PHILOSOPHICAL TRANSACTIONS.

I. *On the Relation of Radiant Heat to Aqueous Vapour.* *By* John Tyndall, *F.R.S., Member of the Academies and Societies of Holland, Geneva, Göttingen, Zürich, Halle, Marburg, Breslau, Upsala, la Société Philomathique of Paris, Cam. Phil. Soc. &c.; Professor of Natural Philosophy in the Royal Institution.*

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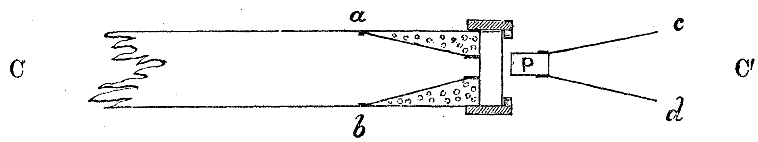
I have already placed before the Royal Society an account of some experiments which brought to light the remarkable fact that the body of our atmosphere, that is to say the mixture of oxygen and nitrogen of which it is composed, is a comparative vacuum to the calorific rays, its main absorbent constituent being the aqueous vapour which it contains. It is very important that the minds of meteorologists should be set at rest on this subject—that they should be able to apply, without misgiving, this newly revealed physical property of aqueous vapour; for it is certain to have numerous and important applications. I therefore thought it right to commence my investigations this year with a fresh series of experiments upon atmospheric vapour, and I now have the honour to lay the results of these experiments before the Royal Society.

Rock-salt is a hygroscopic substance. If we breathe on a polished surface of rock-salt, the affinity of the substance for the moisture of the breath causes the latter to spread over it in a film which exhibits brilliantly the colours of thin plates. The zones of colour shrink and finally disappear as the moisture evaporates. Visitors to the International Exhibition may have witnessed how moist were the pieces of rock-salt exhibited in the Austrian and Hungarian Courts. This property of the substance has been referred to by Professor Magnus as a possible cause of error in my researches on aqueous vapour; a film of brine deposited on the surface of the salt would produce the effect which I had ascribed to the aqueous vapour. I will, in the first place, describe a method of experiment by which even an inexperienced operator may avoid all inconvenience of this kind.

In the Plate which accompanies my former paper, the thermo-electric pile is figured with two conical reflectors, both outside the experimental tube; in my present experiments the reflector which faced the experimental tube is placed *within the latter*, its narrow aperture, which usually embraces the pile, abutting against the plate of rock-salt which stops the tube. Fig. 1 is a sketch of this end of the experimental tube. The

Fig. 1.



edge of the inner reflector fits tightly against the interior surface of the tube at *a b*; *c d* is the diameter of the wide end of the outer reflector, supposed to be turned towards the "compensating cube" situated at C′*. The naked face of the pile P is turned towards the plate of salt, being separated from the latter by an interval of about $\frac{1}{20}$th of an inch. The space between the outer surface of the interior reflector and the inner surface of the experimental tube is filled with fragments of freshly-fused chloride of calcium, intended to keep the circumferential portions of the plate of salt perfectly dry. The flux of heat coming from the source C being converged upon the central portion of the salt, completely chases every trace of humidity from the surface on which it falls.

With this arrangement I repeated all my former experiments on humid and dry air. The result was the same as before. *On a day of average humidity the quantity of vapour diffused in London air produced upwards of* 60 *times the absorption of the air itself.*

It has been suggested to me that the air of our laboratory might be impure; the suspended carbon particles in a London atmosphere have also been mentioned to me as a possible cause of the absorption which I had ascribed to aqueous vapour. With regard to the first objection, I may say that the same results were obtained when the apparatus was removed to a large room at a distance from the laboratory; and with regard to the second cause of doubt, I met it by procuring air from the following places:—

1. Hyde Park.
2. Primrose Hill.
3. Hampstead Heath.
4. Epsom race-course.
5. A field near Newport, Isle of Wight.
6. St. Catharine's Down, Isle of Wight.
7. The sea-beach near Black Gang Chine.

