

From the above table it is seen that in ten cases out of thirteen, the formula I have proposed gives results closer than Mr. Stevenson's, while the means differ by a quite insignificant amount.

If then, as seems probable on all grounds, the higher we ascend, the slower the increase of the velocities with the heights, Mr. Stevenson's formula,  $\frac{v}{V} = \left(\frac{h}{H}\right)^{\frac{1}{2}}$ , should hold for a level somewhat higher than 775 feet, and not below it. Above that, again, a formula,  $\frac{v}{V} = \left(\frac{h}{H}\right)^{\frac{1}{3}}$  should apply; and finally, the formula,  $\frac{v}{V} = \left(\frac{h}{H}\right)^{\frac{1}{4}}$ , recommended in my paper.

I cannot believe, however, that the formula  $\frac{v}{V} = \left(\frac{h}{H}\right)^{\frac{1}{2}}$  holds up to such a comparatively large height as this inference would postulate, since it gives such an excessive value at 1600 feet with Vettin's data (more than twice that observed), and I can only conclude, therefore, until experiments in a free atmosphere corroborate Mr. Stevenson's data from Arthur's Seat, that these latter do not correctly represent the actual rate of increase in the velocity between such levels in the atmosphere, away from the disturbing influences of mountains and valleys.

In any case, however, I must enter a distinct protest against having my name prefixed to the pressure formula  $\frac{f}{F} = \sqrt{\frac{h}{H}}$ .

If Mr. Stevenson carefully examines my paper, he will nowhere find the remotest allusion to such a formula. The formula for the velocity which I there recommended for the higher levels, was in fact shown to be directly deducible from Mr. Stevenson's first formula for the pressure, viz.  $\frac{f}{F} = \sqrt{\frac{h}{H}}$ .

to which it is exactly equivalent on the ordinary assumption that  $\frac{f}{F} = \frac{v^2}{V^2}$ .

Moreover, the paradoxical result which Mr. Stevenson arrived at in violation of this assumption, viz. that the same formula was practically applicable both to force and velocity, is controverted by the conclusion entertained in his letter, that the formula  $\frac{v}{V} = \sqrt{\frac{h}{H}}$  agrees best with the recorded results of velocity, and the formula  $\frac{f}{F} = \frac{h}{H}$  with those of pressure.

While these two formulæ can hardly be called the same, it is somewhat striking to find that on the assumption force varies as (velocity)<sup>2</sup>, which is supposed to be annulled by the diminished density as we ascend, they are identical.

Finally, Mr. Stevenson has evidently quite misunderstood my allusion to sea-level. When I spoke of sea-level, I simply meant the approximate equivalent to the level of the sea on land, as at Berlin for example, where Vettin's observations were made. When Mr. Stevenson therefore maintains that the velocity of the wind at 100 feet above sea-level over land, is probably not so great as that near the surface over the sea, he entirely misses the point of the argument, which lies in the relatively excessive velocity of the wind at 100 feet above, to that near the surface, over land which lies approximately at sea-level.

The very fact mentioned by Mr. Stevenson regarding the greater friction encountered by air in passing over land than over water, as well as the results of his experiments, point to a considerable increase in the velocity from the surface to an elevation of 100 feet above it. For the very same reason, I should expect to find a more moderate increase up to the same height over water.

E. DOUGLAS ARCHIBALD

#### On the Formation of Mudballs

THE letter from Mr. Hart in NATURE, vol. xxvii. p. 483, on the natural formation of snowballs, has recalled to my memory the similar formation of balls of mud.

About eight miles south of Bromley in Kent the soil is clayey, and after rain the country lanes are apt to be very muddy. Some five or six years ago there was a very violent storm of rain, whether or not accompanied with melting of snow I cannot now remember. The steep lanes were in many places regularly scoured with water, and it looked afterwards as though the whole surface had in places been a sheet of water, for the soil was quite washed off and the flints were left bare. After this

storm my brother and I noticed in the lanes a considerable number of mudballs, usually almost perfectly spherical, but in some cases with a tendency to a cylindrical shape. They varied in size from small pellets up to four or five inches in diameter. On seeing the first one or two, they looked to us like the handiwork of some boy with an enthusiasm for mud pies, but the number of them, and the fact that they were always found on the slopes of hills proved them to be a natural formation. They were formed throughout of soft clayey mud, and I do not remember finding any nucleus in the middle when we cut them open. We concluded that they were formed by accretion to pellets of mud washed down the hillside and rolling as they went. I have only once since seen a similar ball, and that was in a furrow in a ploughed field in the same country; it is possible that this ball may have been made inside an agricultural roller, although there were no marks on the field of recent rolling and there had been heavy rain. The comparative rarity of the appearance of these balls seems to show that they can only be formed with some precise degree of stickiness of the mud. Closely similar are the marvellously spherical balls of matted vegetable fibre to be found on the seabeach in some places. Sir Anthony Musgrave informed me that on the beach in Australia, I think near Adelaide, he had seen tens of thousands of such balls, all perfectly spherical. It seems rather obscure why the fibres should begin to mat together in such a form as to be rolled by the surf, but the perfection in shape is clearly due to incessant rolling. It is probable that, with a flat bath and some cocoanut fibre or oakum, the process of formation might be watched, but I have never tried the experiment. It is very common to see after rain matted lines of such objects as pine-leaves or the flowers of lime-trees, but I have never seen any apparent tendency to rolling, and such lines are left lying flat after the water has drained off.

