the fifth, it muft be allowed, that water, when changed into vapour, conftitutes a part of the atmofphere for the time, and weighs with it accordingly; alfo, that when vapour is precipitated in form of rain, the atmofphere lofes the weight of it: but it would be too hafty to conclude from hence, that where evaporation is going forward the barometer muft rife, and where rain is falling it muft fall alfo; becaufe air loaden with vapour is found to be fpecifically lighter than without it. Evaporation, therefore, increaſes the bulk and weight of the atmoſphere at large, though it will not increaſe the weight over any particular country, if it diſplace an equal bulk of air ſpecifically heavier than the vapour: and in like manner, rain at any place may not diminiſh the weight of the air there, becauſe the place of the vapour may be occupied by a portion of air ſpecifically heavier. It ſhould ſeem therefore, that when the air over any country is cleared of vapours, &c. the barometer ought to be higher than uſual, and not lower. ——But we ſhall now proceed to ſtate our own ideas on the ſubject.

It appears from the obſervations, (ſee table, page 16) that the mean ſtate of the barometer is rather lower than higher in winter than in ſummer, though a ſtratum of air on the earth's ſurface always weighs more in the former ſeaſon than in the latter; from which facts we muſt unavoidably infer, that the height of the atmoſphere, or at leaſt of the groſs parts of it, is leſs in winter than in ſummer, conformable to the table, page 83. There are more reaſons than one to conclude that the annual variation in the height of the atmoſphere, over the temperate and frigid zones, is gradual, and depends in a great meaſure upon the mean temperature at the earth's ſurface below; for, clouds are never obſerved to be above 4 or 5 miles high, on which account the clear air above can receive little

little or no heat, but from the ſubjacent regions of the atmoſphere, which we know are influenced by the mean temperature at the earth's ſurface; alſo, in this reſpect, the change of temperature in the upper parts of the atmoſphere muſt, in ſome degree, be conformable to that of the earth below, which we find by experience increaſes and decreaſes gradually each year, at any moderate depth, according to the temperature of the ſeaſon. (See page 30.)

Now with reſpect to the fluctuations of the barometer, which are ſometimes very great in 24 hours, and often from one extreme to the other in a week or 10 days, it muſt be concluded, either that the height of the atmoſphere over any country varies according to the barometer, or otherwiſe that the height is little affected therewith, and that the whole or greateſt part of the variation is occaſioned by a change in the denſity of the lower regions of the air. It is very improbable that the height of the atmoſphere ſhould be ſubject to ſuch fluctuations, or that it ſhould be regulated in any other manner than by the weekly or monthly mean temperature of the lower regions; becauſe the mean weight of the air is ſo nearly the ſame in all the ſeaſons of the year, which could not be if the atmoſphere was as high and denſe above the ſummits of the mountains in winter as it is in ſummer. However, the deciſion of this queſtion need

need not rest upon probability; there are facts, which sufficiently prove, that the fluctuation of density in the lower regions has the chief effect upon the barometer, and that the higher regions are not subject to proportionable mutations in density. In the memoirs of the Royal Academy at *Paris*, for 1709, there is a comparison of observations upon the barometer at different places, and amongst others, at *Zurick*, in *Switzerland*, in latitude 47° N. and at *Marseilles*, in *France*, latitude 43° 15′ N.; the former place is more than 400 yards above the level of the sea; it was found that the annual range of the barometer was the same at each place, namely, about 10 lines; whilst at *Genoa*, in latitude 44° 25′ N. the annual range was 12 lines, or 1 inch; and at *Paris*, latitude 48° 50′ N. it was about 1 inch 4 lines. In the same memoir it is related, that F. *Laval* made observations, for 10 days together, upon the top of *St. Pilon*, a mountain near *Marseilles*, which was 960 yards high, and found that when the barometer varied 2¾ lines at *Marseilles*, it varied but 1¼ upon *St. Pilon*. Now had it been a law, that the whole atmosphere rises and falls with the barometer, the fluctuations in any elevated barometer would be to those of another barometer below it, nearly as the absolute heights of the mercurial columns in each, which in these instances were far from being so. Hence then it may be inferred, that the fluctuations of the barometer are occasioned

chiefly

chiefly by a variation in the denſity of the lower regions of the air, and not by an alternate elevation and depreſſion of the whole ſuperincumbent atmoſphere. How we conceive this fluctuation in the denſity of the air to be effected, and in what manner the preceding general facts relative to the variation of the barometer may be accounted for, is what we ſhall now attempt to explain.

It has been obſerved already that air charged with vapour, or vapourized air, is ſpecifically lighter than when without the vapour; or, in other words, the more vapour any given quantity of atmoſpheric air has in it, the leſs is its ſpecific gravity.—M. *De Sauſſure* has found from experiment, that a cubic foot of dry air, of a certain temperature, will imbibe 12 grains of water; and that every grain of water diſſolved in air becomes an elaſtic fluid capable of ſupporting $\frac{1}{24}$ of an inch of mercury, while its denſity to that of air, is as 3 to 4.—Again, Dr. *Prieſtley* has found from frequent experiments (vid. *Experiments and Obſervations relating to various branches of natural Philoſophy*, Vol. 6, page 390) that different kinds of air, as for inſtance, inflammable air, and dephlogiſticated air, the ſpecific gravities of which are as 1 to 12 nearly, when mixed together, do not obſerve the laws of hydroſtatics; for, the inflammable air, inſtead of riſing to the top of the veſſel, diffuſes itſelf equally

equally and permanently through the dephlogiſticated air, at the ſame time that no chemical attraction takes place betwixt them. The Doctor further obſerves, "that the phlogiſticated "and dephlogiſticated air, which compoſe the "atmoſphere, are of very different natures, "though without any known principle of at- "traction between them, and alſo of different "ſpecific gravities; and yet they are never ſe- "parated but by the chemical attraction of ſub- "ſtances, which unite with the one and leave "the other."—Moreover, Sir *Benjamin Thomſon* has found that moiſt air conducts heat better than dry air. (*Vid.* Philoſophical Tranſactions, 1786.)

From the two firſt mentioned diſcoveries we may venture to infer, that if a cubic foot of dry air were mixed with a cubic foot of moiſt air of the ſame temperature, the compound would occupy a ſpace of two cubic feet, and be of equal elaſticity with the ſimples, the two kinds of air being intimately diffuſed through each other. Hence then a fluctuation of the denſity of the air may happen thus: if a current of warm and vapourized air flow into a body of cold and dry air, it will diſplace a part of the cold air, and diffuſe itſelf amongſt the reſt, by which means the weight of the *ſtratum* will be diminiſhed, whilſt its bulk and ſpring remain the ſame; and *vice verſa*, if dry air flow into vapourized air.

The firſt fact may then be accounted for thus: —the warmer any air is, the more water it will imbibe, in ſimilar circumſtances; hence, the air over the torrid zone, being the hotteſt, will contain the moſt vapour; and the air about the poles, being the coldeſt, will contain the leaſt*: moreover, as the heat within the torrid zone, and the height of the atmoſphere there, remain pretty nearly the ſame all the year round, and all the air approaching the zone from the two temperate zones, is gradually aſſimilated in its paſſage to that of the ſaid zone, it follows, that there can be little fluctuation of denſity in the lower regions of the air, and of courſe little variation of the barometer in the torrid zone.

The ſecond and third facts are the neceſſary reſults of the principles we are aſſerting:—in winter, the ſeaſon when the barometrical range is obſerved to be greateſt, the temperature of the air decreaſes in proceeding from the torrid, through the temperate, to the frigid zones; the decreaſe

* The reader will pleaſe to obſerve, that the terms *moiſt air*, and *vapourized air*, uſed in this and ſome other eſſays, denote air containing a great portion of vapour, though it may not perhaps be characterized as ſuch by a hygrometer. —Thus, a cubic foot of air at the equator, which there is indicated to be dry by a hygrometer, will contain more vapour than a cubic foot of air here, at the freezing temperature, which is indicated to be more moiſt than the former by the hygrometer.—The difference of temperature produces this effect.

decreaſe is at firſt moderate, but grows more and more rapid as we advance; in conſequence of this decreaſe, and the law by which it is regulated, every place in the temperate zone will, then more particularly, be ſituate betwixt the extremes of heat and cold, relative to its own temperature, and the higher the latitude the nearer will be thoſe extremes to the place; beſides, that ſeaſon being liable to the higheſt winds, the air will readily be transferred from one parallel to another; and as the air at all times will endeavour to maintain a proportion of vapour ſuitable to its temperature, it follows, that the air in general in the higher latitudes will then both be *cold* and *dry*, and in the lower latitudes both *warm* and *moiſt*, relatively ſpeaking. The conſequence is obvious, that as a current from one or the other hand prevails, the barometer will riſe or fall accordingly, and the riſe or fall will be greater as the place is ſituate nearer to the extremes of temperature, becauſe the air will in that caſe ſuffer the leaſt change in its paſſage.——In ſummer, the heat all over the northern hemiſphere is brought almoſt to an equality at the different parallels; the whole maſs of air is heated, ſwelled, and repleniſhed with vapour; the air over the northern regions is almoſt brought into the ſame ſtate as within the tropics, and the barometer therefore has almoſt as little variation, in that ſeaſon, here as there.

 The

The fourth fact offers nothing inconfiftent with our theory: winds are the mediate caufe of the variations of the barometer, and the currents of air to and from the torrid zone are not partial, but general, though fubject to confiderable modifications in direction; befides, independent of winds, thofe properties of the air, heat and moifture, will always be diffufing themfelves in every direction, where there is a deficiency of either; from which circumftances, it feems impoffible that the variations of the barometer fhould be local, though the amount of each fluctuation will not be the fame at places confiderably diftant. From the ufual celerity of the winds, the changes will happen upon the fame day at places very diftant; but theory feems to require, that the northern parallels fhould firft experience the higher extremes, and the fouthern parallels the lower, and the obfervations upon the fourth fact countenance the inference. However, a feries of cotemporary obfervations made at two places, differing confiderably in latitude, would afcertain the fact; and if the places were one NE. of the other, they would be ftill more eligible for the purpofe, becaufe the two general currents of air flow in that direction.

The climate of the eaftern coaft of *North America* is fo conftituted, that the decreafe of the mean temperature in the winter feafon, in proceeding northward, is much more rapid than on the weftern coaft of this continent; the confequence is, that any particular place there is liable to great and fudden fluctuations of temperature in that feafon, and thefe produce proportionate fluctuations of the barometer, according as the warm and vapoury, or the cold and dry air predominate.

The fixth fact has not, that I know of, ever been accounted for, or even been adverted to, by thofe who have attempted to explain the caufes of the variation of the barometer; and yet it will admit of a fatisfactory explanation upon the principles we have adopted. Indeed, at firft view, it feems inconfiftent with thofe principles, becaufe we can produce no facts to prove why the air may not deviate from its mean ftate of heat and moifture as much towards one extreme as towards the other; but, allowing what is moft probably the true ftate of the cafe, that the deviations on each fide are nearly equal, ftill the fact of the barometer admits of a rational folution.—Moift air, as has been obferved, conducts heat much better than dry air; now when the loweft extreme of the barometer happens, the air is moift, high winds generally prevail, and the atmofphere is much ruffled by clouds and ftorms; all thefe circumftances tend to diffufe and circulate the heat, by reafon of which the law of decreafe of temperature in afcending, at fuch times, muft be very materially

materially different from what it is in ſerene weather; or, in other words, the decreaſe of temperature in aſcending muſt be much ſlower than at other times; we may venture to ſuppoſe, that, in ſome caſes, the mean ſtate of decreaſe for a few miles of elevation will be 1° for every 150 yards of aſcent, inſtead of 1° for every 100 yards, which is the uſual rate; the conſequence of this muſt be a greater reduction of the barometer than otherwiſe would happen. For, let the weight of the atmoſphere at 3 miles of elevation be ſuppoſed equal to 15 inches of mercury, the heat at the earth's ſurface equal to 45°, and that it decreaſes in aſcending after the uſual rate of 1° for every 100 yards; then, the mean heat of a column of air from the earth's ſurface to 3 miles above it, will be 18°.6, whence the weight of the whole column from the earth's ſurface to the top of the atmoſphere may be found by the theorem, page 82; or $H = \frac{600b + p}{600b - p} \times y$ (y being given in this caſe) $= 28.74$ inches, the height of the mercurial column of the barometer at the earth's ſurface: but if we ſuppoſe the heat decreaſes in aſcending after the rate of 1° for 150 yards, then the mean heat of the column becomes equal to 27°.4, and the height of the barometer equal to 28.30 inches; the difference is .44 of an inch, occaſioned by this change in the temperature, which is greater by .06 of an inch than the difference of the ranges above

and

and below the mean for January, at *Kendal*, as ſtated at page 98.

The ſuppoſition made above, I preſume will not be deemed extravagant, namely, that the mean heat of a column of air 3 miles high will not differ more from that at the earth's ſurface than 17°, on certain occaſions: when we conſider the ſtrong SW. winds during a thaw, (when the loweſt extreme uſually happens) and that the thermometer often riſes to 45° at the ſame time that the froſt is in the earth, and the ground not cleared of ſnow, we muſt conclude, that the then increaſing heat comes from the air above, and not from the earth, and conſequently that the temperature of the air is greateſt at a conſiderable elevation, and decreaſes from thence downward as well as upward; which circumſtance alone will greatly add to what the mean temperature of the column would otherwiſe be. —This irregularity and inverſion of the law of heat in the atmoſphere, by which the loweſt extreme of the barometer is removed farther from the mean ſtate than the higheſt, can only happen in winter, by means of a ſudden influx of warm air into cold; but in ſummer the heat of the air, being chiefly derived from the earth's ſurface, will be more equably diffuſed upwards, and prevent ſuch a diſproportion in the diſtances of the extremes from the mean, agreeably to obſervation.

Having

Having now endeavoured to explain the principal facts relative to the variation of the barometer, we ſhall next advert to ſome other particulars on the ſubject, which tend to illuſtrate and confirm the doctrine we have advanced.

The barometer generally riſes with a wind betwixt the north and the eaſt; it riſes very high during a long and uninterrupted froſt; it was higheſt for the laſt 5 years in January 1789; the mean temperature at *Kendal*, for 4 weeks preceding, was 28°, which was lower than for any other ſimilar interval in the 5 years; there was only 1.643 inches of rain and ſnow for 7 weeks before; theſe were clear proofs of the prevalence both of cold and dry air.

The barometer is often low in winter, when a ſtrong and warm S. or SW. wind blows; the annual extremes for theſe 5 years have always been in January; the loweſt was in January, 1789, about 2 weeks after the above mentioned high extreme; it was accompanied with a ſtrong S. or SW. wind, and heavy rain; the temperature of the air at the time was not high, being about 37°, but the reaſon was no doubt becauſe one half of the ground was covered with ſnow; it was therefore probably warmer above.—Now the reaſon why the low extreme ſhould have at that time, as well as at many others, ſoon ſucceeded the high extreme, ſeems explicable as follows:

follows: the extreme and long continued cold preceding, muſt have reduced the groſs part of the atmoſphere unuſually low, and condenſed an extraordinary quantity of dry air into the lower regions; this air was ſucceeded by a warm and vapoury current coming from the torrid zone, before the higher regions, the mutations of which in temperature and denſity are ſlow, had time to acquire the heat, quantity of matter, and elevation conſequent to ſuch a change below; theſe two circumſtances meeting, namely, a low atmoſphere, and the greateſt part of it conſtituted of light, vapoury air, occaſioned the preſſure upon the earth's ſurface to be ſo much reduced. Hence then, it ſhould ſeem, we ought never to expect an extraordinary fall of the barometer, unleſs when an extraordinary riſe has preceded, or at leaſt a long and ſevere froſt; this, I think, is a fair induction from the foregoing principles; how far it is corroborated by paſt obſervations, beſides thoſe juſt mentioned, I have not been able to learn.

It is obſervable that the high extreme ſome years happens in October or March, but generally in one of the intermediate months; the low extreme is moſtly in December or January. From the obſervations at *Paris* for 20 years, from 1699 to 1718, incluſive, if we take 11 of the loweſt that were made, 10 of them were in December and January, and the eleventh in November.

The month of January, 1791, will be long remembered, on account of the losses at sea, and damage at land, by the extraordinary high winds, which prevailed almost incessantly throughout the month, from the SW.—See page 49 Now a strong and warm SW. wind blowing continually in that season, when the atmosphere was low, ought to have reduced the mean state of the barometer unusually low; the fact therefore may be produced, as an *experimentum crucis* of the theory; accordingly, we find from the observations, that the mean state of the barometer for that month was lower by .14 of an inch, in the north of *England*, and probably lower every where on the western coast of *Europe*, than for any other month in the last 5 years.

It does not appear from the barometrical observations in the first part of this book, that cold alone, independent of every other circumstance, has a tendency to increase the mean weight of the atmosphere over any place; for, if it had, the mean state of the barometer would be higher in winter than in summer, contrary to experience; if, therefore, the mean state of the barometer be lower in the torrid than frigid zones, it is most probably effected by the vapoury air.

ESSAY FOURTH.

