that even with joints all made by fusion of the glass it was well nigh impossible to get rid entirely of hydrogen. Mr. Crookes has, I believe, found that the last traces of moisture adhering to glass can only be expelled by heating to the softening point of the glass. This tallies with my own experience. In a series of experiments on the ultra-violet water spectrum I had occasion to photograph the spectra of sparks in sundry gases wet and dry, and found that in gases which had been passed through a tube full of phosphoric anhydride the water-spectrum still appeared strongly. Even when the gas had been passed very slowly through two tubes each half a meter long filled with calcium chloride, and then through a similar tube full of phosphoric anhydride, and the part of the tube where the wires were sealed had been heated strongly for a long time, while the current of gas was passing, traces of the water spectrum still often appeared. But Dr. Watts did not see the hydrogen lines in his tube. My difficulty has always been to avoid seeing them when the pressure of the gas was sufficiently reduced and a large condenser used with the induction coil. True: tubes of gas may not always show them even when hydrogen is known to be present. The spark takes a selected course of its own, and does not always light up all that is in the tube. Carbonic oxide does not generally show oxygen lines, and in tubes exhausted by a Sprengel pump the lines of mercury do not usually appear until the pumping has been carried far. A real test would be to see whether when the spark gives the line-spectrum of carbon the hydrogen lines do not also appear. The experiment with naphthaline Prof. Dewar and I have repeated and discussed elsewhere, so I will say no more on it than this, that purity in regard to chemicals is a relative rather than an absolute quality, and that it is only from a long series of experiments chosen with a view to eliminate the effects of accidents of all kinds that any safe induction in this kind of spectroscopy can be reached.

Cambridge, January 4 G. D. LIVEING

[To save time we submitted Prof. Liveing's letter to Mr. Watts, who sends the following reply.—ED.]

I SEE no reason why india-rubber stoppers may not be used in the construction of an apparatus to be filled with a gas *at atmospheric pressure*, or nearly so. The case would be altogether different if we were concerned with the construction of a vacuum tube, and I take it that most of these statements of the difficulty of getting rid of the last traces of moisture and of hydrocarbons adhering to the glass refer to cases where the pressure is to be only a few millimetres. But when a current of cyanogen at atmospheric pressure, made from dried mercuric cyanide, is passed through a U-tube filled with phosphoric anhydride, the gas is surely dry to all intents and purposes (I do not say that the glass would not give off traces of moisture, &c., if the pressure were to be reduced to an extreme point); at least there can be so little hydrogen present in the tube that to ascribe the spectrum given by the tube to the hydrogen present in it is to adopt an extreme hypothesis, which must be supported by cogent experimental evidence before it can be accepted.

But if the defect of the experiment be in the use of india-rubber there can be no great difficulty in constructing the apparatus entirely of glass, and if we are to give up the view that the groups δ (5165 to 5082) and γ (5635 to 5478) are due to carbon, it must be shown that they are not present in the spectrum of the spark in cyanogen at atmospheric pressure when sufficient precautions are taken to obtain the gas pure. I have never examined the spectrum of the spark in cyanogen without seeing them, and have every confidence that Prof. Liveing will still find them there after he has taken all the precautions he may think necessary.

But admitting for the sake of argument the justice of Prof. Liveing's contention that the cyanogen in my experiment contained a trace of hydrogen and that the naphthalin contained a trace of nitrogen, then this seems to be the theory offered for our acceptance—that the spark in nitrocarbon gas containing a trace of hydrogen gives the lines of hydrocarbon, and that the spark in hydrocarbon gas containing a trace of nitrogen gives the lines of nitrocarbon. Does Prof. Liveing hold *both* of these hypotheses to be reasonable?

W. M. WATTS

Geological Climates

THE letter of Prof. Haughton in last week's NATURE so bristles with figures and calculations that some of your readers may feel a little puzzled and may be unable to detect the fallacies that lurk among them. The question is far too large a one to be fully discussed in your columns. I shall therefore confine myself to pointing out the erroneous assumptions and false inferences which vitiate all the learned Professor's calculations, having done which my own theory will remain, so far, intact.