The aqueous vapour of the air from these localities exerted absorptions from 60 *to* 70 *times that of the air in which the vapour was diffused.*

I then purposely experimented with smoke, by carrying air through a receiver in which ignited brown paper had been permitted to smoulder for a time, and drying it

* I here assume an acquaintance with my two last contributions to the Philosophical Transactions, in which the method of compensation is described.

afterwards. It was easy, of course, in this way to intercept the calorific rays; but, confining myself to the lengths of air actually experimented on, I convinced myself that, even when the east wind blows, and pours the carbon of the city upon the west end of London, the heat intercepted by the suspended carbon particles is but a minute fraction of that absorbed by the aqueous vapour.

Further, I purified the air of the laboratory so well that its absorption was less than unity; the purified air was then conducted through two U-tubes filled with fragments of clean glass moistened with distilled water. Its neutrality when dry proved that all prejudicial substances had been removed from the air; and in passing through the U-tubes it could have contracted nothing save the pure vapour of water. *The vapour thus carried into the experimental tube exerted an absorption* 90 *times as great as that of the air which carried it.*

I have had the pleasure of showing the experiments on atmospheric aqueous vapour to several distinguished men, and among others to Professor Magnus. After operating with common undried air, which showed its usual absorption, and while the undried air remained in the experimental tube, I removed the plates of rock-salt from the tube and submitted them to the inspection of my friend. They were as dry as polished rock-crystal, or polished glass; their polish was undimmed by humidity; and a dry handkerchief placed over the finger and drawn across the plates left no trace behind it*.

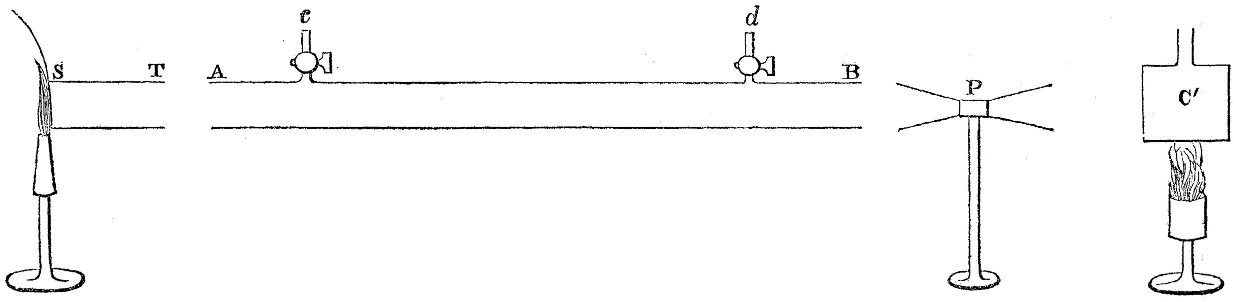
I would make one additional remark on the above experiments. A reference to the Plate which accompanies my two last papers will show the thermo-electric pile standing, with its two conical reflectors, at some little distance from the end of the experimental tube. Hence, to reach the pile after it had quitted the tube, the heat had to pass through a length of air somewhat greater than the depth of the reflector. It has been suggested to me that the calorific rays may be entirely sifted in this interval—that all rays capable of being absorbed by air may be absorbed in the space of air intervening between the experimental tube and the adjacent face of the pile. If this were the case, then the filling of the experimental tube itself with dry air would produce no sensible absorption. Thus, it was imagined, the neutrality of dry air which my experiments revealed might be accounted for, and the difference between myself and Professor Magnus, who obtained an absorption of 12 per cent. for dry air, explained. But I think the hypothesis is disposed of by the foregoing experiments; for here the reflector which separated the pile from the tube no longer intervenes, and it cannot be