On the relation between Heat and other Bodies.

WE have nothing new to offer on this ſubject; but as ſome knowledge of the matter is requiſite in order to underſtand ſome of the phenomena of meteorology, we purpoſe to give a brief explanation of ſuch facts as may be adverted to in the courſe of this work.

Different bodies that are equal in *magnitude*, and of the ſame temperature, do not contain equal quantities of fire; neither do different bodies, that are equal in *weight* and temperature, contain equal quantities of fire.—For example, if a cubic inch of *iron* be heated to 100°, and then thrown into a given quantity of water at 50°, the temperature of the water will be augmented; but if inſtead of *iron, lead* be uſed, the temperature will not be ſo much augmented; on the contrary, if the iron and lead were colder than the water, the iron would diminiſh its temperature moſt. If equal *weights* of iron and lead were uſed, the reſults would be ſomewhat different, but ſtill the temperature of the water would be more augmented or diminiſhed by the iron than by the lead. When equal *weights* are uſed in experiments of this ſort, that body which augments or diminiſhes the temperature the moſt, is ſaid to have the greater capacity for heat; becaufe a greater quantity of heat is required to be added to, or fubtracted from it, in order to vary its temperature equally with the other.

The fame body, under the different forms of *folid*, *fluid*, and *aeriform*, has different capacities for heat; in the folid form its capacity is leaft, and greateft in its aeriform ftate; alfo, when any folid body is converted into a non-elaftic fluid, or any non-elaftic fluid into an elaftic fluid, by heat, it abforbs a portion of heat during its converfion, which does not increafe its temperature; and when the change takes place the contrary way, by cold, it parts with an equal portion of heat, without having its temperature diminifhed. —To inftance in *ice*, *water*, and *aqueous vapour:* if a pound of ice were taken of the temperature of 20°, and a quantity of heat added to it, fo as to augment its temperature to 25°; an equal quantity of heat would augment the temperature of a pound of water lefs than 5°, and of aqueous vapour ftill lefs. Again, if a pound of ice of 32°, and a pound of water of 172° were mixed together, the temperature of the mixture would be 32°, becaufe the ice requires 140° of heat to melt it; that is, it requires as much heat to melt it as would increafe the temperature of a pound of water 140°; whereas, if a pound of water of 32° were mixed with a pound of water of 172°, the temperature of the mixture would be the mean betwixt the two, or 102°. Alfo, it has been found, that aqueous vapour, when condenfed

denfed into water of the fame temperature, gives out 943° of heat.

The capacities of *earth*, *ftones*, and *fand*, for heat, are much lefs than that of water. This is one caufe why the viciffitudes of temperature are greater at land than at fea*.

Another particular relative to heat is, that fome bodies conduct it better than others; in this refpect there is a ftriking refemblance between the electric fluid and fire; for, thofe bodies which conduct the electric fluid well, as metals, water, &c. alfo conducts heat well.—*Glafs*, *fealing-wax*, and other electrics, conduct heat very flowly; alfo *dry land*, whether the furface be ftony, fandy, or earthy, is found by experience to conduct heat flowly.

Sir *B. Thomfon* has by a feries of experiments (fee Philofophical Tranfactions, 1786) found the powers of a few bodies to conduct heat to be proportionate to the following numbers, namely:

Mercury - - - - - - - - -	1000
Moift air - - - - - - - - -	330
Water - - - - - - - - - -	313
Common air, denfity 1 - - - - -	80.11
Rarefied air, denfity $\frac{1}{4}$ - - - -	80.23
Rarefied air, denfity $\frac{1}{24}$ - - - -	78
Torricellian vacuum - - - - -	55

ESSAY

* Thofe who wifh to fee the fubject touched upon above, difcuffed at large, may perufe Dr. *Crawford*'s *Experiments and Obfervations on Animal Heat and the Inflammation of combuftible Bodies*.

ESSAY FIFTH.

On the Temperature of different Climates and Seafons.

MR. *Kirwan* has treated of this fubject in fo able a manner, that we can do little more than extract from his work *.

That the fun is the primary caufe of heat all over the earth, is almoft too apparent ever to have admitted of doubt; though fome philofophers have imagined a *central heat* or body of fire in the earth, which, by its emanations, mitigates the feverity of the winters in the higher latitudes: the opinion is, however, difproved by facts, which fhew, that the temperature of places 30, 40, or 50 feet below the earth's furface, remains nearly the fame all the year round as the mean annual temperature at the furface, and that at a lefs depth the temperature varies, in a fmall degree, with the feafon. The fact feems to be, that in winter the earth gives out to the atmofphere a portion of heat received in fummer.

The earth's furface is the chief medium by which the fun heats the atmofphere; for it is obfervable

* *Eftimate of the Temperature of different Latitudes.*

obſervable that clear air is not heated in any ſenſible degree by the action of the ſun's rays. The direct rays of the ſun falling upon *ſtony* or *ſandy* ground, are found to increaſe its temperature amazingly, partly on account of its ſmall capacity for heat; whilſt the temperature of water is thereby increaſed very little, from its great capacity for heat, the reflection from its ſurface, and evaporation. Water being a much better conductor of heat than land, preſerves a greater uniformity of temperature; whilſt land is more ſubject to the viciſſitudes of heat and cold.

Living vegetables alter their temperature very ſlowly; the evaporation from their ſurfaces is much greater than from the ſame ſpace of land uncovered with vegetables: *foreſts* prevent the ſun's rays from reaching; hence, wooded countries are colder than thoſe open and cultivated.

Evaporation and the condenſation of vapour are made ſubſervient to the more equal diffuſion of heat over the different climates and places: evaporation being great in the torrid zone, a vaſt portion of heat is thereby abſorbed, and rendered inſenſible, till being carried northward or ſouthward, the vapour is condenſed, and gives out its heat again, which being diffuſed in the atmoſphere, augments its temperature very conſiderably.

Mr.

Mr. *Kirwan*, confidering thefe and other circumftances, judges it moft eligible, in comparing the temperature of different places, to fix upon a fituation that may ferve as a ftandard of comparifon, and he judicioufly prefers the fea to the land, as being more free from accidental variations. By combining theory with obfervation, he obtains the mean annual heat of the equator equal to 84°, and that of the pole 31°: and then gives the following theorem for the mean annual temperature of the ftandard fituation in every latitude; namely, if $S =$ the natural fine of any latitude to radius 1; then, $84 - 53 \times S^2 =$ the mean annual temperature of that latitude.

This theorem gives the temperature of different latitudes as by the following table.

Table of the mean annual temperature of the ftandard fituation, for every 5 degrees of latitude.

Lat.	temp.	Lat.	temp.	Lat.	temp.	Lat.	temp.	Lat.	temp.
°	°	°	°	°	°	°	°	°	°
0	84	20	77.8	40	62.1	60	44.3	80	32 6
5	83.6	25	74.	45	57.5	65	40 5	85	31.4
10	82 3	30	70.7	50	52.9	70	37.2	90	31
15	80.4	35	66.6	55	48.4	75	34.6		

It afterwards becomes neceffary to confider the modifications of the ftandard temperature on land, from fituation, &c.

1. Elevation diminifhes the mean temperature of places. Its effects Mr. *Kirwan* ftates as follows:

lows: if the elevation be moderate, or at the rate of 6 feet per mile from the neareſt ſea; then, for every 200 feet of elevation, allow $\frac{1}{4}$ of a degree for the diminution of the mean annual temperature.

If the elevation be	7 feet per mile, allow	$\frac{1}{3}$ of a degree.
	13 feet	$\frac{4}{10}$
	15 feet, or upwards	$\frac{1}{2}$

N. B. The elevation of any inland place may be found ſufficiently exact for this purpoſe, by obſerving how much the mean annual height of the barometer falls ſhort of 30 inches, and allowing for the difference, according to the theorem in page 81; becauſe the mean annual height of the barometer, on a level with the ſea, is nearly 30 inches every where.

2. Next to elevation, diſtance from the ſtandard ocean ſeems to have the moſt conſiderable effect upon the mean annual temperature; its amount Mr. *Kirwan* ſtates, from a compariſon of obſervations, as follows: namely, the mean annual temperature is depreſſed or raiſed, for every 50 miles diſtance, nearly at the following rate:

From lat. 70°	to lat.	35°	*cooled* $\frac{1}{3}$ of a degree.
35	to	30	—— $\frac{1}{8}$
30	to	25	*warmed* $\frac{1}{5}$
25	to	20	—— $\frac{1}{2}$
20	to	10	—— 1°.

R This

This effect of diſtance from the ſtandard ocean Mr. *Kirwan* ſeems to attribute to the unequal capacities of land and water for heat; but, with deference to the opinion of ſo reſpectable a philoſopher, I think this alone inadequate to the effect. For, if land in general receive *more* heat immediately from the ſun in a year than water, the mean temperature of the internal parts of the continent ought to be the greateſt from the equator to the pole. And if land receive *leſs* heat, then, for ought that appears, the mean temperature of the internal parts of the continent might be expected the leaſt in every latitude; but in neither caſe, I think, could we conclude *a priori*, from the mere difference of capacity, that the mean heat of the internal parts of the continent would be greater near the equator, and leſs more northward, than the mean heat upon the coaſt.—To account for the effect in queſtion, we ſhall therefore propoſe the following theory.

Let it be firſt ſuppoſed that water receives a greater quantity of heat, from the ſun's rays, than land in general, under every parallel of latitude*; in the next place, it will be allowed, that a much greater quantity of water is evaporated

* It is generally allowed, I think, that land reflects more light than water, and conſequently imbibes leſs; and the quantity of heat received will doubtleſs be proportionate to the rays imbibed.

rated from the ſea, in the torrid zone, than from an equal area of land in the ſame zone; hence it will follow, that the quantity of heat abſorbed by the vapour may, for ought we know, be ſo great as to reduce the mean temperature of the ſea there below that of the land: in ſuch caſe it is evident, the further any place is diſtant from the ſea, the greater muſt its mean temperature be, all other circumſtances being the ſame. Again, the farther we proceed northward, the leſs is the quantity of water annually evaporated from a given ſurface of the ſea; hence there may be a parallel of latitude where the heat abſorbed by the greater evaporation of the ſea, is equal to the heat which the ſea receives more than the land; in this caſe therefore, the mean temperature of the land and ſea will be every where the ſame in the ſame parallel. Farther than this, the mean temperature of the ſea will become greater than that of the land, and the more ſo as the latitude increaſes. It appears then, that the difference of the capacity of land and water for heat, requires to be joined to the ſuppoſition that water receives more abſolute heat than land from the ſun's rays, before we can produce, *a priori*, a reſult ſimilar to what is ſtated above as deduced from obſervation.

But if we purſue the thought ſtill farther, we ſhall perhaps find, that the above ſtatement of the effect of diſtance from the ſtandard ocean,

is

is not altogether compatible either with theory or obſervation,—and at the ſame time draw a concluſion of much importance to the ſubject we are now diſcuſſing.

It is obſervable, that in the northern temperate zone, the internal parts of the eaſtern continent are generally hotter, in ſummer, than on the coaſt under the ſame parallel, except elevation or ſome peculiarity of ſoil or ſituation diminiſh the temperature; but the cold of winter is ſo much more ſevere, that the mean temperature is greatly reduced below the ſtandard — Now in winter, when the influence of the ſun is ſo weak, it ſhould ſeem that the condenſation of vapour alone affords the northern atmoſphere a very large portion of the ſenſible heat it has in that ſeaſon. And it appears in the former eſſay on winds, that the general current of air from the equator is SW. when it arrives in the northern temperate zone; this current coming from the ſea to the weſtern coaſt of each continent, will there meet with cold air, which condenſes its vapour as it proceeds, affording plenteous rain and heat to the weſtern coaſts: as the current proceeds into the internal parts of the continents it loſes its vapour and heat, till at length the precipitation becomes much leſs in quantity, and in form of ſnow; the current then continues its progreſs, and grows colder and colder till it arrives at the eaſtern coaſt, unleſs the influx of ſea breezes

breezes mitigate the temperature near the coaſt. Hence then it may be inferred, *that in the temperate zones, the weſtern coaſts of all continents and large iſlands, will have a higher mean temperature than the eaſtern coaſts under the ſame parallel, and particularly will have more moderate winters.*

It remains now to ſhew how far this inference is countenanced by obſervation.—We are certain that the eaſtern coaſt of *Aſia* is much colder than the weſtern coaſt of *Europe;* on the eaſtern coaſt of *Kamſchatka*, in latitude 55° N. Capt. *Cook* found ſnow 6 or 8 feet deep, in May, and the thermometer was moſtly 32°; and in January the cold is ſometimes —28°, and generally —8°. At *Pekin*, in *China*, latitude 39° 54′ N. longitude 116° 29′ E. the mean temperature is only 55°.5, the *Atlantic* under this parallel being 62°; the uſual range of the thermometer each year is from 5° to 98°, not unlike what it is at *Philadelphia*, which is under the ſame parallel. —Again, we are certain that the eaſtern coaſt of *North America* is 10 or 12° colder than the oppoſite weſtern coaſt of *Europe;* and hence it may be preſumed, that the weſtern coaſt of *North America*, or that of *California*, is warmer than the eaſtern. The NE. parts of *Siberia* on the one continent, and the country about *Hudſon's Bay* on the NE. ſide of the other continent, ſeem equally ſubject to the moſt rigorous cold in in winter.——But to proceed to the other modifications of the ſtandard temperature.

3. As for the effects of mountains, foreſts, ſeas, &c. upon the mean annual temperature of places, Mr. *Kirwan* obſerves, that all countries lying to the windward of high mountains, and extenſive foreſts, are warmer than thoſe lying to the leeward, in the ſame latitude. Countries that lie ſouthward of any ſea are warmer than thoſe that have that ſea to the ſouth of them. Iſlands participate moſt of the temperature of the ſea, and are therefore not ſubject to the extremes of heat and cold ſo much as continents.

We ſhall here introduce a table containing the mean annual temperature of ſeveral places, as determined by obſervation, from the "Eſtimate, &c." page 113.

	North Lat.	Longitude.	Mean annual Heat.
Wadſo, in Lapland - -	70° 5′		36°
Abo - - - - -	60 27	22°18′E.	40
Peterſburgh - - -	59 56	30 24 E.	38 8
Upſal - - - - -	59 51	17 47 E.	41.88
Stockholm - - -	59 20	18 E.	42.39
Solyſkamſki - - -	59	54 E.	36 2
Edinburgh - - -	55 57	3 W	47.7
{ Keſwick* - - -	54 33	3 3 W.	46
{ Kendal - - - -	54 17	2 46 W.	46.4

Franeker

* Theſe two places are inſerted from page 29 and 30 of this work.

	North Lat.	Longitude.	Mean annual Heat.
Franeker	53° 0′	5°42′ E.	52°.6
Berlin	52 32	13 31 E.	49
Lyndon, in Rutland	52 30	0 3 W.	48.03
Leyden	52 10	4 32 E.	52 25
London	51 31		51.9
Dunkirk	51 2	2 7 E.	54 9
Manheim	49 27	9 2 E.	51.5
Rouen	49 26	1 W.	51
Ratiſbon	48 56	12 5 E.	49.35
Paris	48 50	2 25 E.	52
Troyes, in Champaigne	48 18	4 10 E.	53.17
Vienna	48 12	16 22 E.	51.53
Dijon	47 19	4 57 E.	52.8
Nantes	47 13	1 28 E.	55.53
Poitieres	46 39	0 30 E.	53.8
Lauſanne	46 31	6 50 E.	48.87
Padua	45 23	12 E.	52.2
Rhodes, in Guienne	45 21	2 39 E.	52 9
Bordeaux	44 50	0 36 W.	57.6
Montpelier	43 36	3 73 E.	60 87
Marſeilles	43 19	5 27 E.	61.8
Mont Louis, in Rouſillon	42	2 40 E.	44.5
Cambridge, in New England	42 25	71 W.	50.3
Philadelphia	39 56	75 9 W.	52.5
Pekin	39 54	116 29 E.	55.5
Algiers	36 49	2 17 E.	72
Grand Cairo	30	31 23 E.	73
Canton	23	113 E.	75.14
Tivoli, in St. Domingo	19		74
Spaniſh Town, in Jamaica	18 15	76 38 W.	81
Manilla	14 36	120 58 E.	78.4
Fort St. George	13	87 E.	81.3
Ponticherry	12	67 E.	88
	South Lat.		
Falkland Iſlands	51° 0′	66 W.	47 4
Quito	0 13	77 50 W.	62

The hotteſt place mentioned in this table is *Ponticherry;* the heat there is ſometimes 113 or 115°, which far exceeds that of the human body. The mean heat of June is 95°.4.

In

In ſome parts of *Africa* the heat even exceeds that of *Ponticherry*.

Of all inhabited countries, *Siberia* ſeems the coldeſt; its great elevation and diſtance from the ocean both conſpire to make it ſo. Mercury has often been frozen there by the natural cold, which conſequently exceeded —39°. The mean temperature of *Irkutz*, latitude 52° 15′ N. longitude 105° E. from October 1780 to April 1781, was —6°.8.

