

a long period unless it is applied. I well remember a peculiar case in point. A wild hill-top covered with gorse and bracken was to be taken into cultivation; it had been unturfed, the turves and gorse being piled in heaps and burned on the ground (many acres in extent) now ready for the plough. It was in the month of August. While these heaps were still smouldering, there came two days of heavy rain; immediately after, sprang up like magic an immense crop of mushrooms, chiefly close to the ash-heaps. They were unusually large, and the tops were very brown—scarcely to be distinguished from the bare earth they grew on.

These germs must have been gradually collecting under the turf for years, beaten in by the weather, the moss slowly growing over and hiding them from the air and heat. The removal of the turf exposed them, when, forced by the extraordinary heat of the burning heaps, they suddenly sprang into existence. In after years, when the ground was under cultivation, they were seen no more, for the reason, probably, that when the plant-life was all destroyed, a great part of the insect-life went with it, and thus the means of propagation was lost.

Biarritz, November 23.

R. HAIG THOMAS.

P.S.—I have never actually seen cattle or horses eat mushrooms, but that goats certainly eat some kinds of fungi I can state positively, as last year, in Norway, I had an opportunity of personally observing the fact. A party of us were walking through the pine forest; one of the peasants was leading a goat down the mountain from a *sater* to his farm below. My companions called me to look at the goat, which had stopped in the path-way, and was greedily nibbling at a large piece of sponge-like fungus, such as one finds commonly in the woods. She speedily ate it all up. We expressed some surprise, but the peasants told us goats were very fond of and often eat fungi.—R. H. T.

As stated by Mr. Cooke (NATURE, November 20, p. 57), there is an apparent contradiction between the impossibility of finding out some process of "impregnation" previous to the formation of the spores in Hymenomycetes, and, on the other hand, the occurrence of forms suspected to be "hybrids."

A very remarkable statement in De Bary ("Morphol. u. Physiol. der Pilze," § 1, p. 2), however, may perhaps afford a clue to the mystery, viz. the occurrence of amalgamations between hyphae originally produced from distinct spores. Might not such a process as this possibly lead to "hybrids," if those spores belonged to distinct species? W.

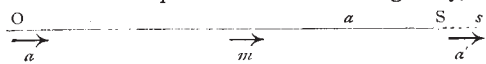
Freiburg, Badenia, November 22.

I WAS shown the other day, in a wine cellar, completely excluding light and fresh air, a remarkably beautiful growth of fungus, covering the wall and floor, to a depth of 4 inches in places, and suggesting cotton-wool in form and colour. When taken up and pressed, it turned brown and emitted the characteristic fungus smell. I should be glad to learn the name, and whether the pure white of the fungus is due to the total exclusion from light. M. H. M.

Doppler's Principle.

THIS subject was referred to in NATURE some months ago, but, although the question is comparatively simple, there is one point of some importance which was not then brought out and to which I have never seen any reference. The change in pitch is, of course, due to the change in the rate at which the cycles of disturbance which constitute the wave-motion fall upon the ear. To determine this change of rate, it is necessary to consider (1) the space occupied by each cycle; (2) the relative velocity of the wave-motion and the observer. Consideration (1) is connected with the velocity of the source of sound, and if wave-length be defined as the shortest distance between two vibrating particles in the same phase, then the space occupied by each cycle may be called the wave-length. If, however, wave-length be defined as the distance which the wave-motion travels through the medium during the "period" of vibration of the sounding body, then the wave-length so defined is unaffected by the motion of the sounding body. It is in connection with this point that there is generally some ambiguity in the usual terms of explanation.

Let s denote the position of the sounding body, and o



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that of the observer. If a denote the velocity of the observer, a' the velocity of the sounding body, m that of the medium, and v the velocity of sound, then during the "period" of the vibration of the sounding body the disturbance travels, *through the medium*, a distance $\frac{v}{n}$, where n is the frequency of the vibration. During this period, however, s is displaced to s' , a distance $\frac{a'}{n}$; and, owing to the motion of the medium, the disturbance originally starting from s , although traversing a length $\frac{v}{n}$ through the medium, only reaches a point a at a distance $\frac{v-m}{n}$ from s . The distance,

$sa = \frac{v+a'-m}{n}$, is thus the actual distance between two particles in the same phase, or gives the *effective* wave-length.

The velocity of the motion through the medium is v , and therefore its velocity relative to o is given by $v+a-m$. Hence, in one second, the number of effective wave-lengths which fall upon the ear is expressed by

$$n' = \frac{v+a-m}{v+a'-m} n.$$

That is, the pitch of the note heard at o is given by n' . This is the formula given by Prof. Everett in NATURE, vol. xlii. p. 81.