* The present Number of the 'Monatsbericht' of the Academy of Berlin contains an account of some experiments executed with plates of rock-salt by Professor Magnus. The plates which stopped the ends of a tube were so far wetted by humid air that the moisture trickled from them in drops. As might be expected, the plates thus wetted cut off a large amount of heat. The experiments are quite correct, but they have no bearing on my results. In the earlier portions of my journal many similar cases are described. In fact, it is by making myself, in the first place, acquainted with the anomalies adduced by Professor Magnus, that I have been able to render my results secure. I may add that the communication above referred to was made to the Academy of Berlin before my friend had an opportunity of examining my rock-salt plates. I do not think he would now urge this objection against my mode of experiment.

supposed that in an interval of $\frac{1}{20}$th of an inch of air an absorption of 12 per cent. has taken place. If, however, a doubt on this point should exist, I can state that I have purposely sent radiant heat through an interval of 24 inches of dry air previous to permitting it to enter my experimental tube, and found the effects to be the same as when the beam had traversed 24 inches of a vacuum.

In confirmation of the results obtained when my tube was stopped by plates of rock-salt, I have recently made the following experiments with a tube in which no plates were used. S is the source of heat, and S T the front chamber which is usually kept exhausted, being connected with the experimental tube at T. This chamber is now left open. A B is the experimental tube, with both its ends also open. P is the thermo-electric

Fig. 2.



pile, the anterior face of which receives rays from the source S, while its posterior surface is warmed by the rays from the compensating cube C′. At *c* and *d* are two stopcocks—that at *c* being connected with an india-rubber bag containing air, while that at *d* is connected with an air-pump.

My aim in this arrangement was to introduce at pleasure, into the portion of the tube between *c* and *d*, dry air, the common laboratory air, or air artificially moistened. The point *c*, at which the air entered, was 18 inches from the source S; the point *d*, at which the air was withdrawn, was 12 inches from the face of the pile. By adopting these dimensions, and thus isolating the central portion of the tube, one kind of air may with ease and certainty be displaced by another without producing any agitation either at the source on the one hand, or at the pile on the other.

The tube A B being filled by the common air of the laboratory, and the needle of the galvanometer pointing steadily to zero, dry air was forced gently from the india-rubber bag through the cock *c*; the pump was gently worked at the same time, the dry air being thus gradually drawn towards *d*. On the entrance of the dry air, the needle commenced to move in a direction which showed that a greater quantity of heat was now passing through the tube than before. The dry air proved more transparent than the common air, and the final deflection thus obtained was 41 degrees. Here the needle stopped, and beyond this point it could not be moved by the further entrance of dry air.

Shutting off the india-rubber bag and stopping the action of the pump, the apparatus was abandoned to itself; the needle returned with great slowness to zero, thus indicating a correspondingly slow diffusion of the aqueous moisture through the dry air within the tube. By working the pump the descent of the needle was hastened, and it finally came to rest at zero.

Dry air was again admitted; the needle moved as before, and reached a final limit of 41 degrees; common air was again substituted, and the needle descended to zero.

The tube being filled with the common air of the laboratory, which was not quite saturated, and the needle pointing to zero, air from the india-rubber bag was now forced through two U-tubes filled with fragments of glass wetted with distilled water. The common air was thus displaced by air more fully charged with vapour. The needle moved in a direction which indicated augmented absorption; the deflection obtained in this way was 15 degrees.

I have repeated these experiments hundreds of times, and on days widely distant from each other. I have also subjected them to the criticism of various eminent men, and altered the conditions in accordance with their suggestions. The result has been invariable. The entrance of each kind of air is always accompanied by its characteristic action. The needle is under the most complete control, its motions are steady and uniform. In short, no experiments hitherto made with solids and liquids are more free from caprice, or more certain in their execution, than are the foregoing experiments with dry and humid air.

