

the freezing-point; and if we take the area of the water as about equal to that of the land, we shall have heat, enough to raise the whole Arctic ocean to a depth of full 180 feet more than 20° F., or to a mean temperature of 52° F., and as this would imply a still higher surface temperature it is considerably more than I require.

Unless therefore Prof. Houghton can prove that the amount of ice now forming *annually* in the Polar regions is *very much more* than an average of five feet thick over the whole area, his own figures demonstrate my case for me, since they prove that the rearrangement of land and sea which I have suggested would produce a permanent mild climate within the Arctic circle and proportionally raise the mean temperature of all north-temperate lands.

Briefly to summarise my present argument:—Prof. Houghton's fundamental error consists in assuming that the true way of estimating the amount of heat required in order to raise the temperature of the Polar area a certain number of degrees is,—first, to suppose an accumulation of ice indefinitely *greater* than actually exists, and then to demand heat enough to melt this accumulation *annually*. The utmost *possible* accumulations of ice in the Arctic area, during an indefinite *number of years*, and under the most *adverse physical conditions* imaginable, are to be all melted in *one year*; and the heat required to do this is said to be the "accurate measure" of that required to raise the temperature of the same area about 20°, at a time when there were no such great accumulations of ice and when all the physical conditions *adverse* to its *accumulation* and *favourable* to its *dispersal* were immensely more powerful than at present!

When this fundamental error is corrected, it will be seen that Prof. Houghton's calculations are not only quite compatible with my views, but actually lend them a strong support.

ALFRED R. WALLACE

By the courtesy of Mr. Ingram I am enabled to say that the tree at Belvoir supposed to be *Araucaria Cunninghami* is in reality, as surmised by Capt. King, *Cunninghamia sinensis*. The *Cunninghamia* is a native of Southern China, whence it has been introduced into Japan. In this country it was originally grown under glass, but, as the instance at Belvoir illustrates, such protection is not absolutely requisite. The tree is however somewhat tender, and so far as I know has never produced its cones in this country in the open air.

As to the Bamboos hardy in this country, it may be well to warn those who are not familiar with the plants not to expect to see the gigantic and rapidly-growing grasses that go under this name in the tropics. Rarely indeed do they attain in this country the dimensions even of the *Arundo donax*, so familiar to travellers in Italy. As accuracy of nomenclature is proved in this and the foregoing instance to be a matter of much moment, it may be well to say on the authority of the late General Munro that the Himalayan plant commonly grown in gardens as *Arundinaria falcata* is more correctly called *Thamnocalamus Falconeri*, that the *Bambusa gracilis* of gardens is the true *Arundinaria falcata* of the Himalayas, and that the Japanese *Bambusa metabé* is *Arundinaria japonica*. General Munro's monograph of this group is to be found in the twenty-sixth volume of the *Transactions* of the Linnean Society, part 1, 1868, while his remarks on the cultivated species may be found in recent volumes of the *Gardeners' Chronicle*, particularly in vol. vi. 1876, p. 773.

The simultaneous flowering of *Thamnocalamus Falconeri* a few years ago in all parts of Europe created much attention, and was indeed a remarkable illustration of hereditary tendency manifested under very varied climatal conditions. The flowering of this grass was by no means looked on with unmixed gratification, as it entailed as a consequence the death or protracted enfeeblement of the plant.

A visit to Kew or to any of our larger nurseries will suffice to show that there are other Bamboos (that is, grasses belonging to the group *Bambuseae*, if not true *Bambusas*) which are hardy enough to withstand even such rigorous winters as those of 1878-9 and 1879-80.

MAXWELL T. MASTERS

Climate of Vancouver Island

THE letters on this subject which have appeared in NATURE (vol. xxiii. pp. 147, 169), have reminded me of a "Prize Essay on Vancouver Island. By Charles Forbes, Esq., M.D., M.R.C.S. Eng., Surgeon Royal Navy," which was published by the Colonial Government in 1862. It consists of sixty-one

closely-printed octavo pages and eighteen pages of Appendix; the latter containing several Tables on the Meteorology of the Colony.

