the camera to any degree of altitude. These supplementary boards are then pivoted at the centre of part of a divided circle, previously inlaid in the wood at the extremities of the base line, in such a manner that a line passing through the axis of the lenses would cut the pivots. The cameras thus furnished can be adjusted with ease to any vertical or horizontal angle. These angular adjustments of the two instruments must always coincide, with the slight exception that the horizontal ones must make internal angles with the base included between them, or, in other words, the lenses of both require to be directed to a point opposite to the centre of the base line.

The cameras also require their rapid exposing shutters to be electrically connected, to ensure the pair of sensitive plates being impressed at the same instant, and each dark slide employed to have a fine wire strained at its centre from top to bottom immediately in front of the prepared plate, and as close as possible to it without touching. The transparent lines produced in the developed negatives by these wires will constitute the zero of distance of any pair, and during the operation of reading off must be made to agree with similar ones on the scale of measurements obtained as follows:—

Upon a large cardboard rule a number of squares in fine black lines, one inside the other, and each one slightly out of the centre of its predecessor to the right hand, the outside square being then divided with a line at a tenth part of its diameter to the left of its centre. This line will indicate the zero of the scale. After placing a distinguishing mark or number in the corner of every square for purposes of identification, the cardboard will be ready to be photographed and reduced at the same time to the intended size of the cloud negatives. Two transparent positives copied from this and observed when placed side by side in a suitable stereoscope with the edges representing the left-hand one of the cardboard together, will appear in that instrument with the lines composing the zero only a few inches away, and the squares as a succession of vertical planes commencing some distance from that and receding from the eye in the order of greater to less, each one representing its own distance in space.

To find the value of these distances it will be necessary to focus the two cameras upon some terrestrial objects whose distances can be measured by any of the known methods, and negatives taken. The two resulting landscapes, when placed in the stereoscope, each superimposed face to face upon its respective scale, and the fine vertical lines of the whole made to occupy one apparent distance, an operation offering but little difficulty, every object or point of the landscape will be found to stand out in the vertical plane suited to its own distance, the relation between them being noted for the values found by measurement of the one to be marked upon the other. As a scale prepared thus would be of no value for any other angle at which the cameras might be placed, it would be most convenient to make use of two or three angles only, more being quite unnecessary, and prepare a scale for each, or one with a reference table of values for the respective angles would suffice. Again, in respect of altitudes. As the terrestrial measurements would only be absolutely accurate for those of clouds in the zenith, or of them, if it were possible, from the earth's centre in any direction, the tables of reference would have to include calculated corrections for altitude, or the graduations could be valued for the most useful degrees by experimental means.

It will be gathered from the above that the constancy of length of the base line can be ascertained, and corrected if necessary, by taking a couple of views of the same landscape for comparison with the preceding pair; slight fluctuations of length would not however be of much consequence in dealing with the comparatively coarse measurements of thick masses of cloud floating in so short a distance as the few miles of atmosphere capable of forming them consists.

To ascertain the height of clouds photograph a pair of negatives, and place these in the stereoscope with a pair of scale plates agreeing with the angle at which they were taken, and adjust as for the landscapes described above. The data required may then be read off by noting the vertical plane each stratum occupies.

Prints of these negatives should afterwards be made for the particulars of height, direction of motion of the respective layers, point of compass, wind rate, state of barometer, thermometer, and general remarks upon the weather, to be recorded upon them for comparison or circulation.

Meteorological observatories fitted with such an addition to their present splendid collection of instruments would have their powers of dealing with the atmosphere and weather changes greatly reinforced. JOHN HARMER

Wick, near Arundel

Correction of an Error in "Island Life"

MY friend Dr. Günther has kindly called my attention to an extraordinary error at p. 322–323 of my "Island Life," where I state that the Loch Killin Charr (*Salmo Killinensis*) inhabits a lake in Mayo County, Ireland; instead of a small lake in Inverness-shire, 2000 feet above the level of the sea, as given in Dr. Günther's original description in the *Proceedings* of the Zoological Society, 1865, p. 698. On referring to my MSS. notes for this part of my work, I find that the habitat was first correctly given, but subsequently scored out and altered to the erroneous Irish locality! Why this was done I cannot now discover; and I can only regret that I should have fallen into so palpable an error, and request such of the readers of NATURE as possess my book to make the necessary alterations.

ALFRED R. WALLACE

Natural Science for Women

WILL you allow me to supplement your kindly reference to the instruction in physical science given to women in Bedford College, London, by the statements that for the last two sessions a class in biology has been conducted there by Mr. Charles Stewart of St. Thomas's Hospital Medical School. The course of study is in every sense a practical one, with special reference to the Preliminary Scientific and First B.Sc. examinations at the University of London, and the best testimonial to the excellence of the instruction in these various subjects is furnished by the remarkable success during the present year of the Bedford College pupils at the University examinations, a success not less marked in the Science than in the Arts examinations.