At *Peterſburgh* the cold has been known —39°: and is one year with another, at an average, —25°; the greateſt ſummer heat, on a mean, is 79°, yet once it amounted to 94°.

General Obſervations and Inferences.

Eſtimate, &c. page 19. "The temperatures of different years differ very little near the equator, but they differ more and more, as the latitudes approach the poles.

"It ſcarce ever freezes in latitudes under 35°, unleſs in very elevated ſituations; and it ſcarce ever hails in latitutes higher than 60°.

"Between latitudes 35° and 60°, in places adjacent to the ſea, it generally thaws when the ſun's altitude is 40°, and ſeldom begins to freeze until the ſun's meridian altitude is below 40°."

Page 28. "The greateſt cold, within the 24 hours, generally happens half an hour before ſun-riſe, in all latitudes. The greateſt heat in all

all latitudes between 60° and 45°, is found about half paſt 2 o'clock in the afternoon; between lat. 45° and 35°, at 2 o'clock; between lat. 35° and 25°, at half paſt 1; and between lat. 25° and the equator, at 1 o'clock.

"On ſea, the difference between the heat of day and night, is not ſo great as on land, particularly in low latitudes.

"The coldeſt weather, in all climates, generally prevails about the middle of January, and the warmeſt in July, though, aſtronomically ſpeaking, the greateſt cold ſhould be felt at the latter end of December, and the greateſt heat in the latter end of June; but the earth requires ſome time to take, or to loſe the influence of the ſun, in the ſame manner as the ſea, with reſpect to tides, does that of the moon."

Page 104, *&c.* "July is the warmeſt month in all latitudes above 48°; but in lower latitudes Auguſt is generally the warmeſt.

"December and January, and alſo June and July, differ but little. In latitudes above 30°, the months of Auguſt, September, October, and November, differ more from each other, than thoſe of February, March, April, and May. In latitudes under 30°, the difference is not ſo great. The temperature of April approaches more, every where, to the annual temperature, than that of any other month: whence we may infer, that the effects of natural causes, that operate gradually over a large extent, do not arrive at their *maximum*, until the activity of the causes begins to diminish; this appears also in the operation of the moon on seas, which produces tides; but after these effects have arrived at their *maximum*, the decrements are more rapid, than the increments originally were, during the progress to that *maximum* *.

"The differences between the hottest and coldest months, within 20° of the equator, are inconsiderable, except in some peculiar situations; but they increase in proportion, as we recede from the equator.

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* The foregoing observations, made at *Kendal* and *Keswick*, afford some remarkable exceptions to the three last general observations.—December is the coldest month in these places; though perhaps a mean of 5 years is not sufficient to determine the point. August is generally the warmest month, and not July; the reason of this last I take to be, our mountains being topped with snow during the spring, which retards the increase of temperature, and throws the *maximum* of heat later in the summer. For the same reason, the month of April is colder than the annual mean: October seems the nearest to it. The standard temperature for those places is 49°; the difference, being between 2 and 3°, must be attributed, I think, chiefly to the extensive ranges of mountains and high lands, in almost every direction; unless, perhaps, we have determined the temperature too low.—See the observations, page 30.

In the higheſt latitudes, we often meet with a heat of 75 or 80°; and particularly in latitudes 59° and 60°, the heat of July is frequently greater than in latitude 51°.

Every habitable latitude enjoys a heat of 60° at leaſt, for 2 months; which heat ſeems neceſſary, for the growth and maturity of corn. The quickneſs of vegetation, in the higher latitudes, proceeds from the duration of the ſun over the horizon. Rain is little wanted, as the earth is ſufficiently moiſtened by the liquefaction of the ſnow, that covers it during the winter; in all this, we cannot ſufficiently admire the wiſe diſpoſition of Providence.

ESSAY SIXTH.

On Evaporation, Rain, Hail, Snow, and Dew.

EVAPORATION is that procefs in nature by which water and other liquids are abforbed into the atmofphere, or are converted into elaftic fluids, and diffufed through the atmofphere; the liquid thus changed, is termed vapour, and the vapour is characterized by the name of the liquid from which it was generated, as *aqueous vapour*, or the vapour derived from water, &c.—Whether the vapour of water is ever chymically combined with all or any of the elaftic fluids conftituting the atmofphere, or it always exifts therein as a fluid *fui generis*, diffufed amongft the reft, has not, I believe, been clearly afcertained.

The following circumftances are found powerfully to promote evaporation; namely, *heat*, *dry air*, and a *decreafed weight* or *preffure* of the atmofphere upon the evaporating furface. The firft and fecond are known to have that effect, from every one's experience; the laft is proved to have fuch an effect, by the air-pump. For, when the air is exhaufted out of a receiver, a large quantity of vapour is raifed from the wet leather upon the pump plate; this vapour is precipitated again when the air is let in, fo as to

appear

appear falling like a ſhower*. If a quantity of warm water be placed under a receiver, when the air is rarefied to a ſufficient degree, the water boils with great violence, and a large portion of it may in this manner be readily raiſed in vapour, which is as ſoon condenſed by the cold of the ſurrounding medium, and falls upon the leather of the pump-plate. The reaſon of this is, that the greateſt heat water is ſuſceptible of, or its boiling heat, depends upon the preſſure of the air upon its ſurface; the leſs the preſſure, the leſs is the boiling heat; and whenever it arrives at the boiling heat, the greater heat applied to augment its temperature, inſtead of doing ſo, converts a portion of it into vapour, which, as has been remarked, abſorbs a great quantity of heat, without any increaſe of temperature†.

As this variation of temperature in boiling water according to the different preſſure of the air, is a circumſtance not foreign to the ſubject we are upon, and perhaps the quantity and mode of the variation may not be generally known, we ſhall here introduce the reſult of a ſeries of experiments made in order to aſcertain what preſſure upon the ſurface of water is requiſite to make it boil at a given temperature; having never ſeen any ſimilar account, though the thing has

* See a note upon this ſubject, page 136.

† Hence we ſee the reaſon of the proviſo, page 20, in determining the boiling point of thermometers.

has probably been done by others with more accuracy.

Heat of the water when boiling.	Preſſure upon its ſurface, in inches of mercury.	Rarefaction of the air.
212°	30.00	1
200	22.8	1.3
190	18.6	1.6
180	15 2	2
170	12.2	2.45
160	9.45	3.2
150	7.48	4
140	5.85	5.1
130	4.42	6.8
120	3 27	9.2
110	2.52	11.9
100	1 97	15.2
90	1.47	20.4
80	1.03	29

N. B. M. *De Sauſſure* found the heat of boiling water upon the ſummit of mount *Blanc*, 186°; the height of the mountain is near 3 miles above the level of the ſea; the barometer was 16 inches $\frac{144}{160}$ of a line (a little above 17 Engliſh inches.

Experiments of this ſort, when made with all the accuracy they will admit of, I am inclined to think will lead to the true theory of evaporation, and to the ſtate of vapour in the atmoſphere; upon conſideration of the facts, it appears to me, that evaporation and the condenſation of vapour are not the effects of chymical affinities, but that aqueous vapour always exiſts as a fluid *ſui generis*, diffuſed amongſt the reſt of the aerial fluids.

fluids.—It is true, the fact that a quantity of common air of a given temperature, confined with water of the same temperature, will only imbibe a certain portion of the water, and that the portion increases with the temperature, seems characteristic of chymical affinity; but when the fact is properly examined, it will, I think, appear, that there is no necessity of inferring from it such affinity.

Granting the truth of the preceding experiments, when the incumbent air is rarefied 29 times, water of 80° is at the point of ebullition; or, in other words, aqueous vapour of the temperature of 80°, can bear no more than 1.03 inches of mercury, without condensation; this, then, is the extreme density of the vapour of that temperature. Now, when a quantity of atmospheric air of 80° imbibes vapour, the vapour is diffused through it, and it may therefore continue to imbibe till the density of the vapour, considered abstractedly, becomes $\frac{1}{29}$ of what it is when under the pressure of 30 inches of mercury, and its temperature 212°; or, till $\frac{1}{29}$ of the bulk of the compound mass is vapour, and then it will be saturated, or imbibe no more; because if it did, the density of the vapour must be increased, which it cannot be in that temperature, without losing its form, and becoming water. Thus then it appears, that upon this hypothesis, there is no need to suppose a chymical

mical attraction in the case; and further, that a cubic foot of dry air, whatever its density be, will imbibe the same weight of vapour if the temperature be the same; and lastly, that it may be determined *a priori*, what weight of vapour a given bulk of dry air will admit of, for any temperature, provided the specific gravity of the vapour be given. For example, let it be required to find the weight of vapour which a cubic foot of dry air of 80° will admit of, or imbibe, supposing the specific gravity of air .0012, and that of vapour to air as 3 to 4:—A cubic foot of water weighs 437500 grains, and the specific gravity of vapour from the *data*, is .0009; now the compound mass being denoted by q, we shall have $\frac{1}{29}q$ = the vapour, and q = 1 foot + $\frac{1}{29}q$; that is, $q = \frac{29}{28}$ foot; and the vapour = $\frac{1}{28}$ foot, = 14 grains. This, it will be observed, is the result of the *hypothesis*. M. *De Saussure* determined by the *experiment* alluded to, page 104, that a cubic foot of dry air of 66° would imbibe 11 or 12 grains of water. Hence then it seems probable that the hypothesis would agree with experiment.—By a like process, we shall find the weight of vapour imbibed by a cubic foot of air of 150°, equal to 131 grains.*

Evaporation

* I cannot forbear remarking in this place, that the fact observed by Dr. *Darwin*, in the Philosophical Transactions for 1788, supports the theory we have here advanced, and indeed, I think, cannot be so rationally accounted for on any other: the fact was, that air during its rarefaction attracts heat

Evaporation from land in general muſt be leſs than the rain that falls upon land; otherwiſe there could be no rivers. In winter the evaporation is ſmall, compared to what it is in ſummer. From a ſeries of experiments made in the preſent year, 1793, I found the mean daily quantity evaporated from a veſſel of water, in a ſituation pretty much expoſed to wind and ſun, for 13 days of March, to be .033 of an inch in depth, the greateſt .064; for 21 days of April the mean daily quantity was .0555 of an inch, the greateſt .1115; for 26 days of May the mean was .0755, the greateſt .1346; for 14 days of June the mean was .063, the greateſt .098; for 8 days of July the mean was .122, the greateſt .195: I never found the evaporation from water any ſummer much to exceed .2 of an inch in 24 hours, in the hotteſt weather. From theſe experiments, and other conſidera-

 tions,

heat from the ſurrounding bodies, and gives off heat during its condenſation; now, the moment any quantity of atmoſpheric air is rarefied, its vapour muſt be rarefied alſo, and hence a portion of moiſture will expand into vapour in order to reſtore that ſtate of denſity which the temperature admits of, and abſorb the requiſite quantity of heat from the bodies adjacent; again, the moment air is condenſed, its vapour is condenſed proportionally, ſo that the abſolute quantity of vapour which retains its form, will always be as the *ſpace* occupied by the condenſed air, and the reſt will be precipitated, giving off its heat to the ſurrounding bodies.—Notwithſtanding what is here ſaid, it is probable that a decreaſed preſſure upon the ſurface of water *accelerates*, if it do not increaſe the evaporation, all other circumſtances being the ſame.

tions, it feems probable, that the evaporation both from land and water, in the temperate and frigid zones, is not equal to the rain that falls there, even in fummer.

When a precipitation (or condenfation, which ever it be) of vapour takes place, if the temperature of the air be above 32°, the matter precipitated is liquid, or in form of *rain;* but if the temperature of the air be lefs than 32°, it is in form of *fnow;* when drops of rain, in falling, pafs throw a *ftratum* of air below 32°, they are congealed, and form *hail.*

If we adopt the opinion, which to me appears the more probable, that water evaporated is not chymically combined with the aerial fluids, but exifts as a peculiar fluid diffufed amongft the reft; whenever any condenfation of it happens, the matter muft be *precipitated*, though not in the chymical fenfe of the word; we would therefore be underftood in this effay to ufe the words *precipitation* and *precipitated* merely to denote the effect, without any allufion to chymical agency.

Different theories to account for thefe precipitations from the atmofphere have been formed; but the principles of none appear to me to be more plaufible, and confiftent with facts, than that which has lately been offered to the public, in the *Edinburgh Philofophical Tranfactions*, by by Dr. *Hutton* of that place. From a ſhort review of the article (for I have not ſeen the original) it appears, that he conſiders the varieties of heat and cold, affecting the ſolvent power of the atmoſphere, as the ſole cauſes of rain. Indeed, when we conſider that evaporation and the precipitation of vapour are diametrically oppoſite, it is reaſonable to ſuppoſe that they ſhould be promoted by oppoſite cauſes; and as heat and dry air are favourable to evaporation, ſo cold, operating upon air replete with vapour, promotes its precipitation. The point upon which we differ, I ſuppoſe will be, that he conſiders water chymically combined with the atmoſphere, and that cold produces a precipitation in a manner ſimilar to what it does in water ſaturated with ſalt, or in other chymical proceſſes; whereas I ſuppoſe, that a portion of the vapour, conſidered as a diſtinct and peculiar fluid, is condenſed into water by cold; the effects reſulting from the two theories will therefore be much the ſame.

The reaſon then that a SW. wind in theſe parts brings rain, ſeems to be, that, coming from the torrid zone, it is charged with vapour, and the heat eſcaping as it proceeds northward, a precipitation of the vapour enſues; but a NE. wind, blowing from a cold into a warmer country, has its capacity for vapour increaſed, and therefore we generally find it promote evaporation.

From the obſervations upon the quantity of rain that falls in different places, it ſeems clearly aſcertained, that there is more rain in mountainous than in level countries. The reaſon ſeems to be, that the inferior, warm, and vapoury *ſtrata* of air, ſtriking againſt the mountains, are made to aſcend into the colder regions, by which means the vapour is precipitated: the ſituation of places, however, may be too high to experience an extreme in this reſpect; thus, the rain in *Switzerland*, and amongſt the *Alps*, is not probably greater than in the north of *England*. It is more than probable too, that the rain in places ſituate near the weſtern coaſt of *Great-Britain*, and of the Continent, is greater than in the more inland parts. Mr. *Clark*, in his Letters on the Spaniſh Nation, obſerves, that there was an inſtance when no rain fell in *Caſtile* for 19 months together; the province is in the centre of *Spain*, and at a great diſtance from the ſea.

In the level parts of this kingdom, and in the neighbourhood of the metropolis, the mean annual rain is only 19 or 20 inches.

Profeſſor *Muſſchenbroek* has given us an account of the mean annual rain at ſeveral places, which we ſhall ſubjoin, together with an account from ſome other places. The inches differ a little in different countries, but the difference is too trivial to merit much notice in this place.

Mean

	Mean annual rain. Inches.
Utrecht, Haerlem, and Lifle, each	24
Delf, and Harderwick, each	27
Dort	40
Middleburgh, in Zealand	33
Paris	20
Lyons	37
Rome	20
Padua	37½
Pifa	34¼
Zurick, in Switzerland	32
Ulm, in Germany	26⅚
Wittenberg	16½
Berlin	19½
In Lancafhire	41
Upminfter, in Effex	19½

Bradford, in New England (2 years)*	31.4
Langholm, } in Scotland†	36 +
Branxholm, } in Scotland†	31 +
Kendal	64.5
Kefwick	68.5

From the table of the mean monthly rain at *Kendal* and *Kefwick*, page 38, it appears, that if we would pitch upon 6 fucceffive months, which together produce more rain than any other 6 fucceffive months, at thefe places, we muft begin with September. At *Kendal*, from September to March there is 37.6 inches of rain, and from March to September only 26.9 inches; at *Kefwick*, the rain in the former period amounts

* American Philofophical Tranfactions.

† Edinburgh Philofophical Tranfactions.

mounts to 40.4, and in the latter to 28.1.— The reafon of this feems to be, that, in the former period, the temperature of the air is decreafing, and confequently its capacity for vapour alfo; which circumftance is an additional caufe of the precipitation of vapour. In the latter period, the capacity of the air for vapour is increafing, which occafions a lefs precipitation.

When a precipitation of vapour takes place, a multitude of exceedingly fmall drops form a cloud, mift, or fog; thefe drops, though 800 times denfer than the air, at firft defcend very flowly, owing to the refiftance of the air, which produces a greater effect as the drops are fmaller, as may be proved thus:—Let $d =$ the diameter of a fmall drop, and $nd =$ that of a larger; then the refiftances, being as the fquares of the diameters when the velocity is given, will be as d^2 and n^2d^2, refpectively; but the magnitudes are as d^3 to n^3d^3, or as 1 to n^3, whence, if the large drop be divided into others of the fame magnitude as the fmall one, the number will be $= n^3$, and the refiftance to them falling, as n^3d^2, whilft the refiftance to an equal mafs in one drop is as n^2d^2; confequently, the refiftance to the large drop is to the refiftance of all the fmall ones, moving with the fame velocity, as the diameter of one fmall drop is to the diameter of the large one, and the force being conftant, the time of falling through a given fpace will be greater

when

when the drops are ſmall than when large. From this it appears, that clouds conſiſting of very ſmall drops may deſcend very ſlowly, which is agreeable to obſervation; if the drops in falling enter into a *ſtratum* of air capable of imbibing vapour, they may be rediſſolved, and the clouds not deſcend at all; and if the air's capacity for vapour increaſe, they may be all imbibed, and the cloud entirely vaniſh. On the other hand, if the precipitation go forward, and the air below have its full quantity of vapour, the ſmall drops meeting one another, will coaleſce, and form larger ones, and deſcend in form of rain to the earth's ſurface.—What is ſaid of rain, will likewiſe hold of ſnow, except that the ſmall particles coaleſcing form flakes, by reaſon of their not being fluid*.

