SYMONS'S

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THE CAUSE OF THE DECREASE OF RAINFALL WITH ELEVATION.

[Although there cannot be two opinions as to the importance of "settling" this question, we feel it to be undesirable that its discussion should occupy so large a portion of our space in future numbers as in the present. Anticipating a somewhat prolonged correspondence, we suggest to our correspondents that no single letter should exceed one page in length, and that not more than three or at the outside four pages, per month, be devoted to this subject.—ED.]

To the Editor of the Meteorological Magazine.

SIR,—Though I am sorry that so experienced a meteorologist as Dr. Burder should not be disposed to accept the conclusions at which I have arrived, I cannot regret that the subject should be thoroughly discussed. I am not unwilling, therefore, to say a few words in reply, more especially as not only my own conclusions are attacked, but the theory according to which all the results of the Rotherham experiments, as well as mine, have been calculated. That theory rests upon an assumption, which some might call an axiom. The assumption is, that the character of the rain is not altered appreciably by any difference in the angle at which it falls, or, in other words, that when falling rain deviates from its original path, the drops are not brought closer together, or the contrary, nor their size thereby increased or diminished. If this be true, Dr. Burder, it seems to me, must be wrong, since it is impossible to deny that a horizontal gauge presents a smaller area of aperture to rain falling obliquely than to that which falls vertically, and if it be the same rain and not altered in density, the gauge must catch less of it. If it be not true, at least approximately, then the calculations hitherto made from the Rotherham experiments must be cast aside as useless—a bold step to take in the face of the very close agreement between calculation and observation exhibited by the results thus obtained.

I do not assert the exact truth of the assumption, but the theory based on it assigns a cause which seems to account, and account adequately, for the facts, and not to be contradicted by any known facts. If it has a fault, it is that it is slightly more than adequate to account for the observed decrease in horizontal gauges, showing apparently some *counteracting* cause. But can as much be said of the "old-fashioned" hypothesis? Does it account for the facts—especially the greater decrease in winter than in summer? Does it not assume what can be disproved—viz., that the temperature of falling rain is almost always below the dew-point of the air near the ground? Is the cause assigned adequate, or even nearly so? And if it were true that the rain-drops gain more than they lose till they reach the ground, would not the beginning of rain dry the air near the ground, by depriving it of moisture, and even warm it by the latent heat given out in condensation? I am much more disposed to believe that, on the contrary, the drops lose volume by imparting moisture to the atmosphere, for it is a matter of common observation that when it begins to rain, the air becomes damp and generally cold.

May I now be permitted to doubt whether Dr. Burder's theory be less beset with difficulties than that on which I have relied? In the first place, clouds do not, I fancy, often discharge rain vertically downwards; but let that pass. Next, it would be a still rarer occurrence for these drops to be deflected to 45°, after falling vertically a certain distance. But admitting all this to be possible, though scarcely representing the ordinary processes of nature, what possible kind of force could "suddenly deflect" to an angle of 45° a body falling vertically, and then make it "maintain that angle," falling, that is, obliquely in a straight line? I should be glad to have this explained, as at present it seems to me that a very extraordinary combination of forces would be required, such as could hardly exist in nature, and it is scarcely desirable to prove or illustrate a theory by impossible suppositions. Next, if the drops are not all of the same size, will not the same force of wind necessarily deflect some more than others, and cause the fall to be spread over a wider space and a longer time?

I should like to hear Dr. Burder's explanation of the results of my "Position Series"—all horizontal gauges carefully kept level—if the angle at which rain falls makes no difference to the amount received on a horizontal surface.

I may point out as a weakness in Dr. Burder's theory, that it would hold good for any angle short of 90°, but would suddenly fail at that point; so that if an inch of rain or snow fell with a very heavy gale at an inclination of 88° or 89°, one gauge might catch the whole inch, while another, tipped over by the wind 1° or 2° from the horizontal, would catch nothing at all.

But if Dr. Burder's proof is sound for horizontal gauges, why not for vertical, or indeed gauges at any angle? Suppose a shower to be falling at an angle of 60° from the vertical, when it meets the upper of two vertical gauges. As it descends the angle diminishes, and it meets the lower gauge at an angle of 45°. By parity of reasoning it follows that the vertical section of the shower intercepted by each gauge would be equal, and the upper gauge catch no more than the lower one. If, therefore, "the quantity of rain received on a horizontal surface does not in theory vary with the angle at which rain falls," neither does the quantity received on a vertical nor any other surface. Certainly the results of my experimental gauges could not in this case have been obtained, and I still think it is "logical" to say that any increase or decrease, from whatever cause, in the density of rain—that is, the amount of it in a given space of air—would affect both vertical and horizontal gauges *in the same way*, proportionately, of course, to the amount which each caught.

On the whole, therefore, I prefer the theory that the ratio of the amount caught by a horizontal gauge varies as the cosine of the angle which the rain makes with the vertical. This gives at first a very slight diminution, only 1½ per cent. in the first 10°, but 6 per cent. for 20°, 13 per cent. for 30°, 30 per cent. for 45°, 50 per cent. for 60°, 100 per cent. for 90°. As the rain falls in summer at a less angle with the vertical than in winter, it is easy to see that the same difference between the angle at which rain falls at different elevations will produce a smaller decrease in summer than in winter in elevated horizontal gauges. This is a point omitted in my paper in *British Rainfall.*

I have thought over Mr. Strachan's remark about the error caused by the oscillation of the vanes to which vertical gauges are attached, but I do not think the error likely to be serious, for several reasons :—(1) The wind itself oscillates, though less than the vane; (2) The greatest oscillations are almost momentary, consequent on gusts of wind, and for nine seconds in every ten, the vane is very nearly true to the wind; (3) It can hardly be supposed that the mouth of a vertical gauge is more than 5° on an average out of the line of the wind's direction, which would, I suppose, diminish the fall not more than one-half per cent.

