Edward W. Wakefield, Esq.

with respects from

The Author

METEOROLOGICAL

OBSERVATIONS

AND

ESSAYS.

BY

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TO

THOMAS HOYLE, ESQ.

OF SPRING COTTAGE, NEAR BURNLEY,

AS A TESTIMONY OF FRIENDSHIP

FOR FORTY YEARS,

AND FOR THE FACILITIES AFFORDED DURING THAT PERIOD

AT MANCHESTER

FOR THE PROSECUTION OF HIS METEOROLOGICAL OBSERVATIONS,

This Second Edition

IS RESPECTFULLY INSCRIBED BY

THE AUTHOR.

PREFACE

TO THE FIRST EDITION, 1793.

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, 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 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 Transactions for 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 atmosphere, I cannot say it will be found entirely so to others. It may be proper to observe, that I had not adopted the theory of vapour which is maintained in the sixth essay, when the third was printed; but I know of no material alteration I would have made in this essay, had it been otherwise.

The fourth and fifth essays are chiefly selected from the publications of others, except that in the latter I have offered some new thoughts on the effect of the situation of countries upon their temperature.

In the sixth essay, amongst other things I have advanced a theory of the state of vapour in the atmosphere, which, as far as I can discover, is entirely new, and will be found, I believe, to solve all the phenomena of vapour we are acquainted with ; I have attempted to solve several, particularly in the appendix.

In the seventh essay the relation betwixt the barometer and rain is investigated, from the observations in the first part : some conclusions are thence obtained in support of theory, and from which several useful and practical observations may be deduced.

The eighth essay is the large one on the *aurora borealis,* which I have divided into six 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 149, 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, 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 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.

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* 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.

I CANNOT help observing here, that the following fact appears to be one of the most remarkable 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 shown, 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 anybody has afterwards noticed it, except AMANUENSIS (see page 149). 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*st*, 1793.

PREFACE

TO THE SECOND EDITION.

Forty-one years have nearly elapsed since these Observations and Essays were first published.—They have long been out of print, and often been eagerly inquired after. Subsequently, a few copies which the author had retained, were gradually dispersed, by solicitations from various quarters, till they were reduced to two or three copies. These circumstances, joined to the consideration that some of the subjects here treated of appeared to the author to be very imperfectly appreciated, or little understood, by some of the modern writers on meteorology, have been the cause of this second edition. It may be expected that such a lengthened series of observations as I have made since, must have afforded abundance of important matter to be added to the work. I have not found this to be the case. This second edition is printed *verbatim* from the first, both text and notes; and I have only added a few notes at the end, under the head of *Appendix to the second edition;* and some observations on clouds, on thunder, and on meteors, particularly the *aurora borealis*, of the appearances of which I have collected a list for the last forty years, in addition to those I had previously obtained when resident at Kendal. I have been the more anxious to preserve the first edition unchanged, as I apprehend it contains the germs of most of the ideas which I have since expanded more at large in different Essays, and which have been considered as discoveries of some importance. For instance, the idea that steam or the vapour of water is an independent elastic fluid, so largely insisted upon in the sixth Essay; and hence that all elastic fluids, whether alone or mixed, exist independently. The great principles of motion in the atmosphere, the theory of winds, their effects on the barometer, and their influence in the production and regulation of temperature and of rain, are of primary consideration in every department of meteorology. These were pretty fully developed in the second, and succeeding Essays. The first part of the work, or the observations, present nothing but what may be met with in numerous meteorological journals both before and since this publication, if we except the observations on the height of clouds, and a few of those on the aurora borealis. It is the second part of the work, or the Essays, that I consider of greater importance. The notions which I entertained of the aurora borealis will be perused with some interest, notwithstanding the lapse of forty years, and the numerous discoveries in electricity, galvanism and magnetism made since.

At the conclusion of my former preface, I alluded to a person who had laid me under great obligations. That gentleman being now no more, I can speak of him without reserve. It was the late John Gough, Esq. of Kendal.

Mr. Gough might justly be deemed a prodigy in scientific attainments, considering the disadvantages under which he laboured. Deprived of sight in infancy (one or two years old) by the small pox, he was destined to live to an advanced age, under this, which is commonly reputed one of the greatest misfortunes that can fall to the lot of man. In his case, however, it may fairly be questioned whether he would have had more enjoyment in himself, and have been of more use to society in the capacity of a manufacturer, his probable destination, than in that which was allotted to him. By the liberality of his father, he had the benefit of a good classical and mathematical education; and, naturally possessing great powers of mind, he bent them chiefly to the study of the physical and mechanical sciences. There are few branches of science in which he did not either excel, or of which he had not a competent knowledge; astronomy, optics, pneumatics, chemistry, natural history in general, and botany in particular, may be mentioned. For about eight years during my residence in Kendal, we were intimately acquainted; Mr. Gough was as much gratified with imparting his stores of science, as I was in receiving them: my use to him was chiefly in reading, writing, and making calculations and diagrams; and in participating with him in the pleasure resulting from successful investigations: but, as Mr. Gough was above receiving any pecuniary recompense, the balance of advantage was greatly in my favour; and I am glad of having this opportunity of acknowledging it. It was he who first set the example of keeping a meteorological journal at Kendal.

Manchester, June 20th, 1834.

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METEOROLOGICAL

OBSERVATIONS AND ESSAYS.

PART I.

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 basin 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 the surface of that in the basin; 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, 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 basin. 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 enclosed 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 quicksilver 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 inquiries 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 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 inventor 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 generally low in windy and rainy weather, and high in serene and settled weather, which circumstance procured the instrument the name of *weather-glass.* This discovery promised to be of the utmost importance to mankind, by enabling them to foresee those changes in the atmosphere, the knowledge of which was so interesting to them; and the most sanguine expectations were entertained on the subject. The experience of a century and a half has now been obtained, from which the barometer does not seem to be that infallible guide that it was once expected to be, though it is certainly a very useful instrument in this respect, in the hands of a judicious and skilful observer.—But of this more hereafter.

Several ingenious contrivances have been used, by different persons, to make barometers of a more ample range, in order to observe minute alterations of weight in the atmosphere; but all these are liable to such objections as render the common upright one preferable.

Those who wish to make barometrical observations, in order to compare them with others, should be well assured of the accuracy of their instruments;—such as incline to make their instruments themselves would do well to attend to the following particulars :—

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

That the quicksilver be strained through a cloth, or rather through leather.

That the inside of the tube be perfectly dry, and the quicksilver dry when put into it.

If there be any moisture in the tube or quicksilver, it expands into an elastic vapour when the pressure of the air is removed, and, ascending into the *vacuum*, depresses the mercurial column sometimes to the amount of one-quarter of an inch, or more, below its proper station. The criterion to discover moisture is to apply the warm hand to the *vacuum*, and the mercury will sink considerably; but it will not be affected if clear of moisture.—Also, if upon a gentle agitation of the barometer in the dark, there appear a light in the *vacuum*, it is a sign there is little or no moisture. If, upon a gentle inclination, the quicksilver rise to the top of the tube, and completely fill it, leaving no speck, it is clear of air.

The scale in strictness ought not to be full inches, but something less, owing to the rising and falling of the surface of the reservoir. If the tube have a bulb, then the area of the surface at the top of the column, divided by the sum of the 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

* *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.

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 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 $\frac{1}{50}$ 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

† 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.

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.

	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 ann. mean			29.85 ann. mean			29.79 ann. 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	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	29.51	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 ann. mean			29.69 ann. mean			29.58 ann. mean		

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 & 30nt	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
Sep.	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	30.42	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	30.26	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 ann. mean			29.87 ann. mean			29.83 ann. mean		

		London		Kendal		Keswick	
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
January	29.33	30.22	28.40	29.23	30.17	28.31
February	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
October	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		

		Kendal		Keswick	
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
March	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
April	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
August	H	20 m	NE 0	19 nt, 20 m	calm
	L	28 m	W 1	28 m	W 0
Sept.	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.			KESWICK.		
	Mean	highest	lowest	Mean	highest	lowest
January	29.60	30.37	28.87	29.53	30.34	28.65
February	29.87	30.47	29.34	29.82	30.43	29.25
March	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
October	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		

		Kendal		Keswick	
Jan.	H	5 all day	calm	5 all day	calm
	L	16 m	SW 0	15 nt	S 1
Feb.	H	16 nt, 17 m	NE 1	17 m and n	NE 0
	L	1 m	SW 1	1 m	SW 0
March	H	12 m and n	NE 1	12 m	NE 0
	L	4 n	SW 0	4 n	W 0
April	H	29 m	SW 0	29 m and n	calm
	L	4 nt	SW 1	4 nt	calm
May	H	5 nt, 6 m	NE 1	5 nt,6m	N 2, NE 1
	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
	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
	L	27 m and n	SW 1	27 m	calm
August	H	1 m&n 29 all d.	NE 0 & 1	1 n, 29 n	NE 0
	L	23 m	SW 1	23 m	SW 1
Sept.	H	16 m	SW 0	15 nt	SW 0
	L	22 m	SW 0	21 nt	SW 0
Oct.	H	24 nt	NE 0	24 nt	N 1
	L	14 n, 15 m	SW 1	14 n and nt	S 1
Nov.	H	24 nt	NE 0	24 nt	calm
	L	14 n and nt	SE 2	14 n and nt	SE 1
Dec.	H	2 nt	calm	2 nt	calm
	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	28.93	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 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, January 1.73 inches, February 1.33, March 0.96, April 0.99, May 0.70, June 0.83, July 0.65, August 0.79, September 1.02, October 0.99, November 1.34, December 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 instrument in meteorology is the thermometer: by which the temperature, or degree of heat, of the air and other bodies, is determined. An instrument under this character was invented prior to the barometer, but never brought to a tolerable degree of perfection till the present century.

Philosophers are generally persuaded, that the sensations of *heat* and *cold* are occasioned by the presence or absence, in degree, of a certain principle or quality denominated *fire* or *heat*:—thus, when any substance feels cold, it is concluded the principle of heat is not so abundant in that substance as in the hand; and if it feel hot, then more abundant. It is most probable, that all substances whatever contain more or less of this principle. Respecting the nature of the principle, however, there is a diversity of sentiment: some supposing it a *substance*, others a *quality*, or property of substance. Boerhaave, followed by most of the moderns, is of the former opinion; Newton, with some others, are of the latter; these conceive heat to consist in an internal vibratory motion of the particles of bodies.

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 constant and uniform temperature at all times and places, provided the barometer be at a certain height; and a mixture of pounded ice, or snow, and water is likewise of a uniform temperature. Hence, we are favoured with the means of finding two sufficiently distant points upon the thermometric scale, without the necessity of another thermometer ; these are called the *boiling* and *freezing points*, and are marked with 212° and 32° respectively, upon the common scale, or that of Fahrenheit, the boiling point being found when the barometer stands at 30 inches. The scale is divided into equal parts, and extended above and below these points, *ad libitum ;* when the degrees go below 0°, they are counted from it, and termed *negative* merely for distinction.* At 55° the word *temperate* is usually placed upon the scale, and *summer heat* at 75° ; 98° denotes the usual heat of the human blood ; 112° the heat of the blood sometimes in an inflammatory fever ; and at 175° spirits boil : quicksilver itself boils at about 600°.

Reaumer's scale, used by some philosophers on the continent, marks the freezing point with 0°, and the boiling point with 80°.

The following is the result of observations on the thermometer, taken three times a day, at *Kendal* and *Keswick*, from 1788 to 1792, inclusive. The morning observations were taken between 6 and 8 o'clock; the mid-day observations about 12 or 1 ; the night observations at *Kendal* about 9 or 10, but at *Keswick* at 6 in summer, and 4 in winter; this circumstance makes the

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

mean temperature of *Keswick* too high when compared with that of *Kendal*, which ought to be noticed in the comparison.

The situation of the thermometers too, is another particular that should be adverted to;—that at *Kendal* was without, in a garden, under the shade of a pretty large gooseberry tree, facing the north: the garden is open to the country in the north, and has houses at the distance of 8 or 10 yards to the south. The thermometer at *Keswick* is situate near, but not in contact with, the wall and window of a house facing the north, which is open to the country: it is about 6 yards above the ground; the sun never shines on it in winter, and only a few weeks in summer, and that early in the morning, long before the observation is taken.

From these accounts it is obvious to infer, that the thermometer at *Keswick* 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, occasionally, owing to the reflection from the ground, though the sun never shines upon the frame for an hour at least before any observation is taken.

The following tables, it is presumed, will be sufficiently 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 KENDAL, 1788.

	MORNING.			NOON.			NIGHT.			month. means.
	Mean	high.	low.	Mean	high.	low.	Mean	high.	low.	
January	37.7°	46°	20°	41°	47°	31°	38.3°	46°	24°	39°
February	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
August	53.5	58	47	63.7	74	57	54.2	60	49	57.1
September	49.5	60	35	59.4	70	50	51.8	62	43	53.6
October	41.5	55	28	52.6	58	47	43.1	57	30	45.7
November	38.3	50	27	44.5	52	33	39.3	52	28	40.7
December	26.0	40	10	33.5	46	23	27.6	40	18	29
Ann. mean	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. means.
	Mean	high.	low.	Mean	high.	low.	Mean	high.	low.	
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
August	58.5	62	52	63.2	75	56	61.6	72	53	61.1
September	54.6	64	46	58.3	68	48	55.9	66	48	56.3
October	44.4	57	28	50.5	59	41	46.8	57	34	47.2
November	42.1	52	28	44.2	52	32	42.5	54	28	42.9
December	26.2	43	8	30.2	44	18	28.5	41	17	28.3

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.	
January	30.8°	47°	4°	36°	50°	19°	32°	47°	5°	33°
February	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
September	49.8	57	32	58.4	67	50	49.2	58	37	52.5
October	42.4	55	27	51	57	39	43.4	55	29	45.6
November	35	44	20	42.5	51	31	35.7	45	23	37.7
December	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. means.
	Mean	high.	low.	Mean	high.	low.	Mean	high.	low.	
January	33°	49°	7°	34.9°	50°	20°	33.8°	50°	16°	33.9°
February	37.7	46	29	40.4	48	34	38.7	47	30	38.9
March	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
September	52.7	61	41	56.4	65	46	54.8	64	46	54.6
October	44.2	52	26	48.9	56	36	46.9	57	32	46.7
November	37	45	23	41.2	48	31	39.1	48	28	39.1
December	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. means.
	Mean	high.	low.	Mean	high.	low.	Mean	high.	low.	
January	36.3°	47°	22°	40°	49°	31°	37.2°	47°	24°	37.8°
February	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
August	52.5	60	38	61.4	71	56	55	63	48	56.3
September	47	56	33	56.7	68	51	49.1	52	42	50.9
October	43.3	55	28	54.3	65	47	45.4	58	33	47.6
November	37.8	50	24	44.1	53	34	37.6	48	21	39.8
December	34.3	46	6	39	49	22	34.9	47	22	36.1
Ann. mean	43.0			52.1			44.2			46.4

At KESWICK, 1790.

	MORNING.			NOON.			NIGHT.			month. means.
	Mean	high.	low.	Mean	high.	low.	Mean	high.	low.	
January	36.4°	48°	29°	39.7°	50°	30°	38.7°	50°	32°	38.6°
February	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
August	54.8	61	51	60.5	69	55	57.5	64	51	57.6
September	49.2	56	41	53.7	62	43	50.8	62	43	51.2
October	46.5	59	35	52.1	62	44	48.8	59	37	49.1
November	37.6	49	25	41.6	50	30	39.9	50	26	39.7
December	35.7	47	18	37.9	49	27	36.8	47	28	36.8
Ann. mean	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. means.
	Mean	high.	low.	Mean	high.	low.	Mean	high.	low.	
January	38.3°	48°	23°	40.4°	48°	32°	38.8°	48°	30°	39.2°
February	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
September	50.3	60	38	63.4	79	52	51.5	60	42	55.4
October	43	57	24	51.8	60	42	43.9	57	24	46.2
November	39.4	49	22	45.2	53	39	39.3	50	28	41.3
December	29	40	10	33	42	20	30.2	46	-10*	30.7
Ann. ms.	43.6			52.0			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. means.
	Mean	high.	low.	Mean	high.	low.	Mean	high.	low.	
January	37.9°	45°	24°	39.3°	49°	31°	39.2°	49°	31°	38.8°
February	35.8	47	25	38.4	47	30	36.7	47	30	37
March	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
August	55.6	64	48	60.9	68	47	58.2	67	45	58.2
September	53.2	66	41	59.1	73	49	56.6	69	46	56.3
October	43.5	55	27	47.7	61	36	46.4	58	32	45.9
November	39.1	47	22	42.3	51	33	40.6	51	32	40.7
December	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.	*Keswick.*	*Kendal.*	*Keswick.*	*Kendal.*	*Keswick.*
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. 6°.

At KENDAL, 1792.

	MORNING.			NOON.			NIGHT.			month. means.
	Mean	high.	low.	Mean	high.	low.	Mean	high.	low.	
January	32.3°	46°	11°	37.5°	48°	26°	32.5°	49°	12°	34.1°
February	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
August	55.6	66	48	69	83	58	56.1	66	48	60.2
September	47.6	59	34	56.8	69	46	48.5	59	36	51
October	43.4	58	28	51.4	63	46	44	56	35	46 3
November	41.6	50	24	47.1	58	34	42	51	30	43.6
December	36.9	50	21	40.3	51	29	37.7	52	24	38.3
Ann. ms.	44.3			52.3			44.3			47.0

At KESWICK, 1792.