From the important obſervations on the height of the clouds (page 41) we learn, that they are ſeldomer above the ſummit of *Skiddaw*, in Nov. Dec. Jan. and Feb. than in the other months; this clearly indicates the effect of cold in reſtraining the aſcent of vapour. Were the meaſurement extended above the ſummit of the mountain, it is probable, from the apparent law of the table, that there could not be many obſervations

* This account of the nature of clouds, and of the mode of their riſing and falling in the atmoſphere, was ſuggeſted by a philoſophical friend and acquaintance; and it appears to me very rational and conſiſtent.

ſervations above 1300 yards in winter, nor above 2000 yards in ſummer. This, it muſt be obſerved, relates to the height of the *under* ſurface of the groſs clouds only. The ſmall white ſtreaks of condenſed vapour which appear on the face of the ſky in ſerene weather, I have, by ſeveral careful obſervations, found to be from 3 to 5 miles above the earth's ſurface.

When vapour is condenſed into ſmall drops upon the ſurfaces of bodies on the ground, it is called *dew;* the only ſeeming difference betwixt dew and rain is, that the condenſation of the vapour in the one caſe is made at or near the ſurface of the body receiving it, and in the other the drops fall a conſiderable ſpace before they reach the earth; the cauſe is the ſame in both caſes, namely, cold, operating upon vapoury air. At firſt view it will ſeem inconſiſtent that a condenſation of vapour ſhould take place in the air reſting upon the earth's ſurface, which is generally ſuppoſed to be warmer than that above; but it is an inconteſtable fact, that after ſun-ſet, and during the night, in ſerene weather, the air is coldeſt at the earth's ſurface, and grows warmer the higher we aſcend, till a certain moderate height (perhaps from 20 to 100 yards, or upwards), this I have often obſerved myſelf, before I happened to ſee it elucidated, by a ſeries of experiments, in the *Lettres phyſiques, &c.* Tom. 5, page 561. And accordingly we find, that

dew

dew and hoar froſt are more copious in valleys than in elevated ſituations. That dew depends upon this circumſtance can hardly be doubted, becauſe when clouds or winds prevent it, there is little or no dew formed.

We ſhould ſcarcely be excuſed, in concluding this eſſay without calling the reader's attention for a moment to the beneficent and wiſe laws eſtabliſhed by the Author of Nature, to provide for the various exigencies of the ſublunary creation, and to make the ſeveral parts dependent upon each other, ſo as to form one well regulated ſyſtem, or whole.—In the torrid zone, and we may add in the temperate and frigid zones alſo, in ſummer, the heat produced by the action of the ſolar rays would be inſupportable, were not a large portion of it abſorbed, in the proceſs of evaporation, into the atmoſphere, without increaſing its temperature; this heat is again given out in winter, when the vapour is condenſed, and mitigates the ſeverity of the cold. The dry ſpring months are favourable to agriculture, and the evaporation, which then begins to be conſiderable, abſorbs a portion of the heat imparted to the earth by the ſun, and thus renders the tranſition from cold to heat ſlow and gradual; in autumn the ſun's influence fails apace, and the condenſation of vapour contributes to keep up the temperature, and prevent too rapid a tranſition to winter.

ESSAY SEVENTH.

On the Relation betwixt the Barometer and Rain.

SINCE the barometer has become an inftrument of general ufe, and is adopted as a guide by moft people interefted in the ftate of the weather, it may be of fervice to inveftigate the relation fubfifting betwixt the weight of the atmofphere and its difpofition for rain, from the facts afforded us by obfervation,—and we may at the fame time confider what further arguments can be obtained in fupport of the foregoing theories.

In the firft place it is remarkable, that, from the table of the mean ftate of the barometer for 5 years, in page 16, we find the higheft mean upon 6 fucceffive months obtained from March to Auguft, inclufive; that is, the mean ftate of the barometer for March, April, May, June, July, and Auguft, taken together, is greater than for any other 6 fucceffive months, being at *Kendal*, for inftance, 29.83, and for the remaining 6 months, only 29.75. But what is more particularly worthy of notice, is, that in this refpect, the rain and the barometer are juft the reverfe of each other; for, in the former period the

the rain was leaſt, and greateſt in the latter, as has been obſerved, in page 141.

Again, by recurring to the tables, page 16 and 38, we ſhall obtain the following arrangements of the months, beginning with that on which the mean ſtate of the barometer was higheſt, and proceeding regularly on to the loweſt; and again, beginning with that month on which there was leaſt rain, and proceeding to that on which there was moſt.

Barometer high.	*Barometer low.*
May, Aug. June, Mar. Sept. April.	Nov. Feb. Octo. July, Dec. Jan.
Dry months.	*Wet months.*
Mar. June, May, Aug. April, Nov.	Octo. Feb. July, Sept. Jan. Dec.*

Now it is obſervable, that the evaporation is greateſt from March to Auguſt; conſequently, the air is then farther from the point of ſaturation, or has a greater capacity for vapour, than in the other period; or, in other words, it is drier, relative to its temperature, than in the other period.—Hence then we have a ſtrong ar-

 gument

* By making the arrangements for *Kendal* alone, and taking in the preſent year, 1793, till Auguſt, and part of 1787, we obtain the following:

Bar. May, Aug. June, Mar. Sep. April, Nov. Feb. July, Oct. Jan. Dec.
Rain. Mar. May, June, April, Aug. Oct. Nov. Feb. Sep. July, Jan. Dec.

gument for the theory of the barometer, as well as for that of rain.

But to be more particular in the inveſtigation: —It will be ſeen that there have been 6 months when the mean ſtate of the barometer at *Kendal* was 30 inches or above; 9 months when it was 29.9, or from thence to 30 inches; 17 months when it was 29.8, or from thence to 29.9, &c. as per the following table.—Now, in order to examine the relation of the barometer and rain, it will be proper to find the mean monthly rain for thoſe diſtributions of the months when the mean ſtate of the barometer was nearly the ſame. This we have done, and the reſult follows.

Mean ſtate of the barometer, at Kendal.	Number of months.	Mean monthly rain in the different diſtributions, in inches.		
		Kendal.	Keſwick.	London*.
30 +	6	2.605	2.511	.212
29.9 +	9	3.362	4.018	.835
29.8 +	17	5.402	5.676	1.846
29.7 +	13	6.184	6.449	2.100
29.6 +	7	7.116	7.198	1.340
29.5 +	6	6.798	7.533	.898
29.4 +	1	3.306	3.600†	3.253
29.3 +	1	8.369	11.357	

The

* The account in this column, is the reſult of the 3 years' obſervations we have inſerted in the firſt part; the firſt mean is for 4 months, when the barometer at *London* was 30.1 plus; the ſecond for 6 months, when it was 30 plus, &c. the reſt are for 7, 11, 5, 2, and 1 months, reſpectively.

† There was no rain-gauge this month at *Keſwick*; the quantity ſet down is got by compariſon only.

The inferences to be drawn from this table are, 1ft, The higher the barometer is above its mean annual ftate, the lefs rain there is. 2d. The farther it is below its mean annual ftate, the more rain there is, till it comes to a certain point, after which the rain feems to decreafe again.

The firft of thefe inferences, being conformable to common obfervation, was expected; but the conclufion in the fecond, that the monthly mean ftate of the barometer may be *too low* to be attended with the *maximum* of rain, was not apprehended till the preceding table, which feems to warrant it, was digefted. However, it was immediately perceived, that the point might be cleared up, by felecting all thofe days which have produced the greateft quantity of rain, and finding the mean ftate of the barometer upon thofe days, which may be taken for that ftate moft conducive to the greateft quantity of rain. —The refult of a careful examination of my own obfervations, at *Kendal*, follows: during the extraordinary fall of rain on the 22d of April, 1792, (fee page 38) the mean of the barometer was 29.62; the other 2 days that gave more than 2 inches of rain each, the barometer was 29.59 and 29.33 refpectively: as for the other 56 days, on each of which there was more than 1 inch of rain, the mean ftate of the barometer upon the whole of them was 29.47, and for 54

of

of thoſe days the barometer was between 29.03 and 29.81; the barometer on the other 2 days was plainly irregular, being on the one 28.5, and it is remarkable, that the rain of that day was barely 1 inch; on the other it was 30.06, attended with an extraordinary circumſtance. (See page 44, upon June 4, 1791).

From this it appears, that the heavieſt rains may be expected when the barometer is about 29.47, at this place, or, in round numbers, 29½ inches, which is a little *above* the mean of the two great extremes obſerved in January 1789, or 29.44.

In the laſt 5 years there have been 1827 days, of which 1082, as per account, had rain, more or leſs, at *Kendal*, and 59 of thoſe gave above 1 inch of rain each; hence, at an average, there has been 1 of ſuch days in every 31, wet and fair, and in every 18 wet days, nearly. The number of days when the mean ſtate of the barometer was below 29 inches, were 40, of which 2 only were fair; and yet there was but 1 of thoſe that gave 1 inch of rain. From theſe facts we may conclude, that when the barometer is very low, the probability of its being fair is much ſmaller than at other times; but that, on the other hand, the probability of very much rain, in 24 hours, is not ſo great as at other times, which is conſiſtent with the concluſion obtained

obtained from the facts ſtated in the preceding paragraphs.

Upon an enumeration it appears, that there have been 78 days in the different months of the laſt 5 years when the mean ſtate of the barometer, at *Kendal*, was above the uſual high extreme for the month, as ſtated at page 16; only 7 of thoſe days were wet, and the rain in very ſmall quantities; hence, the probability of a fair day at that place, to that of a wet one, in ſuch circumſtances, is as 10 to 1.

The preceding facts offer nothing but what appears conſiſtent with the theories of the barometer and rain; when the barometer is above the mean high extreme for the ſeaſon of the year, the air muſt, relatively ſpeaking, be extremely *dry* or *cold*, or both, for the ſeaſon; if it be extremely dry, it is in a ſtate for imbibing vapour, and if it be extremely cold, no further degree of cold can then be expected, and therefore in neither caſe can there be any conſiderable precipitation: on the contrary, when the barometer is very low for the ſeaſon, the air muſt relatively be extremely *warm* or extremely *moiſt*, or both; if it be extremely warm, it is in a ſimilar ſtate to dry air for imbibing vapour, and if it be extremely moiſt, there muſt be a degree of cold introduced to precipitate the vapour, which cold, at the ſame time, raiſes the

the barometer. From which it follows, that no very heavy and continued rains can be expected to happen whilst the barometer actually remains about the low extreme, but they must rather be the consequence of a junction or meeting of extremes, which at the same time effects a mean state of the barometer.

ESSAY EIGHTH.

On the Aurora Borealis.

AS this eſſay contains an original diſcovery, which ſeems to open a new field of enquiry in philoſophy, or rather, perhaps, to extend the bounds of one that has been, as yet, but juſt opened ; it may not perhaps be unacceptable to many readers to ſtate briefly the train of circumſtances which led the author to the important concluſions contained in the following pages.

It will appear, from the obſervations, that the author has been pretty aſſiduous, during the laſt 6 years in noticing thoſe very ſingular and ſtriking phenomena, the *auroræ boreales*, as often as they occurred; in which time he has alſo ſeen and conſidered, with a proper attention, ſeveral conjectures and hypotheſes, endeavouring to account for them; but as no hypotheſis has yet appeared that explains the general phenomena in ſuch a manner as to procure the acquieſcence of any rational enquirer, it was natural to expect that his attention would occaſionally be turned towards an inveſtigation of the nature and cauſe of the *aurora*.

It feemed to be fufficiently proved that the *aurora* was not without the earth's atmofphere, though he had never feen any thing done which afcertained the real height of any one appearance with a tolerable degree of accuracy; and as the atmofphere, or at leaft the grofs part of it, is in all probability confined to the height of 15 or 20 Englifh miles, he was unwilling to admit of the greater height of the *aurora*, unlefs compelled to it by the refult of careful and accurate obfervations. The prevailing idea too that the *aurora* may be *heard*, was another means to induce him to think it was at a moderate height. —Appearances, however, were in direct oppofition to the thought;—that one and the fame *aurora* fhould be feen over a vaft extent of country, with much the fame circumftances, and that fome of them fhould appear in *France*, *Spain*, and *Italy*, whilft they fo very feldom pafs our zenith in the north of *England*, was a very ftrong argument for their great height. The beft obfervations likewife upon thofe large fiery meteors which occafionally fly over the country, and are feen at fuch diftant places, feem to prove the exiftence of an elaftic fluid at the height of 60 or 80 miles at leaft, which far exceeds the height of the atmofphere as prefcribed by the obfervations upon the barometer, or even by the twilight; and if the atmofphere exceed the height of 45 or 50 miles, as determined by the obfervations on the duration of twilight,

twilight, we have no *data* from whence to fix its bounds; it may, for ought we know, amount to 4 or 5 hundred miles.

Theſe conſiderations, it is evident, could not fail of ſuggeſting to the author the expediency of determining, by actual obſervations, the real height of the *aurora borealis*. This he thought might be accompliſhed by the aſſiſtance of his friend and colleague in the buſineſs, Mr. *Croſthwaite*, of *Keſwick*, who having for a long time been accuſtomed to make ſuch obſervations, was the more eligible for the purpoſe; but the manner of doing it was firſt to be determined upon, as the great difficulty was to aſcertain that the obſervations were cotemporary, and made upon one and the ſame object.

As the *aurora* often conſiſts of upright beams, eſpecially when high above the horizon, and theſe ſeldom continue one minute the ſame, the queſtion was, whether to attempt the altitude of the baſe of the beams, or the vertex, or both; this put the author upon conſidering more particularly what the real form of the beams is when ſtript of the optical illuſion, which muſt accompany all objects ſeen at a great diſtance in the atmoſphere, namely, that of appearing to coincide with the blue vault, or ſky, and to conſtitute a part of its ſpherical ſurface. A very moderate ſkill in optics was ſufficient to convince him, that as the luminous beams at all places appear to tend towards one point about the zenith, they muſt in reality be ſtraight beams, parallel,* to each other, and nearly perpendicular to the horizon; and from the appearance of their breadth, they muſt be cylindrical. Theſe circumſtances accounted at once for the *aurora* appearing ſo denſe northward, towards the horizon, and the beams being ſo thin and ſcattered towards the zenith, which is ſo uniformly the caſe. Moreover, as the beams appear to riſe above each other in regular ſucceſſion one ſet above another, in ſuch ſort, that the higher the baſes of the beams are, the higher are their vertices, it ſeemed from this circumſtance probable, that they are all of the ſame length and height; if this be the caſe, by determining the greateſt angle ſubtended by the beams, the relation or proportion of their length to their height above the earth's ſurface may be determined geometrically.—This circumſtance deſerved to be kept in view; and it appeared, from obſervations made upon the *aurora* afterwards, that though the fact could not eaſily be aſcertained, yet ſo much was certain, that the length of the beams bore a very great proportion to their diſtance from the earth, even ſo as to equal or perhaps ſurpaſs the ſaid diſtance.

Thus

* The author did not ſee, before May 1793, the Philoſophical Tranſactions for 1790, in which he finds this idea is ſuggeſted by *H. Cavendiſh*, Eſq. F. R. S. and A. S.

Thus ſtood the author's knowledge and ideas upon the ſubject in the autumn of 1792.—The very grand *aurora* in the evening of the 13th of October, was that which firſt ſuggeſted and led to the diſcovery of the relation betwixt the phenomenon and the earth's magnetiſm. When the theodolite was adjuſted without doors, and the needle at reſt, it was next to impoſſible not to notice the exactitude with which the needle pointed to the middle of the northern concentric arches: ſoon after, the grand dome being formed, it was divided ſo evidently into two ſimilar parts, by the plane of the magnetic meridian, that the circumſtances ſeemed extremely improbable to be fortuitous; and a line drawn to the vertex of the dome, being in direction of the *dipping-needle*, it followed, from what had been done before, *that the luminous beams at that time were all parallel to the dipping-needle.* It was eaſily and readily recollected at the ſame time, that former appearances had been ſimilar to the preſent in this reſpect, that the beams to the eaſt and weſt had always appeared to decline conſiderably from the perpendicular towards the ſouth, whilſt thoſe to the north and ſouth pointed directly upwards, the inference therefore was unavoidable, that the beams were guided, not by gravity, but by the earth's *magnetiſm*, and the diſturbance of the needle that had been heretofore obſerved during the time of an *aurora*, ſeemed to put the concluſion paſt doubt. It

was

was proper however to obſerve whether future appearances correſponded thereto, and this has been found invariably the caſe, as related in the obſervations.