I fancy the close agreement of Sergeant Arnold's two tilted gauges may thus be accounted for. If the exposure of the 6 ft. gauge is good, the angle of rainfall at 30 ft. will not be very different, perhaps from 5° to 8°. It is likely that the bulk of the rain at Aldershott falls at an angle of about 45°. Now the angle at 6 ft. may be below and that at 30 ft. above 45°, and then no difference could be perceived. But at all times of the year it is probably within 20° or 25° of 45° and generally within 10°. If so, a difference of 5° between the angle at 30 ft. and that at 6 ft. could seldom make a serious difference in the amounts collected by the two tilted gauges. Suppose the angle at 6 ft. 45°, and at 30 ft. 50°, then if the gauge at 6 ft. caught an inch, that at 30 ft. ought to catch 0·996 in.

For a similar reason, in the Rotherham experiments, if the difference between the angle for which the maximum amount is calculated and the angle of the inclined gauge which actually catches most does not exceed 10°, as it seldom does, the gauge ought not to catch less than 98½ per cent. of the calculated amount. In 1870 there was a mean difference of less than 7° between the angles, and the nearest inclined gauge should have caught 0·993 for every inch of (R), the calculated maximum. If it actually caught some 4 per cent. too much, the difference is not serious—far too small to be due to an incorrect theory.

Apologizing for the length of this letter,

I am, Sir, your obedient servant,

FENWICK W. STOW.

To the Editor of the Meteorological Magazine.

SIR,—Notwithstanding your high authority, and large experience, I venture to uphold Dr. Burder's views against your own on the above subject. Dr. Burder does seem to me to have realized the problem, and I think proves that it is not because the rain falls at a greater angle with the vertical the greater the elevation of the gauge, that a horizontal gauge collects less rain the more it is elevated. I venture to support his argument with a mathematical reason. It is certain that all the rain which falls into a horizontal gauge in the same small period of time was contained at any moment in a cylinder of the same altitude, whatever the inclination of the path of the rain-drops. It is a mathematical truth, that the volume of all cylinders on equal bases is the same, if their altitudes be the same, whatever the inclination of the axes of the cylinder to the bases. Therefore, the quantity of rain which falls into a horizontal gauge in the same small period of time must be the same, whatever the inclination of the path of the rain-drops to the vertical. I have used the expression *small* period of time to avoid the necessity of supposing the force of the wind to be uniform, or the path of the rain anything but a straight line. I believe the true explanation of the decrease in the amount of rain collected by elevated gauges, to have been given in a paper by Mr. Jevons, to which you called my attention, in the *London and Edinburgh Philosophical Magazine*, for December, 1861. This explanation put briefly is, that the gauge, causing an obstruction to the passage of the wind, breaks up the wind into eddies around the gauge; that, as the force of the wind is generally greater the greater the elevation, therefore the eddies produce greater disturbance, and carry away some of the rain which would have fallen into the gauge had the gauge been nearer the ground and the disturbances less.

This theory seems to me to receive some confirmation from Mr. Griffith's Table on page 25 of *British Rainfall* for 1870, in which I perceive that the 5-inch gauge at a height of 20 feet always received less than the 8-inch gauge at the same elevation: the disturbance caused by the eddies of the wind would produce a much larger effect on small gauges than on larger ones.

J. M. DU PORT.

Mattishall, Norfolk, 27th May, 1871.

To the Editor of the Meteorological Magazine.

SIR,—The recent experiments at Rotherham and Hawsker seem to show that generally—

1.—An elevated rain gauge collects less than one near the ground.

2.—At the elevated point the path of the rain is more inclined to the vertical than below.

3.—As the inclination of the rain increases, so does the difference between the readings of the two gauges, not according to any law, but quite irregularly.

For an instance of this irregularity, see *British Rainfall*, 1870, p. 19. Rain falls on Nov. 14th and Nov. 24th at nearly the same angle, but on the first day the elevated gauge records 8 per cent. less than the ground gauge, whilst on the second day the difference is only 2 per cent. Again, on Nov. 23rd the angle of inclination of the fall is about half, yet the difference is 5 per cent. The Rotherham experiments give similar evidence that some other cause must be also at work. Dr. Burder, in your last number, has, I think, demonstrated that the greater or less inclination of the path of the rain, produced by the horizontal force of wind, cannot affect the amount actually falling on a horizontal surface. Gravitation alone causes the fall of rain, and will not be affected by a force acting at right angles to it. The amount actually collected in a rain gauge is another matter, as here out-splashing and eddying of the wind in the mouth of the gauge may produce some effect. Now this effect ought to remain constant so long as the cause remains constant. But this is apparently not the case, as is shown by the instance quoted above from Mr. Stow's experiments. Hence there must be yet another cause at work. May this not be that indicated in the old hypothesis, that rain-drops gather volume as they fall? Indeed, must they not do so, if cold themselves, from being precipitated from a cold stratum of air above, they pass through a warm stratum of air near the earth, which is quite saturated with moisture? and in this case will they not gather more volume the more their fall is inclined, for as the inclination of the fall increases, so does the path of descent lengthen?

In the unusual case of the lower warm stratum of air being not saturated, the inverse process would take place, as seems to have happened in the case mentioned on page 23, *British Rainfall*, 1869, where the higher gauge collected the most rain.

Your obedient servant, P. P. PENNANT.

May 29th, 1871.

