LETTERS TO THE EDITOR.

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The Eclipse-Wind.

ALTHOUGH meteorological observations during total eclipses of the sun are of secondary importance, the changes of temperature and humidity being well known, yet it is still uncertain whether those changes do occur in atmospheric pressure, and, consequently, of wind, which, theoretically, ought to result from the sudden chilling of the air by the passage of the moon's shadow, and the consequent increased barometric pressure out of which the wind should blow in all directions.

The letter of Mr. J. W. Evans, in *NATURE* of December 28, 1899, describing his observations during the Indian eclipse of 1898 is an interesting contribution to the subject of the "eclipse-wind," and brings to mind the special investigations undertaken by Prof. Winslow Upton and myself during the total solar eclipses of August 19, 1887, in Russia, and of January 1, 1889, in California, the results of which are described, respectively, in *Amer. Meteor. Journal*, vol. iv., and in *Annals Astron. Observatory of Harvard College*, vol. xxix. No. 1. Aneroid barometers, including a recording instrument, all with open scales, were used, and, in addition to the anemometer, a recording wind-vane was taken to California. There the sky was clear, but in the Russian eclipse it was heavily clouded. As regards the atmospheric pressure, it must be said that, while in both eclipses minute rises occurred during the total phase, yet they cannot be attributed with certainty to its influence, since similar fluctuations occurred on other days. As regards the wind, the eclipse appeared to produce an appreciable effect, for the wind backed (contra-clockwise) before totality, and veered (clockwise) to its original direction afterwards, its velocity diminishing as the eclipse progressed. This is what would be expected to happen at a station situated near the central path of a shadow moving north-east. In both eclipses it was nearly calm during totality.

With the hope of settling these questions, the writer joined the Harvard Observatory party that observed the total solar eclipse of April 16, 1893, in Chile, where not only was clear weather assured, but the regular diurnal period of the barometer afforded an excellent opportunity to study any non-periodic disturbance due to the eclipse. Besides the previous instruments, Richard's "statoscope," or differential barograph, was employed, with which, when the temperature is kept constant or allowed to change at a uniform rate, variations of pressure approximating 0.025 millimetre (1/1000 inch) of mercury are recorded. In order to secure a free exposure in all directions for the observations of wind, the station was located on the summit of a mountain. Notwithstanding perfect conditions—the day being clear, and a counterpart of those preceding and following—no unusual changes in pressure during any of the phases of the eclipse could be detected, and if any variation occurred, it was insufficient to disturb the regular diurnal period, and must have been of the order of a thousandth of an inch. The record of wind direction again showed a backing of the wind prior to totality, and a veering round afterwards; but as these oscillations were not infrequent at other times, they cannot be ascribed certainly to the eclipse. The wind reached its minimum velocity soon after the first contact of the moon's limb, and steadily increased until after the fourth contact.

While these observations seem to prove that any change in the atmospheric pressure during a total solar eclipse is so small as to escape measurement, yet there does appear to be evidence of changes in the wind. Mr. Evans observed a contrary rotation of the wind to that described above; and the reports of the changes in wind during many eclipses, which were collected by the late Mr. Ranyard (*Memoirs Roy. Astron. Soc.* vol. xli. chap. xxxv.), are very contradictory.

Therefore, it would be interesting if, on May 28, observers along the path of totality in the United States and elsewhere would make frequent observations of the direction and strength of the wind. Still more valuable data could be obtained from a few self-recording wind-vanes and anemometers exposed high enough above the ground to be free from local influences.

A. LAURENCE ROTCH.

Blue Hill Meteorological Observatory, U.S.A., April 3.

Lord Kelvin's Origin of Granite.

IT is a sound maxim that if you want a thing done you must do it yourself. So, as no expert has replied to my query as to the soundness of Lord Kelvin's theory of granite, propounded in my letter of February 23, I have consulted a big Dana's "Mineralogy" with the following results.

It may be premised that Lord Kelvin assumes for his liquid lava a specific gravity of 2.50, but as according to Dana the basaltic lava of Kilauea is in one case as high as 3.20, the primeval liquid lava may have averaged 2.60.

Although 2.50 will work out well enough, 2.60 is much more striking as an illustration of the effect of the assumed convection currents upon volcanic minerals.

The following is a list of volcanic minerals in the order of their density, with a rough indication of their composition so far as soda, potash, lime, magnesia and iron are concerned:—

Nosean	...	Soda	...	2.25-2.40
Hauyne	...	Soda-lime	...	2.24-2.50
Leucite	...	Potash	...	2.44-2.56
Nepheline	...	Soda	...	2.50-2.65
Sanadine	...	Potash	...	2.56
Labradorite	...	Lime	...	2.67-2.76
Amphiboles	...	Magnesia, lime, iron	...	2.90-3.40
Pyroxenes	...	Magnesia, lime, iron	...	3.23-3.50

Free silica is represented by tridymite 2.28-2.33, instead of by the heavier quartz.

If we take 2.60 to be the specific gravity of the primeval liquid lava, the division between crystals that would float and those that would sink comes between sanadine and labradorite. Some little allowance should be made for expansion on heating.

According to these specific gravities, it would appear that the snow shower produced by the convection currents would not have the effect of silting up the lava ocean with granitic crystals set in a mother liquor of basalt, but would have the effect of differentiating the lava into light and heavy strata, until the convection currents themselves would be checked and the surface stratum, composed largely of potash and soda silicates, left free to freeze. And, we may note, that the upper stratum is composed of the raw materials of granite, while the lower stratum is composed of the raw materials of basalt.

At this point geology and petrology commence work, and what subsequently befalls the primeval crust, after the advent of water and sediment, may be read between the lines of the great works of MM. Daubrée, Fouqué and Lévy.

The question of a floating crust affects no doubt the problem of the age of the earth, but that is beside my point, which is strictly confined to the origin of granite.

Torquay, April 3.

ARTHUR ROOPE HUNT.

Is New Zealand a Zoological Region?

IN your issue of January 11, Mr. H. Farquhar wrote drawing attention again to the incongruity of associating New Zealand with Australia in a zoo-geographic sense. He correctly insists that the New Zealand fauna is *not* most closely allied to that of North-east Australia (Queensland). It is significant that those writers who advocate the alliance of New Zealand to Queensland have not seen either country, while those who deny such relationship have studied or travelled in both or either areas. No observer who had a first-hand knowledge of the two countries could agree with Dr. Sclater that "it is probable that the whole fauna of New Zealand has been originally derived from" Australia.

In the following number (p. 273), Dr. A. R. Wallace, writing in support of his own and Dr. Sclater's views, does not demonstrate or reaffirm their accuracy, but merely lays stress upon the inconvenience of an opposite view.

That an error is convenient is no good reason for its maintenance. Regardless of the direction in which they point, our first care must be the accuracy of facts and deductions.

But, as Dr. Wallace implies, there may be fairly laid upon destructive critics the burden of restoring by constructive work the effects of their ravages. "If," says Dr. Wallace, "antipodean naturalists restrict the 'Australian Region' to Australia and Tasmania, what shall be done with the remainder of his own Australian Region?" I have proposed (*Journ. Malacology*, iv. 1895, p. 55) that New Zealand, New Caledonia and neighbouring groups (inclusive certainly of the Solomons, perhaps of New