Soon after this, the author wrote to Mr. *Croſthwaite*, deſiring him to pay particular attention to theſe phenomena for a ſeaſon, to take the bearings, altitudes, times, &c. of every remarkable appearance, and to obſerve the point to which the beams converged, the bearing of the perpendicular beams, the extent and bearing of the large, northern, horizontal lights, &c. Theſe he performed with much readineſs and ſkill, and his obſervations agree ſufficiently with thoſe made at *Kendal*, though he was entirely unacquainted with the diſcovery, and conſequently his obſervations could not be warped to ſuit the author's purpoſe.

The obſervations on the 15th of February, 1793, are thoſe upon which the height of the *aurora* reſts principally, as none of the others were ſufficiently well timed and circumſtanced to be ſubſervient to this purpoſe, except perhaps that on the 30th of March, 1793.*

We

* It may not be improper here to advert to a circumſtance, which, if not noticed, may be a means of ſubjecting the author, in ſome degree, to the imputation of plagiariſm. ——The advertiſements reſpecting this work were printed on the 10th of April, 1793, in which the diſcovery above mentioned

We ſhall now proceed to ſtate the different parts of this eſſay, diſpoſing them into ſeparate ſections, as follows.

SECTION

mentioned was announced as an original one, and never before publiſhed; the author not knowing that any one had publiſhed the moſt diſtant intimation of their aſcribing the phenomena of the *aurora borealis* to magnetiſm. On the 17th of ſaid month, *George Birkbeck*, of *Settle*, an ingenious and intelligent young man, a ſubſcriber to this work, informed the author, that an anonymous perſon, in a certain periodical publication, had given an eſſay on the *aurora borealis*, in which, amongſt other conjectures, he had advanced the opinion that it might be occaſioned by the earth's magnetiſm;—he was ſo obliging as to tranſmit the author a copy of the eſſay itſelf, which may be ſeen in a work entitled *Mathematical, Geometrical, and Philoſophical Delights*, No. 1." publiſhed May 1, 1792, under the inſpection of a Mr. *Whiting*.

The author, who ſubſcribes himſelf *Amanuenſis*, ſtates his conjectures to the following purport, viz.

1ſt. He ſuppoſes that magnetic effluvia are conſtantly iſſuing from the earth's magnetic pole in the north, and that theſe effluvia, which he conſiders of a ferruginous nature, fly off in every direction along the magnetic meridians; he then conjectures that the ſulphurous vapours, riſing from the many volcanos in the north, mixing with the magnetic effluvia, may catch fire, and fulgurate.

2d. He conjectures that inflammable air having caught fire, may receive a magnetic direction, by the current of magnetic effluvia; he ſubjoins to this conjecture, ſome very juſt obſervations on the *aurora*, which we ſhall have occaſion to mention hereafter.

3d. He conjectures that "a highly ſubtilized aerial nitre "always enters into the compoſition of an *aurora*."

4th.

SECTION FIRST.

Mathematical Propositions necessary for illustrating and confirming those concerning the Aurora Borealis.

PROPOSITION I.

ALL lines or small cylinders, whether straight, curved, or crooked, seen at a considerable distance in the atmosphere, and situate within a plane passing through the eye, must appear arches

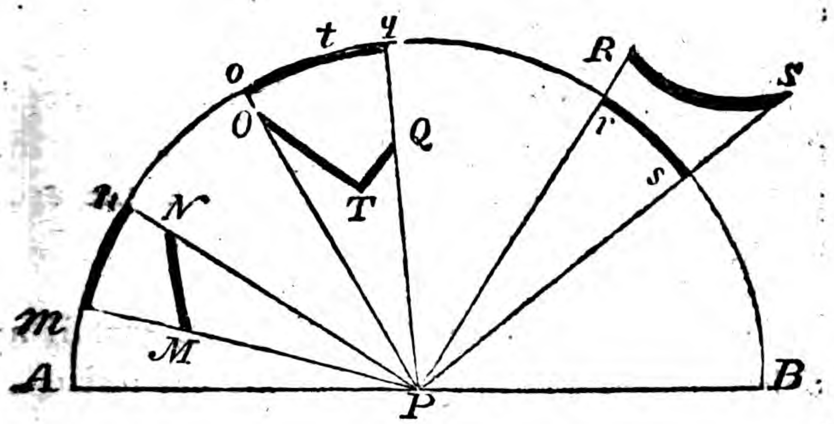
4th. That the *aurora,* like lightning, may be of an electric nature.

5th. He asks, "May the luminosity be conveyed on the "magnetic effluvia, as the electric on an iron wire?"

6th. He conceives the reason why the *aurora* is so frequent now is, because there are more volcanos in the north.

I should suppose that these conjectures, as far as they refer the phenomena of the *aurora borealis* to magnetism, are original; and from the time of the publication it might be suspected that I received the first hint from it; this however was not the case, this work being nearly ready for the press before the 10th of April, and it was not till after, that the letter containing the essay came to hand, which first furnished me with the preceding conjectures; besides, it will be seen that my opinions are, for the most part, very different from those stated above.——It is not meant by this to depreciate the merit of the ingenious *Amanuensis,* who will probably be well satisfied to see that the supposition of a relation between the *aurora borealis* and magnetism, which probably first occurred to him, is capable of being proved to a demonstration.

arches of a circle, in whofe centre is the eye, bounded by lines drawn from the eye to the extremities of the objects.



DEMONSTRATION.

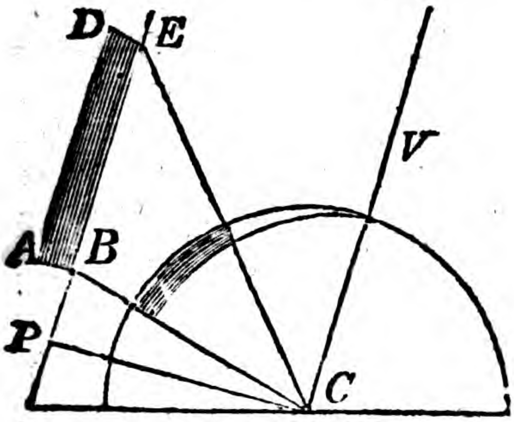
The femicircle *A m n o t q r s B P* reprefents a part of any plane paffing through the eye, fuppofed to be at *P*, the centre; *A P B* the interfection of the faid plane with the plane of the horizon; the arch of the femicircle reprefents the interfection of the firft mentioned plane with the blue canopy or fky; *MN*, *OTQ*, and *RS* reprefent three cylindrical beams feen at a diftance, whofe axes are in the plane *A m n v t q r s B P* indefinitely extended. Then the object *MN* being at a confiderable diftance, as 5, 10, &c. miles, and quite detached from all objects on the earth's furface, it follows, from the principles of optics, that the mind cannot judge with certainty either of the abfolute diftance of the object, or whether the extremity *M* or *N* is more diftant; in fuch a cafe, therefore, nothing appears to the contrary but that both ends are equally diftant, and that *MN* is an arch of a circle in the fky, with the eye in the centre; and this in fact is the judgment that is uniformly made in the cafe. For it is known to every one, that celeftial objects, and objects at a diftance in the air, as the fun, moon, ſtars, meteors, &c. all *appear* at the ſame diſtance, though nothing can be more diſproportionate than their real diſtances; that is, they all appear as if ſituate in the ſky: hence then the object *MN* will appear as the arch *mn*, *OTQ* as the arch *otq*, and *RS* as the arch *rs*. Q. E. D.

Corollary 1. Hence it may eaſily be deduced, that no line that is not wholly ſituate in a plane paſſing through the eye can appear as the arch of a great circle.

Corollary. 2. Hence alſo it follows, that if an object appear to be the arch of a great circle to two obſervers, ſo ſituate that they two, and the object, are not all in the ſame plane, the object muſt be a ſtraight line, or ſmall cylinder, becauſe it muſt neceſſarily be wholly in two planes, and conſequently in their common interſection, which is a ſtraight line (*Euclid*, 11 and 3).

PROPOSITION II.

Imagine a cylindrical beam, as *AE*, elevated in the air, and viewed from a ſtation on the earth, at a diſtance, as in the laſt propoſition; and ſuppoſe the beam ſo ſituate that a perpendicular *CP* from *C* to the ſide of the cylinder *BE* may fall below *B*, or in the prolongation of *EB*; then, I ſay, the beam will apper broadeſt near the bottom, and narrower as it aſcends, that is, its ſides will appear bounded by the circumferences of two great circles, having their common interſection in a line *CV* parallel to *BE*.



DEMONSTRATION.

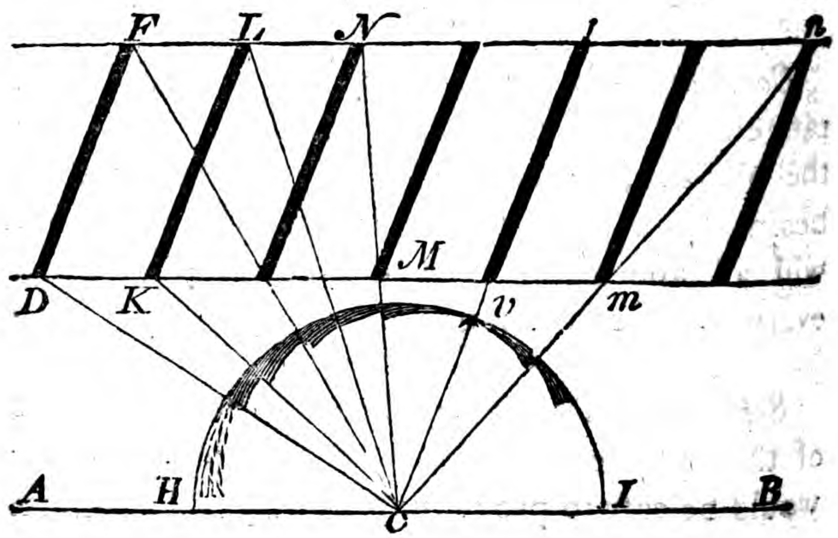
By the laſt propoſition the lines bounding the cylinder longitudinally will appear as arches of great circles; and if the line *BE* be ſuppoſed to be extended indefinitely, the angle *PCE* increaſes, and when *BE* becomes infinite, *CE* coincides with *CV*, and the angle *PCV* = a right one; and the very ſame concluſion will follow if a perpendicular be let fall from *C* upon *AD*, or any other line parallel to *BE*; therefore all right angles parallel to *BE* will appear arches of circles, which, if prolonged, would interſect each other in the line *CV*, and the ſpace bounded by any two arches will grow narrower from *P* towards *V*. Q. E. D.

Corollary. If there be a number of beams ranged all over a tranſparent plane parallel to the horizon, at the height of *AB*; and if theſe beams be parallel to the beam *AE*, then they will all appear to converge towards *V*, from every point of the horizon.

Scholium. The appearances of the extremities of the cylinder are not here conſidered; but it would be eaſy to prove they muſt appear elliptical.

PROPOSITION III.

Let there be a ſeries of cylindrical beams, *DF*, *KL*, &c. equal and parallel to each other, all in a plane perpendicular to the horizon, and at equal diſtances from the horizon; and let *AB* be the interſection of the plane with the horizon; *HvI* its interſection with the ſky; *C* the centre of *HvI*, the place of obſervation; and *Cv* parallel to the beams; then, firſt, the beams will appear to riſe above each other ſucceſſively, in the ſky, in ſuch ſort, that, of any two beams, that which has the higher baſe, will have the higher vertex alſo, except when the beams appear to paſs through, or lie wholly beyond the zenith; ſecond, theſe about the zenith will appear broadeſt, and thoſe neareſt the horizon narroweſt.



DEMONSTRATION.

Join CD, CF, CK, and CL; then the base K will appear higher than the base D by the angle DCK, and the vertex L higher than the vertex F by the angle FCL, and so on for the rest of the beams, till the angle represented by FCL is equally divided by a line from C to the zenith; afterwards the contrary takes place. The angle under which the diameter of the beam appears, being supposed small, will be nearly as the distance inversely, and therefore greatest at the zenith, and less below, in proportion as radius to the sine of elevation, Q. E. D.

PROPOSITION IV.

The same Figure remaining.

If the beams are equidistant, and if CMN, Cmn be drawn on each side of v, so as to touch the bases of two beams in M and m, and the vertices of the two next beams in N and n; then all the beams included in the angle NCn will appear distinct, and all those below, on both sides, will partly cover each other, if opaque; but if luminous, the light of the different beams being blended, will increase in density downward, according to the number of beams crossed by a right line from C.

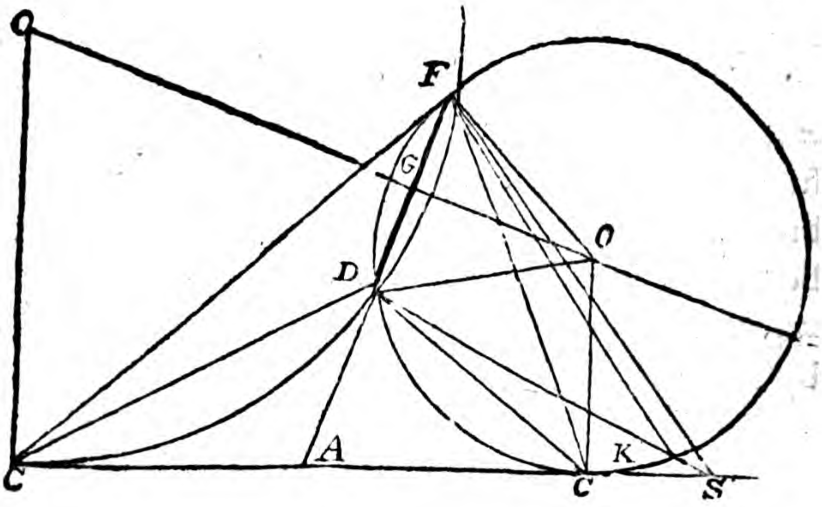
DEMONSTRATION.

The first part is obvious, from the elements of geometry; and from the principles of optics, the distance of the beams makes no difference in their apparent brightness, unless what arises from the want of perfect transparency in the atmosphere,

phere, which ſomewhat obſcures diſtant objects; hence, the greater the number of beams croſſed by a right line from *C*, the denſer will be the light in that direction. Q. E. D.

PROPOSITION V.

The ſame things being ſuppoſed as in Propoſition third; let a circle be deſcribed through the extremities of any one beam, as *DF*, to touch the horizontal line in *c**; and if *cD* and *cF* be joined, the angle *DcF*, ſubtended by the beam, will be greater than that ſubtended by any other beam, as ſeen from *c*; and if *FD* be produced to meet the horizon in *A*, and the quantity of the angle *DcF* be given, the proportion of *AD* to *DF* may be determined.



DEMON-

* To do which, ſee the laſt book of *Simpſon's Geometry*, prob. 42. third edit.

DEMONSTRATION.

Draw any line, DKS, to cut the circle in K, and meet the horizontal line in S; join FK and FS; then the angle DKF = angle DcF (*Euclid*, 3, 21); and angle DKF is greater than DSF (*Euclid*, 1, 21).

Draw oc perpendicular to the horizon from the centre of the circle o, and bisect DF by the perpendicular oG, and join oD; then, since the angle DcF is given, DoG is given also, being = DcF (*Euclid*, 3, 20); also the angles G and Aco being right, and angle A given by hypothesis, angle Goc is given also, and consequently Doc; and the triangle Doc being isoceles, the angles at D and c are both given, and angle AcD also, being the complement of Dco; whence it will be

Sine AcD : side AD :: sine A : side cD;

And sine Doc : side cD :: sine Dco : side Do;

And radius : side Do :: sine DoG : side $DG = \frac{1}{2} DF$, which gives the ratio of AD to DF. Q. E. D.

Scholium. We have here supposed the angle A acute; but if it be taken obtuse, or the observations be made on the other side of A, the proportion of $AD : DF$ may be found equally, but the greatest angle under which the beams appear will be less; thus, if oG be produced to O, so that upon O, as a centre, a circle may be described to pass through F and D, and touch the horizontal line in C; then, the greatest angle DCF will be at C, where the circle touches the horizontal line, as before.

SECTION

SECTION SECOND.

Phenomena of the Auroræ Boreales.

THE appearances of the *aurora* come under four different deſcriptions.——Firſt, a *horizontal light*, like the morning *aurora*, or break of day.—Second, fine, ſlender, luminous *beams*, well defined, and of denſe light; theſe continue $\frac{1}{4}$, $\frac{1}{2}$, or 1 whole minute, ſometimes at reſt apparently, but oftener with a quick lateral motion.—Third, *flaſhes* pointing upward, or in the ſame direction as the beams, which they always ſucceed; theſe are only momentary, and have no lateral motion, but they are generally repeated many times in a minute; they appear much broader, more diffuſe, and of a weaker light, than the beams; they grow gradually fainter till they diſappear. Theſe ſometimes continue for hours, flaſhing at intervals.—Fourth, *arches*, nearly in the form of rainbows; theſe, when complete, go quite acroſs the heavens, from one point of the horizon to the oppoſite point.