To the Editor of the Meteorological Magazine.

SIR,—Is Mr. Stow right, or Dr. Burder? The former, as I think; for if otherwise, whence comes that portion of the low-level rain which was never at a high level? I propose this test:—Put a vertical and horizontal gauge at each level, on a day when the wind is steady—*i.e.*, uniform both in direction and in force, all four gauges having equal apertures.

Let (*a*) be weight of rain which would fall through any of the four apertures at the upper level, if placed at right angles to the direction of the rain.

(*b*) the same at the lower level.

(θ) the angle (supposed constant) made by the direction of the rain with the vertical at the upper level.
(θ') the same at the lower level.
(x) the weight of rain in upper horizontal gauge.
(x') „ „ lower „ „
(y) „ „ upper vertical „
(y') „ „ lower „ „

Then whoever is right, the following equations will be nearly true, and would be strictly true, if the wind were perfectly steady, and the instruments accurate and accurately placed :—

$$\left.\begin{array}{l} x = a \cos \theta \\ y = a \sin \theta \end{array}\right\} \text{which give} \left\{\begin{array}{l} a = \sqrt{x^2 + y^2} \\ \tan \theta = \dfrac{y}{x} \end{array}\right.$$

$$\left.\begin{array}{l} x' = b \cos \theta' \\ y' = b \sin \theta' \end{array}\right\} \text{which give} \left\{\begin{array}{l} b = \sqrt{x'^2 + y'^2} \\ \tan \theta' = \dfrac{y'}{x'} \end{array}\right.$$

If Mr. Stow is right, $a = b, \therefore \sqrt{x^2 + y^2} = \sqrt{x'^2 + y'^2}$. Still better would it be to have at each level three gauges—one horizontal, one vertical facing north or south, and one vertical facing east or west—and we could then similarly prove that the square root of the sum of the squares of the weights in the three gauges would be a constant, if Mr. Stow is right.—Yours, &c., J. B. KEARNEY.

P.S. Of course, the above remarks ignore evaporation, the effect of which is greater at the upper level than it is at the lower; both because there is more wind, which promotes evaporation, and also because there is less atmospheric pressure, which hinders evaporation. We might take account roughly of the effect of evaporation, by providing an additional set of gauges at the upper level, to be kept artificially at the same weight of water as the corresponding gauges at the lower level. The vessel out of which the upper level gauges are replenished (positively or negatively) should be weighed immediately beforehand and immediately afterwards. To get a result as free as possible from the effect of evaporation, we must try our experiment in coldish weather, when there is much constant rain, and little wind. Perhaps Mr. Beckley's ingenuity of contrivance, and Mr. Hicks's skilful workmanship, may suffice to furnish us with a kind of compound gauge, having three equal apertures at right angles to each other, and *three separate* compartments for the rain, and yet recording the three weights by *one* machinery. And, meantime, I respectfully suggest to Dr. Burder that he should, in *settled dry* weather, pour equal weights of water into two similar gauges, and leave them as long as the dry weather lasts at two different levels, when I believe he will find that the upper level gauge contains less water than the lower level gauge. To eliminate evaporation altogether seems to me to be a hopeless task, for I cannot conceive of any way of *admitting* water so as to hinder the constant *outflow* (*a fortiori*) of vapor.

To the Editor of the Meteorological Magazine.

SIR,—Although I have scarcely a right to appear again in your Magazine until my first letter has been answered, I hope you will allow me a word of explanation with reference to your editorial suggestion, that I may not have clearly realized the problem under discussion.

The very concise and lucid form in which you yourself re-state the question satisfies me that I have not misunderstood it, at the same time that it shows clearly, in my humble judgment, where the fallacy lies.

As you put it, the "fact observed" is, that horizontally-placed gauges collect less rain at a height; the "inference" or "assumption" is that less rain falls at a height. But can this be legitimately called an "inference" or an "assumption?" What is rainfall if it is not the quantity of rain that falls on the surface of the earth, that is, on a horizontal surface? Suppose Great Britain to be a horizontal rain-gauge, with a wall round the coast at the water-line, representing the rim. If it will make the illustration simpler, suppose the island to be level throughout. Clearly, the "rainfall" of Great Britain will be the quantity of rain that reaches the ground within the wall, irrespective of any consideration of the angle at which it descends. You may, in imagination, tilt up the island with its wall-rim, as you tilt your gauge, to meet the driving rain, and in the one case, as in the other, you will catch a much larger quantity, but it will be no longer the rainfall of Great Britain, that is, it will be no longer the rainfall proper to the area of Great Britain, for it will include a quantity which was destined to fall into the sea, and which would have fallen there, if, by your tilting process, you had not intercepted it. The illustration is on a large scale, and in the tilting does violence to the position of the clouds, whence the rain issues, but, if preferred, a single county may be taken, or a parish, or a square mile, or a field, it matters not. A small area, as a field or a parish, may indeed be naturally tilted, so that with driving rain from a certain quarter, it shall catch more than its share; but this advantage obtained by one field or parish is obtained at the expense of its neighbour on the other side of the slope, and when the rain drives from the opposite quarter the conditions are reversed. In any case, what we want to measure in our rain-gauges is the depth to which the rain would lie on a horizontal surface, if it all lay as it fell; that is, with respect to a tract of country, the depth corresponding to the horizontal area of the tract, and not to its superficies, which may be increased by undulation. This, and this only, is "rainfall" in any intelligible or measurable sense, and the contested "inference" is, therefore, as it seems to me, only a varied expression of the "fact observed."

GEORGE F. BURDER, M.D.

Clifton, 30th May, 1871.

ERRONEOUS MINIMUM TEMPERATURES.