Cambridge, November 18.

R. W. STEWART.

The Comb of the Hive-Bee.

IN a recent article the Bishop of Carlisle puts forward, as conclusive objections to the perfecting of the cells in the comb of the hive-bee by natural selection: (1) the fact that other kinds of bees continue; (2) that the sterile workers cannot transmit favourable variations.

But (1) other bees, however inferior in comb-making, may have advantages in other respects; thus the humble-bee can reach the nectar of flowers that are not accessible to the common hive-bee. (2) Favourable variations in the workers would presumably or possibly appear in the further swarms thrown off from the hive or home from which these proceeded; and further, seeing that the workers are really females, the queens in the swarms so thrown off may inherit and transmit the favourable tendency. WM. KNIGHT.

Savile Club, 107 Piccadilly, W., November 22.

A Swallow's Terrace?

MR. A. G. VERNON HARCOURT has just shown me, in his boat-house at Cowley Grange, a specimen of swallow's architecture unlike anything I have seen or heard of. The nest, which is itself normal, is placed at the end of a small beam extending from the top of the door to the angle of the building. This beam is about two feet and a quarter in length, and four inches broad. The nest is at the end next the door; the whole of the rest of the surface of the beam is occupied with an adjunct to the nest, which looks as if it had been meant for the family to perch and roost on. It consists, like the nest, of a foundation of dried mud, carefully covered with dry grass; and it is obvious that much care and pains were spent on its construction. Its length (excluding the nest) is nearly two feet.

Mr. Harcourt thinks that the nest was built late last summer, but he did not notice it then, or discover the use of this curious terrace. Can any of your readers parallel or explain it? W. WARDE FOWLER.

Lincoln College, Oxford, November 20.

Araucaria Cones.

IN answer to the Duke of Argyll's inquiry respecting the coning of the *Araucaria imbricata* in the British Isles, I beg to state that there have been, within my own cognizance, several instances of the same during the last thirty or forty years in this country, notably at Maresfield in Sussex, at Bicton in Devonshire, and especially at Chatsworth. The famous avenue of them at Chatsworth frequently produced cones during the last ten years of the trees' existence prior to 1860, when the memorable severe frost on Christmas Eve completely destroyed the whole avenue, despite the artificial screens of branches of evergreen shrubs that had been annually adopted for their protection from severe wintery weather, so I have been informed by a trustworthy

eye-witness, who also stated that the cones produced by the trees in question always proved seedless. The trees, curiously, were all females, and had no opportunity of impregnation. In further reference to the dioecious character of this genus of Conifers, I am informed that the Maresfield trees, as indicated, failed to produce fructiferous cones until males were planted within suitable proximity to them. Pertaining, further, to the sexuality of the *Araucaria*, I believe that a distinguishing character exists in the size of the foliage, that of the cone-bearer being considerably the larger.

Bearing on the sudden fruition of the Inveraray tree, it may be interesting to relate a parallel case, which occurred upwards of twenty years ago, when I was residing in the neighbourhood of Stratford-on-Avon. A fine specimen of that beautiful Spanish silver fir (*Picea Pinsapo*), on one windy day, became prostrate, and exposed, to my surprise, the greater portion of its main roots in a fungous, diseased condition, thus solving the problem why the tree had for the last few years assumed a stunted growth. Fortunately, however, as two or three of the main roots on one side of the tree remained intact, I resolved to raise it to its former position, after having cut away every vestige of diseased or broken roots; which was successfully accomplished by the aid of a stout rope and pulley-block, and a dozen able men. Subsequently the tree did not appear to suffer materially from the trying ordeal it had been subjected to, and my anticipations of its resuscitation were shortly afterwards justified by a healthy renewed growth, and the interesting appearance, in the course of two or three years, of a crop of beautiful cones, specimens of which I exhibited at one of the Royal Horticultural meetings in 1869, and for which a "Special Certificate of Merit" was awarded. Evidently the cause of this abnormal fruition—as in the case of the Inveraray *Araucaria*—was owing to arrested growth. In conclusion, I may add that I failed to discover the real cause of the decay of the *Picea*'s roots, but attributed it to something unsuitable in the almost impervious damp subsoil, the fungous condition being only consequential.

WILLIAM GARDINER.

Harborne, Birmingham, November 15.

P.S.—Respecting the sexuality of the *Araucaria*, it would be instructive as well as interesting could any of your correspondents define any comparative specific character possessed by the plants, such, for instance, as the foliage or general habit, when in their earlier life, and whereby they may be distinguished.—W. G.

