Many striking individual peculiarities were noticed in the course of the experiments, which these general averages fail to show. Three of the male observers were able to detect one part of prussic acid in about 2,000,000 parts of water. Two of these were persons engaged in occupations favouring the cultivation of this sense. Careful chemical tests failed to show the presence of prussic acid in several of the more dilute solutions, in which it could be detected by the sense of smell. We found some of both sexes who absolutely could not detect prussic acid even in solutions of almost overpowering strength. There were several instances of the same peculiarity as regards bromine. Again, our averages show that the sense of smell is in general much more delicate in the case of male than of female observers.

EDWARD L. NICHOLS
E. H. S. BAILEY

University of Kansas, November 4

Tidal Friction and the Evolution of a Satellite

ADVERTING to the correspondence in NATURE (vol. xxxiv. p. 286), I think that Mr. Darwin has not, so far, fully realised the results that would follow from the circumstance that the Martian satellite's period would be affected many hundred times more than that of the planet's rotation, as explained in the correspondence referred to. He argues that, the moon's mass being great, she should recede to an enormous distance before there will be a reversal of the direction of her tides on the earth; while the satellite of Mars, being very small, need only to recede a short distance before a similar tidal reversal ensues. No mention being made of any other supposed difference in the systems at the starting-point, it must be inferred that other things are supposed about equal. But, as a matter of fact, the present position of the Martian satellite is incompatible with an initial rotation of its planet anything like so great as that ascribed to the earth at a like stage. If Mars be supposed to rotate ten times while the satellite, at its present distance, makes nine revolutions, the satellite's period would still be affected or **lengthened much more** than would that of the planet's rotation. The difference between the periods of revolution of the planet and satellite would increase quickly at first, but more slowly as the satellite receded a certain distance, till at a certain time there would be no increase, after which there would be a decrease, and finally a reversal. When the satellite would have receded to a short distance, where she would revolve in the same period as Mars now rotates in, the planet would have lost but little of its original rapid rotation. Now, supposing the satellite tide to go round in the same time as the solar one, the period of the satellite would be affected about thirty times as much as that of the planet's rotation. Allowance being made for the comparative slowness of the satellite's tides, the satellite's period would still be **changed more than ten times** as much as that of the planet. It would be only when the little body got further out, and the planet's rotation slower than it now is, that there could be a reversal of the direction of the satellite's tides. Wherever started, the satellite must either go directly into the planet, or go out a short distance and back into the planet, before the rotation-period can have been much changed by solar tides; or else the satellite must go far out—as when it gets a fair start—and could not possibly turn back until the rotation of Mars be slower than now. Hence it seems that under no conditions could the rotation of Mars, at the birth of her moon, have been twice as rapid as now, and the evidence is very strong that the rotation-period could not have been changed more than a very few hours, if so much. Then, if the rotation of Mars was so slow in the beginning, and so little changed during the whole existence of the satellite, the circumstance does not support the view that the earth's rotation was very rapid in the beginning and so much changed during her past history, but rather inclines the other way.

Respecting the statement that two heavenly bodies cannot revolve about their centre of inertia as parts of a rigid body with their surfaces nearly in contact, unless one be smaller and denser than the other by a certain amount, I can only say, at the present time, that such was the conclusion at which I arrived when investigating the results of the tidal effects of two bodies on one another at close quarters. Without going far into the question, it can be seen that if the rule holds when the two bodies are of the same size and density, it will hold throughout. There will be no difficulty in seeing that the rule holds so far that when the difference in size between the bodies is as great as

between any of the satellites and its primary, the small body must be invariably the denser. Now the argument that was supposed to apply in general would at least apply in the case of the solar system. That argument, as explained in my pamphlet, was that, if a rapidly-rotating body were to separate into two, the small body given off must be denser than the other to withstand the tidal disturbance, and that it would be impossible for the small body to be denser than the primary, since the secondary body must be formed from the surface and therefore lightest part of the other body.

JAMES NOLAN

Dergholm, Victoria, October 5

Seismometry in Japan

I HAVE read, with no small surprise, a paragraph in NATURE of November 11 (p. 36), giving a summary of a letter from Prof. John Milne, with reference to an article by me on the seismographs now manufactured by the Cambridge Scientific Instrument Company. Prof. Milne is represented as saying that, "with the exception of one or two which have been modified, a set of instruments like those recommended by Prof. Ewing are, so far as Japan is concerned, quite obsolete." His letter is not published, and it is possible that the paragraph inadvertently does him an injustice in making him assert what has absolutely no foundation in fact.

In any case the statement cannot be allowed to pass without contradiction. My seismographs have been in regular use at the University of Tokio since they were invented; they are now used for systematic observations by the Japanese Meteorological Bureau; they were sent last year by the Japanese Government to the Inventions Exhibition in London, where they were awarded the highest diploma among Government exhibits: one of them, the comparatively cheap and simple duplex pendulum seismograph, is employed by many private observers in Japan. In a letter received only a few weeks ago, my friend and former assistant, Mr. Sekiya, now Professor of Seismology in the University, says:—

"We are going to start a journal called the *Journal of the Science College of the Imperial University, Japan*. In the first number I will give a paper on 'Comparison of Earthquake Diagrams simultaneously obtained at the same station by two instruments involving the same principle, and thereby proving the trustworthiness of these instruments.' Of course I treat those diagrams recently obtained by two of your seismographs."

Other letters from Prof. Sekiya are full of accounts of the excellent work he is doing with these instruments, and of their continued and extended usefulness in his very able hands. A paper lately received from him describes a rough but effective form of the duplex pendulum, cheaply made in order to bring it within the reach of private observers, and with reference to this the *Japan Mail* of February 2, 1886, says:—

"The duplex pendulum seismograph designed by Prof. J. A. Ewing, has been employed for earthquake observations in the Tokio Daigaku by Mr. S. K. Sekiya, who has improved many of its details during his long use of the instrument. On account of the simplicity and scientific nature of its construction, and its easy management, it has found its way into the hands of many observers."

The *Mail* goes on to mention the name of a native firm by whom the instrument is made and sold. In March last Mr. Sekiya writes:—"The duplex pendulum sells well; some fifteen or twenty of them have been sold."

So much for the duplex pendulum seismograph, which is one of those described in my article, and now made with the utmost refinement of construction by the Cambridge Company. The other is a three-component instrument, of which the principal part is the horizontal pendulum seismograph—consisting of a pair of horizontal pendulums for recording separately two rectangular components of the horizontal motion of the ground on a moving surface driven by clockwork. This method of recording earthquakes was introduced by me in 1880 (*Trans. Seis. Soc. Jap.*, 1880; *Proc. Roy. Soc.*, No. 210), and has been in regular use ever since. The instruments made to my designs by native workmen are still doing good service in Prof. Sekiya's hands. Those now made by the Cambridge Company have the advantage of better workmanship and an improved arrangement of parts. As Prof. Sekiya has recently written to me with regard to the purchase of a set of them by the Japanese Government, it is probable that Mr. Milne will before long have

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. Y. 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

Zouloos, 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