To the Editor of the Meteorological Magazine.

SIR,—In your number for May, it appears that Mr. Casella is annoyed at my remarks upon his Mercurial Minimum Thermometer. This I regret: in a former letter, I stated that I found it most unsatisfactory. I readily admit that it is "a great scientific achievement," the ingenuity of his beautiful arrangement of a supplementary chamber is deserving of all praise, and I believe that the same principle when applied to a mercurial maximum is successful. All that I intended to convey was, that for all practical purposes and for making the usual daily observations, this instrument cannot be recommended for ordinary use. I think everyone that has tried it will come to the same conclusion, and that even Mr. Casella himself would prefer a spirit thermometer to his own delicate instrument. I have used it patiently for a long period, and was often distressed at its inconsistencies. At one time I attached with India rubber bands the tube of an ordinary spirit level to the stem of the thermometer, so as to insure an accurately horizontal position of the instrument, but in the end I did not find it could be depended upon. During all this time there was a spirit minimum alongside for comparison.

As to my acquaintance with the proper method of using minimum thermometers, I am sure I have much to learn on the subject, but having taken some trouble to read all that I could lay my hands on for information, and having purchased several instruments, always from the best makers and at the highest price, the conclusion arrived at is, that spirit thermometers very soon deteriorate, and do not give accurate readings of minimum temperature.

My thermometers are always hung at an angle, the bulb being invariably lower than the upper part of the stem. During the last six weeks, I have firmly screwed to the frame of the minimum in air, a Kew verified mercurial thermometer, so that the two bulbs are quite close to one another. Also, to the grass thermometer I have attached another Kew thermometer, so that when they are laid on the grass, *always in a slanting position*, the two bulbs are nearly in contact. These two couples of instruments are thus plainly under precisely similar circumstances. Every morning the min. temp. is registered, and in a column alongside is placed the difference between the actual temperature of both sets of instruments at the time of observation. The result is, that both minimum spirit thermometers are invariably lower than the corresponding attached mercurial instruments. The error is not uniform, nor does it bear any proportion to the temperature. Are there not grounds here for questioning the trust-worthiness of spirit thermometers? and, also, for the conclusion that we cannot get accuracy until we have a mercurial minimum that can be depended upon. The same ingenuity which has produced the really scientific achievement referred to, ought to overcome the remaining difficulty, and give us observers, what we demand, a *perfect* instrument.

I am, Sir, yours, &c., CHARLETON MAXWELL.

***Leckpatrick Rectory, Strabane, May 20th,* 1871.**

SNOW IN JUNE.

To the Editor of the Meteorological Magazine.

SIR,—On Saturday night the thermometer fell to 35°; Sunday morning, at 9 o'clock, it was 51°, but at 10 a.m. a sharp storm from N.E., of rain, sleet, and snow, reduced it to 41° in a few minutes. It is surprising to me to see vegetation generally bear up so well against such trying weather.—Yours faithfully,

HENRY ST. JOHN JOYNER.

Northwick House, Harrow, Monday, 5th June.

HALOS, &c.

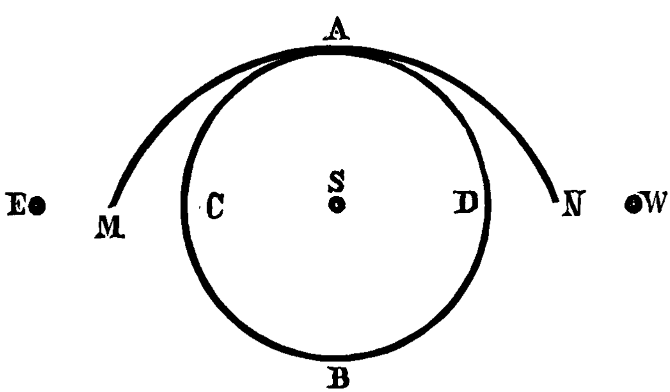
To the Editor of the Meteorological Magazine.

SIR,—I strongly advise everyone who is interested in the subject of halos to buy a copy of the magnificent *Mémoire sur les Halos et les Phénomènes Optiques qui les accompagnent, par* M. A. Bravais, (" Journal de l'Ecole Royal Polytechnique," " Trente Unième Cahier, Tome 18," " à Londres, chez Dulau & Co., 1847.") He adopts the theory that all halos, mock suns, &c., are due to ice prisms, reflecting or refracting the sun's rays. In polar regions, he is no doubt right. In temperate regions, many halos, &c., may be thus accounted for, but, as I think, not all. I have several reasons for my opinion :—

(1) Some phenomena can be accounted for on the supposition of rain-drops, and not on that of ice prisms.

(2) Other phenomena can be accounted for equally on either hypothesis.

(3) These phenomena occur not only in warm countries and in hot weather, but absolutely before the fall and during the fall of *warm* rain. On " 2nd April, 1861," for instance, " at 11.47 and 12.15, the S. wind drove the rain from the halo on me; the colors were red and green." This was on Barham Downs. I made no note to that effect, but I remember that the rain was *not cold.* I was formerly curate of Much Hadham and Perry Green, and resided in Bishop Stortford. On " 4th May, 1862, returning from Perry Green Chapel,



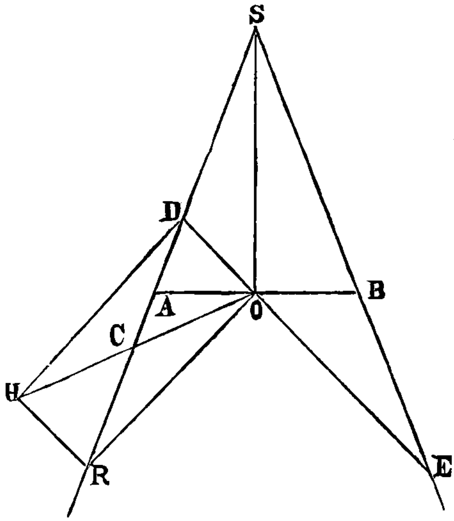
Halos, &c., seen 4th May, 1862, near Bishop Stortford.