ALFRED W. BENNETT

Movements of Leaves

A YEAR ago we had in our conservatory a healthy young plant of *Acacia mollissima*. It bore no flowers, but consisted of a simple axis adorned with the soft feathery leaves of its genus, which closed up at night. Our gardener however thought it would improve in appearance if it could be made to bear a few branches; and with that view he cut it back. His end was achieved: a new stem shot up from the section, and graceful limbs were thrown out in turn by it. But along with this a strange result followed: the fresh leaves borne by the new stem and by the branches now closed at night, while the old leaves below the section ceased to do so. These lower leaves have long since fallen off, but the upper ones kept to their habit, and at the present time all fold up at dusk save a few of the very oldest, which only partially shut, or, in one case, do not shut at all. When our plant was cut back it stood three feet high; now it stands seven: which shows that the vigour of the plant as a whole in no wise diminished by the operation.

Chislehurst, December 23 M. L. ROUSE

ON DUST, FOGS, AND CLOUDS[1]

DUST, fogs, and clouds seem to have but little connection with each other, and we might think they could be better treated of under two separate and distinct heads. Yet I think we shall presently see that they are more closely related than might at first sight appear, and that dust is the germ of which fogs and clouds are the developed phenomena.

This was illustrated by an experiment in which steam was mixed with air in two large glass receivers; the one receiver was filled with common air, the other with air which had been carefully passed through a cotton-wool filter and all dust removed from it. In the unfiltered air the steam gave the usual and well-known cloudy form of condensation, while in the filtered air no cloudiness whatever appeared. The air remained supersaturated and perfectly transparent.

The difference in the behaviour of the steam in these two cases was explained by corresponding phenomena,

[1] Abstract of a paper read to the Royal Society of Edinburgh, December 20, by Mr. John Aitken. Furnished to NATURE by the Council of the Society.

in freezing, melting, and boiling. It was shown that particles of water vapour do not combine with each other to form a cloud-particle, but the vapour must have some solid or liquid body on which to condense. Vapour in pure air therefore remains uncondensed or super-saturated, while dust-particles in ordinary air form the nuclei on which the vapour condenses and forms fog or cloud-particles.

This represents an extremely dusty condition of the air, as every fog and cloud-particle was formerly represented by a dust-particle, which vapour by condensing upon it has made visible. When there is much dust in the air but little vapour condenses on each particle, and they become but little heavier, and easily float in the air. If there are few dust specks each gets more vapour, is heavier, and falls more quickly.

These experiments were repeated with an air-pump, a little water being placed in the receiver to saturate the air. The air was then cooled by slightly reducing the pressure. When this is done with unfiltered air a dense cloudiness fills the receiver, but when with pure air no fogging whatever takes place, there being no nuclei on which the condensation can take place. In this experiment, and in the one with steam, the number of cloud-particles is always in proportion to the dust present. When the air is nearly pure and only a few dust-particles present, then only a few cloud-particles form, and they are heavy and fall like fine rain.

The conclusions drawn from these experiments are: (1) that whenever water vapour condenses in the atmosphere it always does so on some solid nucleus; (2) that dust-particles in the air form the nuclei on which the vapour condenses; (3) that if there was no dust there would be no fogs, no clouds, no mists, and probably no rain, and that the supersaturated air would convert every object on the surface of the earth into a condenser on which it would deposit; (4) our breath when it becomes visible on a cold morning, and every puff of steam as it escapes into the air, show the impure and dusty condition of our atmosphere.

The source of the fine atmospheric dust was then referred to, and it was shown that anything that broke up matter into minute parts would contribute a share. The spray from the ocean, when dried and converted into fine dust, was shown to be an important source. Meteoric matter also probably contributed a proportion. Attention was then directed to the power of heat and combustion as a source of this fine dust.

It was shown that if there is much dust then each particle only gets a little vapour condensed upon it, that when the particles are numerous they become but little heavier, and easily float in the air, and give rise to that close-packed but light form of condensation which constitutes a fog, and therefore whatever increases the amount of dust in the air tends to increase fogs, and that when the dust-particles are not so numerous the cloud-particles are larger and settle down more quickly.

It was shown that by simply heating any substance, such as a piece of glass, iron, brass, &c., a cloud of dust was driven off, which, when carried along with pure air into the experimental receiver, gave rise to a dense fog when mixed with steam. So delicate is this test for dust that if we heat the one-hundredth of a grain of iron wire the dust driven off from it will give a distinct cloudiness in the experimental receiver, and if we take the wire out of the apparatus and so much as touch it with our fingers and again replace it, it will again be active as a cloud-producer. Many different substances were tried, and all were found to be active fog-producers. Common salt is perhaps one of the most active.