When an *aurora* takes place, thoſe appearances ſeem to ſucceed each other in the following order:—Firſt, the faint rainbow-like arches; ſecond, the beams; and, third, the flaſhes: as for the northern horizontal light, it will appear in the ſequel to conſiſt of an abundance of

flaſhes,

flashes, or *beams*, blended together, owing to the ſituation of the obſerver relative to them.

Theſe diſtinctions, and the terms appropriated for them, muſt be kept in view, in attending to the following phenomena.

PHENOMENON I.

The beams of the *aurora borealis* appear, at all places alike, to be arches of great circles of the ſphere, with the eye in the centre, and theſe arches if prolonged upwards would all meet in one point.

This is conformable to my own obſervations, and to all the accounts I have ſeen of the *aurora*.

PHENOMENON II.

The rainbow-like arches all croſs the magnetic meridian at right angles; when two or more appear at once, they are concentric, and tend to the magnetic eaſt and weſt; alſo, the broad arch of the *horizontal light* tends to the magnetic eaſt and weſt, and is biſected by the magnetic meridian; and when the *aurora* extends over any part of the hemiſphere, whether great or ſmall, the line ſeparating the illuminated part of the hemiſphere from the clear part, is half the circumference of a great circle, croſſing the magnetic meridian at right angles, and terminating in the magnetic eaſt and weſt; moreover, the beams perpendicular to the horizon are only thoſe on the magnetic meridian.

Theſe have been the uniform appearances at *Kendal* for a ſeries of obſervations paſt, as has been related; and from recollection, and the notes made upon former appearances, as well as from the inference to be drawn from the later obſervations, I have no doubt the whole liſt of the *auroræ* were conformable to this deſcription.

The accounts from *Keſwick* corroborate the ſame; the horizontal light is deſcribed as extending from WSW. to ENE. and its higheſt part in the middle, or NNW. or, when paſt the zenith, SSE*.—As for the vertical ſtreamers, their declination from the vertical circles being ſo ſmall, except about the eaſt and weſt points, it is no wonder if there be ſome latitude in theſe obſervations, when the eye is to judge; we do not find, however, that this latitude has exceeded 10° from the magnetic meridian.

That this phenomenon agrees with the obſervations made in *England*, *France*, *Germany*, &c. in the beginning of this century, when the *aurora* firſt appeared, we learn from the following extracts from the Tranſactions of the Pariſian Academy.

1707. March 6, between 7 and 10 in the evening, M. *Leibnitz* ſays an *aurora borealis* was obſerved at *Berlin;* there were two luminous arches, one above the other, both directly northward,

* The horizontal arches, indeed, do not always appear to extend juſt to the magnetic eaſt and weſt, but often to fall ſhort of, and ſometimes to ſurpaſs thoſe points; the reaſon is, we judge of its extent from its viſibility above the ſenſible horizon, and the light is either ſo faint by the great diſtance, or objects intervene, that we ſeldom ſee the extremity of the arch, within 2 or 3° of the horizon; this contracts or enlarges its viſible extent amazingly, when the arch makes a ſmall angle with the horizon.

northward, their concavity turned downwards, their chords parallel to the horizon.——The variation of the needle in *Germany*, &c. at that time, was very little from the true north.

1716. M. *Miraldi* defcribes the horizontal lights of the 11th, 12th, and 13th of April, as having all the fame fituation, namely, extent from 45 or 50° W. to 35 or 30° eaft of the meridian.——The variation at *Paris* was then about 1 point wefterly.

——March 17. a rainbow-like arch was feen at *Breft*; it extended from E. to W. croffing the meridian fouth of the zenith; foon after, a horizontal light was feen, extent from NW. to NNE.

——At *Rouen*, the fame night, a horizontal light was feen; its extent from 10° E. to 25 or 30° W.

——At *Newark*, in *Nottinghamfhire*, it was feen between the NW. and N.

One feen at *Copenhagen*, February 1, 1707, is faid to have extended from WNW. to NNE.

September 12, 1621. *Gaffendus* obferved an horizontal light, at *Aix*, in *Provence*; it extended between the fummer rifing and fetting.——N. B. The variation then was a little to the eaftward.

1718. March 4. M. *Miraldi* obferved an horizontal light; extent from NW. to NE. but declining about 10° more to the weft.

Thefe obfervations, compared with thofe recently made, fufficiently indicate that the pofition of the *horizontal lights* and *arches*, changes with the needle, and is now much more wefterly than formerly*.

It

* Since writing the above, I find in the Philofophical Tranfactions of the Royal Society for 1790, vol. 80. feveral accounts of the rainbow-like arches. In Art. 3. Mr. *Hey*, after defcribing feveral arches, fays, "the poles of all the "complete arches which I have feen had a *weftern* variation from the pole "of the equator."——In Art. 5. Mr. *Hutchinfon* defcribes one feen on the 23d of February, 1784, at *Kimbolton*, (63 miles NNW. of *London*) to have extended from ENE. to WSW.; and a defcription of the fame appearance, not differing effentially, is given in Art. 4.

It ſhould, however, be obſerved, that this phenomenon is to be underſtood as *general*, rather than *univerſal;* becauſe the *horizontal lights*, and *arches*, are ſometimes interrupted, which cauſes the *aurora* to be ſeen occaſionally almoſt wholly to the eaſt or weſt of the magnetic meridian; but on all ſuch occaſions I have obſerved the inclination of the *beams* invariably the ſame, in the ſame quarter of the heavens, as far as the eye could judge.——In fact, if the *horizontal lights*, &c. were not interrupted, the zone of light muſt quite ſurround the northern parts of the earth, at every appearance, which we are pretty certain is ſeldom, if ever, the caſe.

PHENOMENON III.

That point in the heavens to which the *beams* of the *aurora* appear to converge at any place, is the ſame as that to which the ſouth pole of the *dipping-needle* points at that place.

Granting the truth of the two preceding phenomena, it follows, that the point of convergency muſt be in the magnetic meridian; and this point, from the beſt obſervations I can make, is between 70 and 75° from the ſouth; which agrees with the obſervations at *Keſwick:* and it appears that the *dipping-needle* in *England* points to that part.—My notes upon the *auroræ* for 4 or 5 years paſt ſtate the point of convergency to the ſouth of the zenith, when a crown was formed, and I believe the remark has been generally made, wherever the appearance was ſeen and attended to.—*Kircher* obſerved the point 29° ſouth of the zenith, at *Berlin.*

In ſupport of the two laſt phenomena I might alſo quote the ingenious *Amanuenſis* whom I have mentioned in the introduction to this eſſay; he ſays, " that the lucid columns, " or radiating flaſhes of the *aurora borealis* almoſt always " ſhoot off from the north to the ſouth, correſponding in a

" great

" great meaſure to the magnetic meridian. And I have " conſtantly obſerved" (adds he) " the *corona*, concourſe, " or concentration, if I may ſo call it, of theſe lucid rays " near the zenith, ſo much to the eaſt of it as anſwered " nearly to the weſtern declination of the common magnetic " needle,—and I think I never obſerved the *corona* to the " weſtward of it.

PHENOMENON IV.

The *beams* appear to riſe above each other in ſucceſſion, ſo that of any two beams that which has the higher baſe has the higher ſummit alſo, or its ſummit nearer the point of concourſe; the angle ſubtended by the length of each beam is not the ſame, it being greateſt about half way from the horizon to the zenith, and leſs above and below; alſo the beams to the ſouth ſubtend leſs angles than thoſe to the north, having the ſame altitude.—The greateſt angle to the north ſeems to be about 25 or 30°; and that to the ſouth 15 or 20°.

PHENOMENON V.

Every *beam* appears broadeſt at or near the baſe or bottom, and to grow narrower as it aſcends, in ſuch ſort that the continuation of its bounding lines would meet in the common centre to which the beams tend; yet the ſummit of the beam is not flat, but pointed.—The higheſt beams ſeem about 3° broad, and the loweſt 1°.

The

The two laſt phenomena are the reſult of my own obſervations chiefly; but there is ſome difficulty and uncertainty in meaſuring the angles ſubtended by the lower beams, by reaſon of their being one behind another; it muſt therefore be left to future obſervations to determine more accurately the angles under which the beams appear in different parts of the hemiſphere.

SECTION THIRD.

Propoſitions concerning the Aurora Borealis.

PROPOSITION I.

THE luminous *beams* of the *aurora borealis*, are cylindrical, and parallel to each other, at leaſt over a moderate extent of country.

The beams muſt be parallel to each other, from Corol. to Prop. 2, and Corol. 2, Prop. 1, Sect. 1; and from Phenom. 1. Hence, and from Prop. 2, Sect. 1, and Phenom. 5, they are cylindrical.

PROPOSITION II.

The cylindrical beams of the *aurora borealis* are all *magnetic*, and parallel to the *dipping-needle* at the places over which they appear.

From the Corol. to Prop. 2, Sect. 1, and Phenom. 3, it follows, that the beams are parallel to the *dipping-needle*; and as the beams are ſwimming in a fluid of equal denſity with themſelves, they are in the ſame predicament as a magnetic bar, or needle, ſwimming in a fluid of the ſame ſpecific gravity

gravity with itſelf; but this laſt will only reſt in *equilibrio* when in the direction of the *dipping-needle*, owing to what is called the *earth's magnetiſm*; and as the former alſo reſts in that poſition only, the effects being ſimilar, we muſt, by the rules of philoſophizing, aſcribe them to the ſame cauſe.— Hence, then it follows, that THE AURORA BOREALIS IS A MAGNETIC PHENOMENON, AND ITS BEAMS ARE GOVERNED BY THE EARTH'S MAGNETISM*.

PROPOSITION III.

The height of the *rainbow-like arches* of the *aurora*, above the earth's ſurface, is about 150 Engliſh miles.

This appears from the calculation made from the obſervations on the 15th of February, 1793,—but other obſervations ought to be made at more diſtant places, to aſcertain the height with more preciſion. Poſſibly the height may be different at different times†.

PRO-

* I am aware that an objection may be ſtated to this;---If the beams be ſwimming in a fluid of equal denſity, it will be ſaid they ought to be drawn down by the action of the earth's magnetiſm. Upon this I may obſerve, that it is not my buſineſs to ſhew why this is not the caſe, becauſe I propoſe the magnetiſm of the beams as a thing demonſtrable, and not as an hypotheſis. We are not to deny the cauſe of gravity, becauſe we cannot ſhew how the effect is produced.---May not the difficulty be leſſened by ſuppoſing the beams of *leſs* denſity than the ſurrounding fluid.

† Since writing the above, I find Mr. *Cavendiſh* has, in Art. 10 of the Philoſophical Tranſactions for 1790, calculated the height of an arch obſerved at different places, on the 23d of February, 1784, to be betwixt 52 and 71 miles.---But, with deference, I would remark, that the obſervations above mentioned appear to me better circumſtanced than thoſe upon which his calculation is founded, and therefore the reſult of them more to be relied upon.

PROPOSITION IV.

The beams of the *aurora* are ſimilar and equal in their real dimenſions to one another.

This is not capable of ſtrict demonſtration, for want of more exact obſervations; it is, however, rendered extremely probable from Prop. 3 and 5, Sect. 1, and Phenom. 4 and 5. —Indeed the phenomena are almoſt irreconcileable to any other ſuppoſition.

PROPOSITION V.

The diſtance of the *beams* of the *aurora* from the earth's ſurface, is equal to the length of the beams, nearly.

Allowing the truth of the laſt propoſition, and comparing Prop. 5, Sect. 1, with Phenom. 4, we ſhall find the phenomenon to agree beſt with the ſuppoſition of the equality of the diſtance and length of the beams.

We have here ſubjoined the reſult of a calculation of the angles ſubtended by the beams, on three different ſuppoſitions, namely, 1ſt, when the length of the beams is equal to their diſtance from the earth; 2d, when the length is but half that diſtance; and, 3d, when it is twice the diſtance.— The calculation is eaſily made by inverting Prop. 5, Sect. 1, and ſuppoſing the point *c* variable, where we have the ratio of AD to DF, inſtead of the angle DcF given; the beams are ſuppoſed to be thoſe in the plane of the magnetic meridian, both north and ſouth of the zenith, and their baſes are taken at 10°, 20°, 30°, &c. altitude. The angle FAc is ſuppoſed 72°.

When accurate obſervations ſhall be made, I have no doubt the angles on the 2d ſuppoſition will be found too little, and thoſe on the 3d too great. Angles

Angles AcD & ACD.	$AD:DF::1:1$ Angle DcF.	Angle DCF.	$AD:DF::1:\frac{1}{2}$ Angle DcF.	Angle DCF.	$AD:DF::1:2$ Angle DcF.	Angle DCF.
10°	10° 30′	8° 27′	5° 14′	3° 25′	20° 51′	15° 23′
20	19 32	13 4	10 7	7 16	35 2	21 27
30	24 52	14 12	13 42	8 22	40 10	21 33
40	26 34	12 50	15 33	7 56	39 46	18 27
50	25 36	9 48	15 43	6 16	36 27	13 36
60	22 48	5 43	14 32	3 45	31 23	7 45
70	18 53	1	12 21	0 40	25 26	1 20
80	14 15		9 28		18 58	
90	9 14		6 11		12 18	
100	4 4		2 44		5 24	

Scholium. It is very probable the *rainbow-like arches* are either at the top or bottom of the *beams*, and I am inclined to think they are at the top, not only becaufe their light is faint, but becaufe the beams fhould be feen at a much greater diftance than it feems they are, if they were 300 miles high, or twice the height of the arches; and the obfervations on the 30th of March, 1793, feem to confirm the opinion of the bafes of the beams being 60 or 70 miles high, or about half the height of the arches.

If the fummits of the beams be 150 miles high, their bafes will, according to this propofition, be 75 miles high, and the whole length of the beams about 75 miles, or, more nearly, 75 miles × $\frac{\text{radius}}{\text{fine of } 72°}$. And if the diameter of the bafe be $\frac{1}{10}$ of the length, each luminous beam will be a cylinder of $7\frac{1}{2}$ miles in diameter, and 75 miles long*.

* If a magnet be required to be made of a given quantity of fteel, it is found by experience to anfwer beft when the length is to the breadth as 10 to 1 nearly: it is a remarkable circumftance that the length and breadth of the magnetic beams of the *aurora* fhould be fo nearly in that ratio.--- Query, if a fluid mafs of magnetic matter, whether elaftic or inelaftic, were fwimming in another fluid of equal denfity, and acted on by another magnet at a diftance, what form would the magnetic matter affume? Is it not probable it would be that of a cylinder, of proportional dimenfions to the beams of the *aurora*?

N. B. An object elevated 75 English miles may be seen at the distance of 10 geographical degrees; if elevated 150 miles, it may be seen 14°; if 300 miles, 20°.

PROPOSITION VI.

That appearance which we have called the *horizontal light*, and which is always situate near the horizon, is nothing but the blended lights of a group of *beams*, or *flashes*, which makes the appearance of a large luminous zone.

The figure to Prop. 3, Sect. 1, represents a series of beams such as those of the *aurora*, situate in the plane of the magnetic meridian, and *C* the place of observation. And it is proved in Prop. 4, Sect. 1, that the lights of the distant beams in that plane will be blended, to a certain elevation, to the observer at *C*. Imagine a series of planes parallel to the plane of the magnetic meridian, with beams situate in them likewise; then, from the principles of optics, the rows of beams in every two of the planes will appear to approach each other, as the distance from the observer increases; and when that distance becomes indefinitely great they will all seem to coincide; hence the beams will appear blended, both horizontally and perpendicularly, and will consequently constitute a large zone of dense light. This zone must appear at right angles to the magnetic meridian, because it is observed (Phenom. 2.) that when the beams of the *aurora* extend over a great part of the hemisphere, they are always bounded by an arch crossing the magnetic meridian at right angles.

SECTION

SECTION FOURTH.

Theory of the Aurora Borealis.

IN the preceding ſection we have deduced the nature of the *aurora*, merely by combining mathematical principles with the phenomena; the concluſions, therefore, are not drawn from *hypotheſis*, but from *facts*, and muſt hold, as far as the facts are well aſcertained, and the principles truly applied.—In this ſection we mean to propoſe ſomething by way of hypotheſis, to account for thoſe phenomena.

The *light* of the *aurora* has been accounted for on three or more different ſuppoſitions: 1. It has been ſuppoſed to be a flame ariſing from a chymical effervefcence of combuſtible exhalations from the earth. 2. It has been ſuppoſed to be inflammable air, fired by electricity. 3. It has been ſuppoſed electric light itſelf.

The firſt of theſe ſuppoſitions I paſs by, as utterly inadequate to account for the phenomena. The ſecond is preſſed with a great difficulty how to account for the exiſtence of *ſtrata* of inflammable air in the atmoſphere, ſince it appears that the different elaſtic fluids, intimately mix with each other; and even admitting the exiſtence of theſe *ſtrata*, it ſeems unneceſſary to introduce them in the cafe, becaufe we know that difcharges of the electric fluid in the atmofphere do exhibit light, from the phenomenon of lightning.—From thefe, and other reafons, fome of which fhall be mentioned hereafter, I confider it almoft beyond doubt that the *light* of the *aurora borealis*, as well as that of *falling ftars* and the *larger meteors*, is electric light folely, and that there is nothing of combuftion in any of thefe phenomena.