	MORNING.			NOON.			NIGHT.			month. means.
	Mean	high.	low.	Mean	high.	low.	Mean	high.	low.	
January	32.1°	45°	12°	35°	47°	20°	33.4°	47°	17°	33.5°
February	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
August	60	71	47	65.2	75	54	59.9	70	48	61.7
September	48.9	59	38	53.9	64	43	51.2	60	42	51.3
October	44.9	57	32	48.8	64	41	46.6	57	37	46.7
November	42.2	55	23	45.7	60	32	44.2	57	33	44
December	36.4	51	20	38.7	51	23	38	49	24	37.7
Ann. ms.	45.0			49.4			46.6			47.0

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 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 THIRD.

Of the Hygrometer.

The hygrometer is an instrument meant to show 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. Sponge, paper, &c. imbibe moisture, and become alternately lighter and heavier by being exposed to the air. Strings, whether made of animal or vegetable fibres, twist and untwist by the moisture and dryness of the air, and consequently are shortened and lengthened alternately.—The force with which a cord contracts is amazingly great. Mr. Boyle, who seems to have been the first that made a series of experiments of this sort, used to suspend a weight of 50 or 100lbs. to the end of a rope, which was alternately raised and lowered by the moisture and dryness of the air, as a small weight would have been.

OBSERVATIONS ON THE HYGROMETER.

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

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
Ann. 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 show a variation in the dryness or moisture of the air; but it is very inadequate to the purpose for which an 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; 2nd, the area of the aperture of the funnel; and, 3rd, 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, $\frac{1728w}{aW} =$ 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	7.2702	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.			
	AT KENDAL.		AT KESWICK.		AT KENDAL.		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
Sept.	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 monthly rain and number of wet days, at Kendal *and* Keswick, *for all the five years.*

	AT KENDAL.		AT KESWICK.	
	Inches of rain.	wet days.	Inches of rain.	wet days.
January	6.403	20	7.3558	22
February	5.671	18	6.1624	22
March	3.219	16	3.7324	18
April	4.909	16	5.0887	20
May	3.825	16	3.9434	18
June	3.549	14	4.1167	18
July	6.717	23	5.9948	24
August	4.681	18	5.2484	18
September	6.286	19	7.2387	21
October	5.179	18	5.9675	20
November	5.267	17	5.7989	18
December	8.800	22	7.9018	22
Total	64.506	217	68.5495	241

The greatest quantity of rain in 24 hours, for these 5 years, was on the 22nd of April, 1792, namely, at *Kendal,* 4.592 inches. The rain at *Keswick* on that day, was something less; but taking both the 22nd and 23rd, the rain was nearly equal at both places.

Besides these 2, there were other 2 days, at *Kendal,* when the rain was betwixt 2 and 3 inches, and 56 days betwixt 1 and 2 inches.

At *Keswick,* for 4 years and 9 months, there were 3 days, besides the 2 above mentioned, when the rain was between 2 and 3 inches, and 52 days between 1 and 2 inches.

SECTION FIFTH.

Observations on the height of the Clouds.

Mr. Crosthwaite, of *Keswick*, has availed himself of his situation in the vicinity of high mountains, to make observations on the height of the clouds; for which purpose he has chosen *Skiddaw*, the highest mountain in the neighbourhood, a very fine view of which his *museum* commands. By means of marks on the side of the mountain, and with the assistance of a telescope, he can discern, to a few yards, the height of the clouds, when they are below the summit, which is very often the case.—Perhaps the following series of observations is the only one of the kind extant, as the labour and difficulty attending such observations in a champaign, or flat country, are sufficient to deter any one from making two or three daily observations for a series of years; and when the whole sky is clouded, they are quite impracticable.

He has determined, by trigonometry, the perpendicular height of *Skiddaw*, above the level of *Derwent lake*, to be **1050** yards, which agrees very nearly with Mr. Donald's observations; and he has noted, in a column of his meteorological journal, every morning, noon, and evening, the height of the clouds, in yards, above the level of the said lake, when their height did not exceed that of *Skiddaw;* and when it did, he has marked it as such.

The result of 5 years' observations is contained in the following table. All the observations when the clouds were between 0 and 100 yards high are placed in one column, and those when they were between 100 and 200 yards high in the next column, &c.—In order to determine what effect the seasons of the year have upon the clouds, in this respect, we have kept the observations in the several months distinct.—It is to be noted, that the column containing the number of observations when the clouds were above *Skiddaw*, includes those observations when there were no clouds visible; but Mr. Crosthwaite has noted this last circumstance also, in the journal, and it appears, that about 1 observation in 30, of those in that column, should be deducted on that account.

	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 observations.
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
March	2	1	6	11	22	40	32	36	24	32	44	184	434
April	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
Sept.	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	46	35	111	460
Total	10	42	62	179	374	486	416	367	410	518	419	2098	5381

It may be proper to observe, that the supposition of the clouds rising or falling with the barometer, or as the density of the air increases or diminishes, is not at all countenanced by these observations.—Also, that in very heavy and continued rains, the clouds are mostly below the summit of the mountain; but it frequently rains when they are entirely above it.*

* See Notes (A), (B), (C), &c. in the *Appendix to the second edition.*

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 3½ P. M. some loud peals of thunder, at Kendal.

May 13.—From 6½ 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 ¾ 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 2½ 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 8½ 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 the E. at Keswick.

May 17.—At 11½ 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.

1790.

June 16.—Distant thunder in the evening, at Kendal.*—At 8½ 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. [B]

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 Keswick.

—— 18.—After 2 P. M. several claps, at Kendal; one of which not unlike that of the 4th of June. At Keswick, 2 claps A. M. and 3 P. M. with excessively heavy showers.

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* 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.

August 15.—Between 8 and 9 P. M. there was the most lightning I ever remember to have seen at one time, at Kendal; some thunder was heard, but it was distant, E.

August 16.—From 5½ to past 7 A. M. much thunder at both places, and heavy rain at Keswick.

October 20.—At 8½ A. M. one loud clap of thunder, at Keswick, and much lightning from 7 to 10 P. M.—heavy rain all day.

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

1792.

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

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

July 9.—At 7 P. M. distant thunder, at Kendal.

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

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

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

August 26.—At 3 P. M. some thunder, at Kendal.

October 14.—In the evening, lightning; and at 10, distant thunder, at Kendal. From 6 to 11 the same evening, lightning, at Keswick; and at the later hour, one long and loud crack of thunder.

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. January 18. March 9. April 26. October 1. November 14. December 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. August 3. Dec. 11 & 15.	1790. Feb. 16. April 11 & 14. July 31. Sep. 2 & 3. Dec. 11 & 13.
1791. January 5, 7, 11 & 13. February 1 & 11. March 21. May 21, 22 & 23. June 4 & 12.	1791. Jan. 2, 4 & 5. Feb. 11, 15 & 18. March 21. May 18, 20, 22, 23 & 25. June 12 & 21. July 5. Oct. 8 & 24. Nov. 5, 16 & 28.
1792. March 19. May 22. October 17. December 6 & 22.	1792. Mar. 7. May 1 & 2. June 30. September 20 & 21. October 18. November 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 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 follow Tables containing the number of observations on the winds each year, in all the different directions, at both places.

WINDS AT KENDAL.

Years.	N.	NE.	E.	SE.	S.	SW.	W.	NW.	Number of Observations
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.	Number of Observations
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 these tables we shall subjoin an account of those days on which the highest winds prevailed, at one or both places.

Highest winds, marked 4, *at* Kendal *and* Keswick,

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 & 21. Oct. 12, Dec. 15 and 23.

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. October 20. November 9, 11, 12, 19, 26 and 27. December 1, 13 and 25.

1792.

February 2. March 18. April 2, 15, 22 and 23. September 10. October 1, 2, 3 and 31. November 18, 19, 20 and21. December 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 circumstance, 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.

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

1788.

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 $8\frac{1}{2}$ 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

1791.

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 NINTH.

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. Crosthwaite has been pretty assiduous in procuring intelligence respecting these phenomena, and in observing any circumstance that might lead to a discovery of their cause; but nothing has occurred yet that promises to throw light on the subject.

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

The following is an account of the times and circumstances of the several observations.

1789.

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

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

August 27.—At 9 A. M. a small 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 strong 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 TENTH.

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 ten o'clock. The third column contains the moon's age at the time, or the number of complete days betwixt the *change* 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 seven 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.	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, still
Oct. 4	10	22	7	still
—— 6	10	24	9	active, faint
—— 7	10	25	10	active, large
——17	11	6		active (*a*)
——19	9	8		still
Nov. 4	9	24	9	large, bright
—— 8	8	28	13	large, bright (*b*)
——28	9	19	3	still, small
——29	9	20	4	still, small
——30	9	21	5	still, small
				Number 27
1788.				
Jan. 9	6	1		still, low
——10	8	2		still, large 2
——11	8	3		still, faint

1788.	Hour P.M.	☽'s age.	☽ past full.	Character.
Jan. 13	8	5		transient
——14	8	6		large, still
——15	9	7		large, still
Feb. 4	9	27	12	still
—— 6		29	14	active, high 1
—— 7		0		small 1
—— 8		1		faint, still
——12		5		active, small 1
Mar. 7	11	0		active, small 2
—— 8		1		active, small
——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	still, low
——28	10	22	8	active, high 2
——29	10	23	9	large, active
——30	10	24	10	still
May 1	10	25	11	a glance, clouds
—— 4	10	28	14	transient
——10	10	4		high. D
——11	10	5		large, still
——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		small
——19	10	18	3	large, still 2

(*a*) A heavy shower, with thunder, just before.

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

(*c*) From 10 to 11 P. M. uncommonly brilliant, active streamers over most of the hemisphere: they were said to be heard.—Not much inferior the next night.

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

1788.	Hour P. M.	☽'s age.	☽ past full.	Character.	1789.	Hour P. M.	☽'s age.	☽ past full.	Character.
Aug. 23		22	7	very grand (*a*) 2	Mar. 16		19	5	still, small
——29	10	28	13	active	——29		3		a glance, clouds
Sep. 2		2		still	——30		4		a glance, clouds
—— 6		6		a glance, clouds	Apr. 12		17	2	still
——10		10		faint, still	——13		18	3	still 2
Oct. 12	4M	12		still	——30		5		still
——21	10	22	6	still	June 12	10	19	5	active
——24		25	9	still	Aug. 13		22	8	active, high 1
——27		28	12	still 1	——14	10	23	9	active, high
——30	9	1		still	——15	9	24	10	still
——31	9	2		large, still	——16	10	25	11	still
Nov. 1		3		still	——17	10	26	12	still
——19	9	21	6	still	——18	10	27	13	active
——27		0		faint, still	——19	10	28	14	active, high
——28		1		bright, still	——20	10	0		active
——30	7	3		still	——25		5		active 1
Dec. 21		24	8	still 2	Sep. 14	10	24	10	still (*d*)
——24		27	11	active	——15	10	25	11	still
					——20	10	1		fine, active 2
				Number 53.	——23		4		fine, clouds
					——26		7		grand (*e*) 2
1789.					Oct. 18		0		active
Jan. 11		15	0	still	——19	11	1		active
Feb. 15		20	5	active, small 1	——20		2		grand (*f*)
——23	11	28	13	active	——23		5		large, still 2
——26	10	1		large, active	——25		7		still
——28		3		large, bright (*b*)	——27		9		still
Mar. 14		17	3	very grand (*c*) 2	——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 23rd 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.

	Hour P.M.	☽'s age.	☽ past full.	Character.
1789.				
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
Sept. 7	9½	28	14	still
Oct. 9		1		still, faint
——18	7½	10		still 1

	Hour P.M.	☽'s age.	☽ past full.	Character.
1790.				
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.

1791.	Hour P.M.	☽'s age.	☽ past full.	Character.
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

1792.	Hour P.M.	☽'s age.	☽ past full.	Character.
June 30	1M	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 13th,* 1792.

In making observations upon any phenomenon in nature, with a view to ascertain its cause, every particular circumstance should

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

be attended to; for, though many may be 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 10th, 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 13*th of October,* 1792, *will be given after the next Section.*

SECTION ELEVENTH.

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 distinguished 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 an oblong bar of tempered steel (it will answer well if five 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 the 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 invented about the beginning of the fourteenth 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 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 22¼° 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 13¼′. He moreover observed, that the needle was disturbed when an *Aurora borealis* was in the atmosphere.

I have myself made a like series of observations for some months, and find them in general to agree with his; but as it is not necessary for my purpose to relate the result of them, any further than what is contained in the subsequent pages, I shall not detain the reader longer on the subject.

ADDENDA

TO THE OBSERVATIONS 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 *south* were remarkably red, and afforded sufficient light to read with, though there was no moon, nor light in the north. The unusual appearance raised my curiosity, and I waited with impatience to see the clouds carried off to the SE. (for the wind was W. or NW. and pretty fresh). In the mean time, having by me a very good *theodolite,* made by Dollond, I took it out to make observations on the bearing, altitude, &c. of any remarkable appearance.

From 9½ to 10 P. M. there was a large, luminous horizontal arch to the southward, almost exactly like those we see in the north; and there was one or more faint, concentric arches northward.—It was particularly noticed, that all the arches seemed exactly bisected by the plane of the magnetic meridian. At half past 10 o'clock, streamers appeared very low in the SE. running to and fro from W. to E. they increased in number, and began to approach the 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, affording 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) 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 track 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. After 9, fine streamers struck out, and ran to and fro a while across the said meridian, and then were converted into flashes, as usual; some rose up to the zenith. The point of convergency, and every other particular, were, to all appearances, the same as have been described before.

The needle was considerably 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 circumstances relating to it follow, supposing the variation of the needle at the noon of that day 25° W.

H. M.	Variation of the needle.	
10 0 P. M.	25° 0′ W.	the arch rising.
10 10 ——	24 54 —	bright streamers, low, with clouds.
10 30 ——	24 42 —	streamers risen; fine, westward.*

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

H. M.		Variation.	
10 35	P. M.	24° 37′ W.	a still light; clouded above.
10 45	——	24 57 —	bright, eastward; clouds above.
10 55	——	25 7 —	light equal, east and west.
11 5	——	25 7 —	bright, low; clouded above.
11 15	——	24 57 —	clouded, but bright eastward.

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

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

H. M.		Variation.	
5 0	P. M.	25° 5′ W.	
6 35	——	24 49 —	altitude of the clear space south 35°.
6 42	——	24 55 —	alt. of do. 20°; streamers bright, east.
6 50	——	25 — —	streamers bright and active all over the illuminated part.
7 2	——	25 28 —	
7 5	——	25 12 —	
7 10	——	24 40 —	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 0	——	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 0	——	24 55 —	
10 15	——	24 57 —	the light growing fainter and fainter.
10 35	——	24 40 —	
8 0	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 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 observations upon the phenomenon, by which its height may be determined with more precision.—In the mean time we shall consider it as 150 English miles.

March 5.—The *aurora* at Kendal was seen at 8 P. M.; it was a bright still light awhile, but soon clouded.—The needle was not attended to.

March 6.—At Kendal, a few fine streamers at 9 P. M. altitude 15°, and extent along the horizon 70°; exactly bisected by the magnetic meridian. It soon dwindled into a faint light. At 9h 35m brightest on the northern side.—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 8 next morning 25° 4′.—There was a brightness northward at 10 P. M. but pretty much clouded; this circumstance, with that of the needle, rendered it probable an *aurora* was in the atmosphere;—it was confirmed by the following account.

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

March 30.—At Kendal, at 8 P. M. there appeared some faint concentric arches of an *aurora;* it was not further noticed till 8h 35m.——

H. M.		Variation.	
8 35	P. M.	25° 5′ W.	a grand horizontal arch, altitude 6°.
8 40	——	25 25 —	streamers to 30° past the zenith.
8 48	——	25 5 —	bright eastward.
8 55	——	25 5 —	streamers faint.
9 0	——	25 5 —	dense light north; rare above.
9 5	——	25 10 —	ditto.
9 10	——	24 55 —	bright westward.
9 15	——	24 55 —	a fine, perfect, horizontal arch.
9 20	——	24 55 —	altitude of its upper edge 30°.
9 30	——	24 30 —	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 0	——	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 0	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 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 $13\frac{1}{2}$°, bearing NW. $\frac{1}{2}$ 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. $\frac{1}{2}$ W.; one minute after, bright streamers from NE. to WNW. the greatest altitude of their base $5\frac{1}{2}$°, the bearing of the same NW. by N. $\frac{1}{4}$ N.—From this to 10h 30m, bright streamers at intervals, low

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* 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 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.

in the NW. quarter.—After 10h 30m grown faint; horizontal extent from WNW. to NE. by N.

April 14th.—At Keswick, about midnight, or soon after, a still, horizontal light, altitude of the under edge 5°, of the upper edge $9\frac{1}{2}$; bearing of the centre NW. $\frac{1}{2}$ N.