I saw the combination of halos and mock suns in the figure, at

1.45 p.m. Colors (*m. s.*) red and green; (*h.*) *r. g.* yellow. A C B D a circular halo about S the true sun; M A N another circular halo touching the former at its highest point A. Two mock suns, E. W., at the same height as S. So far as I could judge E M = M C and D N = N W nearly." The weather (I speak from memory) was *extremely hot.* On "29th June, 1862, at 9.15 to 9.30 a.m., going to Much Hadham, a white horizontal halo through the sun. At first I only saw about one-fourth of it in the north; but when the wind drove the clouds towards the sun, this northern part vanished, and was replaced by a portion on the west, which terminated in the mock sun (3) (23°)" (By this I meant a mock sun at a distance of about 23° from the sun, and in the same direction from it as the figure 3 on the face of a clock is from the centre of the face.) This mock sun was "coloured red and green. The top of the 23° halo round the sun was seen occasionally, also red and green. My children also saw the halo, and said it was white." At Bourton, "28th September, 1868, about 5 p.m., two mock suns, E and W., red, orange; *warm* rain." Bravais's theory would be perfect if we could go up and arrange the ice prisms after our own fancy; putting all those in one region with their axes vertical, all those in another region with their axes horizontal, and all those in a third region with their axes in every possible direction; always supposing that there is a sufficiently large number of ice prisms.

Bravais follows Babinet, and supposes that there are very long and very flat ice crystals, in enormous numbers, in the upper strata of the atmosphere, and that those strata are quite calm, and further that the ice crystals fall slowly and regularly through the air; and he argues that by reason of the resistance of the atmosphere, the axes of the long crystals will be arranged vertically, and those of the flat crystals horizontally. Bravais reasons beautifully and to a certain extent conclusively, moreover he follows up his reasoning by judicious and careful experiments, and he candidly and fully states the views of other scientific men, even when he most differs from them; so that his book of 270 pages is invaluable, nor can anyone expect without it to acquire a sound knowledge of halos, &c. I admire the book enthusiastically, and I shall be extremely happy, for the mere pleasure of doing it, and without any pecuniary remuneration, to translate the whole of it into English, if any one else will pay the expense of printing and publishing it. Without wishing, however, to derogate in the least from a treatise which it would be a high honor to any man to have written, and which it is a great privilege to read, I nevertheless venture to put in a claim on behalf of rain-drops, not as causing all halos, &c., not perhaps as causing the greater number of them, but at least as causing some of them. And, in particular, I claim for rain-drops the power of producing vertical, horizontal, and circumsolar halos, the three white mock suns which have the same height as the sun, and distant from it 90°, 90°, and 180°, and several of the colored mock suns. With your kind permission, I propose to enter more at length into the subject in your pages. Meantime, the following theorem applies as much to horizontal halos as to vertical ones :—

THEOREM.—The base A B of an isosceles triangle S A B is less than any other straight line, D O E, which passes through the middle point O of the base, and is terminated by one side A D S, and the other side produced S B E.



Join S O, make the angle A O R = B O E, complete the parallelogram O H, draw O C H. Then the diagonal H O is bisected in C, and from the triangles A O R, O B E, O R = O E .·. also H D = O E .·. H D + D O = D E, but the two sides H D, D O are greater than H O .·. D E is greater than H O. Again, the angle C A O = O B E, which is greater than the inner and opposite S A B, and S A B is greater than the inner and opposite A C O, much more .·. is C A O greater than A C O .·. also the side C O is greater than A O; but H O = 2 C O & A B = 2 A O .·. also H O is greater than A B; much more .·. is D E greater than A B.

I reserve the application for a future letter.

Faithfully yours,

J. B. KEARNEY.

Bourton, Shrivenham.

THERMOMETER STANDS.

To the Editor of the Meteorological Magazine.

SIR,—Letters have from time to time appeared in your Magazine and other scientific periodicals, complaining of the great discrepancy in thermometrical readings at stations within short distances of each other. Two causes have been assigned for this. One is the probable defect in the instruments themselves; the other, and, in my opinion, the most plausible, is the effect of the sun's rays on the large surface of wood-work presented by the thermometer stand. To obviate this, I would recommend a plan I have for some time adopted, and found to answer admirably. I cover the whole of the exterior surface of that part of the stand exposed to the sun with strong cotton canvas of close texture, keeping it by means of small strips of wood from half-an-inch to an inch above the wood-work, thus permitting the air to play freely between the wood and the canvas. The result is that on a hot summer's day the stand instead of being too hot to touch, is as cool as though placed in the most perfect shade. The construction of my stand (which I some time since sent you a model of, and of which you were pleased to express your approbation), is much better adapted than the stands in general use, for being thus screened. I think if this simple plan was generally adopted we should have less complaints of discrepancy in readings, and certainly fewer records of temperatures of 98° and 100° (!) in the shade. The stand is also protected from upward radiation by a screen of cotton canvas.

Yours truly,
JOHN THRUSTANS, F.M.S.

Merridale, Wolverhampton, May 16, 1871.

REVIEWS.

Commission Hydrométrique et des Orages de Lyon, 1867. 24me. *Année.* Large 8vo., 218 pages, 2 plates.