Heat, it is well known, destroys the motes in the air, and it might be thought that flame and other forms of combustion ought to give rise to a purer air. Such however is not the case. Gas was burned in a glass receiver, and supplied with filtered air for combustion, and it was found that the products of combustion of pure air and dustless gas gave rise to an intensely fog-producing atmosphere. It may be mentioned here that the fog-producing air from the heated glass, metals, and burning gas were each passed through the cotton-wool filter, and the air was in all cases made pure, and did not give rise to cloudiness when mixed with steam

It will be seen that it is not the dust motes which are revealed to us by a beam of sunlight when shining into a darkened room, that form the nuclei of fog and cloud-particles, as these may be entirely removed by heat, and yet the air remain active as a cloud-producer. The heat would seem to break up the larger motes which reflect the light into smaller and invisible ones. When speaking of dust, it is to these infinitesimally small and invisible particles we refer. The larger motes which reflect the light will no doubt be active nuclei, but their number is too small to have any important effect.

It is suggested, and certain reasons are given for supposing, that the blue colour of the sky is due to this fine dust.

Other experiments were made to test the fog-producing power of the air and gases from different sources. The air to be tested was introduced into the experimental receiver and mixed with steam, and the relative densities of the fog produced were noted. It was always found that the air of the laboratory where gas was burning gave a denser fog than the air outside, and that the air outside varied, giving less fog during wet than during dry weather. The products of combustion of gas burned in a Bunsen flame, a bright flame, and a smoky flame, were all tested and found to be about equally bad, and all much worse than the air in which they were burned. Products of combustion from a clear fire and from a smoky one gave about equal fogging, and both much worse than the air of the room.

Experiments were made by burning different substances. Common salt when burned in a fire or in alcohol flame gave an intensely fog-producing atmosphere, but burned sulphur was the most active substance experimented on. It gave rise to a fog so dense it was impossible to see through a thickness of 5 cm. of it.

The vapours of other substances than water were tested to see if they would condense in the cloud form without nuclei on which to deposit. All the substances experimented on, which included sulphuric acid, alcohol, benzole, and paraffin, only gave a cloudy condensation when mixed with ordinary unfiltered air, and remained perfectly clear when mixed with filtered air, all these acting like water vapour.

Before referring to fogs, which have now become so frequent and aggravated in our large towns, it was pointed out that caution was necessary in applying the results of the experiments.

The conditions of a laboratory experiment are so different, and on so small a scale, that it is not safe to carry their teaching to the utmost limits and apply them to the processes which go on in nature. We may, however, look to the experiments for facts from which to reason, and for processes which will enable us to understand the grander workings of nature.

It having been shown that vapour, by condensing on the dust-particles in the air, gives rise to a fogging, the density of which depends on the amount of fine dust in the air; the more dust the finer are the fog-particles, and the longer they remain suspended in the air. It having been also shown that all forms of combustion, perfect and imperfect, are producers of fog nuclei, it is concluded that it is hopeless to expect that, adopting more perfect forms of combustion than those at present in use, we shall thereby diminish the frequency, persistency, or density of our town fogs. More perfect combustion will, however, remove the pea-soup character from the fogs and make them purer and whiter, by preventing the smoke which at present mixes with our town fogs and aggravates their character, and prevents them dissolving when they enter our rooms. Smoke descends during a fog, because the smoke particles are good radiators, and soon get cooled and form nuclei on which the water vapour condenses. The smoke thus becomes heavier and falls. This explains why falling smoke is often a sign of coming rain. It indicates a saturated condition of the atmosphere.

Sulphur when burned has been shown to be an intensely active fog-producer. Calculation shows that there are more than 200 tons of sulphur burned with the coal every winter day in London, a quantity so enormous as quite to account for the density of the London fogs. It is suggested that some restriction ought to be put on the amount of sulphur in the coal used in towns.

Before utterly condemning the smoke and the sulphur, it was pointed out that it would be necessary thoroughly to investigate and fully to consider the value of smoke as a deodoriser, and also the powerful antiseptic properties of the sulphurous acid formed by the burning sulphur. The air during fogs is still and stagnant. There is no current to clear away the foul smells and deadly germs that float in the air, which might be more deadly than they are, were it not for the suspended soot and burned sulphur. We must therefore be on our guard lest we substitute a great and hidden danger for an evident but less evil.

ON THE SPECTRUM OF CARBON

ALTHOUGH fifteen years have passed since the possibility of one substance possessing more than one spectrum was first suggested by Plücker and Hittorf, the question of the existence of double spectra cannot yet be considered as decided. One of the elements to which multiple spectra have been attributed is carbon, which was at one time supposed to possess four different spectra: of these one has been shown to be due to oxide of manganese, a second to oxides of carbon, the origin of a third (obtained only from oxides of carbon) has hardly been discussed (though it may prove to be one of the true carbon spectra), and the other "carbon" spectrum—the best known of all—is the one first attributed to carbon by Attfield, but ascribed to acetylene by Ångström.