The quantity of heat absorbed in the above experiments, expressed in hundredths of the total radiation, was found by screening off one of the sources of heat, and determining the full deflection produced by the other and equal source.

By a careful calibration, repeatedly verified, this deflection was proved to correspond to 1200 units of heat,—the unit being, as before, the quantity of heat necessary to move the needle of the galvanometer from 0° to 1°. According to the same standard, a deflection of 41° corresponds to an absorption of 50 units. From these data we immediately calculate the number of rays per hundred absorbed by the aqueous vapour,

$$1200 : 100 = 50 : 4{\cdot}2.$$

An absorption of 4·2 per cent. was therefore effected by the atmospheric vapour which occupied the tube between the points *c* and *d*. Air *perfectly saturated* on the day in question gave an absorption of $5\frac{1}{2}$ per cent.

These results were obtained in the month of September, and on the 27th of October I determined the absorption of aqueous vapour with the above tube when stopped with plates of rock-salt. Three successive experiments gave the deflections produced by the aqueous vapour as 46°·6, 46°·4, 46°·8. Of this concurrent character are all the experiments on the aqueous vapour of the air. The absorption corresponding to the mean deflection here is 66. The total radiation through the exhausted tube was on this day 1085; hence we have

$$1085 : 100 = 66 : 6{\cdot}1;$$

that is to say, the absorption of the aqueous vapour of the air contained in a tube 4 feet long, was on this day 6 per cent. of the total radiation.

The tube with which these experiments were made was of brass, polished within; and it was suggested to me that the vapour of the moist air might have precipitated itself on the interior surface of the tube, thus diminishing its reflective power, and producing an effect apparently the same as absorption. In reply to this objection, I would remark that the air on many of the days on which my experiments were made was at least 25 per cent. under its point of saturation. It can hardly be supposed that air in this condition would deposit its vapour upon a polished metallic surface, against which, moreover, the rays from our source of heat were impinging. More than this, the absorption was exerted even when only a small fraction of an atmosphere was made use of, and found to be proportional to the quantity of atmospheric vapour present in the tube. The following Table shows the absorptions of humid air at tensions varying from 5 to 30 inches:—

Humid Air.

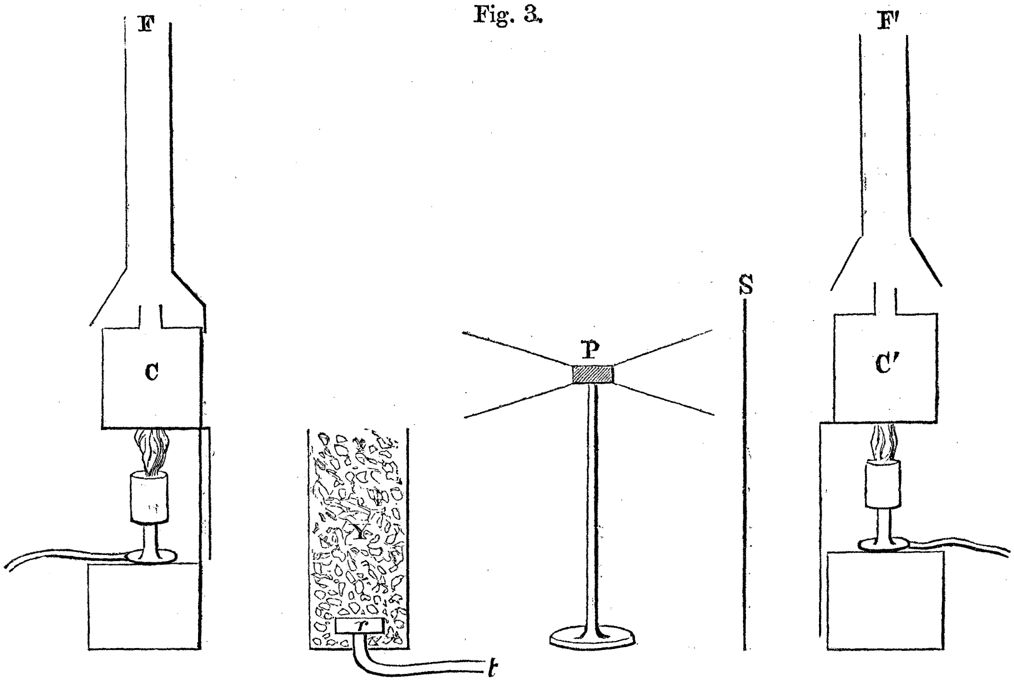
Tensions in inches.	Absorption.	
	Observed.	Calculated.
5	16	16
10	32	32
15	49	48
20	64	64
25	82	80
30	98	96