The following is a portion of the "Abstract of Meteorological Observations, taken at the Royal Engineer Camp, New Westminster, during the year 1861, by order of Col. R. C. Moody, R.E., Commanding the Troops. Lat. 49° 12' 47" N., Long. 122° 53' 19" W." (p. 3, Appendix):—

Max. temp. of air in shade at 9.30 a.m., July 9,	74.3° F.
" " " 3.30 p.m. "	84.0 " "
Mean " " 9.30 a.m. "	48.8 " "
" " " 3.30 p.m. "	52.2 " "
Min. " " 9.30 a.m., Jan. 21,	20.0 " "
" " " 3.30 p.m., Dec. 23,	24.0 " "
Min. temp. on grass on January 21	10.0 " "

All the observations were made at 9.30 a.m. and 3.30 p.m. 122° 53' 19" W." (p. 3, Appendix):—

Torquay, January 6

WM. PENGELLY

Dimorphic Leaves of Conifers

IT is now generally believed that some of the varying forms assumed by individual plants or animals in the course of their development are as it were the reflex of an ancestral state of things. From this point of view the different forms of leaves assumed by some *Araucarias*, as well as by many other conifers, become of particular importance. The *Retinosporas* now so common in our gardens and on our balconies represent an immature stage of some *Thuya*, the proof of which statement is occasionally furnished by the plants which suddenly assume the foliage characteristic of that genus. In various species of juniper, notably in the Chinese juniper, two forms of leaf representing the juvenile and the adult condition occur together on the same branch.

Assuming that the juvenile, or "larval" forms, as they have been called, do really represent previous conditions in the history of the species, it might be expected that some of the fossil conifers would be characterised by the possession of this larval foliage to the exclusion of any other. But if I mistake not both forms of foliage have been met with in fossil as in recent conifers, and the pedigree of these plants is by so much the more pushed back.

The resemblance in the form and arrangement of the adult leaves in some *Thuyas* and allied plants to the disposition of the leaves in *Selaginella* should not be overlooked in this connection nor the close resemblance between the foliage of some species of *Lycopodium* proper and the "larval" leaves of many conifers as above referred to.

MAXWELL T. MASTERS

Dust and Fogs

THE meteorological conclusions of Mr. Aitken's important paper, published in NATURE, vol. xxiii. p. 195, will, if adopted without further examination, even temporarily, exercise an unfortunate influence upon the present attempts to rid the atmosphere of our large towns of their ever-recurring fogs, glooms, and mists, and those conclusions certainly are not supported by such evidence as we already have as to the production of fogs on a great scale, however much indicated by experiments in the laboratory. It is stated that, "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." Now, first as to frequency: what are the facts with regard to localities differing in their methods or materials for producing heat? Every one living in or near London knows that fogs, thick mists, and dark days are far more frequent within than without its circumference, and experiment has shown that sunshine is both less frequent and much less intense within the metropolis. And, according to Mr. Aitken's theory, something of the same kind ought to be observed wherever large quantities of fuel are burned, whether smokeless or not. Thus, the large towns of the Continent, where wood and charcoal are in general use, would have their peculiar urban fogs. But they are free from any fogs beyond those which are common to the country. And Paris, before coal was much used, ought to have been distinguished by more frequent fogs than the surrounding country. But it was not so marked out. No oasis of fog prevailed there when the sun shone brightly beyond its precincts, as in our own capital. And Philadelphia, which burns

anthracite, ought not to rejoice in a pure and transparent atmosphere.

Similarly, the South Wales coal and iron districts would be centres of fog-clouds and mist, like Birmingham and Newcastle. But they are as free from fog as the purely pastoral valleys of Wales.

Next, as to persistency. Early in the morning of January 31 last, in some districts of London the fog extended considerably above the tops of the houses, in others only about 10 or 20 feet from the ground in any intensity. Where the fog extended high the smoke mixed with it and produced a yellow fog, but where it remained low the smoke escaped into the upper air and drifted away, leaving a white fog below, so pure as to be a very unusual phenomenon at 10 a.m. in a London street. Now it was remarkable, that wherever the white fog prevailed in the morning, the sun soon obtained the mastery and dispelled it more or less, but in the smoke-obscured districts a dark yellow fog continued throughout the day.

White fogs may doubtless be exceedingly dense. But will not an admixture of smoke increase its density?

A humid atmosphere is not necessary for the production of mist and haze. The frequent long-continued prevalence of blue haze over the whole country, not excepting the east coasts, in the driest east winds of spring, would be a subject deserving investigation. They sometimes extend to a height much above the tops of our highest mountains. Experiments such as those of Mr. Aitken will, we may hope, ultimately solve this problem of meteorology.

R. RUSSELL

Low Temperature

THE reading of the thermometer here last night, January 15, 16, was the lowest ever recorded at this observatory in the course of thirty three years. The reading was $4^{\circ} \cdot 6$ F., the previous minimum having occurred on December 24, 1860, when the mercury stood at $6^{\circ} \cdot 7$ F.