In a paper read before the Royal Society, and of which an abstract is given in NATURE, vol. xxii. p. 620, Professors Liveing and Dewar describe experiments to prove that this spectrum is that of a hydrocarbon, and not of carbon itself; and also that certain blue bands, best seen in the flame-spectrum of cyanogen, are due to compounds of carbon and nitrogen, and not to carbon itself. They attribute to hydrocarbon (amongst others) the yellowish-green group, which we will call γ, of wave-lengths from about 5635 to 5478, and the emerald-green group, which we will call δ, of wave-lengths from about 5165 to 5082; and they attribute to nitro-carbon the two blue groups of wave-lengths 4600 to 4502 and 4220 to 4158, which we will call θ and ζ respectively.

As these result are directly opposed to my own experience, I have thought it necessary to repeat two of the experiments described in my paper on the carbon spectra in the *Philosophical Magazine* for October, 1869, under such conditions as to exclude (as far as lay in my power) all trace of hydrogen in the one case, and of nitrogen in the other.

The difficulty of supposing carbon to be present in the state of vapour at any temperature which we can command seems to be the chief reason why so many investigators think it necessary to attribute the spectrum in question (with experimental evidence or without it) to compounds of carbon. I am not aware that Ångström ever gave any experimental proof of his assertion that this spectrum was caused by acetylene.

On the other hand, the evidence that the spectrum is due to carbon is that first stated by Attfield, that if these lines "are absent in flames in which carbon is absent, and present in flames in which carbon is present," if they are "observable equally in the flame of the oxide, sulphide, and nitride as well as in the hydride of carbon," and if "present whether the incandescence be produced by the chemical force, as in burning jets of the gases in the open air or by the electric force, as when hermetically-sealed tubes of the gases are exposed to the discharge of a powerful induction-coil," then they "must be due to incandescent carbon vapour"; and if this is borne out by experiment the conclusion that the lines are due to carbon (as gas, liquid or solid) cannot be resisted, whatever may be the apparent impossibility of volatilising or even liquifying carbon, even by the most powerful current of electricity directed through it.

We must bear in mind how very small a quantity of a gas is often sufficient to give us a spectrum, and when the carbon spectrum is obtained by the decomposition of olefiant gas or cyanogen by passing sparks through the gas, the carbon certainly exists as gas in the compound which is decomposed, and before the liberated atoms unite together to form the molecules of the solid, there is surely no impossibility in their existing for the moment as gas—as gaseous carbon.

On an examination of Professors Liveing and Dewar's paper to ascertain the experimental evidence upon which the bands γ and δ are attributed to hydrocarbon and not to carbon itself, we find it stated that "the green and blue bands characteristic of the hydrocarbon flame seem to be always present in the arcs, whatever the atmosphere. This is what we should expect if they be due, as Angström and Thalèn suppose, to acetylene, for the carbon electrodes always contain, even when they have been long heated in chlorine, a notable quantity of hydrogen."

Since then it is impossible to completely expel hydrogen from the carbon-poles, we must reject all the experiments in which the electric arc was observed in atmospheres of different gases, although "the green and blue hydrocarbon bands were seen more or less in all of them."

Turning then to other methods of producing the spectrum, we find it stated that in the flame of carefully-dried cyanogen "the hydrocarbon bands were almost entirely absent" (they should have been *entirely* absent); "only the brightest green band was seen, and that faintly." Hence we are to infer, I suppose, that the bands γ and δ, so brilliant in the flame of cyanogen in air or oxygen, are due to the accidental presence of hydrogen (see the extract from Morren's paper, NATURE, vol. xxii. p. 7. Dibbits also speaks of this spectrum as "by far the most magnificent" he has seen).

Next we have the experiment of burning hydrocyanic acid, in which, as we have hydrogen present, we expect to find the hydrocarbon bands brilliantly developed. But we find the result stated as "very much the same as that of cyanogen." The flames of hydrogen and sulphide of carbon, and of hydrogen and carbonic oxide, do not give the hydrocarbon bands (their spectra being continuous); a mixture of hydrogen and carbon tetrachloride gives them faintly, and a mixture of hydrogen and chloroform gives them strongly.

In all this we have no *proof* of the point in question, nor even any special probability that the bands are due to hydrocarbon; and yet, in the face of experiments in which the spectrum is obtained from cyanogen, when care has been taken to exclude hydrogen, we are asked to attribute the bands to the hydrocarbon formed by combination with some trace of hydrogen (as water or otherwise), supposed to be present as impurity. In the same way the presence of the bands θ and ζ obtained under circumstances when nitrogen has been intentionally excluded, is to be explained by "the extreme difficulty of