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 as follows:—the variation was 24° 57′ at noon; 24° 35′ at 5¾ P. M.; 24° 35′ at 5h 50m; 24° 20′ at 5h 58m; 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 II.

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 of various elastic fluids, or *gases,* 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 a 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 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

* Estimate of the Temperature of different Latitudes.

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 law of decrease in density above mentioned, and greatly contracts the height of the atmosphere, as deduced from such law; though this circumstance has perhaps no effect upon the figure of the atmosphere.

Philosophers have attempted to find the height of the atmosphere by two methods; namely, by the duration of twilight, and by experiments upon the descent of the barometer on high mountains. The former determines the height about 45 miles, as follows:—the twilight disappears when the sun is

18° below the horizon; hence it is argued, that a ray of light emitted from the sun, so as to be a tangent to the earth's surface, after passing through the atmosphere, is reflected from its external surface so as to be a tangent to the earth's surface again, at 18° distance from the former place of contact. This argument being admitted, affords *data* to find the height of the atmosphere, a proper allowance for refraction being first made.—Several objections to this conclusion however may be stated; amongst others, it may be said, 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 itself; in this case, therefore, we can determine no more from the twilight, than that the height of the atmosphere, or of that 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 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 quicksilver $\frac{1}{10}$th of an inch high, when the barometer stands at 30 inches, for every 5° of temperature from 32 to 80.* For any other height of the barometer it will be in the inverse 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, corresponding to the mean height of the two thermometers; x = the height of the mountain, in feet: then, we shall have this theorem, $\frac{30ab}{A} = x$, the height required.

* 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 surface.

EXAMPLE.

Suppose 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 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:—supposing 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 process, for a larger also, sufficiently accurate for the purpose of explaining the theory of the variation of the barometer; though we cannot from this fix the

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

boundary of the atmosphere with precision. To what height the very thin and rare medium in the higher regions rises, we cannot ascertain ; but there is sufficient reason 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 considered, with reason, as having a principal share in producing changes of weather, and therefore they demand a particular regard in meteorology.

Most people know that the winds are not every where so changeable as in these 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 south latitude, the *trade winds*, as they are called, blow pretty uniformly from east to west, all the year round, with a small variation in the different seasons.

The cause of these constant winds, within the tropics, the ingenious and learned Dr. Halley has endeavoured to explain, and his explication seems to have been universally adopted by others since its publication.—The chief physical principle he uses, is the undeniable and well known one, that the air is rarefied by heat; and, as the earth, in revolving from west to east, exposes the torrid zone every day to the direct rays of 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 rotatory 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 Newton'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 rotatory 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 of the atmosphere 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 reasoning, or any other physical principle than that Dr. Halley uses; 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 causes of all winds, both regular and irregular; in comparison with which all the rest are trifling and insignificant. The trade winds in the torrid zone, and the variable winds every where else, seem to be the natural effects of these two causes, and might have been deduced from them *a priori*, if the facts had never been ascertained by the navigation of the torrid zone. Notwithstanding, as we are in possession of many facts relative to the winds, it may be proper first to state them, and then to consider how they result from the causes above mentioned.

Facts relating to the Winds.

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

2. When the sun is on the equator, the trade winds, in sailing northward, veer more and more from the east towards the north; so that about their limit they become nearly NE.: and, *vice versa*, in sailing southward, they become at last almost SE.

3. When the sun is near the tropic of cancer, the trade winds north of the equator become more nearly east than at other times, and those south of the equator more nearly south: and, *vice versa*, when the sun is near the tropic of capricorn.

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

5. The winds in the northern temperate zone are variable, but the most general are the SW. and W. and the NE. and E.—See page 47.

6. In the northern temperate and frigid zones,

and doubtless in the southern also, the winds are more tempestuous in winter than in summer.—See page 48.

Now in order to perceive the reason of these facts, it must be remembered, that the heat is at all times greatest in the torrid zone, and decreases in proceeding northward, or southward ; also, that the poles may be considered as the centres of cold at all times : hence it follows, that, abstracting from accidental circumstances, there must be a constant ascent of air over the torrid zone, as has been observed, which afterwards falls northward and southward ; whilst the colder air below is determined by a continual impulse towards the equator. And, in general, wherever the heat is greatest, there the air will ascend, and a supply of colder air will be received from the neighbouring parts.—These, 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 surface, when apparently at rest or calm, will have the same rotatory velocity as that part, or its velocity will be as the co-sine of the latitude ; but if a quantity of air in the northern hemisphere, receive an impulse in the direction of the meridian, either northward or southward, its rotatory velocity will be greater in the former case, and less in the latter, than that of the air into which it moves ; consequently, if it move northward, it will have a greater velocity eastward than the air, or surface of the earth over which it moves, and will therefore become a SW. wind, or a wind between the south and west. And, *vice versa*, if it move southward, it becomes a NE. wind. Likewise in the southern hemisphere, it will appear the winds, upon similar suppositions, will be NW. and SE. respectively.*

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

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

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 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 rotatory 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 anything 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, somewhere 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 tempestuous 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 rotatory 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 rotatory velocity of the parallels of latitude for every 10 degrees, together with their differences, agreeable to the above supposition.

Degrees of latitude	Hourly rotatory 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 extended, 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 sixty miles an hour; and a brisk gale will travel at the rate of twenty or thirty 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 economy 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 earth on its axis, the air over the torrid zone, and particularly about the equator, would have been in effect stagnant; and in the other zones the winds would have had little variation either in strength or direction; navigation, in this case, would have been greatly impeded, and a communication between the two hemispheres, by sea, rendered impracticable. On the present system of things, however, the irregularity of winds is of the happiest consequence, by being subservient to navigation; and a general circulation of air constantly takes place between the eastern and western hemispheres, as well as between the polar and equatorial regions; by reason of which, that diffusion and intermixture of the different aerial fluids, so necessary for the life, health, and prosperity of the animal and vegetable kingdoms, is accomplished:—such is the transcendent wisdom 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 satisfactory, and indeed conclusive argument for the earth's rotation on its axis; for, the trade winds blowing from east to west, we must conclude, *a posteriori,* that the earth revolves the contrary way, or from west to east.

ESSAY THIRD.

On the variation of the Barometer.

The causes of the variation of the barometer have never yet been discovered, so as to admit of demonstration; though several eminent philosophers have given the public the result of their reasoning and experience on the subject. We propose to consider the principal of their allegations; but in the first place it will be proper to lay down the chief *facts* respecting the variation, which are the result of observation, and not of any hypothesis.

Facts relating to the Barometer.

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

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

2. Within the northern temperate zone, and doubtless the southern also, the range of the barometer increases 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 greatest range, or difference between the highest and lowest observations, for the same term, was 2

* By *annual range* I mean the difference between the highest and lowest observations each year.

inches. (Vide *Martyn's Abridgment of the Parisian 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. 15 and 16.

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 15.

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 *Williamsburgh* in *Virginia,* latitude 37° 20′ N. the annual range is above one inch, which is the same as at *Genoa*, latitude 44° 25′ N.

6. In the temperate zones the mean state of the barometer in the summer months is nearly equidistant from the extremes in that season; but in winter the mean is much nearer the higher extreme than the lower.

According to the observations at *Kendal* (see page 15*) the mean height of the barometer in July is distant from the higher extreme .33 of an inch, and from the lower extreme .37; in January the mean is distant from the higher extreme .79, and from the lower 1.17: the ratio of the former distances is as 11 to 12, and of the latter as 8 to 12, nearly.

Professor Musschenbroek, in his Elements of Natural Philosophy, (translated by Colson,) published about 50 years ago, has endeavoured to account for those changes of weight in the atmosphere; he has adverted to all or most of the causes that have ever been considered as agents in producing the effects: he enumerates the following causes, namely; First, the opposition of winds; second, the north wind blowing, which cools and condenses the air; third, the winds blowing upward or downward; fourth, an increase or diminution of heat, which rarefies or condenses the air, in consequence of which the air's distance from the earth's centre is increased or diminished, and its weight, as well as centrifugal force, thereby affected; fifth, the air

* The mean for July, uncorrected, is 29.77, and for January 29.66, which must be used in this case, because 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 chemical 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, the idea of opposite winds, mechanically accumulating or dispersing the air, be inconsistent

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

with the first fact, it will certainly fail of explaining the rest. Besides, it would not be difficult to prove, *a priori,* that the opposition of winds, admitting the fact at the time, could not produce those great and long continued accumulations of air which we often experience.

The second cause, or that of a cold north wind blowing, has doubtless an effect upon the barometer, though perhaps not altogether in the manner that has been conceived.—We shall consider this in another point of view by and by.

The third cause, supposing it to exist at any time, can only be local and transitory at most; but the rise or fall of the barometer is general, and of considerable duration: it cannot, therefore, produce the effect.

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

With respect to the fifth, it must be allowed, that water, when changed into vapour, constitutes a part of the atmosphere for the time, and weighs with it accordingly; also, that when vapour is precipitated in form of rain, the atmosphere loses the weight of it; but it would be too hasty to conclude from hence, that where evaporation is going forward the barometer must rise, and where rain is falling it must fall also; because air loaden with vapour is found to be specifically 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 15,) 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 80. 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 four or five miles high, on which account the clear air above can receive 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 29.)

Now with respect to the fluctuations of the barometer, which are sometimes very great in twenty-four hours, and often from one extreme to the other in a week or ten 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 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 ten 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 1¾ 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 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 (vide *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 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 attraction between them, and also of different specific gravities; and yet they are never separated but by the chemical attraction of substances, which unite with the one and leave the other."—Moreover, Sir Benjamin Thomson has found that moist air conducts heat better than dry air. (Vide *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

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* 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 an hygrometer.—Thus, a cubic foot of air at the equator, which there is indicated to be dry by an 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.

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 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 fourth fact offers nothing inconsistent with our theory: winds are the mediate cause of the variations of the barometer, and the currents of air to and from the torrid zone are not partial, but general, though subject to considerable modifications in direction; besides, independent of winds, those properties of the air, heat and moisture, will always be diffusing themselves in every direction, where there is a deficiency of either; from which circumstances, it seems impossible that the variations of the barometer should be local, though the amount of each fluctuation will not be the same at places considerably distant. From the usual celerity of the winds, the changes will happen upon the same day at places very distant; but theory seems to require, that the northern parallels should first experience the higher extremes, and the southern parallels the lower, and the observations upon the fourth fact countenance the inference. However, a series of cotemporary observations made at two places, differing considerably in latitude, would ascertain the fact; and if the places were one NE. of the other, they would be still more eligible for the purpose, because the two general currents of air flow in that direction.

The climate of the eastern coast of *North America* is so constituted, that the decrease of the mean temperature in the winter season, in proceeding northward, is much more rapid than on the western coast of this continent; the consequence is, that any particular place there is liable to great and sudden fluctuations of temperature in that season, and these produce proportionate fluctuations of the barometer, according as the warm and vapoury, or the cold and dry air predominate.

The sixth fact has not, that I know of, ever been accounted for, or even been adverted to, by those who have attempted to explain the causes of the variation of the barometer; and yet it will admit of a satisfactory explanation upon the principles we have adopted. Indeed, at first view, it seems inconsistent with those principles, because we can produce no facts to prove why the air may not deviate from its mean state of heat and moisture as much towards one extreme as towards the other; but, allowing, what is most probably the true state of the case, that the deviations on each side are nearly equal, still the fact of the barometer admits of a rational solution.—Moist air, as has been observed, conducts heat much better than dry air;

now when the lowest extreme of the barometer happens, the air is moist, high winds generally prevail, and the atmosphere is much ruffled by clouds and storms; all these circumstances tend to diffuse and circulate the heat, by reason of which the law of decrease of temperature in ascending, at such times, must be very 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 79; 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 below the mean for January, at *Kendal*, as stated at page 94.

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 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: 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 48. 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 five 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. [c]

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; because a greater quantity of heat is required to be added to, or subtracted from it, in order to vary its temperature equally with the other.

The same body, under the different forms of *solid, fluid,* and *aeriform,* has different capacities for heat; in the solid form its capacity is least, and greatest in its aeriform state; also, when any solid body is converted into a non-elastic fluid, or any non-elastic fluid into an elastic fluid, by heat, it absorbs a portion of heat during its conversion, which does not increase 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 diminished.—To instance 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, so as to augment its temperature to 25°; an equal quantity of heat would augment the temperature of a pound of water less than 5°, and of aqueous vapour still less. 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°, because the ice requires 140° of heat to melt it; that is, it requires as much heat to melt it as would increase 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 the two, or 102°. Also, it has been found, that aqueous vapour, when condensed into water of the same temperature, gives out 943° of heat.

The capacities of *earth, stones*, and *sand*, for heat, are much less than that of water. This is one cause why the vicissitudes of temperature are greater at land than at sea.*

Another particular relative to heat is, that some bodies conduct it better than others: in this respect there is a striking resemblance between the electric fluid and fire; for, those bodies which conduct the electric fluid well, as metals, water, &c. also conduct heat well. — *Glass, sealing-wax*, and other electrics, conduct heat very slowly; also *dry land*, whether the surface be stony, sandy, or earthy, is found by experience to conduct heat slowly.

Sir B. Thomson has by a series of experiments (see *Philosophical Transactions*, 1786) found the powers of a few bodies to conduct heat to be proportionate to the following numbers, namely:—

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

* Those who wish to see the subject touched upon above, discussed at large, may peruse Dr. Crawford's *Experiments and Observations on Animal Heat, and the Inflammation of Combustible Bodies.*

ESSAY FIFTH.

On the Temperature of different Climates and Seasons.

Mr. Kirwan has treated of this subject in so able a manner, that we can do little more than extract from his work.*

That the sun is the primary cause of heat all over the earth, is almost too apparent ever to have admitted of doubt; though some philosophers have imagined a *central heat* or body of fire in the earth, which, by its emanations, mitigates the severity of the winters in the higher latitudes: the opinion is, however, disproved by facts, which show, that the temperature of places 30, 40, or 50 feet below the earth's surface, remains nearly the same all the year round as the mean annual temperature at the surface, and that at a less depth the temperature varies, in a small degree, with the season. The fact seems to be, that in winter the earth gives out to the atmosphere a portion of heat received in summer.

The earth's surface is the chief medium by which the sun heats the atmosphere; for it is observable that clear air is not heated in any sensible degree

* Estimate of the Temperature of different Latitudes.

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. Kirwan, considering these and other circumstances, judges it most eligible, in comparing the temperature of different places, to fix upon a situation that may serve as a standard of comparison, and he judiciously prefers the sea to the land, as being more free from accidental variations. By combining theory with observation, 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 standard situation in every latitude ; namely, if $S =$ the natural sine 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 standard situation, for every five 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	74.0	45	57.5	65	40.5	85	31.4
10	82.3	30	70.7	50	52.9	70	37.2	90	31.0
15	80.4	35	66.6	55	48.4	75	34.6		

It afterwards becomes necessary to consider the modifications of the standard temperature on land, from situation, &c.

1. Elevation diminishes the mean temperature of places. Its effects Mr. KIRWAN states as follows : 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 78; because the mean annual height of the barometer, on a level with the sea, is nearly 30 inches everywhere.

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°.

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 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 everywhere the

* 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.

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 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 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 show 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 six or eight 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 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." see page 113 of that work.

	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	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

* These two places are inserted from page 28 and 29 of this work.

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 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 latitudes 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 twenty-four hours, generally happens half an hour before sun-rise, in all latitudes. The greatest heat in all latitudes between 60° and 45°, is found about half-past two o'clock in the afternoon ; between lat. 45° and 35°, at two o'clock ; between lat. 35° and 25°, at half-past one ; and between lat. 25° and the equator, at one 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, everywhere, 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 of 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.

" 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 two 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.

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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 five 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 29.

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." [D]

ESSAY SIXTH.

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

Evaporation is that process in nature by which water and other liquids are absorbed into the atmosphere, or are converted into elastic fluids, and diffused through the atmosphere; 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 chemically combined with all or any of the elastic fluids constituting the atmosphere, or it always exists therein as a fluid *sui generis*, diffused amongst the rest, has not, I believe, been clearly ascertained.

The following circumstances are found powerfully to promote evaporation; namely, *heat*, *dry air*, and a *decreased weight* or *pressure* of the atmosphere upon the evaporating surface. The first and second are known to have that effect, from every one's experience; the last is proved to have such an effect, by the air-pump; for, when the air is exhausted out of a receiver, a large quantity of vapour is raised from the wet leather upon the pump plate; this vapour is precipitated again when the air is let in, so as to 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

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* See a note upon this subject, page 129.

† Hence we see the reason of the proviso, page 19, in determining the boiling point of thermometers.

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 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°	30.0	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 *Mont Blanc*, 186°; the height of the mountain is near three 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 chemical affinities, but that aqueous vapour always exists as a fluid *sui generis,* diffused amongst the rest of the aerial 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 chemical 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 chemical 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 437,500 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 100, 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.*

* 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 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

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 considerations, it seems 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 summer.

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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.

When a precipitation (or condensation, whichever 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 less than 32°, it is in form of *snow;* when drops of rain, in falling, pass through a *stratum* 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 chemically combined with the aerial fluids, but exists as a peculiar fluid diffused amongst the rest; whenever any condensation of it happens, the matter must be *precipitated,* though not in the chemical sense of the word; we would therefore be understood in this essay to use the words *precipitation* and *precipitated* merely to denote the effect, without any allusion to chemical agency.

