no power whatever to enforce the laws it might make, and could not be expected to put an end to discussion on these points. The knot must be untied, not cut.

2nd. That the binomial system of nomenclature should not be arbitrarily considered to have commenced at any given date; but that recognisable names in all works in which this system is methodically employed should be used according to the rule of priority.

3rd. That it is not necessary to suppress a generic name in zoology because it has been previously used in botany (or *vice versa*); but that it is much to be regretted that any generic name should thus be in double use, and it should always be made matter of reproach to an author that he has committed an act of this nature.

4th. That names must be Latin to the extent that renders them capable of being written or used in scientific Latin; but that classical emendations beyond this are entirely inadmissible; no line except this can be drawn between emendation, alteration, and total suppression. The laws of classical languages have, *per se*, no more right over scientific nomenclature than has the Hindoo language. As regards the much talked-of "Amphionycha knownothing," it should be latinised in the simplest manner, as *Amphionycha knownothinga;* and I would further suggest that its barbarian author be well hissed whenever he ventures to show his face in a scientific assembly.

5th. That as regards placing an author's name after a species, the name so placed should always be that of the first describer of the species; not because he has any right in the matter, but as an additional means of certainty, and as a security against change.

6th. That the specific name is the name of an object, and therefore a noun, and should be changed in gender, or any other manner, when removed from one genus to another.

7th. That it is very undesirable to use the same specific name in two closely-allied genera; but that where this has been done already no alteration should be made till the two names actually come into collision on account of the two genera being united as one genus. Surely to act otherwise is like cutting one's throat for fear somebody else should do it.

8th. That as regards placing an author's name after a genus, the name so placed should be that of the author who established the genus in the sense in which it is actually used. *Carabus* of Linnæus included all the insects now comprised in the family *Carabidæ*, at present divided into several hundreds of genera. To write, therefore, *Carabus* Linn., when we mean something entirely different, may be usual but is not desirable.

I may add, that I consider it useless to expect a perfectly stable zoological nomenclature, until zoology itself is complete and perfect; but that in order to reduce changes to a minimum, classical and other secondary claims must not be allowed any great importance.

D. SHARP

Thornhill, Dumfriesshir

Deep-Sea Soundings

IN reference to the very interesting article in NATURE for February 22, "American Deep-Sea Soundings," may I be permitted to make the following remarks:—It is there stated that the water-collecting cylinder is apt to lead to incorrect conclusions in regard to the gaseous ingredients of sea water obtained by its means from great depths, owing to the escape of a portion of the gases when the pressure is relieved by the cylinder being drawn to the surface. As a member of the *Porcupine* expeditions of 1869 and 1870, I had nearly eight weeks' constant daily experience in the examination of samples of abyssal water thus obtained, and I believe that I was the first to adapt the gas analysis apparatus of the late Prof. W. A. Miller to the exigencies of a laboratory on board ship. The general result of these experiments for 1869 will be found as an appendix in No. 121 of the Proceedings of the Royal Society. My object in writing now is to point out that if there were such an escape of gaseous ingredients as is indicated above, the abyssal water would be so saturated with them at the ordinary atmospheric pressure (*i.e.* after the sample was removed from the water cylinder in the laboratory), that the least elevation of temperature would be sufficient to cause a further quantity to be given off. This, however, never was the case, since I invariably noticed that there was no appearance of bubbles of gas, until the water had been heated above 120° Fahr., and frequently still hotter. I may add that the only samples of water which appeared saturated with gaseous ingredients were those taken at the surface, afte several hours of strong wind. I must confess that after giving a good deal of thought to the subject, and conversing with friends whose knowledge of physics is far greater than mine, who agree with my view of the matter, I am unable to see any reason why we should expect to find any greater quantity of gaseous ingredients in abyssal than in surface water. No doubt, if the excess were there the enormous pressure would retain it, but where is the source of the supply of the supposed excess? I have never seen a satisfactory answer to this question. The solvent is exposed to excessive pressure, but the gases to be dissolved in it are not, unless there is any evolution of gas at those depths. It is probable that this abyssal water was at some point in its circulation near the surface, when an interchange would take place between some of its dissolved carbonic acid and the oxygen of the atmosphere. And it appears to me that it is only when the particles of sea water are near the surface, and exposed to no excess of pressure, that they dissolve their gaseous ingredients, which are afterwards modified in their composition by the animal life on the sea bottom.

WILLIAM LANT CARPENTER

Clifton, Bristol, February 26

Snow at the Mouth of a Fiery Furnace

IT would be interesting to ascertain the temperature of the saltatory drops noticed by Mr. H. W. Preece. Sudden and excessive evaporation may have produced actual congelation.

HENRY H. HIGGINS

ON THE SPECTRUM OF THE ATMOSPHERE

DURING the voyage out to India of the Eclipse Expedition, I took every opportunity of observing carefully the spectrum given at sunrise, compared with that at sun-high, and obtained the following results, which, though poor in themselves, will show the wide field open for further research.

When leaving England, and for some way into the Mediterranean, the length of the spectrum as seen at sunrise extended generally from about B in the red to near G in the violet. Great differences were, however, presented in the absorption-lines according to the state of the weather, or perhaps rather according to the state of the sky when the sun rose.

If the sun rose among yellow tinted clouds, the absorption bands about B, C, between C and D, and near D, were exceedingly well defined; at the same time the blue end did not extend so far as usual, showing that there was more absorption of the blue, while probably the greater quantity of aqueous vapour in the air reflected the red and yellow rays. In these cases the tint of the clouds generally changed to a rosy red shortly after sunrise.

A clear sunrise, on the contrary, showed an extension of the violet end, whilst the aqueous bands at B, C, and D were less defined, as if the red and yellow light were not so strong to show them out by contrast.

On passing through the Suez Canal and down the Red Sea the spectrum was shortened at both ends, leaving from little beyond C to a third from F to G; this would seem to show a general absorption going on in the atmosphere from some cause, probably light dust in the air. This idea is strengthened by the beautiful purple colour of the distant mountains, as if, though the violet rays were greatly absorbed, the red rays were so to a less degree, whilst the want of aqueous vapour allowed nearly all the yellow rays to be transmitted.

When clear of the Red Sea in the Indian Ocean, the blue became greatly reduced, and the red end extended to A; the aqueous bands were very strong indeed, so much so that on two mornings D_1 and D_2 could hardly