S. J. PERRY

Stonyhurst Observatory, January 16

A "Natural" Experiment in Polarised Light

BREAK off a plate of ice and hold it between the sky and a pool of water. Its reflected image will show the beautiful colours due to polarised light. The incident rays should come from a part of the sky about 90° from the sun, and reflection should take place at the polarising angle for water, and the plate will probably require adjusting to bring out the maximum effect. Water, vaporous, solid, and liquid, thus furnishes us with polariser, crystal, and analyser. I do not remember to have read any account of this very simple experiment, for which Nature provides all the materials.

CHAS. T. WHITMELL

9, Beech Grove, Harrogate, January 10

STATICS AND DYNAMICS OF SKATING

MANY years ago, when skating was but in its infancy, skates were made of bone, and if they could be made to stay on the feet they were considered to answer their purpose sufficiently well.

More recently iron runners with wooden beds came into use, and accuracy of adjustment on the foot, horizontally and longitudinally, was made easier by means of leather straps and a screw passing into the heel of the boot; and these adjustments, made haphazard, were quite sufficient for the skating of those days, namely forward skating.

Within the last twenty years however skating has made enormous strides, back skating becoming an essential qualification of a finished skater; and hence not only more perfect forms of skate are demanded from the maker, but also the adjustment of them on the boot becomes an important part of his duty.

There are three points to be attended to in the adjustment of the skate, besides the obvious one of placing the skate medially on the foot.

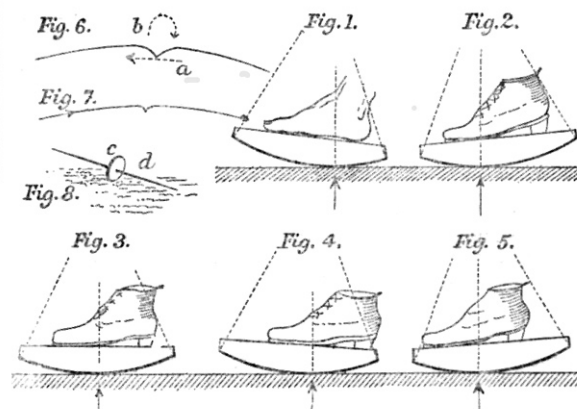
1. Height of foot off the ice where the greatest breadth of the sole of boot occurs.
2. Height of foot off ice at the heel.
3. Position of the skate longitudinally or lengthwise on the foot.

First. The height of the foot from the ice should be such as will enable the skater to lean over sufficiently when on a curve, and such that he may be able to get a powerful enough stroke. If he is too low the edge of the boot will come in contact with the ice in leaning overt and also in taking a stroke: a fall ensuing in the first case, and a disagreeable and dangerous overstrain in the second. To avoid these the sole of the boot should subtend an angle at the bottom of the runner of about 96° *i.e.* for a sole $3\frac{1}{2}$ inches broad the edge of the runner should be $1\frac{1}{2}$ inch from the sole, instead of varying from $1\frac{1}{8}$ to $1\frac{1}{2}$ inch, which are the heights of skates commonly met with.

This angle of 96° will be found to clear the ice in both striking and leaning over for most skaters, and any greater height than is given by this angle should not be used, as it is not necessary, and only throws an additional strain on the ankle.

Second. The height at the sole having been fixed, the next point is what should be the height at the heel? In fact is the foot to be parallel to the skate, or is it to rest on an incline?

Dove was the first person, in his "Skater's Monitor," published in Edinburgh in 1846, to write on the position of the skate on the foot, summing up his remarks by saying, "Level woods then are for shoes whose heels



and soles are equally prominent, but high heels must be sunk into the skate-woods." This was quite correct at that time, when back skating was little practised, and when the skate which was then worn was made very flat, in fact almost straight at and near the heel. Now, by universal consent for figure-skating, the iron is made a segment of a single circle from toe to heel, $7\frac{1}{2}$ feet being the radius. Yet, notwithstanding these changes, Vandervell and Witham, as lately as January, 1880, in their "Figure Skating," recommend the very same parallelism of the foot to the skate instead of parallelism of the top of the blade to the ice, as it should be for modern skating, as I shall subsequently show.

In Fig. 1 is shown the result of adopting Dove's or Vandervell and Witham's position, *i.e.* no heel. It might be thought that a person standing on a curve would balance comfortably at the middle of the curve, but this cannot be, for a person standing naturally on a level surface does not distribute the weight of his body equally over the length of his foot, but by far the greater part comes on the heel, and therefore the centre of pressure of his body is nearer the heel than the toe, and consequently if he is standing on a curve the curve must roll up in front and down behind till the upward pressure of the ice just passes through the centre of pressure of his body. The point of contact of the skate on the ice will therefore not only be much behind the centre of the skate, but will be a little behind the centre of pressure of his body when standing on a level surface, as he now