G. H. DARWIN

Cambridge, March 23

#### Snow Rollers

THE phenomenon described in NATURE, vol. xxvii. p. 483, under the title of "Natural Snowballs," is known to British meteorologists under that of snow rollers, and as the latter agrees more closely with the phenomenon, I venture to plead for its adoption.

I believe that the first person who carefully examined their formation was that excellent and venerable observer, the Rev. Dr. Clouston of Sandwick Manse, Orkney, and I am under the impression that he published a description of their formation in an early number of the *Philosophical Magazine*. He has observed them on the lawn at Sandwick more than once, and has always noticed the hollowness at the ends; in fact, he described them to me as resembling ladies' white muffs.

I remember only one instance of their being reported in England, namely in the following letter from the late Admiral Sir F. W. Grey, which appeared in the *Meteorological Magazine* for May, 1876.

G. J. SYMONS

62, Camden Square, N.W.

SIR,—The snowstorm of Thursday night (April 13, 1876) was marked by one circumstance which I have never witnessed before, though it may not be uncommon. It was this:—

On Friday morning I observed that for a considerable distance, and following a regular line, the lawn, to leeward of the house, was strewn with masses of snow like boulders, varying from the size of a snowball to a cubic foot at least, and as the snow melted, a track either straight or curved led up to the large ones, following, apparently, the direction of the wind. I had observed before dusk that the eddies of the wind and the swirls of the snow were very marked, and I have since heard from a friend who observed the same thing, that he saw the snow rolled along by the wind, and forming masses such as I have described.

As I have said, I know not whether this has been observed in other cases, and perhaps it may interest you to have this account of it.—Yours faithfully,

F. WM. GREY

Lynwood, Sunningdale, Staines, April 16

#### Incubation of the Ostrich

IT seems strange that there should have existed an uncertainty in the mind of an ornithologist as to the mode of incubation of the ostrich in confinement at the Cape of Good Hope. The habits of the birds are of course as familiar to the ostrich-farmers

as those of barn-door fowls to ourselves. I have stayed at a farm at Cape Point, where a pair of the birds were nesting within fifty yards of the house, in a small paddock, and have seen the hen on the nest.

An interesting subject of inquiry, however, seems to me to be still open in the matter. It is, How far do the habits of nidification of the ostrich vary in the different climates through which it ranges? The nest of the ostrich is commonly described as a heap of sand, and so no doubt it is in warm desert regions; but the nest which I saw at the Cape was carefully built of grass and other warm materials, so as to aid in retaining heat. The birds kept the nest almost constantly covered between them.

In warmer regions, however, the hen appears often to leave the nest in the daytime, and it is just possible that where the temperature is very high the hen may not incubate at all, and the cock alone may do so at night. I merely wish to point out that it should not be assumed that the habits of the ostrich as to incubation are necessarily the same in the various climates of Africa with those to be observed in the Cape region.

I have noticed that at the Zoological Gardens the ostriches at the breeding season are supplied every year with a cartload of silver sand as the traditional nest. It would not be amiss to try them with some more substantial materials as an experiment, and prove whether in our climate they would not build a warm nest as at the Cape.

That birds' eggs can be hatched like those of turtles in mere sand is undoubtedly a fact. The Megapodius inhabiting Cape York, Australia, makes, as is well known, a huge mound of vegetable matter, which by decomposition supplies the necessary warmth to hatch the eggs; but at the Philippine Islands another Megapodius buries its eggs in the perfectly clean calcareous sand near the seashore.

The habits of the emu in nesting have been carefully watched at Blenheim. The head keeper told me not long ago that the cock alone incubates. The hens lay their eggs anywhere about in the grass, the cock builds a nest, and rolls the eggs to it, the hen sometimes endeavouring to prevent him and to break them. I believe an account of observations on the habits of the emu at Blenheim were published by Mr. Frank Buckland.