THIS useful and interesting volume possesses (in addition to its intrinsic merits, which are equal to any of the previous ones), special claims to notice in the fact that it contains the latest contributions to meteorology of the late president of the Commission, the talented and lamented M. Fournet. In addition to the usual meteorological and rain tables, it contains the daily height of the Rhone at Pont Morand, from 1855 to 1865. Daily meteorological observations at Lyons Observatory, at 9 a.m., during 1867, from which we may take two items: (1) that the annual evaporation only amounts to 19 inches; (2) that the density of every fog is indicated by numbers denoting the distance in mètres at which objects were perceptible. We find that Lyons occasionally has fogs which even Londoners would recognize as such, for there are in the year's tables two instances in which objects distant only 50 mètres (say 55 yards) were imperceptible.

The volume also contains an abstract, by M. Delocre, of observations made during the last two centuries as to the density of snow, and the quantity of water which it will yield on melting. The series is not very extensive, but it contains quantities quite as variable as those given in *British Rainfall*, 1865, the range being from 24 to 1 down to 5 to 1. As with our English observers, the equivalent of 12 to 1 occurs oftener than any other, and the mean is 10·36, which closely agrees with the value assigned in the *Meteorological Magazine* for April, 1867.

Passing two short articles, one on the best means of restoring the French rivers to their original fish-yielding condition, and another on some antiquities dug out of the river at Lyons, we come to one by M. Chacornac, on the influence of atmospheric electricity, or of the currents derived from telegraph lines upon the daily movement of a magnetic needle, which, being short and suggestive, we freely translate :—

"Since I have started a magnetic needle at my residence at Villeurbanne, I have convinced myself that the daily variation far exceeds that ordinarily observed. For a long time I asked myself why the variation here should be 1°, while at other observatories it was only one-sixth as much, or from 9′ to 12′. I endeavoured to account for it, and the following is the result at which I have arrived :—

"Firstly, my needle is of small diameter and powerfully magnetized by the voltaic process. Its length is 13·41 inches, and its diameter ·274 inches (say 1 foot long by ¼ inch in diameter), and it is suspended by Gambey's method. It is of steel slightly tempered and highly magnetic. Under the influence of the temperature of a cloudless sky, it immediately undergoes considerable deviations, whilst under a cloudy sky and with a damp atmosphere they are at a minimum. These facts seem to me extremely interesting, and the daily variation being observed by a new magnifying process, I began observing with a second and still lighter needle to see if I could clear up the difficulty. It is well known that in the observatories the magnetic instruments are observed by telescopes from a distance; in my case, I was close to the needle, and the final result was that the needle of small mass was moved in a jerky manner by the presence of the observer. It was subsequently found that the presence of some persons and their movements affected the needle, even when they were 33 feet distant, whence I conclnde that the presence of observers near magnetic needles is incontestably condemned.

"In searching for the cause of these perturbations, I am led to think that the currents derived from telegraph lines accumulating on the nervous systems of certain persons may, by joining induced currents from Ruhmkorff's coils, give rise to real magnetic disturbances, as I have noticed with myself. The large number of these coils in use, spread throughout the atmosphere a quantity of dynamic electricity, which accumulates on the persons of those towards whom the currents are directed. In conclusion, I may draw attention to the singular fact that currents from telegraph lines should arrive and make themselves felt at Villeurbanne-lès-Lyon, although I am unprovided with an inductive coil.

"As it is easy to repeat these observations at any place where it seems desirable, I have hastened to publish these observations."

Concerning this paper we have only one remark to offer. If some persons are so magnetic as to affect the needles at a distance of 33 feet, should not the admission of strangers to magnetic basements be strictly prohibited? Or is it possible that M. Chacornac forgot the keys in his pocket?

The report concludes with three articles, by M. Fournet, on the characteristic features of the storms of South-West France, written with even more than his usual clearness.

MAY, 1871.