Air, and all elaftic fluids, are reckoned amongft the non-conductors of electricity. There feems, however, a difference amongft them in this refpect; dry air is known to conduct worfe than moift air, or air faturated with vapour. Thunder ufually takes place in fummer, and at fuch times as the air is highly charged with vapour; when it happens in winter, the barometer is low, and confequently, according to our theory of the variation of the barometer, there is then much vapourized air: from all which it feems probable, that air highly vapourized becomes an imperfect conductor, and, of courfe, a difcharge made along a *ftratum* of it, will exhibit light, which I fuppofe to be the general cafe of thunder and lightning.

Now, from the conclufions in the preceding fections, we are under the neceffity of confidering the *beams* of the *aurora borealis* of a *ferruginous* nature,

nature, becaufe nothing elfe is known to be magnetic, and confequently, that there exifts in the higher regions of the atmofphere an elaftic fluid partaking of the properties of *iron*, or rather of *magnetic fteel*, and that this fluid, doubtlefs from its magnetic property, affumes the form of cylindric beams.—It fhould feem too, that the rainbow-like arches are a fort of *rings* of the fame fluid, which encompafs the earth's northern magnetic pole, like as the parallels of latitude do the other poles; and that the beams are arranged in equidiftant rows round the fame pole. At firft view, indeed, it feems incompatible with the known laws of magnetifm, that a quantity of magnetic matter fhould affume the form of fuch rings, by virtue of its magnetifm; but it may take place in one cafe at leaft, if we fuppofe the rings fituate in the middle, between two rows of beams, fo that the attraction on each fide may be equal. As for the beams, in their natural ftate, when not acted upon by caufes hereafter to be mentioned, they muft all be guided by the *earth's magnetifm* (I mean the caufe that guides the needle, whether it is in the earth or air I know not), and confequently all have their *north poles* downward; but whether any two neighbouring beams have the poles of the fame denomination, or of different denominations, acting upon each other, ftill the effect will be the fame, and their mutual action upon each other not difturb their parallelifm, nor the pofition

pofition of the rings; becaufe, whether the poles mutually attract or repel each other, is of no moment in this cafe, and the attraction of each pole is alike upon the rings.

Things being thus ftated, I moreover fuppofe, that this elaftic fluid of magnetic matter is, like vapourized air, an *imperfect conductor* of electricity; and that when the equilibrium of electricity in the higher regions of the atmofphere is difturbed, I conceive that it takes thefe beams and rings as conductors, and runs along from one quarter of the heavens to another, exhibiting all the phenomena of the *aurora borealis.*—The reafon why the diffufe flafhes fucceed the more intenfe light of the beams is, I conceive, becaufe the electricity difperfes the beams in fome degree, which collect again after the electric circulation ceafes.

Many of my readers, I make no doubt, will be furprifed to find, after having formed a conception that the relation betwixt the *aurora* and magnetifm was to be explained and demonftrated, chiefly if not folely, from the obfervations on the difturbance of the needle during the *aurora,* that no mention or ufe whatever is made of thofe obfervations, in the preceding fections. In fact, the relation above mentioned is demonftrable without any reference to them; notwithftanding which, they not only corrobo-

rate

rate the proof of it, but almoſt eſtabliſh the truth of the hypotheſis we are here advancing.

The variations of the needle during the *aurora*, as may be ſeen in the obſervations, are ſo exceedingly irregular, that after conſidering them a while, one would conclude this is the only fact aſcertained by theſe obſervations. However, I think we may deduce the following:

1. When the *aurora* appears to riſe only about 5, 10, or 15° above the horizon, the diſturbance of the needle is very little, and often inſenſible.

2. When it riſes up to the zenith, and paſſes it, there never fails to be a conſiderable diſturbance.

3. This diſturbance conſiſts in an irregular oſcillation of the horizontal needle, ſometimes to the eaſtward, and then to the weſtward of the mean daily poſition, in ſuch ſort that the greateſt excurſions on each ſide are nearly equal, and amount to about half a degree each, in this place.

4. When the *aurora* ceaſes, or ſoon after, the needle returns to its former ſtation.

Now, from theſe facts alone, independent of what is contained in the preceding ſections, I think

think we cannot avoid inferring, that there is ſomething magnetic *conſtantly* in the higher regions of the atmoſphere, that has a ſhare at leaſt in guiding the needle; and that the fluctuations of the needle during the *aurora* are occaſioned by ſome mutations that then take place in this magnetic matter in the incumbent atmoſphere; for, it is certainly improbable, if not abſurd, to ſuppoſe that the *aurora produces* this magnetic matter, at its commencement, and *deſtroys* it at its termination. Moreover, abſtracting from a chymical ſolution of the metal, nothing is known to affect the magnetiſm of *ſteel*, but *heat* and *electricity;* heat weakens, or deſtroys it; electricity does more, it ſometimes changes the pole of one denomination to that of another, or inverts the magnetiſm. Hence, we are obliged to have recourſe to one of theſe two agents, in accounting for the mutatious above mentioned. As for heat, we ſhould find it difficult, I believe, to aſſign a reaſon for ſuch ſudden and irregular productions of it in the higher regions of the atmoſphere, without introducing electricity as an agent in thoſe productions; but rather than make ſuch a ſuppoſition, it would be more philoſophical to ſuppoſe electricity to produce the effect on the magnetic matter *immediately*. Hence then were we obliged to form an *hypotheſis* of the *aurora borealis*, without any other facts relative to it than the *four* above mentioned, we ought to ſuppoſe it a phenomenon produced in ſome manner by the united agency of magnetifm and electricity.

It appears then, that the difturbance of the needle during an *aurora* equally countenances the conclufions drawn in the laft fection, and the hypothefis adopted in this; and it may be accounted for on the hypothefis, as follows.

The beams of the *aurora*, being magnetic, will have their magnetifm weakened, deftroyed, or inverted, *pro tempore*, by the feveral electric fhocks they receive during an *aurora*; or perhaps the temporary difperfion and diffufion of the magnetic matter thereby, may confiderably alter its influence; when, therefore, the alterations on each fide of the magnetic meridian do not balance each other, the confequence will be a difturbance of the needle*.

* I conceive that a beam may have its magnetifm inverted, and exift fo for a time, becaufe the repulfion, acting longitudinally upon it, will only impel it in that direction, and not turn it round; juft as the north pole of a magnet may be applied to the north pole of a magnetic needle, without turning it round, by keeping the magnet exactly in the fame line with the needle, and thus making the needle act upon the centre. And I further conceive, that when the beam is reftored to its natural pofition of the north pole downward, it is effected, not by inverting the beam, wholly as a beam, (for this is never obferved in an *aurora*) but by inverting the conftituent particles, which may eafily be admitted of a fluid.

In fine, the conclusions in the last section, and the hypothesis in this, afford a very plausible reason for the appearance of the *aurora* being so much more frequent now than formerly in these parts; if the earth's magnetic poles be like the centres of the *aurora*, as the phenomena indicate, it is plain the *aurora* must move along with them, and appear or disappear at places, according as the magnetic poles approach or recede from them; and hence it may be presumed that the earth's magnetic pole in the northern hemisphere is nearer the west of *Europe* in this century than it was in the last or preceding.—The observations upon the dip of the needle, however, if they have been accurately made, seem to indicate the approach of the magnetic pole to have been very little; the dip at *London*, according to Mr. *Cavallo*, was 71° 50′ in 1576, and 72° 3′ in 1775; but there is reason to suspect the accuracy of the instruments at so early a period as 1576; besides, we do not know in what proportion the dip of the needle keeps pace with the approach of the pole.

It may perhaps be necessary here, before the subject is dismissed, to caution my readers not to form an idea that the *elastic fluid of magnetic matter*, which I have all along conceived to exist in the higher regions of the atmosphere, is the same thing as the *magnetic fluid or effluvia* of most writers on the subject of magnetism.

netifm. This laft they confider as the efficient caufe of all the magnetic phenomena; but it is a mere hypothefis, and the exiftence of the *effluvia* has never been proved My *fluid of magnetic matter* is, like magnetic fteel, a fubftance poffeffed of the properties of magnetifm, or, if thefe writers pleafe, a fubftance capable of being acted upon by the magnetic *effluvia*, and not the magnetic *effluvia* themfelves.

Whether any of the various kinds of air, or elaftic vapour, we are acquainted with, is magnetic, I know not, but hope philofophers will avail themfelves of thefe hints to make a trial of them.

SECTION FIFTH.

An inveftigation of the fuppofed effect of the Moon in producing the Aurora Borealis*.

SOME time after the author began his obfervations on the *aurora borealis*, it occurred to him that the phenomenon had more frequently happened about the change of the moon than at any

* An effay on this fubject was firft publifhed by the author in the beginning of 1789, in Mr. *Davifon's Mathematical and Philofophical Repofitory.*

any other time; this produced the fuſpicion that the ærial tides occaſioned by the moon might have ſome influence upon it. Granting this to be the caſe, it was obvious, the full moon muſt have an equal ſhare with the new, though the phenomenon may often be then inviſible, owing to the light of the moon.——Having now an enlarged liſt of obſervations, we ſhall reſume the ſubject afreſh, and examine what countenance the obſervations give to the ſuppoſition.

In the liſt of obſervations we have placed the moon's age, both with reſpect to change and full; collating, therefore, the whole number of obſervations to each particular number expreſſing the age, we ſhall have the following ſeries:

Days paſt change and full.	0	1	2	3	4	5	6	7
No. of obſervations.	14	25	21	20	19	20	15	21
Days paſt change and full.	8	9	10	11	12	13	14.	
No. of obſervations.	18	23	15	6	10	13	9. (12)	

As the lunar revolution is completed in 29½ days nearly, one half of a lunation is 14¾ days; hence the obſervations under 14 do not ſtand the ſame chance as the reſt, there being only ¾ of the number of periods that have a day correſponding to this number: the number of obſervations under it ought therefore to be increaſed in the

ratio

ratio of $\frac{3}{4}$ to 1, or be 12 inſtead of 9, in order to make a fair diviſion of the terms of the ſeries. Now the ſpring tides will fall almoſt wholly in the firſt half of this period, and the neap tides in the laſt; dividing the terms of the ſeries, therefore, into two equal portions, taking half of the odd intermediate one to each, the ſums of the portions are as under.

	Spring tides.		*Neap tides.*
No. of *auroræ.*	$144\frac{1}{2}$.		$107\frac{1}{2}$.
Ratio	4	:	3, nearly, which

is favourable to the ſuppoſition.

It may be objected, that as the latter diviſion contains the whole of the *ſecond* quarter of the moon, when its light is ſtrong, and when it is above the horizon all the time there is to obſerve the *aurora*, the phenomenon is not noticed as often as it takes place in that quarter.—This may be right, but it ſhould be obſerved, that the laſt quarter of the moon, which is wholly exempt from this objection, falls in the ſame diviſion; and both the firſt and third quarters, conſtituting the other diviſion, are in part liable to the ſame objection.

However, in order to determine whether this objection is of ſuch import as to counterbalance the apparent concluſion contained above, it may be proper to find and compare the number of obſervations

obſervations in the firſt and laſt quarters only.— This being done, on the principle above uſed, the numbers ſtand,

Firſt quarter, or ſpring tides.	Laſt quarter, or neap tides.
$93\frac{1}{2}$.	81.

From which it appears the phenomenon is obſerved more frequently in the firſt quarter of the moon, though liable in part to the above objection, than in the laſt quarter, which is wholly free from it.

Preſuming then from what is done above, that thoſe periods of the lunar months, when the higher tides are in the air, are moſt ſubject to the phenomenon in queſtion, it ſhould be expected, that thoſe times of the *day* when ſuch tides are in the atmoſphere, ſhould likewiſe be more ſubject to it than others. Now the ſpring tides in the afternoon always happen in the interval from 2 to 8 o'clock; conſequently, the opportunity of making obſervations upon the phenomenon in this interval will *often* occur in in winter, and *never* in ſummer, owing to the twilight.—It ſhould ſeem then, that the winter obſervations ought to favour the hypotheſis more than the ſummer ones.—In fact, we find this the caſe. The obſervations in the months of November, December, and January, being arranged and ſummed up as above, give,

Spring

Spring.	*Neap.*
40½.	24½.

And thofe in the months of May, June, July, and Auguft, give,

Spring.	*Neap.*
25½.	24½.

As the tides are higher in fpring and autumn than in fummer and winter, the phenomenon ought, according to hypothefis, to occur more frequently in the two former feafons than in the two latter. The number of obfervations in the different months ftand thus:

Jan.	Feb.	Mar.	April	May	June	July	Aug.	Sep.	Oct.	Nov.	Dec.
18	18	26	32	21	5	2	21	23	36	38	9.

The fmall number in June and July is undoubtedly owing in great part to the twilight; but the deficiency in December, January, and February, cannot be owing to the fame caufe.

Upon the whole, I think it is not improbable that the agitations caufed by the moon in the very high regions of the atmofphere, which we may fuppofe are not much agitated by the tempefts in the lower regions, may have fome effect upon the phenomenon in queftion; and the fuppofition is evidently countenanced by the feveral facts ftated above.

SECTION

SECTION SIXTH.

An inveſtigation of the effect of the Aurora Borealis *on the Weather ſucceeding it.*

VARIOUS have been the conjectures on this ſubject offered to the conſideration of the public: ſome aſſert that the *aurora* has no ſenſible effect upon the weather; others that it is very frequently followed by rain ſoon after.

In the American Philoſophical Tranſactions, we find it obſerved that the barometer *falls* after an *aurora*.

Having a large number of obſervations on the *aurora*, together with thoſe on the barometer and rain, we are prepared to examine theſe opinions, and we do it the rather becauſe if any thing can be aſcertained on this head, it muſt be regarded as a valuable diſcovery, conſidering the preſent very imperfect ſtate of meteorological prognoſtication.

Since the ſpring of 1787 there have been 227 *auroræ* obſerved at *Kendal* and *Keſwick;* 88 of the next ſucceeding days were *wet*, and 139 *fair*, at *Kendal;* now, in the account of rain, the mean yearly number of wet days there is ſtated at 217, and of courſe the fair days are 148; hence the chances of any one day, taken at ran-

dom,

dom, being wet or fair, are as these numbers. But it appears the proportion of fair days to wet ones succeeding the *auroræ*, is much greater than this general ratio of fair days to wet ones; the inference therefore is, that the appearance of the *aurora borealis* is a prognostication of *fair weather*.

The only objection to this inference which occurs to us as worth notice is, that the *aurora* being from its nature only visible in a clear atmosphere, this circumstance of itself is sufficient to cast the scale in favour of the succeeding day being fair, without considering the *aurora* as having any influence either directly or indirectly. —This objection has undoubtedly some weight; but upon examinining the observations, it appears that the *aurora* not only favours the next day, but indicates that a series of days to the number of 10 or 12, are more likely to be all fair, than they would be without this circumstance.

Of 227 observations, 139 were followed by 1 or more fair days, 100 by 2 or more &c. as under.

1	2	3	4	5	6	7	8	9	10	11	12
139	100	69	52	38	30	21	16	10	6	2	1.

According to the laws of chance, the probability of any number of successive fair days is found by raising $\frac{148}{365}$ to the power, whose index is the proposed number of fair days; these probabilities being multiplied by 227 will give what

 the

the above ſeries ought to have been, if the *aurora* had no influence; it is as under.

1	2	3	4	5	6
92	38	15	6	2	1

From which it appears, there ſhould not have been more than 1 *aurora* out of 227 followed by 6 fair days, and yet in fact there were 30; whence the inference above made is confirmed.

As for the different ſeaſons of the year, I find the *aurora* is more frequently followed by fair weather in ſummer than in winter; but the diſtinction is not very conſiderable.

It may be obſerved that the largeſt and moſt ſplendid appearances of the *aurora*, as they uſually happen in rainy and unſettled weather, they are frequently ſucceeded by 1 or more wet days; but I do not find any of thoſe very remarkable ones which happened on a fair day, was ſucceeded by a wet one.

Upon examination of the effect of the *aurora* upon the barometer, I find, that ſince the 19th of September, 1787, there have been 219 obſervations, and that in 120 of theſe inſtances the barometer was riſen next morning, and fallen in 99.—This circumſtance, therefore, corroborates the inference before made, that the *aurora* is a ſign of fair weather.

General

General Rules and Obſervations for judging of the Weather.

NOTWITHSTANDING we have departed pretty much from our original deſign of expatiating on this ſubject, we think it may not be amiſs to collect ſome of the facts and obſervations that are diffuſed through the work, which relate more immediately to the ſubject, and to add thereto a few more obſervations.

1. The barometer is higheſt of all during a long froſt, and generally riſes with a NE. wind; it is loweſt of all during a thaw following a long froſt, and is often brought down by a SW. wind. See page 112.

2. When the barometer is near the high extreme for the ſeaſon of the year, there is very little probability of immediate rain. See page 151.