The third column here is calculated on the assumption that the absorption, within the limits of the experiment, is sensibly proportional to the quantity of matter in the tube. The agreement with observation is almost perfect. It cannot be supposed that results so regular as these, agreeing so completely with those obtained with small quantities of other vapours, and even with small quantities of the permanent gases, can be due to the condensation of vapour on the surface of the tube. When 5 inches were in the tube it had less than one-sixth of the quantity of vapour necessary to saturate the space. Condensation under these circumstances is not to be assumed, and more especially a condensation which should produce such regular effects as those above recorded.

The subject, however, is so important that I thought it worth while to make the following additional experiments:—

C is a cube of boiling water, intended for our source of heat; Y is a hollow brass cylinder, 3·5 inches in diameter and 7·5 inches in depth; P is the thermo-electric pile, and C′ the compensating cube; S is an adjusting screen, used to regulate the amount of heat falling on the posterior surface of the pile. The apparatus was entirely surrounded by boards, the space within being divided by tin screens into compartments which were loosely stuffed with paper or horsehair. The formation of air-currents near the cubes or the pile was thus prevented, and irregular motions of the external air were intercepted. A roof, moreover, was bent over the pile, and this was flanked by sheets of tin. The action here sought I knew must be small, and hence the necessity of excluding every disturbing influence.

Fig. 3.



The cylinder Y was first filled with fragments of quartz moistened with distilled water. A rose burner *r* was placed at the bottom of the cylinder, and from it the tube *t* led to a bag containing air. The bag being subjected to gentle pressure, the air passed upwards amid the fragments of quartz, imbibing moisture from them, and finally discharged itself in the open space between the cube C and the pile. The needle moved and assumed a permanent deflection of 5 degrees, indicating that the opacity of the intervening space to the rays of heat was augmented by the discharge of the saturated air.

The moist quartz fragments were now removed, and the vessel Y was filled with fragments of the chloride of calcium. The rose burner being, as before, connected with the india-rubber bag, air was gently forced up among the calcium fragments and discharged in front of the pile. The needle moved and assumed a permanent deflection of 10 degrees, indicating that the transparency of the space between the pile and source was augmented by the presence of the dry air. By timing the discharges the swing of the needle could be augmented to 20 degrees. Repetition showed no deviation from this result—the saturated air always augmented the opacity, and the dry air always augmented the transparency of the space between the source and the pile.

Not only, therefore, have the plates of rock-salt been abandoned, but also the experimental tube itself, the displacement between dry and humid air being effected in the open atmosphere. The experiments are all perfectly concurrent as regards the action of the aqueous vapour upon radiant heat.

The power of aqueous vapour being thus established, meteorologists may, I think, apply the result without fear. That 10 per cent. of the entire terrestrial radiation is absorbed by the aqueous vapour which exists within ten feet of the earth's surface on a day of average humidity, is a moderate estimate. In warm weather and air approaching to saturation, the absorption would probably be considerably greater. This single fact at once suggests the importance of the established action as regards meteorology. I am persuaded that by means of it many difficulties will be solved, and many familiar effects, which we pass over without sufficient scrutiny because they are familiar, will have a novel interest attached to them by their connexion with the action of aqueous vapour on radiant heat. While leaving these applications to be made in all their fullness by meteorologists, I would refer, by way of illustration, to one or two points on which I think the experiments bear.