Div.		STATIONS. [The Roman numerals denote the division of the Annual Tables to which each station belongs.]	RAINFALL. Total Fall.	Difference from average 1860–5	Greatest Fall in 24 hours. Dpth	Date.	Days on which ·01 or more fell.	TEMPERATURE. Max. Deg.	Max. Date.	Min. Deg.	Min. Date.	No. of Nights below 32° In shade	On grass
			inches	inches.	in.								
I.	ENGLAND.	Camden Town	·93	— 1·47	·36	8	7	79·0	25	35·0	12	0	0
II.		Maidstone (Linton Park)	1·20	— 1·04	·41	19	8	81·0	26	32·0	12‖	2	4
,,		Selborne (The Wakes)	·21	— 2·27	·09	27	4	74·0	25	28·0	12	3	5
III.		Hitchen	·92	— 1·01	·32	25	9	73·0	25	32·0	13	0	...
,,		Banbury	·98	— 1·24	·57	25	5	77·0	24	32·0	14	1	...
IV.		Bury St. Edmunds (Culford)	1·63	— ·53	·87	27	9	78·0	24	29·0	15	2	7
V.		Bridport	·99	— 1·04	·51	24	5	74·0	24	32·0	12	1	...
,,		Barnstaple	·96	— 1·48	·75	24	5	79·0	25*	38·5	12	0	...
,,		Bodmin	·49	— 1·97	·30	24	5	74·0	29	39·0	11§	0	2
VI.		Cirencester	1·70	— ·58	·74	25	4	...	...	...	...	...	...
,,		Shiffnal (Haughton Hall)	1·18	— 1·08	·66	25	8	74·0	24	33·0	11	0	...
,,		Tenbury (Orleton)	·97	— 1·91	·55	25	5	80·4	30	30·0	11	3	6
VII.		Leicester (Wigston)	1·34	— ·78	·80	25	7	80·0	24	30·0	13	2	...
,,		Boston	1·43	— ·51	·85	25	10	77·7	25	35·0	2	0	1
,,		Grimsby (Killingholme)	1·23	..	·76	25	9	67·5	25	35·5	17	0	...
,,		Derby	1·68	— ·48	1·15	25	10	77·0	24	35·0	17	0	...
VIII.		Manchester	2·08	— ·58	...	...	8	...	...	...	...	...	...
IX.		York	1·31	— ·64	·83	25	8	76·0	24	34·0	17	0	...
,,		Skipton (Arncliffe)	2·03	— 1·32	·57	3	11	80·0	29†	25·0	17	2	...
X.		North Shields	1·59	— 1·05	·56	25	13	66·0	22	29·0	17	1	1
,,		Borrowdale (Seathwaite)	2·63	— 6·91	·91	2	9	...	...	...	...	...	...
XI.	WALES.	Cardiff (Town Hall)	...	...	...	...	...	...	...	...	...	...	...
,,		Haverfordwest	·53	— 2·19	·20	24	4	76·6	29	32·3	11	0	1
,,		Rhayader (Cefnfaes)	·98	— 1·87	·50	24	10	77·0	...	30·0	...	3	...
,,		Llandudno	1·19	— 1·19	·59	24	6	74·1	24	37·6	13‖	0	...
XII.	SCOTLAND.	Dumfries	1·03	— 1·36	·33	24	10	78·5	29†	28·0	17	...	...
,,		Hawick (Silverbut Hall)	1·06	...	·23	25	11	...	...	...	...	...	...
XIV.		Ayr (Auchendrane House)	·85	— 2·26	·26	3	12	75·0	24	26·0	17	2	7
XV.		Castle Toward	2·44	— ·95	1·09	25	7	...	...	...	...	...	...
XVI.		Leven (Nookton)	1·30	— ·71	·49	25	10	71·0	30	26·0	17	2	15
,,		Stirling (Deanston)	1·10	— 1·55	·39	3	11	75·0	29	25·0	17	3	9
,,		Logierait	·69	...	·14	24	9	...	...	...	...	...	...
XVII.		Ballater	·95	...	·17	17	7	73·0	22‡	28·5	12	4	...
,,		Aberdeen	·79	...	·17	17	12	68·2	22	32·8	17	0	14
XVIII		Inverness (Culloden)	·45	...	·19	18	5	69·9	24	35·4	17	0	...
,,		Portree	2·06	— 3·59	·45	2	19	...	...	...	...	...	...
,,		Loch Broom	1·11	...	·24	16	11	...	...	...	...	...	...
XIX.		Helmsdale	·74	...	·22	29	12	...	...	...	...	...	...
,,		Sandwick	1·12	— 1·14	·29	16	13	60·0	22	31·8	17	1	4
XX.	IRELAND.	Cork	·65	...	·34	24	...	...	...	...	...	...	...
,,		Waterford	·71	— 1·54	·19	27	10	72·0	29	37·0	17	0	...
,,		Killaloe	·99	— 2·19	·35	24	12	77·0	29	30·0	1	1	...
XXI.		Portarlington	·63	— 2·57	·11	5	14	74·5	30	32·0	16	1	...
,,		Monkstown	·35	— 1·56	·14	2	6	...	...	...	...	...	...
XXII.		Galway	1·28	...	·47	24	12	76·0	31	36·0	1	0	...
,,		Bunninadden (Doo Castle)	1·42	...	·42	24	11	75·0	8	27·0	1, 17	2	...
XXIII		Bawnboy (Owendoon)	...	...	..	...	...	...	...	...	..	..	..
,,		Waringstown	·48	...	·19	24	7	79·0	22	27·0	16	1	...
,,		Strabane (Leckpatrick)	·68	.	·23	24	12	75·0	22	..	...	...	...

* And 30, 31. † And 30. ‡ And 29. ‖ And 16. § And 12.

\+ Shows that the fall was above the average ; —that it was below it.

METEOROLOGICAL NOTES ON MAY.

ABBREVIATIONS.—Bar for Barometer; Ther. for Thermometer; Max. for Maximum; Min. for Minimum; T for Thunder; L for Lightning; T S for Thunderstorm; R for Rain; H for Hail, S for Snow.

ENGLAND.

CAMDEN TOWN.—T S and heavy R from 3.22 to 4 p.m. on 8th, unusually dark at the time; T S from 1.40 to 3.20 p.m. on 27th.

LINTON PARK.—A dry ungenial month, the N. and N.E. winds having retarded vegetation. T, mostly distant, on 8th, 19th, 26th and 27th. Frosts, 12th and 16th, and slight ones on 4th and 5th. Fruit, and many other trees, sadly infested with insects.

HITCHIN.—T on 8th and 27th.

BANBURY.—Strong gust of wind between 3 and 4 p.m. on 8th. [Electric breeze?—ED.] T S on 27th; S on 17th.

CULFORD.—Hail-storm on 1st, and on 27th in a severe T S, during which the L struck and slightly injured the Church of St. John, Bury St. Edmunds.

BRIDPORT.—Very fine but cold month; French beans and potatoes slightly cut by the frost of the 12th.

BODMIN.—Average temp. 2°·8 above the average; average difference between the wet and dry bulb 5°·1; max. diff. 12° on 29th.