3. When the barometer is low for the ſeaſon, there is ſeldom a great weight of rain, though a fair day in ſuch a caſe is rare. See page 150. The general tenor of the weather at ſuch times is, ſhort, heavy, and ſudden ſhowers, with ſqualls of wind from the SW. W. or NW.

4. In ſummer, after a long continuance of fair weather, with the barometer high, it generally falls gradually, and for one, two, or more days, before there is much appearance of rain.—If the

fall be ſudden and great for the ſeaſon, it will probably be followed by thunder.

5. When the appearances of the ſky are very promiſing for fair, and the barometer at the ſame time low, it may be depended upon the appearances will not continue ſo long. The face of the ſky changes very ſuddenly on ſuch occaſions.

6. Very dark and denſe clouds paſs over without rain when the barometer is high; whereas, when the barometer is low, it ſometimes rains almoſt without any appearance of clouds.

7. All appearances being the ſame, the higher the barometer is, the greater the probability of fair weather.

8. Thunder is almoſt always preceded by hot weather, and followed by cold and ſhowery weather.

9. A ſudden and extreme change of temperature of the atmoſphere, either from heat to cold or cold to heat, is generally followed by rain within 24 hours.

10. In winter, during a froſt, if it begin to ſnow, the temperature of the air generally riſes to 32°, and continues there whilſt the ſnow falls; after which, if the weather clear up, expect ſevere cold.

11. The *aurora borealis* is a prognoſtic of fair weather. See Eſſay 8, Sect. 6.

Appendix,

Appendix, containing additional Notes, &c. on different parts of the Work.

PAGE 8.

THE height of *Kendal* above the ſea was ſet down 25 yards, by eſtimation only; I have ſince found, by levelling with the barometer, that *Stramongate bridge*, at *Kendal*, is 46 yards above *Levens bridge*, to which the tide flows; though it ſeems the ſurvey for the intended canal makes the height leſs: I am not aware of any circumſtance that could lead me into an error.

Mr. *Croſthwaite* has lately determined, by levelling with a very good theodolite, that *Baſſenthwaite-lake* is 70 yards above the level of the ſea, and that *Derwent-lake*, which is 10 yards below his barometer, is 76 yards above the level of the ſea; I make the laſt mentioned lake 81 yards above the level of the ſea, from barometrical obſervations; but if I have made an error by determining *Kendal* 5 yards too high, the reſults of our obſervations will be reconciled *.

Page

* The height of the following places above the level of the ſea have been determined as under; the obſervations with the barometer were made by myſelf, and thoſe with the theodolite by Mr. *Croſthwaite*.

	From barom. obſ.	From the theodolite.
Windermere lake - - - -	26 yards.	———
Dunmail-raiſe, barrow of ſtones in the boundary of *Cumberland* and *Weſtmorland*	245 ———	275.
Leathes lake - - - - - -	171 ———	182.

My

Page 29.—The greateſt heat experienced for the laſt 5 years, at *Kendal*, was on the 1ſt of Auguſt 1792; but the heat of the preſent year, 1793, exceeded; the thermometer in the ſhade was 83°½ on the 11th, and 84½ on the 15th of July.

Page 39.—There is a great diſcordance in the height of *Skiddaw*, as determined by the obſervations of different perſons; I have remarked that Mr. *Croſthwaite* made it 1050 yards above *Derwent lake*, I find ſince that Mr. *Donald* made it 1090 yards above the ſea, and 958 above *Baſſenthwaite lake*. Mr. *Croſthwaite*, by a later admeaſurement, determines its height 1000 yards above *Derwent lake*.—On the 26th of Auguſt, 1793, I attempted its height by the barometer, for which purpoſe the following obſervations were made.

At 3h 30m P. M. the barometer upon the ſummit of *Skiddaw*, when the proper allowance for the riſe of the mercury in the reſervoir was made, ſtood at - - - - - - - - - - - - 26.79 inches.
Mr. *Croſthwaite*'s barometer at *Keſwick*, allowing for the ſmall difference of the barometers when together, at the ſame time, ſtood at - - 29.715——.
A detached thermometer above was, in the ſhade, - 46°.
A detached thermometer below was, in the ſhade, - 60°.

Now,

My obſervations were taken both in going to and returning from *Keſwick*, and compared with nearly cotemporary obſervations at *Kendal* and *Keſwick*; at the former time the air was dry, and at the latter moiſt: the elevations were found ſomething leſs by the later obſervations, but the difference was only 2 yards in *Leathes lake* and 9 in *Dunmail-raiſe*.

Now, by applying the theorem at page 81, we find the elevation of the upper barometer above the lower $945\frac{1}{2}$ yards; whence, adding $10\frac{3}{4}$ yards, we get the height of *Skiddaw* above *Derwent lake* = $956\frac{1}{4}$ yards, and its height above the ſea comes out $1037\frac{1}{4}$ yards.—It had been a good deal of rain on the morning of that day, and the clouds were juſt broken off at the time of the obſervations, the air remaining ſtill very ſoft; from which circumſtance I am inclined to think that the height above determined is rather too little; for I have found by repeated obſervations upon a hill 310 yards high, that the heights are found leſs by the theorem as the air is ſofter, *cæteris paribus*; I think therefore we may conclude *Skiddaw* to be nearly 1000 yards above *Derwent lake*, agreeable to Mr. *Croſthwaite*'s laſt meaſurment, till its height can be more exactly aſcertained by a repetition of obſervations *.

AFTER

* Mr. *Croſthwaite* makes the height of *Latrig*, another mountain in the neighbourhood of *Keſwick*, to to be 319 yards above *Derwent lake*: by obſervations on the barometer the above mentioned day, I found its height 312 yards, which, for the reaſon aſſigned above, is probably too little.

Helvellyn is a mountain cloſe by the road leading from *Kendal* to *Keſwick*, about 8 miles from the latter place; it has always juſtly been conſidered higher than *Skiddaw*. On the 27th of Auguſt I made the following obſervations to determine its height.

At 1h 30m P. M. barometer at the ſummit, corrected as above,	26 69.
Barometer below, 10 yards above *Leathes lake*, - - - - -	29.39.
A detached thermometer at the ſummit was - - - - - -	$42°\frac{1}{2}$.
A detached thermometer below was - - - - - - - -	54.

From

AFTER I had observed the *aurora borealis* to to disturb the needle so greatly, as is related in the *addenda* to the observations on that head, I conjectured, *a priori*, that thunder-storms would do the same; accordingly, I watched the needle for a considerable time during the only thunder-storm we had at *Kendal* in the summer of 1793, namely, on the evening of the 3d of August; but, far from perceiving any unusual fluctuation, I could not discover the needle was perceptibly disturbed all the while, and it continued at the same station the next morning.

On the state of Vapour in the Atmosphere, &c.— See page 134, and following.

AFTER making some experiments upon the effects of the condensation of atmospheric air, in a glass vessel, by means of a syringe, from which I find that repeated condensation produces a deposition of water upon the inside of the glass, and repeated rarefaction removes the same; also, having

From which the elevation of the upper barometer above the lower comes out 869⅓ yards; to which adding 171 and 10, we get the height of *Helvellyn* above the sea = 1050⅓ yards. But it should be observed the state of the air was still more moist than when I was upon *Skiddaw*, and the observation at top was taken during a shower; from which it is probable the height of *Helvellyn* above the sea is nearly 1100 yards: Mr. *Donald* makes it 1108 above the sea.—About 200 yards below the summit there is a very fine spring, from which a large stream of water descends all the year round, with little variation in quantity at the different seasons, as my guide informed me; its temperature I found to be 38°.

having made ſome experiments upon the effect of heat on water thrown into the *vacuum* of a common barometer, which tend to confirm thoſe the reſult of which is given at page 134,—I am confirmed in the opinion, that *the vapour of water (and probably of moſt other liquids*), exiſts at all times in the atmoſphere, and is capable of bearing any known degree of cold without a total condenſation, and that the vapour ſo exiſting is one and the ſame thing with* ſteam, *or vapour of the temperature of* 212° *or upwards*. The idea, therefore, that vapour cannot exiſt in the open atmoſphere under the temperature of 212°, unleſs chymically combined therewith, I conſider as erroneous; it has taken its riſe from a ſuppoſition that *air* preſſing upon *vapour* condenſes the vapour equally with *vapour* preſſing upon *vapour*, a ſuppoſition we have no right to aſſume, and which I apprehend will plainly appear to be contradictory to reaſon, and unwarranted by facts; for, when a particle of vapour exiſts between two particles of air, let their equal and oppoſite preſſures upon it be what they may, they cannot bring it nearer to another particle of vapour, without which no condenſation can take place, all other circumſtances being the ſame; and it has never been proved that the vapour in a receiver from which the air has been exhauſted is precipitated upon the admiſſion of perfectly dry air. Hence, then, we ought to conclude, till the con-

* Dr. *Prieſtley* obſerves in the fifth volume of his Experiments, page 225, that quickſilver evaporates not only *in vacuo* but when expoſed to the atmoſphere.

trary can be proved, that *the condensation of vapour exposed to the common air, does not in any manner depend upon the pressure of the air.*

All the facts, however, conspire to prove that the *temperature* of the air bears a relation to the condensation of vapour; thus, the utmost force which vapour of 212° can exert, is equivalent to the weight of 30 inches of mercury, and any greater force than this, acting upon vapour alone of that temperature, will condense the whole into water; and, if the temperature be less, then the utmost force or spring of vapour is less, as is indicated by the table in page 134; and no doubt as the utmost force decreases, the utmost density will decrease also, though probably not in the same ratio. Hence, then, atmospheric air, saturated with vapour, is such wherein the vapour, considered abstractedly from the air in which it is diffused, is at its utmost density for the temperature; in such case, if a quantity of atmospheric air and vapour be taken, and mechanically condensed, a portion of the vapour will be condensed into water, and give off heat; on the contrary, if it be expanded, or, which amounts to the same thing, if a quantity be taken out of a receiver, the remainder will have its capacity for vapour increased, as has been already observed.

Though the pressure of the air does not promote the condensation of vapour, yet when the pressure is removed, evaporation is promoted; for under the receiver of an air-pump we find

that

that the vapour from the wet leather rifes as faſt as it can be pumped out, when the rarefaction has proceeded to a certain degree.

In order the more to illuſtrate and confirm the notion of vapour here laid down, we ſhall now attempt to explain ſeveral facts, which have been conſidered as involving difficulties, and we believe ſome of them have never been accounted for by others.

Dr. *Alexander*, in his *Experimental Eſſays*, page 102, informs us, that from ſome experiments he was induced to think, that blowing upon the bulb of a thermometer with a pair of hand-bellows would cool it, but upon trial found it was always heated 1 or more degrees by the operation.—Now, if a thermometer that has juſt been dipped in water of the ſame temperature as the air, be blown with a pair of hand-bellows as above, it will be cooled ſeveral degrees. Theſe two facts I have proved frequently, from expement.—Again, Dr. *Darwin* (ſee the note, page 136) found that air having been for ſome time condenſed, upon ruſhing out againſt the bulb of a thermometer, cooled it ſeveral degrees, and a dew was depoſited upon the bulb.

The reaſon of theſe apparently diſcordant facts may be explained thus: the condenſation of vapour in a pair of hand-bellows will precipitate a portion of the infuſed vapour, which gives off its heat to the air; and thus the temperature of the air in the bellows being increafed, that of the thermometer, expofed to the current, will be increafed accordingly. In the fecond inftance, the water on the bulb of the thermometer being expofed to the current of air, quickly evaporates, and at the fame time abforbs the neceffary heat from the quickfilver. But in the third inftance the heat confequent to the condenfation was fuffered to efcape, whilft the condenfed vapour or water remained in the air-gun; the air rufhing out was therefore of the fame temperature as the furrounding air, and probably a great portion of the condenfed vapour remained mechanically mixed therewith; a depofition of water upon the bulb of the thermometer was of courfe unavoidable, and this being refolved into vapour by its expofition, reduced the temperature of the thermometer.

In the Philofophical Tranfactions for 1777, there is a very interefting feries of experiments fhewing the effects of vapour in the receiver of an air-pump, when the air is exhaufted; the experiments were made by *Edward Nairne*, F. R. S. upon a pump on Mr. *Smeaton*'s conftruction. He ufed two gages, one of which was the common barometer gage, which was of courfe an accurate meafure of the force or elafticity of the medium of air or vapour within the receiver; the other, called the *pear gage*, from its fhape, confifted of a glafs tube, capacious in the middle, and ending in a narrow neck, which was cloſe; the other, or open end, was, by a contrivance for the purpoſe, let into a baſon of mercury before the air from without was ſuffered to enter, and upon its admiſſion the quickſilver was forced into the gage; the ſpace occupied by the air being then compared with the whole capacity of the gage, gave the rarefaction of the permanent elaſtic fluid or air.—The chief facts obſerved were the following,

1. When the pump-plate leather was ſoaked in water, and the barrel of the pump well cleared of moiſture, then, after working the pump for 10 minutes, the rarefaction indicated by the pear gage was very great, and exceeded what was obſerved in any other circumſtance, whilſt that indicated by the barometer gage was often not $\frac{1}{200}$th part as great as the other; alſo, it was obſerved that the rarefaction by the pear gage was *leſs* every time the experiment was repeated, but that of the barometer gage was always the *ſame* at the ſame time.

2. When the pump-plate leather was ſoaked in water mixed with ſpirit of wine, the rarefaction by both gages was leſs than in the former caſe; but the reſults in other reſpects were ſimilar.

3. The effects of different temperatures of the air upon the rarefaction were as follow:

Pump-plate leather being ſoaked in water.

Air in the room 46°—barometer gage 84—pear gage 20000.
———— 57 ———— 56 ———— 16000.

Pump-plate

Pump-plate leather being soaked in water mixed with spirit of wine.

Air in the room 46°—barometer gage 76—pear gage 8000.
——— 57 ——— 49 ——— 1200.

4. When the leathers of the *piston* were soaked in water, the two gages nearly corresponded; but the utmost rarefaction in this circumstance was very small, being, for instance,

In one pump — barometer gage 37——pear gage 38.
In another pump ——— 34 ——— 37.

5. When the pump, &c. were dry, the barometer gage was sometimes lower after working the pump 5 minutes, than after the operation was continued 5 minutes longer.

6. When the pump and plate were both dry, and the receiver cemented on to the pump-plate, the two gages nearly agreed, the rarefaction by both being about 600, in *damp* weather; but in *dry* weather, and in a still greater degree when a quantity of vitriolic acid was in the receiver, (which was always found to gain weight by such its exposure) the barometer gage indicated a greater rarefaction than the pear gage.

These facts, some of which the ingenious artist who made the experiments accounted for, seem most or all of them capable of a satisfactory explanation upon the theory of vapour we are here maintaining.—When the pump-plate leather is soaked in any liquid, and the pressure is so far diminished that the liquid boils, or turns into vapour, it is plain the pressure can be no further diminished;

diminished; and in such case, when the pump is wrought, it must draw each time a portion of the remaining air along with the vapour, and thus the air in the receiver admits of a diminution almost *ad infinitum*, and vapour generated instantaneously supplies the place of the air withdrawn; when air is let in, the vapour in the pear gage is condensed, and there remains nothing but the small portion of air, with its saturating portion of vapour, at the top of the gage.—The reason why the repetition of the experiment decreased the rarefaction by the pear gage, was, that the frequent condensations of air and vapour in the barrel of the pump must have produced a deposition of water there, by which the effect was sooner at its *ne plus ultra;* for, when the vacuum of the barrel is not perfect, the quantity drawn from the receiver in a given time must be less than otherwise. I have no doubt if the experiments had been repeated often enough, the leather of the piston and the valves would have been in effect soaked with water, and the result as stated in the 4th fact: in this case, as soon as the spring of the air in the receiver is weakened to a certain degree, working the pump does not avail, because the vapour in the barrel, together with the resistance of the valves, is just sufficient to counteract the spring of the air within; hence the rarefaction by the pear gage is then scarcely greater than by the barometer gage.

Experience proves that spirit of wine rises sooner into vapour than water; consequently the rarefaction

rarefaction by the pear gage, when the pump is wrought a given time, must be less than when water is used. Also, it follows *a priori*, that the cooler the circumambient air, other circumstances being the same, the greater must the rarefaction be by both gages.

When by long pumping a quantity of vapour is collected in the barrel of the pump, I conceive a portion of it may, during the operation, escape again into the receiver, this will account for the 5th fact.

I do not see how the 6th fact can be explained without supposing that the elasticity of dry air, when greatly expanded, decreases in a greater proportion than its density; it is true that the increase of cold in the receiver, and the less vapour there is in the air at first, the more will the rarefaction indicated by the barometer gage exceed that of the pear gage; for, it cannot be reasonably supposed that when the rarefaction is at its utmost degree, the proportion of vapour to air in the receiver is no greater than at first; I conceive, therefore, that the air condensed in the pear gage is always saturated with vapour, unless perhaps when the vitriolic acid is in the receiver, and of course its bulk, *cæteris paribus*, greater than before: but this alone is not sufficient to account for the observed differences of the gages.

THE END.