The whole argument against me is based upon an "ideal ice-cap," extending from the Pole to lat. 60°. A considerable but unknown thickness is given to this imaginary field of ice, and it is then calculated that the three great ocean streams, even if admitted to the Arctic area in the manner I suggest, would not get rid of this mass of ice. There are however several important misconceptions and illogical deductions underlying the whole argument, and when these are exposed the results, however accurately worked out, become completely valueless.

We first have it stated that if heat and cold were uniformly distributed over the Polar regions the whole would be permanently frozen over, and an ice-cap be formed of great but varying thickness, diminishing from the Pole to about lat. 60°. But even this preliminary statement is open to serious doubt; for ice cannot be formed without an adequate supply of water, and over a large part of the Polar area no more snow falls than is annually melted by the sun and by warm southerly winds blowing over the heated land-surfaces of Asia and America. Admitting however that any such ice-cap could be formed, it would certainly not form in *one year* but by the accumulations of a long series of years; and any estimate of the *total* heat required to melt it has no bearing whatever on the *annual* amount that would be sufficient, since this depends solely on the average thickness of the ice *annually* formed, of which Prof. Haughton says nothing whatever.

The amount of rainfall in the Arctic regions (mostly in the form of snow) is certainly very small. It is estimated by Dr. Rink to be only twelve inches in Greenland, and this is probably far above the average. All that falls on the inland plains of Asia, Europe, and America is however melted or evaporated by the action of the sun and air far from the influence of the Gulf Stream. The thickness of ice formed annually over the whole area of the Arctic Ocean I have no means of estimating. In open water in very high latitudes it may be considerable, but perennial ice-fields can only increase very slowly. I should therefore very much doubt if the thickness of ice now formed annually over the whole Arctic area averages nearly so much as five feet; and Prof. Haughton himself calculates that our own Gulf Stream is now capable of melting this quantity.

The first assumption, therefore—that the amount of heat required to be introduced into the Arctic regions in order to raise their mean temperature above the freezing-point is "accurately measured" by the amount required to melt an "ice-cap" covering the whole area to a thickness of several hundred feet—is grossly erroneous; and it is so because it takes the hypothetical *accumulated* effects of *many years* Arctic cold under altogether impossible conditions, and then estimates the amount of heat required to melt this whole accumulation in *one year!*

But we find a second and equally important error, in the assumption (involved in all Prof. Haughton's arguments and figures) that all the ice of the alleged "ideal ice-cap" must be melted by that portion of the Gulf Stream which actually enters the Polar area, where its temperature is taken to be 35° F. or only 3° above the melting point of ice. A large quantity of the Arctic ice, however, even now floats southward to beyond lat. 50° in both the Atlantic and Pacific, and is melted by the warmer water and atmosphere and the hotter sun of these lower latitudes. Now, as it is an essential part of my theory that much of Northern Asia and North America were under water at those early periods when warm climates prevailed in the Arctic regions, it is clear that whatever Arctic ice was then formed would have a freer passage southwards, and as the south-flowing return currents would then have been more powerful and more extensive than at present, a much larger proportion of the ice would have been melted by the heat of temperate instead of by that of Arctic seas.

Prof. Haughton admits that the Kuro Siwo and the Mozambique currents together, if they entered the Polar seas, would be equal to the melting of a layer of ice more than thirteen feet thick over the whole area down to lat. 70°. But if our own Gulf Stream is sufficient to get rid of the whole of the ice that now forms annually—as Prof. Haughton's figures show that it would probably be, and as it would be still more certainly were Greenland depressed, thus ceasing to be the great Arctic refrigerator and ice accumulator—then the heat of the other two currents would be employed in raising the temperature of the Arctic seas above 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. Haughton 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. Haughton'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. Haughton'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 metaké* 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 *Bambuseæ*, 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. daily throughout the year.

WM. PENGELLY

Torquay, January 6

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 coniferæ 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