And first it is to be remarked that the vapour which absorbs heat thus greedily radiates it very copiously. This fact must, I think, come powerfully into play in the tropical region of calms, where enormous quantities of vapour are raised by the sun, and discharged in deluges upon the earth. This has been assigned to the chilling consequent on the rarefaction of the ascending air. But if we consider the amount of heat liberated in the formation of those falling torrents, the chilling due to rarefaction will hardly account for the entire precipitation. The substance quits the earth as vapour, it returns to it as water; how has the latent heat of the vapour been disposed of? It has in great part, I think, been radiated into space. But the radiation which disposes of such enormous quantities of heat subsequent to condensation, is competent, in some measure at least, to dispose of the heat possessed prior to condensation, and must therefore hasten the act of condensation itself. Saturated air near the surface of the sea is in circumstances totally different from those in which it finds itself in the higher atmospheric regions. Aqueous vapour is a powerful radiant, but it is an equally powerful absorbent, and its absorbent power is a maximum when the body which radiates into it is vapour like itself. Hence, when the vapour first quits the equatorial ocean and ascends, it finds, for a time, a mass of vapour above it, into which it pours its heat, and by which that heat is intercepted and in part returned. Condensation in the lower regions of the atmosphere is thereby prevented. But as the mass ascends it passes through successive vapour-strata which diminish far more speedily in density than the associated strata of air, until finally our ascending body of vapour finds itself lifted above the screen which for a time protected it. It now radiates freely into space, and condensation is the necessary consequence. The heat liberated by condensation is, in its turn, spent in space, and the mass thus deprived of its potential energy returns to the earth as water. To what precise extent this power of aqueous vapour as a radiant comes into play as a promoter of condensation, I will not now inquire; but it must be influential in producing the torrents which are so characteristic of the tropics.

The same remarks apply to the formation of cumuli in our own latitudes. They are the heads of columnar bodies of vapour which rise from the earth's surface and are condensed to cloud at a certain elevation. Thus the visible cloud forms the capital of an invisible pillar of saturated air. Certainly the top of the column, piercing the sea of vapour which hugs the earth, and offering itself to space, must lose heat by the radiation from its vapour, and in this act alone we should have the necessity for condensation. The "vapour plane" must also depend, to a greater or less extent, on the chilling effects of radiation.

The action of mountains as condensers must, I think, be connected with these considerations. When a moist wind encounters a mountain-range it is tilted upwards, and condensation is no doubt to some extent due to the work performed by the expanding air; but the other cause cannot be neglected; for the air not only performs work, but it is lifted to a region where its vapour can freely lose its heat by radiation into space. During the absence of wet winds the mountains themselves also lose their heat by radiation, and are thus prepared for actual surface condensation. We must indeed take into account the fact that this radiant quality of water is persistent throughout its three states of aggregation. As vapour it loses its heat and promotes condensation; as water it loses its heat and promotes congelation; as solid it loses its heat and renders the surfaces on which it rests more powerful refrigerators than they would otherwise be. The formation of a cloud before the air which contains it *touches* a cold mountain, and indeed the formation of a cloud anywhere over a cold tract of land, where the cloud is caused by the cold of the tract, is due to the radiation from the aqueous vapour. The uniformly diffused fogs which sometimes fill the atmosphere in still weather may be due to cold generated by uniform radiation throughout the mass, and not to the mixture of currents of different temperatures. The cloud by which the track of the Nile and Ganges (and sometimes the rivers of our own country) may be followed on a clear morning is, I believe, due to the chilling of the saturated air above the river by radiation from its vapour.

