magnitude of a star, the light of which, if diffused over a circle one minute of arc in diameter, would be equal in brightness to that of the nebula.

The star employed as a standard in the present instance was *α Cygni*. The comparisons were made upon three evenings, and three observers took part in the measurements. The number of determinations is six. If we assume the magnitude of *α Cygni* to be 1·7, as in the *Durchmusterung*, that of the nebula is 4·6 on the system just explained. The average deviation of the separate results is 0·4, and the probable error of the mean 0·2. The scale of stellar magnitudes here adopted is that of Pogson, in which the ratio of light corresponding to one magnitude is that the logarithm of which is 0·4. Accordingly, it appears that the brightness of *α Cygni* would be equal to that of the nebula, if the light of the star were diffused over a circle 3′·8 in diameter.

In the position angle 140°, the diameter of the nebula is about 11″, and the diameter perpendicular to this is about 8″. The border of the nebula is not sharply defined, and the fainter light around it is not very regularly distributed about its central portions. In a smaller telescope it would probably look smaller and more nearly circular.

From the measured dimensions and brightness of this nebula, its total light may be computed. The result is that, according to these observations, we receive 590 times as much light from *α Cygni* as from the nebula. Hence, regarding the nebula as a star, its magnitude may be expressed by $1\cdot7 + \frac{2\cdot77}{0\cdot4}$, or 8·6. The magnitude assigned to it in the *Durchmusterung* is 8·5. This close agreement must of course be regarded as accidental.

Like most of the planetary nebulæ observed here, this nebula shows a faint continuous spectrum, not due to the light of the sky, in addition to the lines denoting its gaseous character. This continuous spectrum is largely due to the nucleus.

EDWARD C. PICKERING

Cambridge, U.S., January 24

Electricity of the Blowpipe Flame

COL. ROSS's experiment on the above subject seemed of such importance that I thought it advisable to repeat it, and it may be of interest to some of your readers to hear of the result and of the way in which my experiment was conducted.

A compass in a closed box, to prevent the influence of air currents, was placed close to a brass Herapath blowpipe, and after the position of the needle was noted the gas was lighted and air was blown through the flame; no deflection of the needle was observed. As the compass is an old one and there was probably some friction on the pivot, it was replaced by a piece of magnetised watch-spring attached to a mirror, and suspended in a glass case by a single silk fibre; this apparatus being placed on a stone slab, light from a lamp was reflected from the mirror on to a screen. The arrangement was so delicate that the needle was set in oscillation by the movement of the iron rod connecting the blowpipe with the treadle; so, to avoid any possible disturbing cause, the air was supplied by water pressure from a copper gas holder. When the jet was brought near the needle, the flame being either in the magnetic meridian or at right angles to it, not the least movement of the spot of light was perceived, although the screen was at a distance of about eight feet from the mirror.

As this result is so much at variance with that of Col. Ross, it would be interesting to know exactly how his experiment was performed.

HERBERT M'LEOD

Cooper's Hill, February 4

Triassic Footprints

IN the *Quarterly Journal* of the Geological Society for August last there is an interesting notice by Mr. Sollas, accompanied by a figure, of a set of footprints from the Triassic beds of South Wales. These footprints Mr. Sollas says he has compared with those of the emu taken in modelling-clay; and so complete was the agreement that, other considerations out of the question, he would not have felt much hesitation in declaring for the avian, and indeed ratitous, character of the animal that produced them; but that because no remains of birds have occurred in the trias of the south-west of England, while those of reptiles have, he refers them to either *Thecodontosaurus* or *Palæosaurus*.

I wish, therefore, to call attention to the fact that in these footprints there is shown that character of the crossing of one leg over the other, and of turning out the toe, which persons who have kept poultry may have noticed as conspicuous in the walk of the domestic fowl; that is to say, it places the foot, not directly forward, but across the opposite leg, turning the toe well out. Now this is distinctly shown in the relative positions of these Triassic footprints. The first, or lowermost in the figure, is that of the right foot, and the toes point to the right; the next (2) is that of the left foot, and crosses the median line of the animal's path, and the toe of this (for only the middle one remains unobliterated), points well to the left; the third, being that of the right foot, crosses the median line in the same way, its toes pointing well to the right; but the fourth (left), though it thus crosses, has not the toe turned out, because the animal at that point began to bend its course to the right hand.

This track is thus, I venture to say, one made by the jaunty step of the light-limbed bird, and not by the slouching stride of the heavy-limbed dinosaur, even if this kind of reptile did (as has not yet, notwithstanding its ornithic affinities, been shown) walk erect, and exclusively on two legs; and I am induced to trouble you with these remarks, because just twenty years ago (*Quart. Jour. Geol. Soc.*, vol. xvi. p. 328) I contended that the existing *Ratitæ* and other wingless (or, more accurately, flightless) birds are the direct, and but little altered, descendants of those which inhabited Triassic continents in the southern hemispere, of which one portion, that formed by Australia and New Zealand, has been preserved in complete, and other portions, such as South Africa and South America, in less complete isolation since that remote period; and it seems to me that the footprints figured by Mr. Sollas furnish very satisfactory evidence of the case.

SEARLES V. WOOD, Jun.

Martlesham, near Woodbridge, January 30

Rainfall in the Tropics

MY studies on the distribution of rain on the earth have often caused me to regret our want of knowledge about the quantity of water falling on the oceans, especially in the tropics. The observations on the continents and large islands are very apt to mislead us as to what takes place on the open sea. As there seem to be very great difficulties about observing rain-gauges at sea, I have thought it would be possible to gain some insight into the matter by placing rain-gauges on the smallest and lowest islands to be found on the ocean, the meteorological conditions of which differ but very little, if at all, from those of the ocean. In the Pacific such islands are to be found in plenty; in the Atlantic I would especially recommend the island of St. Paul ½° N. and 29½° W.; in the Indian Ocean, the Southern Maledives, the Chagos, and Keeling Islands, &c.

The rain-gauges for this purpose should be made of strong metal, the lower part, instead of the ordinary glass measuring-vessel, being also of metal. Such rain-gauges could be put on islands, especially uninhabited, and taken up and the amount of water fallen measured after some months, or even a year or more. The measurement would be but a rough one, as the evaporation could not be strictly accounted for, and we would certainly know very little as to the distribution of rain during the year; but with all these drawbacks, even an approximate knowledge of the quantity of water falling in strictly oceanic climates, far from the disturbing influence of land, would be very important for meteorology. Even a few figures as to the total annual rainfall in parts of the ocean, which are for some months included in the "doldrums," and those where the trade-winds blow steadily the whole year, would very much increase our knowledge, more than a great number of observations taken on mountainous islands, where local conditions modify the quantity in the extreme.

I refrain from further practical details, as these will be better provided for by British meteorologists and seamen, in case they should accept my suggestion.

A. WOEIKOF

St. Petersburg, January 21

Mountain Ranges

THE reply which Mr. H. B. Medlicott has made to me in NATURE, vol. xxi. p. 301, seems only to obscure rather than set aside or remove my objections. In the second sentence it is said that I "take geologists to task for not making their descriptions to fit in with my delineation of purely superficial features." But my complaint was based, not on my delineation, but on a trigonometrical survey; and it was caused by a description—not of