Different theories to account for these precipitations from the atmosphere have been formed; but the principles of none appear to me to be more plausible, and consistent with facts, than that which has lately been offered to the public, in the *Edinburgh Philosophical Transactions,* 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 chemically 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 chemical 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 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 annual rain. Inches.
Utrecht, Haerlem, and Lisle, each	24
Delf, and Harderwick, each	27
Dort	40
Middleburgh, in Zealand	33
Paris	20
Lyons	37
Rome	20
Padua	$37\frac{1}{2}$
Pisa	$34\frac{1}{4}$
Zurick, in Switzerland	32
Ulm, in Germany	$26\frac{5}{6}$

	Mean annual rain. Inches.
Wittenberg	16½
Berlin	19½
In Lancashire	41
Upminster, in Essex	19½
Bradford, in New England (2 years)*	31.4
Langholm, in Scotland†	36+
Branxholm, in Scotland†	31+
Kendal	64.5
Keswick	68.5

From the table of the mean monthly rain at *Kendal* and *Keswick*, page 37, it appears, that if we would pitch upon six successive months, which together produce more rain than any other six successive months, at these places, we must 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 *Keswick*, the rain in the former period amounts to 40.4, and in the latter to 28.1.—The reason of this seems to be, that, in the former period, the temperature of the air is decreasing, and consequently its capacity for vapour also; which circumstance is an additional cause of the precipitation of vapour. In the latter period, the capacity of the air for vapour is increasing, which occasions a less precipitation.

When a precipitation of vapour takes place, a multitude of exceedingly small drops form a cloud,

* American Philosophical Transactions.

† Edinburgh Philosophical Transactions.

mist, or fog; these drops, though 800 times denser than the air, at first descend very slowly, owing to the resistance of the air, which produces a greater effect as the drops are smaller, as may be proved thus:—Let $d =$ the diameter of a small drop, and $nd =$ that of a larger; then the resistances, being as the squares of the diameters when the velocity is given, will be as d^2 and n^2d^2, respectively; 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 same magnitude as the small one, the number will be $= n^3$, and the resistance to them falling, as n^3d^2, whilst the resistance to an equal mass in one drop is as n^2d^2; consequently, the resistance to the large drop is to the resistance of all the small ones, moving with the same velocity, as the diameter of one small drop is to the diameter of the large one, and the force being constant, the time of falling through a given space will be greater 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 also 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 40) we learn, that they are seldomer above the summit of *Skiddaw*, in November, December, January and February 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 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 three to five 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

* 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. [The late Mr. JOHN GOUGH, of *Kendal*.]

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 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 instrument of general use, and is adopted as a guide by most people interested in the state of the weather, it may be of service to investigate the relation subsisting betwixt the weight of the atmosphere and its disposition for rain, from the facts afforded us by observation,—and we may at the same time consider what further arguments can be obtained in support of the foregoing theories.

In the first place, it is remarkable, that, from the table of the mean state of the barometer for five years, in page 15, we find the highest mean upon six successive months obtained from March to August, inclusive; that is, the mean state of the barometer for March, April, May, June, July, and August, taken together, is greater than for any other six successive months, being at *Kendal*, for instance, 29.83, and for the remaining six months, only 29.75. But what is more particularly worthy of notice, is, that in this respect, the rain and the barometer are just the reverse of each other; for, in the former period the rain was least, and greatest in the latter, as has been observed, in page 134.

Again, by recurring to the tables, page 15 and 37, 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. Oct. July, Dec. Jan.

DRY MONTHS.	WET MONTHS.
Mar. June, May, Aug. April, Nov.	Oct. Feb. July, Sept. Jan. Dec.*

* 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. Sept. April, Nov. Feb. July, Oct. Jan. Dec.
Rain Mar. May, June, April, Aug. Oct. Nov. Feb. Sept. July, 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 argument 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 six months when the mean state of the barometer at *Kendal* was 30 inches or above; nine months when it was 29.9, or from thence to 30 inches; seventeen 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.*
30.0 +	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 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, 1st. The higher the barometer is above its mean annual state, the less rain there is. 2nd. The farther it is below its mean annual state, the more rain there is, till it comes to a certain point, after which the rain seems to decrease again.

The first of these inferences, being conformable to common observation, was expected; but the conclusion in the second, that the monthly mean state of the barometer may be *too low* to be attended with the *maximum* of rain, was not apprehended, till the preceding table, which seems to warrant it, was digested. However, it was immediately perceived, that the point might be cleared up, by selecting all those days which have produced the greatest quantity of rain, and finding the mean state of the barometer upon those days, which may be taken for that state most conducive to the greatest quantity of rain.—The result of a careful examination of my own observations, at *Kendal*, follows: during the extraordinary fall of rain on the 22nd of April, 1792, (see page 37,) the mean of the barometer was 29.62: the other two days that gave more than two inches of rain each, the barometer was 29.59 and 29.33 respectively: as for the other fifty-six days, on each of which there was more than one inch of rain, the mean state of the barometer upon the whole of them was 29.47, and for fifty-four of those days the barometer was between 29.03 and 29.81; the barometer on the other two days was plainly irregular, being on the one 28.5, and it is remarkable, that the rain of that day was barely one inch ; on the other it was 30.06, attended with an extraordinary circumstance. (See page 43, upon June 4th, 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, $29\frac{1}{2}$ inches, which is a little *above* the mean of the two great extremes observed in January 1789, or 29.44.

In the last five 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 one inch of rain each : hence, at an average, there has been one of such days in every thirty-one, wet and fair, and in every eighteen wet days, nearly. The number of days when the mean state of the barometer was below twenty-nine inches, were forty, of which two only were fair ; and yet there was but one of those that gave one 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 twenty-four hours, is not so great as at other times, which is consistent with the conclusion obtained from the facts stated in the preceding paragraphs.

Upon an enumeration, it appears, that there have been seventy-eight days in the different months of the last five years when the mean state of the barometer, at *Kendal*, was above the usual high extreme for the month, as stated at page 15; only seven 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 ten to one.

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 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 inquiry 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 six 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 inquirer, it was natural to expect that his attention would occasionally be turned towards an investigation of the nature and cause of the *aurora.*

It seemed to be sufficiently proved that the *aurora* was not without the earth's atmosphere, though he had never seen anything done which ascertained the real height of any one appearance with a tolerable degree of accuracy; and as the atmosphere, or at least the gross part of it, is in all probability confined to the height of fifteen or twenty English miles, he was unwilling to admit of the greater height of the *aurora*, unless compelled to it by the result of careful and accurate observations. 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 opposition to the thought;—that one and the same *aurora* should be seen over a vast extent of country, with much the same circumstances, and that some of them should appear in *France*, *Spain*, and *Italy*, whilst they so very seldom pass our zenith in the north of *England*, was a very strong argument for their great height. The best observations likewise upon those large fiery meteors which occasionally fly over the country, and are seen at such distant places, seem to prove the existence of an elastic fluid at the height of sixty or eighty miles at least, which far exceeds the height of the atmosphere as prescribed by the observations upon the barometer, or even by the twilight; and if the atmosphere exceed the height of forty-five or fifty miles, as determined by the observations on the duration of twilight, we have no *data* from whence to fix its bounds; it may, for aught we know, amount to four or five 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

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* 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.

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 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 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.*

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* 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 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 1st, 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

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

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the magnetic meridians; he then conjectures that the sulphurous vapours, rising from the many volcanoes in the north, mixing with the magnetic effluvia, may catch fire, and fulgurate.

2nd. 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.

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

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 volcanoes in the north.

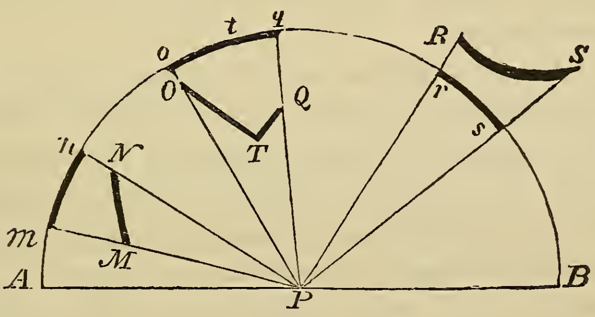
I should suppose that these conjectures, as far as they refer the phenomena of the *aurora borealis* to magnetism, are original; and from the time of the publication it might be suspected that I received the first hint from it; this however was not the case, this work being nearly ready for the press 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.

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



DEMONSTRATION.

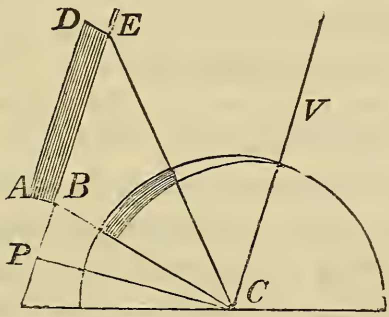
The semicircle *A m n o t q r s B P* represents a part of any plane passing through the eye, supposed to be at *P*, the centre; *A P B* the intersection of the said plane with the plane of the horizon; the arch of the semicircle represents the intersection of the first mentioned plane with the blue canopy or sky; *M N*, *O T Q*, and *R S* represent three cylindrical beams seen at a distance, whose axes are in the plane *A m n o t q r s B P* indefinitely extended. Then the object *M N* being at a considerable distance, as 5, 10, &c. miles, and quite detached from all objects on the earth's surface, it follows, from the principles of optics, that the mind cannot judge with certainty either of the absolute distance of the object, or whether the extremity *M* or *N* is more distant; in such a case, therefore, nothing appears to the contrary but that both ends are equally distant, and that *M N* is an arch of a circle in the sky, with the eye in the centre; and this in fact is the judgment that is uniformly made in the case. For it is known to every one, that celestial objects, and objects at a distance in the air, as the sun, 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 *M N* will appear as the arch *m n*, *O T Q* as the arch *o t q*, and *R S* as the arch *r s*. 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 *A E*, 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 *C P* from *C* to the side of the cylinder *B E* may fall below *B*, or in the prolongation of *E B;* then, I say, the beam will appear 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 *C V* parallel to *B E*.



DEMONSTRATION.

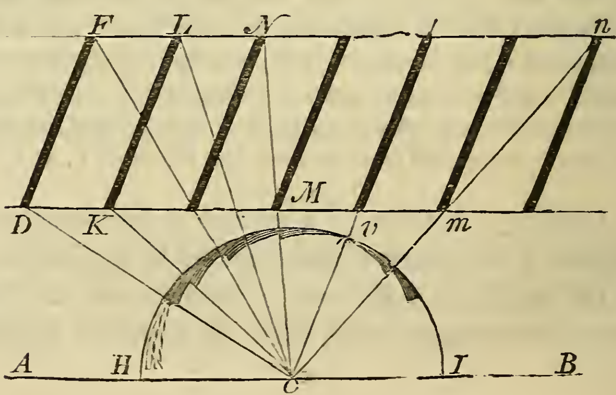
By the last proposition the lines bounding the cylinder longitudinally will appear as arches of great circles; and if the line *B E* be supposed to be extended indefinitely, the angle *P C E* increases, and when *B E* becomes infinite, *C E* coincides with *C V*, and the angle *P C V* = a right one; and the very same conclusion will follow if a perpendicular be let fall from *C* upon *A D*, or any other line parallel to *B E;* therefore all right angles parallel to *B E* will appear arches of circles, which, if prolonged, would intersect each other in the line *C V*, 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 *A B;* and if these beams be parallel to the beam *A E*, 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 eliptical.

PROPOSITION III.

Let there be a series of cylindrical beams, *D F*, *K L*, &c. equal and parallel to each other, all in a plane perpendicular to the horizon, and at equal distances from the horizon; and let *A B* be the intersection of the plane with the horizon; *H v I* its intersection with the sky; *C* the centre of *H v I*, the place of observation; and *C v* 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; and 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.

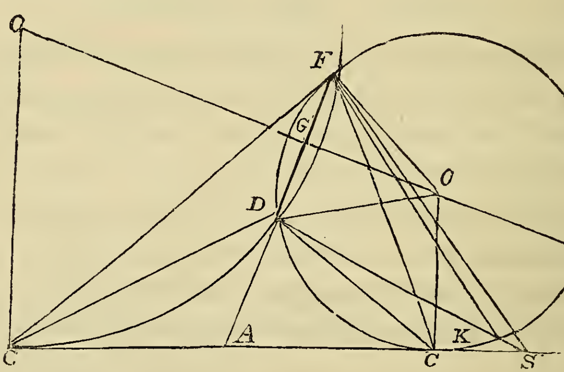
If the beams are equidistant, and if CMN, Cmn be drawn on each side of v so as to touch the bases of two beams in M and m, and the vertices of the two next beams in N and n; then all the beams included in the angle NCn will appear distinct, and all those below, on both sides, will partly cover each other, 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, 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.



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;

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

then, since the angle $D\,c\,F$ is given, $D\,o\,G$ is given also, being $= D\,c\,F$ (*Euclid* 3, 20;) also the angles G and $A\,c\,o$ being right, and angle A given by hypothesis, angle $G\,o\,c$ is given also, and consequently $D\,o\,c$; and the triangle $D\,o\,c$ being isoceles, the angles at D and c are both given, and angle $A\,c\,D$ also, being the complement of $D\,c\,o$; whence it will be

Since $A\,c\,D$: side $A\,D$ :: sine A : side $c\,D$;

And sine $D\,o\,c$: side $c\,D$ :: sine $D\,c\,o$: side $D\,o$;

And radius : side $D\,o$ :: sine $D\,o\,G$: side $D\,G = \frac{1}{2}\,D\,F$, which gives the ratio of $A\,D$ to $D\,F$. 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 $A\,D : D\,F$ may be found equally, but the greatest angle under which the beams appear will be less; thus, if $o\,G$ 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 $D\,C\,F$ will be at C', where the circle touches the horizontal line, as before.

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,* 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

* 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

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, 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 describes the horizontal lights of the 11th, 12th, and 13th of April, as having all the same situation, namely, extent from 45 or 50° W. to 35 or 30° east of the meridian.—The variation at *Paris* was then about 1 point westerly.

—— March 17th, a rainbow-like arch was seen at *Brest;* it extended from E. to W. crossing the meridian south of the zenith; soon after, a horizontal light was seen, extent from NW. to NNE.

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

—— At *Newark, in Nottinghamshire,* it was seen between the NW. and N.

One seen at *Copenhagen,* February 1st, 1707, is said to have extended from WNW. to NNE.

September 12th, 1621. Gassendus observed a horizontal light, at *Aix* in *Provence;* it extended between the summer rising and setting.—N. B. The variation then was a little to the eastward.

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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.

1718. March 4th, M. Miraldi observed a horizontal light; extent from NW. to NE. but declining about 10° more to the west.

These observations, compared with those recently made, sufficiently indicate that the position of the *horizontal lights* and *arches*, changes with the needle, and is now much more westerly than formerly.*

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 the 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

* Since writing the above, I find in the Philosophical Transactions of the Royal Society for 1790, vol. 80, several accounts of the rainbow-like arches. In Art. 3. Mr. Hey, after describing several arches, says, "The poles of all the complete arches which I have seen had a *western* variation from the pole of the equator."—In Art. 5. Mr. Hutchinson describes one seen on the 23rd of February, 1784, at *Kimbolton*, (63 miles NNW. of *London*,) to have extended from ENE. to WSW.; and a description of the same appearance, not differing essentially, is given in Art. 4.

at *Keswick:* and it appears that the *dipping-needle* in *England* points to that part.—My notes upon the *auroræ* for four or five 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 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 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 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.†

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* 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 show 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 show 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 23rd 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; 2nd. when the length is but half that distance; and, 3rd. 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 2nd supposition will be found too little, and those on the 3rd too great.

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 because their light is faint, but because the beams should be seen at a much greater distance than it seems they are, if they were 300 miles high, or twice the height of the arches; and the observations on the 30th of March, 1793, seem to confirm the opinion of the bases of the beams being 60 or 70 miles high, or about half the height of the arches.

If the summits of the beams be 150 miles high, their bases will, according to this proposition, be 75 miles high, and the whole length of the beams about 75 miles, or, more nearly, 75 miles $\times \frac{\text{radius}}{\text{sine of } 72^{\circ}}$. And if the diameter of the base 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.*

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°.

* If a magnet be required to be made of a given quantity of steel, it is found by experience to answer best when the length is to the breadth as 10 to 1 nearly: it is a remarkable circumstance that the length and breadth of the magnetic beams of the *aurora* should be so nearly in that ratio.—Query, if a fluid mass of magnetic matter, whether elastic or inelastic, were swimming in another fluid of equal density, and acted on by another magnet at a distance, what form would the magnetic matter assume? Is it not probable it would be that of a cylinder, of proportional dimensions to the beams of the *aurora*?

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 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 effervescence 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 case, because we know that discharges of the electric fluid in the atmosphere do exhibit light, from the phenomenon of lightning.—From these, and other reasons, some of which shall be mentioned hereafter, I consider it almost beyond doubt that the *light* of the *aurora borealis*, as well as that of *falling stars* and the *larger meteors*, is electric light solely, and that there is nothing of combustion in any of these phenomena.