EARLY this summer the *Araucarias* of large size around Terregles House, near Dumfries, were in fruit. Many of the shed cones were lying at the base of the plants. Several years ago I saw a fine *Araucaria* in fruit in the manse garden, Colvend, Kirkcudbrightshire; but learned from the incumbent that the sight was a rare one. About the middle and end of October, this year, we had numerous trees of the mountain ash from which the leaves had fallen, but which stood glittering, laden with red berries. Clouds of fieldfares arrived, at first noisy and shy, perching on the tops of larch-trees. They devoured these berries, and, getting bolder, invaded my garden, and clustered on a mountain ash in such numbers that there could not be less than 200 at one time. At two visits of one hour each, in one day, every berry disappeared from that tree. Now the flocks of fieldfares are no longer visible, and the berries of the hawthorn and other wild fruit do not seem to attract them, while not a berry of the mountain ash could be picked up for many miles.

JAMES SHAW.

Dumfries-shire.

THE GENESIS OF TROPICAL CYCLONES.

ACCORDING to the views of Dr. Hann, as explained in a previous number of this journal, (Nov. 6, p. 15) the storms of the temperate zone originate, not in the convective ascent of warm damp air (an explanation, however, which he appears to admit in the case of tornadoes), but in great vortical movements of the upper air-currents, which commence over the equator as the anti-trades, and set continuously towards the poles, being gradually diverted eastwards in consequence of the earth's rotation. Owing to the spherical form of the earth's surface, these

currents become irregularly congested as they necessarily converge on reaching higher latitudes, and thus give rise to anticyclones, or tracts of excessive accumulation and pressure, and to cyclonic vortices in the intervals. Admitting this view as at least highly probable, the question now to be considered is how far similar conditions hold good in low latitudes. Do the cyclones of the tropical zone originate in like manner, or are they not rather primarily due to the conditions of the lower atmosphere, to the production and condensation of vapour over a calm region, and the creation of an upcast current?

In the first place, it is to be observed that in low latitudes those causes which impede the even flow of the upper currents are at a minimum. Their tendency to congestion must vary as the contraction of the degrees of longitude in successive parallels of latitude; and whereas between latitude 40° and 50° , for instance, this amounts to 16 per cent. of the length of the degree, and between 50° and 60° to 22 per cent., between 5° and 15° it is little more than 3 per cent. Accordingly, the non-periodic oscillations of the barometer, which, in Europe, frequently amount to an inch in the course of a day or two as cyclones and anticyclones successively sweep past, in the latitude of Madras (13° N.) rarely much exceed a tenth of an inch in the whole course of a month. But cyclones originate certainly as low down as latitude 8° , and instances have been recorded in 7° and even 6° .¹

On the other hand, the supposed alternative cause, viz. the production and condensation of vapour, is at a maximum in low latitudes, and the facts recorded by Eliot, Pedler, and others who have traced out the early history of Bay of Bengal cyclones, go to show that their formation is determined by the inrush of a saturated current from the equatorial sea, and that this inrush is preceded by at least one or two days of disturbed squally weather in the birthplace of the storm. Moreover, the evident relations of these storms to the features of the terrestrial surface, always in the early stages of their existence, and frequently after they have been maturely developed, seem to admit of no other conclusion than that they are, primarily at least, phenomena of the lower atmospheric strata, even though at a later period the vortical movement may be imparted to the greatly elevated anti-trade, and so be carried forward into higher latitudes. And lastly, as Dr. Hann has himself shown, the temperature test, which he rightly appeals to as crucial, and which in his hands has led to the overthrow of the condensation theory of extra-tropical storms, does not fail when applied (as far as the data admit of) to the case of tropical cyclones. On each of these points some further elucidation is necessary.

First, as regards the place of their origin; and in these remarks I shall restrict myself to the storms of the Bay of Bengal and the adjacent Indian continent, which have been more closely studied than those of other tropical seas. A chart given by Mr. Eliot in his recently published "Hand-book of Cyclonic Storms in the Bay of Bengal" shows that they are generated with about equal frequency in all parts of the bay between N. latitudes 8° and 18° . Between latitude 18° and the Bengal coast they are much more frequent, though generally of less intensity. But they are formed very rarely indeed over any part of the Indian peninsula. I can remember but one such case during an experience of many years' daily study of the weather charts. And although they originate somewhat more frequently in Lower Bengal during the height of the monsoon, even these instances are rare in comparison with those of storms generated at the head of the bay during the same season. With but few exceptions, therefore, they are formed only over the sea, and these exceptions are nearly all restricted to the low plain immediately north of the bay. If the original impulse were a vortical movement of the higher atmosphere, it

¹ See the list of storms in Appendix II. to the "Weather and Climates of India."