

an opportunity of seeing the latest forms of the instruments in Japan.

No one knows better than Prof. Milne that the horizontal pendulum seismograph is not obsolete. He adopted it himself soon after I introduced it, and he has used it freely in his own investigations. His letter will be understood to mean that since I left Japan in 1883 there has been a new departure in seismometric methods which has made my apparatus fall out of date. There has been nothing of the kind. Can Mr. Milne point to any methods involving novel features of importance, and say what their novel features are? It would be odd for instruments to become obsolete when they answer their purpose very well, and when there is nothing better to take their place.

J. A. EWING

University College, Dundee, November 13

Ozone Papers in Towns

I TAKE the opportunity of mentioning that I have experimented with Moffatt's ozone papers in London for the past month, and find that on exposing the papers already previously coloured they all become bleached to their original white. They were previously exposed to the air at Brighton and Hastings, on the sea-coast, and were then coloured, and afterwards preserved closely shut up for trial in the mephitic air of towns.

Some more stained papers were also received from Cheltenham, which also became bleached on open-air exposure in London, though as highly stained as 8 degrees. They were not washed by rain, but kept dry in the usual cage in the open air and out of the sun; and they were of various shades of colour, from 2 to 8, as already marked on them. I should like to know or ask for opinion as to the chemical changes that had taken place, and if these had been due to an antozone causing a recombination of the ingredients (starch and iodide potassium) to their original constitution. It may be likely, therefore, that in Moffatt's papers, coloured previously, we may have the means of testing the impure condition of the air of any locality by exposing them in it for a few hours.

Other papers had already been prepared for testing the sulphurous impregnation of the town air, as by compounds of lead, tin, &c.; but, though they became stained in the laboratory, yet they failed on trial in the open air. As to the influence of the wind, the quickest effect seemed to be produced by easterly winds, while those from the south-westerly direction were slower in action on the papers; but this, I think, may be merely due to the air from the east in London blowing first over a greater expanse of city, carrying with it adulterating emanations.

W. J. BLACK

London, November

The Similarities in the Physical Geography of the Great Oceans

IN the abstract of my paper read at the Royal Geographical Society on the 8th inst., which was published in *NATURE* of Nov. 11, there is a statement (p. 34) that the weight of the column of water between 20 fathoms and 70 fathoms from the surface under the westerly equatorial current is only 88 per cent. of the weight of the same column under the easterly counter equatorial current. I regret that a serious arithmetical error occurs in the calculations on which this statement was founded. There is no such considerable difference of weight in the two columns of water.

J. V. BUCHANAN

Edinburgh, November 22

Lung Sick

DR. E. J. DUNGATE, with compliments to the Editor of *NATURE*, begs to inclose him a letter which he has just received from Prof. Smets, of Hasselt. It refers to the letter on "lung sick," which appeared in *NATURE* for November 11 (p. 29), and contains most important evidence on the subject. Dr. Dungate is sure, from the genial tone of the letter, that the Editor of *NATURE* is at liberty to publish it, if he desires.

6, Marchmont Road, Edinburgh, November 17

Hasselt (Belgique), le 14 Novembre, 1886

MONSIEUR DUNGATE,—J'ai lu votre demande dans la *NATURE* du 11 Novembre.

L'inoculation préventive de la pleuropneumonie exsudative a commencé à Hasselt, et la méthode, suivie déjà chez les

Zouloous, a été préconisée, en premier lieu, par un médecin de Hasselt, M. le Dr. Willems. Je vous communiquerai, avec plaisir, ses travaux si vous les désirez.

On a essayé, à diverses reprises, à Hasselt, les inoculations au fanon, à la poitrine, etc.; elles ont eu des conséquences mortelles.

Je crois que cette pratique permettait au microbe d'envahir rapidement les poumons, et d'étouffer le bœuf. Mais quand on pratique l'inoculation à la queue, le microbe a passé par les divers stades de son existence, et est déjà à son déclin avant d'arriver au poumon. Il est possible aussi que le microbe, que je crois fortement aérobie, a été atténué dans sa virulence par suite de son passage dans des organes où l'oxygène est plus rare.

Plusieurs fois, néanmoins, l'inoculation est encore mortelle, en moyenne 1 cas sur 100 inoculations à Hasselt.

La perte de la queue est due, à mon avis, à ce que l'on fait usage d'un virus impur, obtenu empiriquement, sans culture. La gangrène, qui emporte une partie de la queue, peut être causée par un autre organisme inoculé simultanément avec le microbe de la maladie. Car, parfois, plusieurs bêtes inoculées en même temps, avec le même virus, ne perdent pas la partie inférieure de la queue, tandis que d'autres fois cet accident est fréquent. On prévient partiellement cet accident en faisant des incisions longitudinales dans l'engorgement qui se produit.

Agréez, Monsieur Dungate, l'expression de ma considération distinguée et de mon entier dévouement.

DR. GÉRARD SMETS,
Professeur à Hasselt (Belgique)

Meteor

PASSING along Kensington Gore yesterday at 7.20 p.m., I saw the finest meteor I have ever seen in my life. It descended from near the zenith perpendicularly through the constellation of the Great Bear. It was much larger than any planet. About half-way on its downward course it gave out a second meteor of a red colour, being itself of a pale yellow. The atmosphere was rather foggy at the time, but I could see the stars through the mist. It was, no doubt, the same meteor as is mentioned in to-day's *Times* as having been seen at Reading.

P. L. SCLATER

3, Hanover Square, London, W., November 18

The Origin of Species

MR. CATCHPOOL, writing in *NATURE* (vol. xxxiv. p. 617) on this subject, says:—"If B is separated from A by being nearly infertile, and C from B in the same way, C is likely to be still more infertile with A." This is quite a mistake. Suppose B to be the cat species, and A and C two varieties of dogs; A and C are quite fertile with each other, and infertile with B.

It is certain that mutual infertility is not caused by mere visible unlikeness. The horse and the ass, which do not produce fertile offspring, are much less visibly unlike than many of the varieties of dogs or of pigeons, which are mutually quite fertile.

May not mutual infertility be a result of long-continued separation, quite independently of any unlikeness arising? I do not know whether this conjecture is supported by any observations on the mutual relations of kindred species or varieties in lands separated by oceans.

JOSEPH JOHN MURPHY

Belfast, November 8

MR. MURPHY has mistaken my meaning, which I will try to make clear by an example. Suppose one brood of an ancient species of Gallinæ to have exhibited, as a sport, a partial infertility with the rest of the species, while the birds composing the brood remained abundantly fertile among themselves. Suppose the main body of that species to have become, by natural selection, our pheasants, while the isolated brood became the ancestors of our grouse. Suppose one brood of these grouse to have become partially infertile with the main body of grouse, and to have been the ancestors of our red grouse, while the main body of the grouse became, by natural selection, our black grouse. If, as I believe, variation does not produce or increase infertility, the black grouse will still be only partially infertile with the pheasant, and the red only partially infertile with the black grouse; but it seems probable, *primâ facie*, that the second spontaneous infertility would remove the red grouse