Observation proves the radiation to augment as we ascend a mountain. MARTINS and BRAVAIS, for example, found the lowering of a radiation-thermometer 5°·7 Cent. at Chamouni; while on the Grand Plateau, under the same conditions, it was 13°·4 Cent. The following remarkable passage from HOOKER's Himalayan Journals, 1st edit. vol. ii. p. 407, bears directly upon this point:—"From a multitude of desultory observations I conclude that, at 7400 feet, 125°·7 or 67° above the temperature of the air, is the average maximum effect of the sun's rays on a black-bulb thermometer. . . . . . These results, though greatly above those obtained at Calcutta, are not much, if at all, above what may be observed on the plains of India [because of the dryness of the air.—J. T.]. The effect is much increased with the elevation. At 10,000 feet, in December, at 9 A.M. I saw the mercury mount to 132° [in the sun], with a difference [above the shaded air] of 94°, while the temperature of shaded snow hard by was 22°. At 13,100 feet, in January, at 9 A.M. it has stood at 98°, with a difference of 68°·2, and at 10 A.M. at 114°, with a difference of 81°·4, *whilst the radiating thermometer on the snow had fallen at sunrise to* 0°·7." This enormous chilling is fully accounted for by the absence of aqueous vapour overhead. I never under any circumstances suffered so much from heat as in descending on a sunny day from the so-called Corridor to the Grand Plateau of Mont Blanc. The air was perfectly still, and the sun literally blazed against my companion and myself. We were hip deep in snow; still the heat was unendurable. Immersion in the shadow of the Dôme du Goûté soon restored our powers, though the *air* of the shade was not sensibly colder than that through which the sunbeams passed. Notwithstanding the enormous daily accession of heat from the sun, terrestrial radiation at these altitudes preserves an extremely low temperature at the earth's surface.

Without quitting Europe we find places where, even when the day temperature is high, the hour before sunrise is intensely cold. I have often experienced this even in Germany; and the Hungarian peasants, if exposed at night, take care, even in hot weather, to prepare for the nocturnal chill. The *range* of temperature augments with the dryness, and an "excessive climate" is certainly in part caused by the absence of aqueous vapour.

Regarding Central Australia, Mr. MITCHELL publishes extremely valuable tables of observations, from which we learn that, when the days are at the same time calm and clear, the daily thermometric range is exceedingly large. The temperature at noon being 68° on the 2nd of March 1835, that at sunrise next morning was 20°, showing a difference of 48°. The 7th and 8th were also clear and calm; the difference between noon and sunrise on the former day was 38°, while on the latter it was 41°. Indeed between April and September a range of 40° in clear weather was quite common—or more than double the amount which it is in London at the corresponding season of the year.

A freedom of escape similar to that from bodies at great elevations would occur at any other level were the vapour removed from the air above it. Hence the withdrawal of the sun from any region over which the atmosphere is dry, must be followed by quick refrigeration. This is simply an *à priori* conclusion from the facts established by experiment; but I believe all the experience of meteorology confirms it. The winters in Tibet are almost unendurable from this cause. The isothermals dip deeply from the north into Central Asia during the winter, the earth's heat being wasted without impediment in space, and no sun existing sufficiently powerful to make good the loss. I believe the fact is well established that the desert of Sahara, which during the day is burning hot, is often extremely cold at night. This effect has been hitherto referred in a general way to the "purity of the air;" but purity, as judged by the eye, is a very imperfect test of radiation, for the existence of large quantities of vapour is consistent with a transparent atmosphere. The purity really consists in the absence of aqueous vapour from those so-called rainless districts, which, when the sun is withdrawn, enables the hot surface of the earth to run speedily down to a freezing temperature.