Air, and all elastic fluids, are reckoned amongst the non-conductors of electricity. There seems, however, a difference amongst them in this respect; dry air is known to conduct worse than moist air, or air saturated with vapour. Thunder usually takes place in summer, and at such times as the air is highly charged with vapour; when it happens in winter, the barometer is low, and consequently, according to our theory of the variation of the barometer, there is then much vapourized air: from all which it seems probable, that air highly vapourized becomes an imperfect conductor, and, of course, a discharge made along a *stratum* of it, will exhibit light, which I suppose to be the general case of thunder and lightning.

Now, from the conclusions in the preceding sections, we are under the necessity of considering the *beams* of the *aurora borealis* of a *ferruginous* nature, because nothing else is known to be magnetic, and consequently, that there exists in the higher regions of the atmosphere an elastic fluid partaking of the properties of *iron*, or rather of *magnetic steel*, and that this fluid, doubtless from its magnetic property, assumes the form of cylindric beams.—It should seem too, that the rainbow-like arches are a sort of *rings* of the same fluid, which encompass the earth's northern magnetic pole, like as the parallels of latitude do the other poles; and that the beams are arranged in equidistant rows round the same pole. At first view, indeed, it seems incompatible with the known laws of magnetism, that a quantity of magnetic matter should assume the form of such rings, by virtue of its magnetism; but it may take place in one case at least, if we suppose the rings situate in the middle, between two rows of beams, so that the attraction on each side may be equal. As for the beams, in their natural state, when not acted upon by causes hereafter to be mentioned, they must all be guided by the *earth's magnetism*, (I mean the cause that guides the needle, whether it is in the earth or air I know not,) and consequently all have their *north poles* downward: but whether any two neighbouring beams have the poles of the same denominations, acting upon each other, still the effect will be the same, and their mutual action upon each other not disturb their parallelism, nor the position of the rings; because, whether the poles mutually attract or repel each other, is of no moment in this case, and the attraction of each pole is alike upon the rings.

Things being thus stated, I moreover suppose, that this elastic 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 atmosphere is disturbed, I conceive that it takes these 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 reason why the diffuse flashes succeed the more intense light of the beams is, I conceive, because the electricity disperses the beams in some degree, which collect again after the electric circulation ceases.

Many of my readers, I make no doubt, will be surprised to find, after having formed a conception that the relation betwixt the *aurora* and magnetism was to be explained and demonstrated, chiefly if not solely, from the observations on the disturbance of the needle during the *aurora*, that no mention or use whatever is made of those observations, in the preceding sections. In fact, the relation above mentioned is demonstrable without any reference to them; notwithstanding which, they not only corroborate 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 awhile, 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 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 chemical 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 mutations 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 magnetism and electricity.

It appears then, that the disturbance of the needle during an *aurora* equally countenances the conclusions drawn in the last section, and the hypothesis adopted in this; and it may be accounted for on the hypothesis, as follows.

The beams of the *aurora*, being magnetic, will have their magnetism weakened, destroyed, or inverted, *pro tempore*, by the several electric shocks they receive during an *aurora;* or perhaps the temporary dispersion and diffusion of the magnetic matter thereby, may considerably alter its influence; when, therefore, the alterations on each side of the magnetic meridian do not balance each other, the consequence will be a disturbance of the needle.*

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

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* I conceive that a beam may have its magnetism inverted, and exist so for a time, because the repulsion, acting longitudinally upon it, will only impel it in that direction, and not turn it round; just 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 same line with the needle, and thus making the needle act upon the centre. And I further conceive, that when the beam is restored to its natural position of the north pole downward, it is effected, not by inverting the beam, wholly as a beam, (for this is never observed in an *aurora*,) but by inverting the constituent particles, which may easily be admitted of a fluid.

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. This last they consider as the efficient cause of all the magnetic phenomena; but it is a mere hypothesis, and the existence of the *effluvia* has never been proved. My *fluid of magnetic matter* is, like magnetic steel, a substance possessed of the properties of magnetism, or, if these writers please, a substance capable of being acted upon by the magnetic *effluvia*, and not the magnetic *effluvia* themselves.

Whether any of the various kinds of air, or elastic vapour, we are acquainted with, is magnetic, I know not, but hope philosophers will avail themselves of these hints to make a trial of them.

SECTION FIFTH.

*An investigation of the supposed effect of the Moon in producing the Aurora Borealis.**

Some time after the author began his observations on the *aurora borealis*, it occurred to him that the phenomenon had more frequently happened about the change of the moon than at any other time; this produced the suspicion that the aerial 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

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* An essay on this subject was first published by the author in the beginning of 1789, in Mr. Davison's *Mathematical and Philosophical Repository*.

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 $29\frac{1}{2}$ days nearly, one half of a lunation is $14\frac{3}{4}$ days; hence the observations under 14 do not stand the same chance as the rest, there being only $\frac{3}{4}$ 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 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 follows:—

	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 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 two to eight o'clock; consequently, the opportunity of making observations upon the phenomenon in this interval will *often* occur 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.	NEAP.
40½	24½

And those in the months of May, June, July, and August, give,

SPRING.	NEAP.
25½	24½

As the tides are higher in spring and autumn than in summer and winter, the phenomenon ought, according to hypothesis, to occur more frequently in the two former seasons than in the two latter. The number of observations in the different months stand 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 small 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 same cause.

Upon the whole, I think it is not improbable that the agitations caused by the moon in the very high regions of the atmosphere, which we may suppose are not much agitated by the tempests in the lower regions, may have some effect upon the phenomenon in question; and the supposition is evidently countenanced by the several facts stated above.

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 random, 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 examining 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 to 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 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 one 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 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 107.

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 143.

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 142. 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 twenty-four 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. [D]

APPENDIX,

CONTAINING ADDITIONAL NOTES, ETC. 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.*

* 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.

Page 28.—The greatest heat experienced for the last five 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 83½° on the 11th, and 84½° on the 15th of July.

Page 38.—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 in 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°.

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 two yards in *Leathes-lake* and nine in *Dunmail-raise*.

Now, by applying the theorem at page 81, we find the elevation of the upper barometer above the lower 945½ yards; whence, adding 10¾ yards, we get the height of *Skiddaw* above *Derwent-lake* = 956¼ yards, and its height above the sea comes out 1037¼ 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 measurement, till its height can be more exactly ascertained by a repetition of observations.*

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* Mr. CROSTHWAITE makes the height of *Latrig*, another mountain in the neighbourhood of *Keswick*, 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 eight 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, ten yards above *Leathes-lake*............	29.39
A detached thermometer at the summit was	42½°
A detached thermometer below was.......................	54.

From which the elevation of the upper barometer above the lower

After I had observed the *aurora borealis* 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 3rd 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 127, *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

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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°.

rarefaction removes the same; also, 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 127,—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 chemically 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

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* 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.

been exhausted, is precipitated upon the admission of perfectly dry air. Hence, then, we ought to conclude, till the contrary 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 127; 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 the vapour from the wet leather rises 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 one 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 experiment.—Again, Dr. Darwin (see the note, page 129) 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 increased, that of the thermometer, exposed to the current, will be increased accordingly. In the second instance, the water on the bulb of the thermometer being exposed to the current of air, quickly evaporates, and at the same time absorbs the necessary heat from the quicksilver. But in the third instance the heat consequent to the condensation was suffered to escape, whilst the condensed vapour or water remained in the air-gun; the air rushing out was therefore of the same temperature as the surrounding air, and probably a great portion of the condensed vapour remained mechanically mixed therewith; a deposition of water upon the bulb of the thermometer was of course unavoidable, and this being resolved into vapour by its exposition, reduced the temperature of the thermometer.

In the Philosophical Transactions for 1777, there is a very interesting series of experiments showing the effects of vapour in the receiver of an air-pump, when the air is exhausted; the experiments were made by Edward Nairne, F. R. S. upon a pump on Mr. Smeaton's construction. He used two gauges, one of which was the common barometer gauge, which was of course an accurate measure of the force or elasticity of the medium of air or vapour within the receiver; the other called the *pear gauge*, from its shape, consisted of a glass 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 gauge; the space occupied by the air being then compared with the whole capacity of the gauge, 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 ten minutes, the rarefaction indicated by the pear gauge was very great, and exceeded what was observed in any other circumstance, whilst that indicated by the barometer gauge was often not $\frac{1}{200}$th part as great as the other; also, it was observed that the rarefaction by the pear gauge was *less* every time the experiment was repeated, but that of the barometer gauge 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 gauges 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 gauge	84—	pear gauge	20000.
..	57	..	56	....	16000.

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

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

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

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

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

6. When the pump and plate were both dry, and the receiver cemented on to the pump-plate, the two gauges 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 gauge indicated a greater rarefaction than the pear gauge.

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; 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 gauge is condensed, and there remains nothing but the small portion of air, with its saturating portion of vapour, at the top of the gauge.—The reason why the repetition of the experiment decreased the rarefaction by the pear gauge, 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 gauge is then scarcely greater than by the barometer gauge.

Experience proves that spirit of wine rises sooner into vapour than water; consequently the rarefaction by the pear gauge, 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 gauges.

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 gauge exceed that of the pear gauge; 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 gauge is always saturated with vapour, unless perhaps when the vitriolic acid is in the receiver, and of course its bulk, *cœteris paribus*, greater than before: but this alone is not sufficient to account for the observed differences of the gauges.

APPENDIX

TO THE SECOND EDITION.

NOTE [A] SEE PAGE 40.

FROM the table, at page 40, it may be seen the clouds are lowest in the three first and three last months of the year, evidently pointing out temperature as the regulating cause. Indeed the clouds are seldom a mile high in our climate in winter; in summer they may perhaps occasionally be two or three miles high. The thickness of a stratum of clouds, or distance from the under to the upper surface, is also variable from a few yards to three or four hundred or more; at least it is so with such as I have had occasion to observe. A stratum of cloud of greater depth is probably of rare occurrence anywhere; it would produce a greater degree of darkness on the earth's surface than is ever observed. In cloudy and rainy weather, in the summer season, when vapour is abundant, the process of nature seems to be to produce two, three, or more parallel strata of clouds, with clear transparent strata of air intervening: when rain commences from the uppermost cloud, it causes an extension of the cloudy strata into the clear strata, till the whole of the rainy region of the atmosphere becomes, especially during the fall of a thunder storm, nearly one continued cloud. Soon after the rain ceases, the strata of clouds either disappear, or become one stratum, or two or more strata, disposed probably at nearly equal intervals of distance. On the 7th of July, 1833, I had a remarkable illustration of three strata of clouds, all moving in the same current of air. Being in Wythburn, at the foot of Helvellyn, in Cumberland, I observed the upper part of the mountain covered all the morning with clouds extending about one-third part down. The wind was moderate, and blowing across the valley, or at right angles to the extensive mountain range, and the morning was fine and gleamy: the passing cloud was slowly detached from the mountain side, and commenced its motion across the valley; but it became gradually attenuated as it proceeded, and never reached further than the middle of the valley before it vanished; so that for some hours this low stratum never made any progress towards the west, further than to the middle of the valley. The west side of the valley, all this time, was quite clear, or more properly free from this stratum. There was a second and higher stratum above this, however, which was moving in the same direction, but much higher. From the less apparent velocity, I judged it to move in the same current as the lower stratum, but about twice the height; it continued its course over the summits of the opposite mountains uninterruptedly.

From occasional breaks in this stratum I could see a third and still higher stratum of very light, fleecy clouds, moving in the same direction, and probably with the same velocity as the other two strata, but *apparently* much slower, on account of their greater elevation. I judged them to be about three times the height of the lowest stratum, or rather more than a mile. There was thunder this day at Manchester, and other places in the north of England.

No person can have an adequate knowledge of clouds but one who has been above them, and in them, as well as below them, in calm weather, in windy weather, in summer, and in winter. This has been my lot frequently. Visiting the mountainous parts of Cumberland and Westmorland annually, and generally ascending one or more of the highest mountains, I had various opportunities of investigating the nature of clouds, especially in summer. Enveloped in a fog on the summit of a mountain in summer, one may see to read, or view divisions on an instrument, or in short any object within two or three yards, but all beyond that distance is hid in impenetrable darkness. The summer fogs are much more dense than the winter ones which we have on the plains. In these one may often see houses, trees, &c. at the distance of ten or twenty yards; but the former are usually much more dense. Notwithstanding this greater density of the vapour, in a strong wind the mass of vapour is evidently moving with the velocity of the wind. To most people there is somewhat of novelty in being in a dense fog and in a hurricane at the same time. When the obstruction of the mountain causes a break in the cloud, the scene at first is awful. The mountain appears like a volcano. Every vista shows a volume of smoke carried off by a whirlwind, and more smoke issuing from various points. In a few moments impenetrable darkness resumes its reign, till a fresh break succeeds. In this way the mountaineers, whose object is an extensive view and beautiful prospect, are sometimes tantalized; but the Meteorologist, adopting for his motto, *ex fumo dare lucem*, may find some important points of science elucidated in such circumstances. In general however, tourists, after being for some time buffeted by the wind and moistened by the humidity, begin to feel and to express their anxiety to get safely down again. Nor is this wish very unreasonable; for even the most experienced guides are sometimes at a loss how to direct their course. A compass on such occasions is useful. The object is to reach the under surface of the cloud; when all the country below at once becomes visible, and the *cloud* above is often found to become *rain* beneath.

Sometimes we reach the summit of a mountain in a calm; when, if a fog sets in, it is almost hopeless to expect it to clear off in a short time. I was on Scawfell, the 8th of July 1812, in these circumstances, in mid-day, and remained for two hours without being able to see in what quarter of the heavens the sun was, and possessing a view around me of only a few yards in diameter. In time the sun pierced the fog, and became just visible; for half an hour it was alternately brighter and darker, till at length the fog presented a smooth white sea on a level with the summit of the mountain, extending all around to an indefinite distance, and was most brilliantly illuminated by the sun. The day continuing fine, this white sea gradually sunk into the valleys, and left the guide and me on a rock in the midst of an island situated in this ocean. Soon after, other mountain summits projected out of the ocean; and the views were wonderfully grand: at last the mist vanished from the valleys, leaving only one of them, which happened to be shaded by the mountain, with a level stratum of cloud as white as snow, at the bottom. The clouds thus dissolved left the atmosphere in the most transparent state I had seen it. Both sea and land views were uncommonly fine. Several ships were seen at sea, and the whole of the Isle of Man and part of the sea beyond it were full in view.

In the above instance we see an example of the heat of the sun dissolving a cloud, or converting it into steam, beginning at the *upper* surface of the cloud and proceeding downward. There are other times when the heat operates on the *under* surface of the clouds first, and the solution proceeds *upward*. In this case the cloud appears to *ascend*, contrary to the law of gravitation; the phenomenon is not unfrequent after a foggy morning in the summer. It is considered a prognostic of a fine day. I have seen it in great perfection at Lowwood Inn, near Winandermere Lake. A thick fog has concealed the lake at 6 A. M.; by and by the lake became visible, next the opposite bank appeared, then the summit of the high ground beyond the lake; in time the base of Coniston Fell was seen, and the cloud gradually ascended the mountain, or more properly *appeared* to ascend it, till it rose above the summit, and soon after vanished into air.

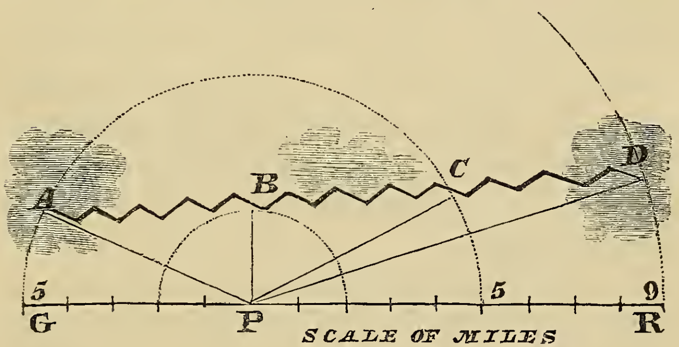
On the whole I think it must appear that a mountainous country is the fittest of all for studying the nature of clouds, though perhaps a plain country may be more advantageous for ascertaining the force and direction of the winds, and some other phenomena of meteorology.

Note [B] See page 43.

Since the year 1791, I do not remember an instance of a thunder crack so remarkable as the one which occurred at the time to which this note refers.

The cause of the long continued and irregular sound of thunder formerly appeared to me difficult to be accounted for; and all the attempts I had seen to give an explanation of the phenomena utterly inadequate. The most common account of the continuance of the sound refers to the echo of the electric explosion occasioned by the ground, the mountains, the clouds, &c. which is almost destitute of plausibility.

