

sciences, will be able to read infinitely grander legends in wild and mountainous scenery than he who looks upon it alone through the glamour thrown over it by mythology or genius. At all events, we welcome the spreading love of travel as one of many signs of a great intellectual awakening, although doubtless at present it has a good deal about it which lays it open to the sneer of the cynic, as have all new movements. There is a considerable, and we think ill-natured outcry in certain quarters, that all the accessible tourist grounds will become more and more crowded by the followers of the beneficent Cook. But there will always be some spot to which he who does not wish to be counted one of the common herd of tourists can retreat until he has gained vigour and nerve enough to feel in a mood to mix again with "the kindly race of men." Such a retreat is, and will for long be afforded by the "Abode of Snow" which Mr. Wilson and this Lady Pioneer have so attractively described; by and by, no doubt, it will be made more accessible by roads either from our own or from the other (is it premature to say the Russian?) side.

### LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

#### The Article "Birds" in "Encyclopædia Britannica"

MR. GARROD'S article on the new edition of the "Encyclopædia Britannica" in last week's NATURE contains the following passage:—

"As another example of the different teaching of the artificial and the natural classifications, the Swifts (*Cypselidae*) and the Hummingbirds (*Trochilidae*) may be referred to. These two groups, from the details of their internal structure when examined one by one, are most certainly related as intimately as are the Woodpeckers with the Toucans. There is, in fact, not a family difference between them, and yet, from their palates, Professors Huxley and Parker place them in quite different divisions, because the vomer is truncated in the one and pointed in the other."

In a previous part of the article Mr. Garrod refers to my paper on the Classification of Birds, published in the Proceedings of the Zoological Society in 1867, which he criticises as if he had studied it with a care proportioned to the labour it cost. Nevertheless, I can but think that his acquaintance with its contents must be somewhat superficial, inasmuch as any careful reader will find at p. 459, the following passage under the head of *Cypselomorpha*, or Swift-like birds:—

"This group contains three very distinct families—the *Trochilidae*, the *Cypselidae*, and the *Caprimulgidae*. The first two families have a length of the manus and a brevity of the humerus which is peculiar to themselves."

Thus, so far from placing the Swifts and the Hummingbirds in "quite different divisions," I placed them in the same division, and took pains to point out their close affinity; and in asserting the intimate relations of the *Cypselidae* and *Trochilidae*, Mr. Garrod is reiterating a view which, unless I mistake, was first definitely put forward by myself, and not, as the readers of his article would be led to imagine, controverting my opinions.

Mr. Garrod takes pains to show that "the structure of the skull does not alone suffice to determine the mutual affinities of birds." The implication appears to be that Mr. Parker and I assert the contrary. I have no right to speak for Mr. Parker, but I may remark that my knowledge of his works would not have led me to Mr. Garrod's conclusion, while it would have compelled me to treat any opinion of his, however much I might be disposed to differ from it, in a manner different from that adopted by Mr. Garrod. As to the facts, so far as I am concerned, those who will take the trouble to read my paper on the Classification of Birds, and an article by the editor of the *Ibis*, with a letter addressed to him by me, published in the *Ibis* for 1868, will see that the classification in question is not based upon cranial structure alone, and that, seven years ago, we went a little deeper into the question of the principles to

be followed in taxonomy than the point at present attained by Mr. Garrod.

Jan. 23

T. H. HUXLEY

### D-Line Spectra

IN reply to a question propounded to you by a correspondent (vol. xiii. p. 224) as to my reasons for believing that sodium is free in the flame of a spirit-lamp with salted wick, I have to state as follows:—

1. We now know that the flame exercises a specific absorption, and is capable of producing dark D. If this were due to vapour of chloride of sodium, we should expect, in accordance with what observation shows in other cases, that solution of chloride of sodium, or at least the solid chloride, would more or less absorb the orange or yellow part of the spectrum, though not in the same definite way, and we find it does not.

2. We know, by direct experiment, that vapour of sodium does exert the very peculiar absorption indicated by dark D. Different salts of the same metallic oxide agree in the mode in which their solutions absorb light, or at least there is a strong family likeness; but when we pass from one oxide to another of the same metal, there is a complete change. Much more should we expect a complete change when there is such a profound difference of chemical character as there is between sodium itself and chloride of sodium.

3. Lastly, Mr. A. Mitcherlich has proved, by direct experiment, that vapour of chloride of sodium within a tube heated to bright redness neither emits bright D nor produces dark D by absorption (*Poggendorff's Annalen*, vol. 116, pp. 504, 505).

It need not surprise us that sodium should be temporarily free in an ordinary flame, since the metal is prepared by heating carbonate of soda with charcoal, and in the flame we have hydrocarbons at a high temperature. Perhaps the heat alone would suffice to set it free by dissociation.

Cambridge

G. G. STOKES

### The True Nature of Lichens

THE editorial note on this subject in NATURE, vol. xiii. p. 168, was thoroughly disappointing to those who, like myself, may have had hopes that the confident allusion by the reviewer of Haeckel to the "clearing up" of the "true nature of Lichens" had reference to some demonstration—of which we had not heard—of the part played by *Spermogonia* and *Pycnidia* in *Lichen-Reproduction*. Having long had in contemplation the publication of a volume of "Outlines of Lichenology," it has been my business for years to note carefully all publications of any importance on the Natural History of Lichens. Those of Prof. Schwendener of Bâle and his disciples could scarcely have escaped me; so that I find the papers mentioned in the editorial note aforesaid, as well as others, duly recorded, with abstracts and relative criticisms, in my Lichenological memorandum book.

My opinion of the speculations of Schwendener and his followers has all along been, and still is, that so far from "clearing up" the "true nature of Lichens," they introduce elements of very decided confusion; and that they are to be regarded merely as illustrations of German transcendentalism, comparable to the fanciful notions of his countryman Bayrhafer, in 1851, concerning Lichen-Reproduction.\* The dogmatic assertions of anonymous critics concerning the "clearing up" of the "true nature of Lichens" by mere Speculations notwithstanding—I hold what I have always held—that the Lichens as an Order are quite as natural, important, and distinct as any other Order of the Cryptogamia. And in so saying I do not forget the fact that they overlap both the *Algæ* and the *Fungi*. On the contrary, I have over and over again pointed out, in my own publications on the Natural History of Lichens, the affinities, or points of affinity, between Lichens, and Algæ on the one hand, Fungi on the other. In order that sight might not be lost of organisms of doubtful character, possessing elements of structure usually regarded as both algal and lichenoid, or fungoid and lichenoid, or either the one or the other, I long since proposed the establishment of *intermediate and provisional groups of Algo-lichenes and Fungo-lichenes*. Such groups would have the advantage of attracting attention to those *passage-forms*, which appear to me to be of the highest interest to the philosophical botanist.

I have not myself had an opportunity of perusing Haeckel's

\* "Einiges über Lichenen und deren Befruchtung," von J. D. W. Bayrhafer, Bern, 1851; an illustrated 4to.