On the most serene days the atmosphere may be charged with vapour; in the Alps, for example, it often happens that skies of extraordinary clearness are the harbingers of rain. On such days, no matter how pure the air may seem to the eye, terrestrial radiation is arrested. And here we have the simple explanation of an interesting fact noticed by Sir JOHN LESLIE, which has remained without explanation up to the present time. This eminent experimenter devised a modification of his differential thermometer, which he called an *Æthrioscope*. The instrument consisted of two bulbs united by a vertical tube, of a bore small enough to retain a little liquid index by its own adhesion. The lower bulb was protected by a metallic coating; the upper or sentient bulb was blackened, and was placed in the concavity of a polished metal cup, which protected it completely from terrestrial radiation. "This instrument," says its inventor, "will at all times during the day and night indicate an impression of cold shot downwards from the higher regions. . . . . But the cause of its variations does not always appear so obvious. Under a fine blue sky the *Æthrioscope* will sometimes indicate a cold of 50 millesimal degrees; yet on other days, *when the air is equally bright*, the effect is hardly 30°." It is, I think, certain that these anomalies were due to differences in the amount of aqueous vapour in the air, which escaped the sense of vision. LESLIE himself connects the effect with aqueous vapour by the following remark:—"The pressure [apparently a misprint for *presence*] of hygrometric moisture in the air probably affects the indications of the instrument." In fact, the moisture opened and closed an invisible door for the radiation of the "sentient bulb" of the instrument into space. The following observation in reference to radiation-experiments with POUILLET's pyrheliometer, now receives its explanation. "In making such experiments," says M. SCHLAGINTWEIT, "deviations in the transparency are often recognized which are totally inappreciable to the telescope or the naked eyes, but which afterwards announce themselves in the presence of thin clouds," &c.

In his beautiful essay on Dew, WELLS gives the true explanation of the formation of ice in India, by ascribing the effect to radiation. I think, however, his theory needs supplementing. Given the same day-temperature here as at Benares, could we, even in clear weather, obtain a sufficient fall of temperature to produce ice? I think not. The interception of the calorific rays by our humid air would too much retard the chill. It is apparent, from the descriptions given of the process, that a dry still air was the most favourable for the formation of the ice. The nights when it was formed in greatest abundance were those during which the dew was not copious. The flat pans used in the process were placed on dry straw, and if the straw became wetted it was necessary to have it removed. WELLS accounts for this by saying that the wetted straw is more dense than the dry, and hence more competent to transfer heat from the earth to the basins. This may be to some extent true; but it is also certain that the evaporation from the moist straw, by throwing over the pans an atmosphere of aqueous vapour, would check the radiation and thus tend to diminish the cold.

MELLONI, in his excellent paper "On the Nocturnal Radiation of Bodies," gives a theory of the *serein*, or excessively fine rain which sometimes falls in a clear sky a few moments after sunset. Several authors, he says, attribute this effect to the cold resulting from radiation of the air during the fine season immediately on the departure of the sun. "But," writes MELLONI, "as no fact is yet known which distinctly proves the emissive power of pure transparent elastic fluids, it appears to me more conformable to the principles of natural philosophy to attribute this species of rain to the radiation and subsequent condensation of a thin veil of vesicular vapour distributed through the higher strata of the atmosphere"*. Now, however, that the power of aqueous vapour as a radiant is known, the difficulty experienced by MELLONI disappears. The former hypothesis, however, though probably correct in ascribing the effect to radiation, was incorrect in ascribing it to the radiation of "*the air*."

Dr. HOOKER encourages me to hope that this newly discovered action may throw some light on the formation of hail. The wildest and vaguest theories are afloat upon this subject. But the same action which produces *serein* must, if augmented, freeze the minute rain, and the aggregation of the small particles thus frozen would form hail. I cannot think the hail that I have had an opportunity of examining to be due to the freezing of drops of water, each hailstone being merely the ice of the drop. The "stones" are granular aggregates, the components of which may, I think, be produced by the chill of radiation. I will not, however, dwell further on this subject, but will now commit the entire question to those who are more specially qualified for its investigation.

* TAYLOR's Scientific Memoirs, vol. v. p. 551.