About the year 1808 or 1809 (I do not find any memorandum of the precise time) a thought occurred to me, that if we could admit of an electric discharge to be made *instantly* from one cloud to another, through an extensive range of atmosphere, say twelve or fourteen miles, rather than from one cloud to another in the immediate vicinity, as within one hundred feet or yards, we might give a much better explanation, both of the continuance and the irregularity of the sound. I made a sketch somewhat like the annexed one, in which the line *G R* represents an interval of fourteen miles upon



a horizontal ground plane; *A*, *B*, *C*, *D*, a region of air interspersed with thunder clouds, the zig zag line *A B C D* a discharge of lightning from a cloud at *A* to a cloud at *D*, or *vice versa;* *P*, the place of an observer, *P B* the nearest distance from *P* to the line *A D*, *P A* the distance of one cloud and *P D* the distance of the other, being supposed two miles, five miles, and nine miles respectively; then the whole line of air *A*, *B*, *C*, *D* being instantly convulsed by an electric discharge, the observer at *P* will first hear the sound from *B*, being the nearest point, in about nine seconds after he perceives the flash of lightning; the sound will afterwards arrive from two equidistant parts, right and left, for thirteen or fourteen seconds; after which it will die away gradually to the right, in about eighteen seconds more, exactly as it would be found to do if a volley of musquetry was instantly fired in a like line of fourteen miles in length, along the ground, in a similar position as to distance from the observer at *P*.

Hence also we may account for the same peal of thunder being at some moments soft, and at others violently harsh, and abrupt in succession, according as the electric fluid has met with less or more obstruction from the various states of the successive portions of atmosphere through which it had to force its passage.

On this principle I gave an explanation of the phenomena of thunder in a lecture at the Royal Institution, Albemarle-street, London, in 1810. Sir H. Davy repeated the explanation in his next lecture, and gave his opinion of it, as being both original and most satisfactory. It was afterwards, in 1812, published in Dr. Rees's Cyclopedia, in the article Meteorology, which I wrote for that work.

A very striking confirmation of the truth of this view was afforded, from observations on a remarkable peal of thunder, which occurred on the 27th of June, 1819. Being on an eminence, about three miles west of Manchester, between three and four o'clock P. M., a few drops of rain fell from the west end of a narrow stratum of clouds visibly extending little farther than Stockport, or about ten miles to the east. On a sudden a vivid and unexpected flash of lightning took place, and almost instantly a loud peal of thunder commenced, which gradually died away in the direction of Stockport, in the space of half a minute. No more thunder was heard that afternoon, and the clouds soon dispersed. On conversing with a neighbour, a few days after, who had been at Stockport the same afternoon, he incidentally mentioned a very loud peal of thunder that had taken place at Stockport that afternoon, which, from its suddenness after the lightning, and loudness, had caused unusual alarm in that place. On further inquiry, I learned it had occurred between three and four o'clock, and that no more than that one peal had been heard.

Those loud reports like thunder, which are mostly heard a few minutes after the appearance of large meteors, or fire balls, moving with great rapidity through the high regions of the atmosphere, are, I conceive, only to be accounted for in the same manner as the peals of thunder. These reports have hitherto been mostly, if not always, ascribed to the *bursting* of the meteors ; but it would be difficult to assign a reason why this should cause a greater concussion, in such highly rarefied air, than the prodigious velocity of the meteors themselves. The great meteor of 1783, which moved from N.

to S. nearly on the magnetic meridian, could not, from my own observation, have moved with a velocity much differing from ten miles in a second, which is probably greater than that of the thunderbolt. Whilst I saw the meteor, the marked and well defined front of the ball, the long train or tail gradually diminishing in brilliancy as it receded, and a wavering motion of the whole meteor, all denoted motion in a resisting medium. I did not hear any report after the meteor was gone, probably because I did not wait for, or expect one. But a report resembling thunder was noticed by two of those persons who described the meteor from York, one or more from Windsor; and reference is made to similar accounts from Kent. Now it appears to me much more rational to ascribe the commencement of the reports heard at York, at Windsor, and in Kent, to the concussion of the atmosphere at the nearest point of the path of the meteor to those places respectively, and that the sound died away as the two equal distances of those points from the observers increased, as has been explained above for thunder.

There were about ten of those large meteors seen in the course of the last century, as described in the Philosophical Transactions of the Royal Society. Mr. Brydone saw one, February 10th 1772, at Berwick; he counted 5 min. 7 seconds between the appearance and sound from the meteor; another gentleman in the neighbourhood also saw the same, and heard the report some minutes afterwards, but was not aware that it was connected with the meteor. Mr. B. calculates the height of the meteor, or rather its distance at the moment of the supposed explosion, to be sixty-six miles, from the time elapsed before the sound arrived, supposing that sound moves down the atmosphere with the same velocity as it moves horizontally. See Abridg. Trans. vol. 13. I calculated the height of the meteor of 1783 to be eighty miles, from the newspaper observations at the time; but it was calculated variously by others, from forty to eighty miles. In all probability it was about sixty miles.

The light of these meteors, it can scarcely be doubted, is electrical; but how it becomes accumulated and dispersed, it is not easy to discover.

Besides these large fire balls, there are other smaller balls seen at times, of intermediate *apparent* magnitude between the large balls and the very small ones, or *falling stars* as they are called. The intermediate balls, called *bolides* by the ancients, are of still more rare occurrence than the large balls. When they occur, they usually spread themselves over a large extent of country; sometimes in the eastern and western hemispheres at the same time. Humboldt, in his Travels in South America, mentions a grand display of these, early in the morning of the 12th November, 1799, at Cumana, lat. 10° N., long. 63° W.; they were also seen by an American gentleman, (Andrew Ellicot, Esq.,) in a Voyage from New Orleans to Philadelphia, lat. 25° N. and 80° W. long. An extract from his Journal follows. (Vide Transactions American Philosophical Society, vol. 6, page 28.) "About 3 A. M. I was called up to see the shooting of the stars, as it is commonly called. The phenomenon was grand and awful; the whole heavens appeared as if illuminated with sky-rockets, which disappeared only by the light of the sun after day-break. The meteors, which at any one instant of time appeared as numerous as the stars, flew in all possible directions, except from the earth, towards which they all inclined more or less, and some of them descended perpendicularly over the vessel we were in, so that I was in constant expectation of their falling among us."—He adds, "I have since been informed that the above phenomenon extended over a large portion of the West India islands, and as far north as Mary's, in lat. 30° 42′, where it appeared as brilliant as with us off Cape Florida."

Humboldt ascertained that these meteors were seen in various parts of South America, from the Equator northwards. Similar ones were seen at Nain, in Labrador, and in Greenland at the same time. On his return to Europe, he was astonished to find they had been seen, at the same time, in various parts of this continent, particularly in Germany, near Weimar.

On perusing these accounts for the first time, last winter, (1833,) and knowing that I had a journal at the time of these appearances, I was eager to find whether they had been noticed here; on turning to my journal for 1799, I was agreeably surprised to find, on the 12th of November a "N. B. Several large meteors and much lightning in the morning from five to seven o'clock." The day's account was, "Showery; violent wind P. M.," and the barometer was near the low extreme.

A similar exhibition of meteors was seen over Great Britain, and part of the Continent as far as Geneva, on the morning of the 12*th of November*, 1832. And what is certainly very remarkable, though probably fortuitous, a like exhibition seems to have taken place in North America, on the morning of the 12*th of November*, 1833.

These meteors, it would seem, are of the same nature as the smaller ones, called falling stars, and the larger ones or fire balls, and probably belong to the same region of the atmosphere, or that region which lies between the clouds and the *aurora borealis*.

NOTE [C] SEE PAGE 109.

FORTY years have elapsed since this Essay on the variation of the Barometer was written ; and I have met with no arguments since, that appear to me more cogent than those which I have used in it to explain the phenomena. Another *fact* respecting the variation of the barometer indeed appears to be well established, by the attentive and careful observations of that instrument. I mean the diurnal variation first observed in the torrid zone, and since then traced through the temperate zone, though it is there blended with other and more powerful variations, from other causes.

Generally speaking, the fact is this, that early in the morning, about sun-rise, or soon after, the barometer is higher, all other circumstances the same, than it is afterwards; that it droops a little, as the heat of the day advances, and is lowest nearly in the warmest part of the day; after which, it rises as the air cools, and in the evening nearly recovers what it had lost since morning.

The sun's power being greatest in the torrid zone, this effect of it, (for it is evidently an effect of temperature,) is there a maximum; and on this account it is more conspicuous there, as well as on account of the other variations being of less magnitude than in the temperate and frigid zones. The effect diminishes, in leaving the equator, in some proportion as the latitude, the seasons, and other circumstances.

The sun is constantly heating the earth and air, successively from east to west: the air being heated expands in various directions, to restore an equilibrium of pressure; if this expansion was only in a perpendicular direction, it would not disturb the barometer; but as the air will go in any direction where the pressure is least, it has a lateral motion as well as a perpendicular one; and hence the column pressing on the mercury is less in quantity during the high temperature; but when the excess of temperature is withdrawn, the air falls back into its former position.

It will be perceived that the principle we have adopted in the Essay on the variation of the Barometer is, that an equality of *elasticity* in two vertical columns of air, will, in great part, counteract an inequality in their *weight*. To illustrate this position: suppose a cylindric vessel of indefinite length were filled with hydrogen gas, and placed perpendicular to the ground plane, having no communication with the atmosphere; suppose then that a small hole were made in the side of said vessel, at a point where the elasticities of the two gases were equal. A communication being now opened, an intercourse would immediately commence; but this would not be occasioned by the specifically heavy gas rushing into the light gas, nor the light gas into the heavy gas, exclusively. The two gases, having equal elastic forces, would, by virtue of those forces, be diffused through each other slowly and gradually, according to the law which I have pointed out in another Essay—that of one gas being as a vacuum to another in regard to their mutual diffusion.

The application of this principle, in accounting for a temporary existence of a warm vapoury volume of atmosphere, supporting itself against heavier but colder volumes of atmosphere on the right and left of it, is too obvious to be insisted upon.

Of all the phenomena of meteorology, there is none more difficult to explain, than those relating to the fluctuations of the pressure of the atmosphere indicated by the barometer. After forty years' additional observations and occasional reflections on the subject, I am persuaded that the explanation already before the public in this third Essay, is sufficient, and will be generally considered so, when properly understood. The fact probably is, that the Essay has fallen into the hands of few who have paid much attention to meteorology; or who have had their attention directed more particularly to this part of the subject. Indeed the explanation itself involves certain physical facts by anticipation, which were not known at the time the Essay was first published. I allude to the mutual action of different elastic fluids. It has been long known, that if a tall jar, of two or more inches in diameter, be filled with a *heavy* elastic fluid, and held with its mouth *downward*, it will be emptied in a moment or two, and at the same time filled with common air; but if placed with its mouth *upward*, it will remain for a considerable time in the jar, and be *gradually* diluted with common air, till at length it becomes filled with the same; and vice versâ, if filled with a *light* elastic fluid. But it was not known, I believe, till I published my Chemistry in 1810, that if a vessel of any shape be filled with any either light or heavy gas, and it be made to communicate with the atmosphere by a tube of one tenth of an inch diameter, it matters little whether the tube be held up or down, as regards the time of the exit of the gas. It will be slow and gradual, in both positions.

From these observations it is not difficult to conceive, that a vertical column of warm vapoury air may be projected into a heavier column of cold dry air, such that their *elasticities* may be nearly equal for a time, but that the adjustment of their *weights* may require a slow and gradual operation, sufficient to account for the interval of time observed between the extreme and mean state of the barometer.

Note [D] See page 125.

In 1817, the celebrated traveller Humboldt published his Essay on Isothermal Lines on the Globe, in the third volume of the Memoirs of the Society of Arcueil. The author's object was to ascertain facts, rather than to account for them; he has furnished a more copious collection than that given by Kirwan. The effect of elevation in reducing the mean temperature of any place, is now well known to be about 1° for every hundred yards above the level of the sea; and this is occasioned by the temperature of the atmosphere diminishing, in ascending, at the same rate. It is pretty obvious too, that distance from the equator must cause a diminution of temperature, though it may not be easy to discover the law of that diminution; but it is not so obvious why the same parallel of latitude should have such great variations of temperature in its different parts. In most parallels of latitude, between the tropics and the polar circles, there appear to be two or more *waves* of temperature, or ascents and descents. Humboldt has noticed the fact; and he has referred to my explanation of the cause. (Memoirs, page 508.) The reference is to the part in italics at page 125 of my Essays, (first edition,) or at page 119 of the present edition. My own explanation given in that place, I have reason to think, is too brief to be generally intelligible, and I shall therefore unfold it a little here. It has been shown in the Essay on winds that air drawn from the polar regions towards the equator, by reason of its superior specific gravity, would go on the meridian, were it not for the diurnal rotation of the earth; but this rotation or motion of the earth's surface, from West to East, causes the air relatively to move westward, so that, uniting the two motions of North or South with West, the real tract of the air over the surface of the earth is, in the northern hemisphere, towards the S. and towards the W. or a N. E. current, and in the Southern hemisphere, towards the N. and W. or a S. E. current. These currents do not move over the surface of the earth in *great circles* nor *less circles* of the sphere, but in *spiral lines*, or directions somewhat like the oblique *rhumb-lines*, and always *convex* towards the East. What we have just described relates to the *under* currents of the atmosphere. The *upper* currents will be *spiral lines*, in diametrically opposite directions, or S. W. and N. W. From this statement it will follow, that a strong S. W. wind blowing on the West coast of

Europe, must have proceeded, in all probability, from the West Indies; that is, it must have traversed a space of 3000 miles over a warm sea, and consequently have acquired nearly its full charge of vapour; when, meeting with a cold continent, it must deposit its excess of vapour, and give out heat; and the further it extends over the continent, the less will be its temperature, as well as its vapour. If it should reach to the opposite, or E. coast, it must arrive cold and dry, having lost both its heat and vapour in the progress. Thus it appears that a S. W. wind can have little influence in warming the *East* coast of Europe. The other current of air, which meets the east coast, is one blowing from the polar regions, or a N. E. wind, which cannot be expected to bring either heat or moisture, in excess: hence the east coast must be colder and drier than the west. And the same reasoning will apply, with equal effect, to the east and west coasts of the American continent. The same reasoning will also apply to large islands, as well as to continents: thus in Great Britain, the west coast has a higher temperature, and more rain, than the east, on the same parallel of latitude.

If the idea suggested by Sir David Brewster in the Transactions of the Royal Society of Edinburgh, Vol. 9, 1821, be correct, (and there seems great reason to believe it to be so,) namely, that *there are two poles of greatest cold in the northern hemisphere,* the above observations will enable us to see the natural cause of this remarkable fact. The lands within the arctic circle, in the absence of the sun, must depend upon the S. W. winds from the two great oceans, for their winter heat. Those parts of the eastern and western continents, which are most remote from the ocean as measured along the curvilinear track of the current of air, must receive that air, in great measure deprived both of its vapour and its temperature. Accordingly it is found, that the temperature of the N. E. parts of such continents exhibits the extreme of cold. Probably a latitude of 75° N. and a longitude of 90° E. and 90° W. would be found nearly equally cold, and to exceed any other place on the surface of the globe, in this respect;—and it would be a curious coincidence, if Professor Hansteen's two supposed northern magnetic poles should be found in the same positions as the two poles of extreme cold.

ADDENDA

TO THE ESSAY ON THE AURORA BOREALIS.

1834.

Since my removal from Kendal to Manchester, I have not had so good opportunities of making observations on the appearances of the *Aurora borealis*, partly from the obstructions of view by buildings in a large town, and partly from the effects of greater distance from those regions where the auroræ most abound, but principally, from the late period being one in which the phenomena were less common than they had been previously. I have not, however, been inattentive to the subject, and have noted in my journal all such appearances as seemed well authenticated, whether communicated by friends or through the medium of the public journals.

It may be proper, in this place, to subjoin a list of those appearances, with their dates, as the present work was published originally with more especial reference to this peculiar meteor.

Explanation of the abbreviations used, as reference to authorities, in the following list of Auroræ Boreales. Where no reference is given, it is to be considered the Author's own observation.

R. Refers to such observations as were *reported* to have been made by persons deemed competent.

H. Refers to Luke Howard, Esq., F. R. S., and to his second edition of the Climate of London, 1833.

A. P. Refers to Dr. Thomson and Mr. Phillips's Annals of Philosophy.

Br. Refers to Dr. Brewster's Edinburgh Journal of Science.

J. J. Refers to Professor Jameson's Edinburgh New Philosophical Journal.

Bl. Refers to John Blackwall, Esq., F. L. S., of Crumpsall Hall, three miles N.W. of Manchester, to whose kindness and favourable residence, the Author is indebted for many observations.

Ph. Mag. Refers to the Philosophical Magazine, and Annals of Chemistry, &c. edited by Messrs. Taylor and Phillips.

P. T. Refers to the Philosophical Transactions of the Royal Society.

Phil. M. Lond. & Edin. Refers to the Journal of Sir D. Brewster and Messrs. Taylor and Phillips.

Auroræ Boreales observed in Great Britain and Ireland since 1793.