H. N. MOSELEY

Bonchurch Hotel, Isle of Wight, March 26

### Holothurians

My experience of about three months in Bermuda and Jamaica fully bears out Mr. Guppy and Mr. Kent's view that the Holothurians do not feed on living coral. They were moderately common in both localities close to the shore, where corals are comparatively scarce, and are mainly of the massive kinds, such as the *Astræas*, against which the tentacles of a Holothurian would be useless. There were a few branching *Oculinas* here and there, but not enough to support the Holothurians. But further, some of the latter bury their bodies in the mud or sand, leaving only the tentacles exposed; and I have watched these thrusting their tentacles into their stomachs right up to the base in the comical way described by Mr. Kent. It is quite clear that these were not feeding on living coral. I did not, however, see them actually taking up sand and shell and thrusting it down, as Mr. Kent did; in fact I was puzzled as to what they were feeding on. From the way the tentacles were set, standing nearly erect, I fancied they were catching swimming creatures, as other tentacled animals do. This idea is supported, though not proved, by a fine specimen from the Zoological Station at Naples, which has a half-swallowed fish protruding from its mouth. The specimen is in the Bristol Museum. It proves at all events that they do not reject this kind of food. Possibly in default of it they may fall back upon sand and shell, and the minute organisms contained in these. Some of my experiences with these creatures were interesting. At Bermuda two large kinds used to lie quite exposed in shallow water. I might have guessed from this that they would probably be protected in some way. I used to wade along shore carrying a fishing-basket and a landing-net, and one day, as my basket was full, I put a couple into the landing-net to carry home. As their skins were quite hard, I thought they would travel well so. After handling them, I found my hands smarted a little, and the irritation lasted till bedtime. I found that little bits of their skin had got under mine, and this caused the irritation. As I was going home, I found my Holothurians were literally melting away; long streamers of a colourless gelatinous substance were

hanging down between the meshes. Of course I threw the nasty things away, and had a dreadful job to get the net clean. I attributed my misfortune to the sun, so another day I packed a couple up comfortably at the bottom of my basket, which is very closely made. After an hour or two I was horrified to find long streamers hanging down from the basket of the same horrible substance. They had literally gone to pieces again, and spoilt everything in the basket. Shortly after, I left for Jamaica, and there I took out a wide-mouthed bottle and brought one home in triumph. Being engaged that evening, I left the Holothurian in the bottle all night. Next morning the creature was all there, but he had cleared out the whole of his inside; his intestinal canal and the beautiful tree-like organ were perfect. The latter was still alive and was waving about in the water in the prettiest way, and looking remarkably like branchiae. Some accessory organs along the intestinal canal were exhibiting rhythmic pulsations. Altogether it was a most interesting sight. But my poor Holothurian was only a tube. I did not know at the time that he could grow a complete new inside.

Clifton College

J. G. GRENFELL

### The British Circumpolar Expedition

SUPPLEMENTARY to the very interesting notice in NATURE (p. 484) of the above expedition, permit me to give a brief extract from a letter recently received from Capt. Dawson, as follows:—"I have heard of a large cavern about a day from this (Fort Rae), which I shall try and explore. There are some eyeless fish that live there, that I hope may turn out to be a new species." I do trust Capt. Dawson may be able to carry out his intention, but he must be heavily weighted with work, in which he appears to take a deep interest. I had long ago been told of this cave and its fish, but had no time to visit it, never having been within one or two hundred miles of the place.

March 24

J. RAE

### Meteor

MR. MASHEDER's account in your last number of NATURE (p. 483) of the meteor seen by him at Ashby-de-la-Zouch on March 17, corresponds in some particulars with the inclosed note of one seen by myself on the same evening at Malvern. I am therefore inclined to send it you.

The discrepancies are in the time, which Mr. Masheder states to have been 7.5, while here the meteor passed at 6.56 p.m.; also in his description of "pieces dropping," I noticed no such appearance, but simply the not unusual one of rapidly recurring scintillations in the train.

Great Malvern, March 17, 6.56 p.m.

This evening a bright flame-coloured meteor with a short scintillating train, nucleus the brightness of Jupiter, passed rapidly across the sky. When first seen it was beneath the moon, then shining brightly, and was apparently about the altitude of Betelgeux, at that time nearly 10° past the meridian. It disappeared behind the hills almost due west, but so quickly that it was difficult to determine its course with any exactitude.

Lambert House, Great Malvern, March 25 E. BROWN

### Mimicry

SUCH remarkable instances of mimicry as that described by the Duke of Argyll in NATURE, vol. xxvii. p. 125, as occurring in a moth, make heavy demands upon the faith of the non-scientific reasoner, since it seems to him impossible that organic Nature in her "blind groping in the dark" could, under any imaginable combination of circumstances, have succeeded in taking the successive steps requisite to bring her to such a state of perfect adaptation to condition. But the proverbially keen sight of birds, as at present organised, is apt to lead to erroneous inferences with regard to the evolution of protective mimicry in their insect prey, seeing that the high development of this faculty now attained by them renders nugatory any disguise that is not almost perfect. The theory of natural selection, however, requires the gradual perfecting of this, no less than of other structural and physiological acquirements; and there is no reason for supposing that the Ornithoscelidan ancestors of the feathered tribes possessed exceptional visual powers, but rather that the reverse was the case; so that it may be concluded that improvement in vision and in rapidity of flight proceeded *pari passu*. This being granted, the initiatory steps of mimicry in