HAUGHTON HALL, SHIFNAL.—Very cold for the first three weeks, but, owing to the absence of positive frost, vegetation proceeded quicker than might have been expected; the wheat much improved by the welcome rain of the 25th and 26th. Hollies blossom most abundantly at the beginning of the month; potatoes cut on lower grounds on 11th; hawthorn, a sheet of white, on 18th; oats in full leaf on 19th; seringea blossoms on 28th. Turtle dove arrives on 2nd; orange-tipped butterfly appears on 8th; unusually few queen wasps.

ORLETON.—Very fine and dry, with a low but variable temp. and some frosty nights till the 19th, then warmer to the end with sudden changes from heat to cold. Temp. of month about half a deg. above the average; nearly all the R fell on the 24th and 25th; distant T on 27th.

WIGSTON.—Heavy T S on the 8th, passed over to the N. of us; slight fall of S on the morning of 17th, which continued for three hours; the prospect for fruit is not good.

GRIMSBY.—Polar current scarcely interrupted and nights cold, still the land, both in grass and tillage, is looking remarkably well. Fruit of all kinds scarce. H on 4th; T and L at 7.5 p.m. on 25th; distant T on 27th. Lesser white-throat heard on 6th; garden warbler on 8th. Air very dry on 17th. Old moon visible on 23rd. Monthly rose on 25th.

DERBY.—A very fine month, no frost indicated 5 ft. above ground; refreshing rains, but somewhat below the average. A meteor seen at 9.45 p.m. on 23rd, near the tail of Ursa Major.

ARNCLIFFE.—H on 17th, and hills white with S.

NORTH SHIELDS.—H and T on 26th.

SEATHWAITE.—S on the mountains on the 4th and 17th.

WALES.

HAVERFORDWEST.—The first three weeks cold and ungenial, bright cloudless skies and N.E. winds; the last week bright and very warm, rain much wanted, the driest May during the last 22 years.

CEFNFAES.—Very dry, cold nights and frosts, with wind N.E. and N.W., generally the former. Hills very bare and brown. Hay crops light. Sheep dying of disease in the head.

LLANDUDNO.—On 6th a dense sea fog, from 11 a.m. to 8 p.m. On 24th a heavy shower at 4 p.m.

SCOTLAND.

DUMFRIES.—Drier than usual; the night temp. colder, but day warmer than average, and so the mean is 0°·5 above May of last year. On 17th there was S on the hills and sharp frost, in some places 10° below freezing. Early potatoes cut down. Beech trees and hedges quite brown. T on 26th. Crops looking well.

HAWICK.—A very dry month with much E. wind; hail shower and very keen frost on night of 16th. Cuckoo has not been heard here this season; landrail first heard on 14th.

AUCHENDRANE, AYR.—With bar. pressure and temp. above the May mean, and bar. range below it, the R is the lowest recorded since 1859; the rivers are very low; potatoes, hay, orchards, &c., injured by frost on 17th.

DEANSTON.—Gale of wind from S.E. on 3rd; some R till 6th, then dry and bright; nights cold and frosty, and on 10th, sharp frost, destroying potato leaves, and many young leaves of trees; very dry and bright till the 14th, when, and on the 15th and 17th, some R; sharp frost on 16th and 17th; much E. wind.

LOGIERAIT.—Cold E. winds marked the first part of the month, latterly the temp. has been very high; R wanted; crops looking well; landrail first heard on the 21st.

BALLATER.—R below the average: early part of month dry and parching; sharp frost on the 12th, nipping tender plants; last ten days more seasonable, and vegetation made rapid progress. Cuckoo on 6th, swifts on 25th. Hailstorm on afternoon of 26th.

PORTREE.—Very cold, more or less frost almost every night; gale from S.W. on 3rd. S on 16th and 17th; vegetation backward, trees and shrubs have not got over the frosts of April; fruit trees and bushes are quite brown, and many of them have died.

LOCHBROOM.—The driest May since I have registered, and indeed the driest of any month except March of last year, the difference being ·01. Better weather for tilling the ground the farmer never experienced, nor the hill grazier for his lambs and fleecy flocks.

IRELAND.

MONKSTOWN.—An unusually dry May.

DOO CASTLE.—Month hard and dry; the deficiency of R and prevalence of N. and N.E. winds, have seriously affected vegetation; grazing lands bare of pasture, meadows short and presenting a poor prospect; oats (particularly the late sown crops) in anything but a flourishing condition; potato crop good. Severe frost on 17th; potato tops destroyed.

WARINGSTOWN.—Very dry with N.E. winds; R much wanted.

LECKPATRICK.—Driest May ever registered here, during the last 8 years; every month's rainfall has exceeded 1·00 in. except this May, July 1863, and June 1865. The drought has been very injurious to the flax and grass, both hay and pasture. The rainfall for the first five months of the year is only about 1·00 inch below the average.

THE SUMMER.

To the Editor of the Meteorological Magazine.

SIR,—When April is very wet we often have a summer rainfall in excess of the average, and when May is very dry we frequently have an unsatisfactory season follow, but when the April rainfall at Greenwich is excessive (say 3 inches or more), and the following month is very dry (say has below an inch of rain), we always have an unfavourable season follow. In 1848 (which was a very bad season), the April rainfall at Greenwich was 3·4 in., and in the following month only 0·4 in. was registered at that Observatory. In 1829, the rainfall at the same station was 4·8 in. in April and 0·6 in. in May, and the summer which followed was very cold and bad. In 1809, according to Luke Howard, the rainfall of April, near London, was 3·8 in. and that of May 0·8 in., and the following summer was very cold and unfavourable. In the present year the rainfall of April at Greenwich was 3·03 in. and the rainfall of May, at the same place, 0·68 in. These are all the instances I can find in the present century with reference to the above rule.—Yours, &c., GEORGE D. BRUMHAM.