1794.	Jan.	7	A few fine streamers
		22	A horizontal light
	Mar.	8	A low aurora
		29	A low aurora most of the evening, with some streamers.
	Dec.	8	At 5h. 30m. P. M. an aurora with streamers—it soon disappeared.
		19	At 11 P. M. a bright aurora, and some beams
1795.	Sept.	8	A horizontal light
		14	A horizontal light 10 or 12° alt.
	Oct.	18	At 10 P. M. a faint streamer or two
1796.			None observed
1797.	Jan.	22	A low horizontal light
	Feb.	1	At 9 P. M. an aurora, with a few beams
		18	A horizontal light most of the evening
		27	A faint light
		28	From 10 to 12 P. M. a fine luminous aurora
	March	1	Evening luminous in the north

1797.	March 2	Evening luminous in the north
	10	A fine aurora in Yorkshire—R.
	April 24	At 11 a bright horizontal light, with streamers
	Nov. 18	At 9 a small aurora
	21, 22 & 23	Auroræ
1798.		None observed
1799.	Sept. 3	An aurora at 12, seen at Harrogate—R.
	Oct. 25	An aurora at 9 P. M.
1800.	Nov. 2 & 7	Faint auroræ
	Dec. 10	A fine bright aurora, with beams
1801.	Jan. 4	An aurora—R.
	25	A beam or two of an aurora—R.
	Feb. 22	An aurora—R.
	Oct. 6	A bright low aurora
1802.	Feb. 3	An aurora—R.
	Mar. 29	A low aurora
	Sep. 27	An aurora at sea, off Liverpool—R.
	Dec. 13	At 8 P. M. an arch of an aurora 10° alt.
1803.	April 12 & 13	Auroræ—R.
	Sept. 11	At 10 P. M. a low aurora
	17	Ditto
	Oct. 12	Ditto
	Dec. 3	An aurora—R.
1804.	Feb. 2	An aurora at 8 P. M.—R.
	April 1	A fine luminous aurora at 11 P. M.
	4	A faint aurora
	May 2	A splendid and active aurora at 11 P. M.
	Nov. 22	A fine aurora at 9 P. M.
	25	A horizontal light, and two or three beams at 8 P. M.
1805.	Jan. 1	An aurora
	Feb. 23	A remarkable aurora to the south, with flashes of light amongst the clouds
	April 30	Aurora at 11 P. M.—R.
	Sept. 24	Aurora most of the evening
1806.	Feb. 7	A grand aurora over greatest part of the heavens at 10 P. M.

1806.	Sept. 10	An aurora
	Nov. 2	An aurora—R.
1807.		None observed
1808.	Mar. 25	An aurora.—R.
1809, 1810, 1811, 1812 & 1813		None observed
1814.	April 17	An aurora—H. A. P. iii. page 400
	Sept. 10 & 11	Auroræ
1815.		None observed
1816.	Sept. 15	An aurora—R.
1817.	Feb. 8	An extensive aurora—A. P. ix. page 250. H. vol ii. page 324
1818.	Oct. 26	At 8 P. M. an aurora—H. & A. P. xii. page 476
	31	An aurora at Sunderland—H. & A. P. xiii. page 71
1819.	Oct. 12	Said to have been an aurora—R.
	17	A fine aurora about 7 P. M.—H. & A. P. xiv. page 395
	Dec. 14	Fine aurora, seen at Manchester, London, &c.—H. & A. P. xvi. page 160
1820.	Jan. 14	A brilliant aurora, London—H. & A. P. xv. page 160
	Feb. 11	From 8 to 10 P. M. a bright aurora in the magnetic north.
1821.	Nov. 26	An aurora at 6 or 7 P. M.
	Dec. 12	An aurora—R.
1822. 1823 & 1824		None observed
1825.	Mar. 19	Fine auroral arch, Edin.—Br. vol. iii. page 181
1826.	Jan. 21	After 9 P. M. an auroral arch, Edin.—Br. vol. ix. page 129
	Mar. 29	At 9 an auroral arch, and streamers, Edin. &c.—P. T. 1828. Ph. Mag. vol. iv. page 418
1827.	Jan. 16	At 9 P. M. a fine luminous arch, Scot.—J. J. vol. iii. page 342
	18	A fine aurora, Manchester, Gosport—Ph. Mag. vol. i. page 239
	Feb. 17	An aurora after 10 P. M. Manchester, Gosport—Ph. Mag. vol. i. page 317

1827. Aug. 29		An aurora from 11 to 12, Berwicks.—J. J. vol. iii. page 379
Sept. 9		An aurora, *seen in the day*, and also in the evening from 9 to 12, England, Scotland—J. J. iii. 378
	25	Very fine aurora, Great Britain, Switzerland, Baltic, &c. &c.—Ph. Mag. vol. ii. page 395 & iii. page 75
Oct. 6		An aurora seen in Scotland.—Br. vol. viii. p. 171
Nov. 18		An aurora, Scotland—Br. viii. page 171. Gosport—Ph. Mag. vol. iii. page 78
	19	An aurora, Scotland ditto.
Dec. 8		Fine aurora in Denmark.—R.
1828. June 2		An aurora, Yorkshire.—R. Ph. Mag. vol. ii. page 396
Sept. 8		A fine aurora, Essex.—See Lond. Cour. Sep. 12
	15	A fine aurora, Scotland.—Br. vol. x. page 177
	29	A grand auroral arch, Great Britain, Ireland, many places in the State of New York—Br. vol. i. page 256; vol. x. page 146; and Ph. Mag. vol. v. page 153
Oct. 9		An aurora seen amongst clouds at 9½ P. M.—R.
	15	An auroral arch, Scot.—Br. vol. x. page 179
	29	Fine streamers, Perth.—Br. vol. x. page 179
Dec. 1		An aurora, Manchester, Wirksworth—Ph. Mag. vol. v. page 153. Also at New York
	22	An aurora—Bl.
	26	An aurora—Bl.
	28	An aurora—Bl. Do. Alford, Aberdeen.—P. T. 1829, page 118
1829. March 4		An aurora, with streamers, 7 P. M.—Bl.
	6	An aurora at 8—Bl.
	22	An aurora at 7½—Bl.
May 4		An aurora, with streamers—Bl.
June 17		An aurora, with streamers—Bl.
July 25		An aurora, with streamers—Manch. Cour. & Bl.
Sept. 19		An aurora, with streamers, at 8h 25m P.M.—Bl.

1829. Sept.	21	An aurora, Alford, Aberdeen—P. T. 1830, page 110
Oct.	3	At 9½ P. M. an aurora, with streamers—Bl.
	11	At 6½ P. M. brilliant streamers, at Aberdeen—P. T. 1830, page 102
	17	From 6 to 11 P.M. an aurora, and streamers—do.
	25	An aurora—do.
Nov.	17	From 6 to 11 P. M. a low aurora—do.
	18	An arch, and streamers—do.
	19	At 8 P. M. an aurora—do.
Dec.	14	From 6 to 9 a low aurora, London, Gosport—Ph. Mag. vol. vii. page 158. Fine at Aberdeen—P. T. 1830, page 99
	19	At 11½ a low aurora, Aberdeen—P. T. 1830, page 103
	20	From 8½ to 11 P. M. an aurora, low, Aberdeen—P. T. 1830, page 104
1830. Jan.	25	A low aurora 7 to 10 P. M. Aberdeen—P. T. 1830, page 108
	28	A low aurora 8 to 9 P. M. Aberdeen—P. T. 1830, page 108
Mar.	18	Aurora, cloudy—R.
	24	A fine aurora before 10 P. M., Aberdeen—P. T. 1830, page 113
April	19	From 9 to 12 P. M. a grand aurora in Great Britain. Also in America very brilliant—Br. vol. v. page 262
	24	An aurora at Leeds—Newspaper
Aug.	20	An aurora, with streamers, at 10 P. M.—Bl. Grand in America
Sep.	7	A fine aurora, Manchester—Bl. Bedford—Ph. Mag. vol. ix. page 393. Isle of Man—J. J. vol. x. page 176. Also in America—Br. vol. v. page 262
	8	An aurora, Manch.—Bl. Bedford—Ph. Mag. vol. ix. page 394
	10	An aurora, Isle of Man—J. J. vol. x. page 176

1830. Sep. 12 An aurora, Isle of Man—J. J. vol. x. page 176

13 An aurora, ditto ditto

17 An aurora at Kelso, arch and streamers—Br. vol. v. page 262. Isle of Man—J. J. vol. x. page 176. Bedford—Ph. Mag. vol. ix. page 394. America—Br. vol. v. page 363

19 An aurora, Isle of Man—J. J. vol. x. page 176.

20 An aurora, ditto ditto

21 An aurora, ditto ditto

25 An aurora, ditto ditto

Oct. 5 An aurora at 7h 20m Gosport—P. Mag. vol. viii. page 465. Arch with streamers — Br. vol. v. page 253. Scotland, Carlisle, Kendal—Br. vol. iv. page 187. Bedford—Ph. Mag. vol. ix. page 394. America—Br. vol. v. page 263

16 At 9½ P. M. aurora at Bedford—Ph. Mag. vol. ix. page 394. At 10 P. M. at Gosport—Ph. Mag. vol. viii. page 465. Also in America—Br. vol. v. page 263

17 A low aurora at Bedford—Ph. Mag. vol. ix. page 394. At Gosport—Ph. Mag. vol. viii. page 465. Also in America—Br. vol. v. page 263

22 An aurora—R. Bl. ?

Nov. 1 An aurora—Bl. Also Bedford—Ph. Mag. vol. ix. page 394. Gosport—Ph. Mag. vol. ix. page 79

3 An aurora at Kendal—Br. vol. iv. page 187

4 An aurora—Bl. Also Gosport—Ph. Mag. vol. ix. page 79. Bedford — Ph. Mag. vol. ix. page 394

7 A faint aurora at Gosport—Ph. Mag. vol. ix. page 79. At Bedford—Ph. Mag. vol. ix. page 79

10 An aurora—Bl.

1830. Dec. 11 A grand aurora from 7 to 12 P. M. in England, and still more so in the State of New York, America—Howard, vol. iii. page 373. Ph. Mag. vol. ix. pages 159, 394. Br. vol. v. page 263

12 Another aurora nearly equally fine and extensive in Europe and America. Same ref.

13 & 14 Faint auroræ at Gosport and Bedford—Ph. Mag. vol. ix. page 159 and 395

22 An aurora, Ackworth, Yorkshire—H. vol. iii. page 373

25 A fine aurora, Manch. Gosport—Ph. Mag. vol. ix. page 159. Kendal—Br. vol. vi. page 184

1831. Jan. 5 An aurora—Bl.

6 An aurora, Manchester, Gosport—Ph. Mag. vol. ix. page 235

7 From sunset till morning, one of the finest auroræ ever seen; England, Scotland, Paris, Brussels—H. vol. iii. page 373. Ph. Mag. vol. ix. pages 127, 151, 233. Scotsman Newspaper, &c. J. J. vol. x. page 381

8 An aurora—Bl. Gosport—Ph. Mag. vol. ix. page 235. Bedford—Ph. Mag. vol. ix. page 395

10 An aurora—Bl.

11 An aurora at Kendal—Br. vol. vi. page 184 Bedford—Ph. Mag. vol. ix. page 395 Gosport—Ph. Mag. vol. ix. page 239

21 An aurora at Manchester, at 10 P. M.—Mr. Burton

Feb. 11 An aurora at Manchester—Bl.

14 An aurora—Bl. Also Lancaster Gazette

17 An aurora—Lancaster Gazette

March 6 A low aurora—Bl.

7 A fine aurora at 7 P. M.—Bl. Also at Gosport from 7 to 10—Ph. Mag. vol. ix. page 399

1831.	March 8	An aurora from 11 P. M. to 1 A. M. Manchester —Bl. Gosport at 11 P. M.—Ph. Mag. vol. ix. page 399
	11	An aurora, Manchester—Bl. Marsden, Lancashire—T. Hoyle. Gosport from $8\frac{1}{2}$ to $9\frac{1}{2}$ P. M. like that of the 7th—Ph. Mag. vol. ix. page 399
	12	An aurora—Bl.
	15	An aurora—Bl.
	April 3	An aurora—Bl.
	19	An aurora, Manchester, at 11 P. M. Gosport, from $8\frac{1}{2}$ P. M. to 1 A. M.; fine and elevated—Ph. Mag. vol. ix. page 466
	20	A faint aurora, Gosport, low from 10 to 12 P. M. —Ph. Mag. vol. ix. page 466
	23	An aurora at 11 P. M. Manchester—Mr. Burton
	May 30	A slight aurora in the North at $10\frac{1}{2}$ P. M. Gosport—Ph. Mag. vol. x. page 79
	Sept. 12	An aurora, or horizontal light, Manchester—Bl. A faint aurora from 8 to $10\frac{1}{2}$ P. M., Gosport—Ph. Mag. vol. x. page 398
	Oct. 29	An aurora, or horizontal light, Manchester—Bl. From 10 to 11 P. M. an aurora, reaching about 16° alt., Gosport—Ph. Mag. vol. x. page 571
1832.	Jan. 27	An aurora, Manchester—Bl.
	Sept. 23	An auroral arch, and streamers, Mr. R. Potter, Smedley Hall, Manchester—Lond. & Edin. Ph. Mag. & Journal of Science, vol. ii. page 233
	Nov. 1	Evening, luminous N., Manchester. Aurora at Bradford, Burnley, Kendal, &c.—See the above Mag. & Jour. vol. ii. page 239
	4	Aurora at Lincoln—London Paper
	Dec. 21	At 7 P. M. an auroral arch, Newcastle-upon-Tyne, Mr. R. Potter—See above Journal, page 233

1833. Feb.	11	An aurora—Mr. Peter Clare, Manchester
	18	An aurora—Bl.
	19	An aurora 11 P. M.—Mr. Hadfield, Manchester
Mar.	13	An aurora at Cambridge—Phil. M. Lond. & Edin. vol. ii. page 315
	21	A luminous aurora, Manchester—Phil. M. Lond. & Edin. vol. iii. page 422. Seen in Scotland and Ireland, *ibid.*
Aug.	6	An aurora—R.
Sept.	1	An aurora—Carlisle Journal
	12	An aurora, Greta-bridge—Mr. John Phillips, York
	15	An aurora at Bury, &c.—R.
	17	A fine aurora from 8 to 11, Manchester. Very extensively seen in Great Britain and Ireland—Phil. M. Lond. & Edin. vol. iii. page 461
	18	An aurora inferior to the one last night, Manch.
Oct.	12	A most splendid aurora, seen throughout Great Britain and Ireland, and reported in most of the Newspapers
1834. Jan.	5	An aurora at Catteric-bridge, Yorkshire—R.
Feb.	20	An aurora (supposed) seen about noon at Kendal—Kendal Paper

OMITTED.

1814. Sept.	11	A fine auroral arch in Great Britain and Ireland —A. P. vol. iv. page 362

On the height of the Aurora Borealis.

Ever since the phenomena of the aurora borealis have been observed, the height and distance of the meteor have naturally been subjects of inquiry. Yet, to the no small discredit of Meteorology, there are, at this day, some persons who hold the height of the aurora to be *one thousand miles*, others who hold that *one thousand feet* may be nearer the truth, and others who think its height may, in particular instances, be the one or the other of these extremes, as well as all the intermediate heights.

Some excuse for this diversity of opinion may be adduced, from the circumstance that the phenomena are subject to long periods of intermission. In some periods, they are rarely seen for great part of an age; in other periods, they may be viewed thirty, forty, or fifty times in each successive year, as was the case forty years since, in Great Britain; but when they do occur, it is only one time in ten perhaps in which the phenomena excite universal attention, and afford such data as are more particularly useful for obtaining their height above the earth's surface.

M. de Mairan, a member of the Royal Academy of Paris, in the beginning of the last century, has left us a history of the aurora borealis, from the first records of it up to the year 1732. From this, it appears that sixty-six observations had been made, or rather recorded, betwixt the years 500 and 1716. Since 1716, there has scarcely been a year without a number of these appearances. He records one hundred and sixty-three, which happened between 1716 and 1732. In 1728, there were thirty; in 1724, only two. Dr. THOMSON, in the 4th Vol. of the Annals of Philosophy, page 427, has given an article on the aurora borealis. In this, we find a table of the heights of several auroræ, as estimated by the observers of the last century, most of which are taken from a work of BERGMAN, rarely to be met with I believe in this country. Their heights are very discordant, no doubt owing chiefly to the want of correct *data* for the calculation. They seem mostly to have estimated the heights as being some aliquot part of the earth's radius, as $\frac{1}{4}$, $\frac{1}{5}$, $\frac{1}{6}$, $\frac{1}{8}$, or $\frac{1}{12}$ of the same; that is, from one thousand miles to three hundred, and under. Notwithstanding this, the brilliant auroræ that occur in these times, have just the same appearances as those which were observed a century ago; and the description which Dr. HALLEY gave of the one in 1716, would equally apply to any grand display we have had since, either in this country, or any part of Europe. There is not then any reason, from the optical appearances, to conclude that the auroræ are materially variable in their heights at different times and places.

Though the Astronomers and Mathematicians of the last century failed in ascertaining the exact height of the aurora, they had abundant proof it was far above the height of the atmosphere, supposing that to be only forty or fifty miles. For, in the one of October 19th, 1726, the meteor was seen at the same moment at Moscow, Petersburgh, Warsaw, Rome, Naples, Madrid and Lisbon, and some said at Cadiz, a space of three thousand five hundred miles ; and in all these places it was characterized as an aurora borealis, or light, seen in the *North*. In our own country, we know that an aurora is seen in London, in the Isle of Wight, and at Plymouth, in the magnetic north ; the same aurora is frequently seen in the north at Manchester, Carlisle, Edinburgh, Aberdeen, the Orkneys, and probably in Iceland ; for we are informed by Mr. Henderson, who resided there one winter, (Dr. Brewster's Journal of Science, vol. viii. page 170,) that he had an opportunity of contemplating the aurora borealis almost every clear night. The aurora was strongest in the north and north east ; he only saw it once or twice in the south, and those very faint and stationary. Now the distance between London and Iceland is nearly one thousand miles on the magnetic meridian, and yet Iceland does not seem far enough north to reach the centre of the luminous region. Every one, who has seen an extensive and brilliant aurora in any one of the places just enumerated, must have been struck with the amazing difference in the intensity of the light, according as he was situate on the north or south side of a large building.

