

atmospheres, according to the chemical compounds or their spectra, are coolest.

A. BRESTER.

Delft (Holland), November 30, 1895.

THE above amplification of Dr. Brester's views, as to the cause of variability in red stars, calls for a few remarks. Although opinions may differ as to the constitution of stars of Group II., it is generally agreed that those of Group VI. are in an advanced state of condensation, with cool, absorbing atmospheres, and it is in the latter group of stars that we find the full development of the fluted absorption spectrum which Dr. Brester ascribes to acetylene. Experimental evidence does not certainly indicate that this gas is responsible for the dark flutings observed, for other compounds of carbon give a similar spectrum; but, to whatever the flutings may owe their origin, they are a result of Kirchhoff's law. Notwithstanding that it is in these stars of Group VI. we should most expect the bright lines on Dr. Brester's theory, no bright lines have been recorded in variable stars belonging to the group; that is, there is no visible chemical luminescence.

It is in the variables of Group II. that the bright line phenomena occur, but the associated dark flutings of metals, or their compounds, do not lead us to suppose that the temperature is lower than in the other group of stars with fluted spectra. Hence, luminescence does not seem to be more probable in Group II. than in Group VI., unless we accept the view put forward by Mr. Lockyer, that the physical constitutions of the two groups of stars are essentially different. If the stars of Group II. consist of uncondensed swarms of meteorites, it is certainly conceivable that the luminosity of the hydrogen in the interspaces may be partly due to electrical excitation; this view involves the supposition that these stars are becoming hotter.

It may be further remarked that there is abundant experimental evidence to show that the line spectrum of hydrogen can coexist with a fluted spectrum. Acetylene, for instance, exhibits such a mixture, and hence luminescence is not more necessary to explain the luminous phenomena in the case of hydrogen than in the case of the absorbing vapours which give the flutings associated with them in stars.

THE WRITER OF THE NOTE.

Mount Wosho.

As my name has been mentioned in NATURE for December 5, 1895 (p. 107), in connection with recent explorations in Africa, allow me to say that snow clothing on Mount Wosho must have been added by an outsider, for in page 110 of my work on Ethiopian geography, published five years ago, I have expressly said that snow does not exist in that country. In page 387 of the same volume, I have given up all Mount Wosho's claims to an immense height; but I still think it exists as a mountain, and that it ought not therefore to be wiped off the map.

Your criticism induces me to explain how I tried to sketch one in Upper Ethiopia. I first employed the common method by recording hours of travel and bearings by compass; but I was then forced to suppose the variation of the needle. Disturbing attractions could not be eliminated, and it was seldom possible to check my road by observed latitudes. However, as a warning to my successors, I published this tedious work in my "Géodésie d'Ethiopie" (Paris, 1873), a volume of 502 quarto pages.

This first attempt to map the country being fruitless, I turned all my attention to using a theodolite, and collected in 318 stations more than 4000 bearings, besides 500 of the sun, taken at proper hours to get true azimuths, each of these being followed by an angular zenith distance. I obtained three base-lines from differences of latitudes observed on two heights situated near the same meridian and connected by azimuths. The northern base is nearly 51 miles long; I got there my longitude by eight occultations of stars. The second base is 52 miles calculated from a quadrangle, and the southern base, still more indirect, reaches 51 miles. I have thus carried a continuous chain of triangles from the Red Sea to the frontier of Kaffa, a distance slightly greater than from Calais to Bayonne. These triangles, mostly with only two observed angles, have been checked here and there by latitudes and independent longitudes. I got the latter by a few occultations, but chiefly according to the Russian method of lunar observations. By this method I have calculated 857 positions in Ethiopia. They are given in my pages 423-440, the heights being obtained by a *supposed* coefficient for terrestrial refraction.

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In my "Géodésie" (page 195) I give the first mention of Mount Wosho as *probably* Wosho, and the following page says, "flat mountain which I suppose (*que je prends pour*) Wosho." Page 438 points in No. 805 to a note (p. 448) which adds, "placé par renseignements et par ces azimuts réitérés." This hearsay evidence is taken from a list of days' journeys between Bonga and Wosho (published in my "Géographie") without details. The profile of the mountain measured from Falle is given in the collection of sketches belonging to my 325 *tours d'horizon*. Moreover, the zenith distance was $90^{\circ} 21'$, nearly the very worst to be corrected by the ordinary rule for terrestrial refraction. In spite of all these drawbacks, I published the resulting enormous height in order to call attention to Wosho. Let us hope that some explorer, after throwing a net of triangles over Walamo, may put in its proper place and height the real Wosho.

ANTOINE D'ABBADIE.

Hendaye, December 13, 1895.—

I AM aware that M. d'Abbadie did not still maintain the exaggerated importance once attached to Mount Wosho, for which, indeed, he is by no means solely responsible. My remark was suggested by the fact that the great mountain reported by Grixoni's expedition had again called attention to Mount Wosho, and led to the idea that the earlier reports were possibly, after all, correct. But until Dr. Donaldson Smith's maps are published, it is no use attempting to reconcile the previous maps of this region.

THE WRITER OF THE NOTE.

Cactaceæ in the Galapagos.

ALTHOUGH the Editor has kindly replied to that part of Mr. Agassiz's letter [*ante* p. 199] relating to the *Albatross* Expedition, I should like to say a few words on the other points raised by him. In the first place, I regret that my communication on the subject should have produced the impression of unfairness towards any person or persons. Such was not my intention. For the sake of my own reputation, as well as for the information of those who, like Mr. Agassiz, might, from my way of expressing myself, think otherwise, I may add that I did not intend to convey the idea that *Opuntia* and *Cereus* are limited to Chili on the west coast of America. The contrary is so well known, that it did not appear necessary to me to enter into particulars. Of course Mr. Agassiz will hold me guiltless of suppressing anything respecting the branches of *Opuntia* and *Cereus* which he says he collected, though he does not know what became of them.

Perhaps I may also be permitted to add that Dr. Baur has since sent me some very fine photographs of *Opuntia* and *Cereus*, on a comparatively large scale, together with some notes, which I laid before a recent meeting of the Linnean Society.

Herbarium, Kew.

W. BOTTING HEMSLEY.

A Luminous Centipede.

IN Mr. Lloyd Bozward's letter on the above subject in this week's NATURE, he says that the light of the *Scolopendra electrica* "is the same as that of the glow-worm." My experience is that the light of the glow-worm is a clear little *spark* of light; whereas that of the *Scolopendra electrica* (which I have usually found in the autumn) is more of a phosphorescent light, and streaks of this light are left for a few seconds in the trail of the animal as it crawls about, so that it is often difficult to say exactly where the creature is. I have no doubt others have observed the same thing.

T. PLOWMAN.

Enfield, N., January 11.

The Critical Temperature of Hydrogen.

WHAT I object to is not Mr. Bryan's reference to Wroblewski's work, but his statement (explicitly founded upon the absence of "fresh experiments") that my conclusions "are not results of independent original investigation." In his letter to NATURE of January 9, Mr. Bryan does not even attempt to justify this statement. My *Bulletin* paper was an abstract; in the full paper (*Trans. Crac. Acad.*, vol. xxvii. p. 375), published May 1895, Wroblewski's work was quoted and discussed.

Cracow University, January 12.

L. NATANSON.

A Fog Scale.

CONSIDERING the important part that fog plays in determining the character of a health resort, it is remarkable that no