The phenomena of the aurora have been already described (page 158). They consist of beams, nearly perpendicular to the horizon, (or rather parallel to the dipping-needle, at the place where they are incumbent,) and rainbow-like arches crossing the magnetic meridian at right angles; the arches seem to precede the beams in point of time, and, soon after the beams are formed, the arches usually disappear, so that the relative situations of an arch, and row of beams originating from it, are not easily determined; it may be presumed the beams either rise from the arches or fall from them: I am inclined to think the former is the case.

Now the arches are generally steady for considerable intervals of time; but they are observed mostly to move along the meridian, either southward or northward, before they vanish; this motion seems to be rather by starts than slowly progressive. If, therefore, two or more observers, situate on the same magnetic meridian, should be so fortunate as to take the altitudes of the highest point of any arch at the same moment of time, the height of the arch above the earth might readily be found by well known rules. The beams, in general, are quite too fugitive to be observed for this purpose.

From the observations, page 68, the altitude of such arch was taken at Kendal and Keswick, and the result of calculation brings out the height one hundred and fifty miles. The base of twenty-two miles is, however, too short to place much reliance upon. A much finer appearance was observed, on the 29th of March, 1826, at various places in

England and Scotland, so as to afford a base of much greater length. I collected the accounts of these observations, and the result of them will be found in the Philosophical Transactions of the Royal Society, for 1828. The height of the arch was found one hundred miles or upwards.

Several auroral arches have been seen since the one from which I deduced the height of the meteor in 1826; but I have not met with better *data* than those which I used on that occasion. Some strictures on that Essay appeared in the Philosophical Transactions for 1829, which I did not think required any animadversion, as far as regarded the author of that paper, supposing that a little more reflection and experience would amply satisfy him. But as persons of established reputation in physical and mathematical science have imbibed notions so very opposite to mine, and so nearly allied to those of the author above alluded to, in regard to the *height* of the aurora borealis, I shall here make a few observations on the subject. I do not however wish it to be understood, that I apprehend those strictures to have influenced their judgment.

After a suitable introduction, Mr. Farquharson adverts to the "numerous observations" he has made on the aurora, and particularly to one observation, which "appears decisive of the question of height." Mr. F. next quotes a paragraph from the Edinburgh Philosophical Journal, vol. viii. page 303, April, 1823. In this he has endeavoured to convey his own ideas on the nature, and constitution of the aurora, namely, "that the aurora borealis has, in all cases, a determinate arrangement of figure, and follows an invariable order in its appearance and progress," &c. So far I think I understand the author, and have nothing material to object to it; but I cannot comprehend the remainder of the description, and therefore cannot say whether Mr. F.'s ideas and mine are the same or not. But as I do not find one word in it as to the height of the phenomenon, or its distance from the surface of the earth, or to its being within, or beyond the limits of the atmosphere, I may presume that the author can accommodate his hypothesis to any one height, to that of one thousand feet, one thousand yards, or one thousand miles above the earth, as circumstances may require.

At page 113 of the Philosophical Transactions, we come to the consideration of height. Mr. F. had inferred, in the essay in 1823, from seeing a phosphorescent light *apparently* emerge from a cloud, that the light really arose from the cloud; and the like on another occasion, in 1825. This last was so clear, that "it was impossible for a spectator to refer the aurora to a distance more remote than that of the mass of clouds; or to believe that the former, and the light of the latter, were not parts of the same phenomenon." This may be thought sound reasoning with some; but to me it is somewhat the same as inferring that the moon, emerging from behind a dense cloud, is actually rising out of the body of the cloud, and is in *reality*, as well as in *appearance*, at the same distance from the spectator as the cloud is. The author next presents us with an aurora, seen on the 22nd of November, 1825, under such a combination of circumstances as, he thinks, seems decisive of this question. Besides the small detached clouds, this evening, there was " another of a quite different character extended along the whole western part of the sky, to about 25° or 30° above the horizon. It was one dense sheet or stratum comparatively with the other clouds, very dark below, waved or furrowed from north to south, and cut off, at its east side, in an apparently straight edge, tending nearly north and south. It was coming on very slowly towards the east, and had, before next morning, prevailed over the other clouds, covering the heavens, and accompanied with a fresh westerly breeze, after a frosty night, which the 22nd of November was. The large sheet of cloud was much more elevated than the small detached ones, as was fully proved by some of the latter being projected in perspective on its dark under surface, and there appearing as white masses fully enlightened by the moon."*

There were two auroral arches that evening, with streamers rising from them and slowly pro-

* This great black cloud which the author describes, was nothing more than the one which is usually seen (or rather imagined) under the low brilliant auroræ, and through which, stars of the first magnitude may be seen. It is a mere contrast of light and shade, and the cloud vanishes when the aurora does.

gressing southward along the magnetic meridian; these, at their eastern ends were abruptly terminated by the black clouds: the streamers came in seeming contact with the edge of this seeming black cloud, "and sometimes I [Mr. F.] conceived they stretched before its eastern edge; but that part being considerably illuminated by the moon, prevented me from being quite positive. Independently, however, of this uncertainty, the appearances are surely decisive of the fact, that the aurora did not extend into the region occupied by the western cloud; and being seen over it at an angle not much higher than its own, occupied therefore a region of nearly equal elevation above the surface of the earth." This proof that the aurora is no higher than the clouds, needs no further comment than has been made. Mr. F. observes the harmony of his results with the famous observation of Capt. PARRY and Lieuts. SCHERER and ROSS, at Port Bowen, of which more in the sequel.

Mr. F. having thus established his hypothesis of arches the height of the clouds, and fringes or streamers rising from them, supposed about the same altitude above the arches as these are above the ground, next proceeds to account for the phenomena detailed in my paper.

He supposes five arches instead of one, with their fringes or streamers arranged as under, namely, one over or near Edinburgh, another near Jedbergh, Hawick and Kelso, another near Cockermouth, Keswick and Whitehaven, another at Kendal, and another at Lancaster,—to which I would add, one over Carlisle, and another about one mile north of Warrington.

I find it necessary now to assume some given height for the arches; I cannot well allow them to be less than one thousand yards from the ground, because that is the height of Skiddaw; and Mr. Crosthwaite, who reported to me a great number of observations on the aurora, never saw one betwixt Keswick and that mountain, though they frequently rested on various parts of the mountain apparently, from its N. W. aspect. To follow up Mr. Farquharson's plan, we must next assume the length of the fringes or streamers to be, suppose another one thousand yards, making the summit of each streamer two thousand yards above the ground: we must also assume the angle which the thickness of the rainbow-like arch subtends at the earth's surface when in the zenith, say 5°. Then the absolute thickness of the ring will be found about eighty-seven yards; if it is moved one mile from the zenith, or (which is the same thing) if we move one mile from the ring, its altitude will be no more than 30°; if two miles, then 16°; if three miles, 12°; if four miles, 8°, &c.; at which great distance it would probably be invisible, only subtending about $\frac{7}{10}$ of a degree. The arch being in the zenith, the streamers over it would be invisible; at one mile distance a row of streamers would be seen running off east and west, till they gradually died away, from distance and apparent diminution of magnitude, the brightest streamer on the arch subtending an angle of 19°, and the rest gradually less and less till they became insensible. At two miles from the arch, the vertical streamers would subtend an angle of 14° ; at three miles, 10°; at four miles, $7\frac{3}{4}$° ; at five miles, $6\frac{1}{3}$° ; and at ten miles, 3°+; and and at twenty miles, $1\frac{1}{2}$° ; but whether they could be visible at this great distance would be doubtful. Every person situate on the south side of one of these arches, and not more than two or three miles from it, would see a beautiful aurora *borealis ;* and every one, two or three miles north of it, would see a beautiful aurora *australis*, the same in every respect as the former person, except its being seen in the south, whilst the other is seen in the north.

Whenever five or six of such arches as these we have been describing, or even one of them, is seen stretching over the country from E. to W., showing to the inhabitants of one narrow slip of land a fine aurora to the north, and to another but contiguous narrow slip, a fine aurora to the south, whilst $\frac{19}{20}$ths of the island see no aurora of any sort, we may conclude Mr. FARQUHARSON'S explanation deserves attention.

It was objected to the observations that there were discrepances in respect to time. No doubt there were; but how could we expect it to be otherwise? Differences of meridians; clocks and watches, some regulated to mean time, some to apparent time, and many of them, perhaps, without any regulation, are sufficient to account for those discrepances.

There are much graver objections to Mr. FARQUHARSON's explanation than that above is to mine. For instance, one arch is to serve for the three towns, Kelso, Hawick and Jedbergh; but Kelso and Jedbergh are six miles different in magnetic latitude; so that an arch in the zenith at one place would only be about 5° alt. at the other.

We come now to the remarkable observation of Captain PARRY, at Port Bowen, lat. 73° 13′ N., long. 88° 55′ W., which Mr. F. and other advocates of low auroræ bring forward as a most unquestionable proof of the truth of their hypothesis: and, in order to do Captain PARRY justice, instead of giving *three lines* only of his account we shall give the whole paragraph. "About midnight, on the 27th of January, [1825,] this phenomenon [the aurora] broke out in a single compact mass of brilliant yellow light, situated about a S. E. bearing, and appearing only a short distance above the land. This mass of light, notwithstanding its general continuity, sometimes appeared to be evidently composed of numerous pencils of rays, compressed as it were, laterally into one, its limits both to the right and left being well defined, and nearly vertical. The light, though very bright at all times, varied almost constantly in intensity; and this had the appearance (not an uncommon one in the aurora) of being produced by one volume of light overlaying another, just as we see the darkness and density of smoke increased by cloud rolling over cloud. While Lieuts. SHERER and ROSS and myself were admiring the extreme beauty of this phenomenon, from the observatory, we all simultaneously uttered an exclamation of surprise at seeing a bright ray of the aurora shoot suddenly downward from the general mass of light, *and between us and the land*, which was there distant only three thousand yards. Had I witnessed this phenomenon by myself, I should have been disposed to receive, with caution, the evidence even of my own senses, as to this last fact; but the appearance conveying precisely the same idea to three individuals at once, all intently engaged in looking towards the spot, I have no doubt that the ray of light actually passed within that distance of us."—Journal of a Third Voyage, &c. page 61.

Captain PARRY had just before, in describing another appearance of an aurora, given his readers a significant note how they were to understand his descriptions: namely, "I am aware that this appearance is usually referred to the effect of viewing the phenomenon in perspective; but I here describe *appearances* only."

Again, at page 170, Captain PARRY observed an aurora on the 20th of September, at sea: "a bright arch of it passed across the zenith from S. E. to N. W., *appearing to be very close to the ship*," &c.

From the above extracts it must be evident that Captain Parry had imbibed a notion that the aurora borealis is generally much nearer the earth's surface than was calculated by all the observers of the last century.—The real distances of luminous objects in the heavens can never be ascertained by their appearances merely. The distance of a spark from a chimney in a dark night, is scarcely to be distinguished from that of a fixed star. We must therefore examine what *data* Captain Parry's observation has afforded us on the 27th of January, from which we are to deduce the height of the aurora, or of the beam that shot down between the observers and the land.

The height of the land, over which the aurora appeared to be, must have been about seven hundred feet, (Mount Cotterell,) and its distance is stated to be three thousand yards; hence it is probable the high land was seen under an angle of about 4°. The body of the aurora "appearing only a short distance above the land" must be understood to mean its angular elevation, or that the lower extremity of the mass of light was elevated a few degrees more than the land; we may presume its elevation to be 6° or 8°. Now the auroral beam might drop down till it seemingly touched the high land, or till it sunk below it 1°, 2° or more.

All depends upon the precision with which these observations were made: if it seemingly touched the land, then its distance from the observer might be either one hundred yards or one thousand miles; if it sunk 1°, 2° or 3° below the summit of the land, then its distance could not exceed two thousand two hundred and fifty, one thousand five hundred, or seven hundred and fifty yards respectively, and might have been half of those distances or less. It is to be regretted that this so favourable an opportunity should have been let slip for want of more precise observation: how far did the beam descend below the mountain? how long did it continue there? did it ascend again, or did it die away there? These are inquiries which every one would wish to make upon such an extraordinary event, but we find no answer. It is evident then that nothing certain, nor even probable, can be obtained as to the real height of this beam; the observation may just as readily be adapted to one extreme of height as to the other, or to any intermediate one.

On the whole, I have very little addition to make to the essay on the aurora borealis, as far as regards its theory given in 1793, in the fourth section, page 167. The aurora consists of luminous arches or rings, drawn round the magnetic poles, in the manner of parallels of latitude around the poles of the earth on a terrestrial globe, and of luminous beams arising from them or amongst them, nearly perpendicular to the surface of the earth, or rather parallel to the dipping needle at the subjacent places. These concentric arches extend to 20°, sometimes 30°, but very rarely to 40° from the magnetic poles: thus, the aurora is seldom seen to the south, in Iceland, which is about 25° from the magnetic north pole; still more rarely in the Orkney and Shetland islands, which are 35° from the pole; and very rarely over the middle of Great Britain and Ireland, which is nearly 40° from the pole: thus, in the list recently given of one hundred and eighty-four auroræ, only five or six arches were seen to pass the zenith, in this country. Auroræ are more numerous in the State of New York than in Britain, because that State is only 30° from the magnetic pole. This fact shows the latitude, or distance from the equator, is not the regulating principle of the aurora, as no aurora has ever been seen, that I am aware of, in Europe, on the parallel of New York. In the years 1828 and 1830, there were one hundred and two appearances of the aurora in the State of New York; in this country we have only registered forty-two.

As auroral arches are seen in Great Britain, Ireland and America at the same time, (page 221, Sept. 29,) it may be presumed the arch extends sometimes uninterruptedly from Europe to America. If the arch be one hundred and fifty miles high, its visible extent, at any one place, from the eastern horizon to the western, will be one thousand eight hundred miles.—(See page 166.) Hence, if an arch is seen from the west of Ireland to descend to the west horizon, and from New York to descend to the east horizon, the parties will see $\frac{2}{3}$ of their distance covered by the arch.

When it is considered that the Icelanders usually see the aurora to the north, and we in Britain on the same magnetic meridian, but 15° further from the pole, see the same aurora to the north, it is an unanswerable argument for its great elevation. If the summits of the beams of the aurora be three hundred miles above the earth in Iceland, they will be visible at Paris, (page 166;) and in Iceland, if the aurora extends 5° beyond the zenith to the south, it may still be characterized there as an aurora borealis or *northern lights*, as the probability will be that more than three fourths of the illumination will be derived from the northern half of the hemisphere.

With regard to the exciting cause of the aurora, I believe it will be found in change of temperature. The phenomenon occurs most frequently in those months when the transitions of temperature are most rapid. In the following Table, the numbers of auroræ seen in each month of the year are arranged according to the sources from which they are derived. The first (1) is the list from page 178, or those observed at Kendal during five years; the second (2) is from the list just before given; the third (3) is MAIRAN's or those observed in Europe before 1732; and the fourth (4) is from the American list of those seen in the State of New York, in 1828 and 1830.

	Jan.	Feb.	Mar.	Apr.	May.	June	July	Aug.	Sept.	Oct.	Nov.	Dec.
(1)	18	18	26	32	21	5	2	21	23	36	38	9
(2)	21	18	23	13	3	2	1	3	35	22	22	21
(3)	21	27	22	12	1	5	7	9	34	50	26	15
(4)	5	6	4	8	10	7	6	14	14	17	5	6
	65	69	75	65	35	19	16	47	106	125	91	51

Total number.............. 764.

From what is now known of electricity, there is great reason to believe that most, if not all, that class of meteors denominated *fiery*, are of electric origin. Thunder takes place in the lower regions of the atmosphere, or amongst the clouds of one or two miles elevation. Lightning, taking place in an atmosphere of great density, is vivid and dazzling in the extreme. The large fire-balls, the *bolides* or smaller ones, and the shooting or falling stars, are found vastly more distant than the thunder; their light is less intense than lightning, and their velocity is measurable; they appear, however, from distant observations on the same meteor, to traverse a high region of the atmosphere, probably from fifty to eighty miles of elevation. The aurora borealis exhibits a light infinitely more attenuated than the other meteors; it may spread over one half of the hemisphere, and not yield more light than the full moon ; this arises from the extreme rarefaction of the air, which is almost tantamount to a Torricellian vacuum ; in fact the light of the aurora exactly corresponds with that of the electric spark, when sent through a tube in which the air has been rarefied to as high a degree as can be effected by a good air-pump.

N. B. I find from the information of a friend, that the explanation of the lengthened sound of thunder, which I have given at page 203, is much the same as was formerly given by Boscovich.—See Priestley's History of Electricity.

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