air as nuclei is essential to the condensation of aqueous vapour, it is by no means improbable that *ice* may be associated with these phenomena. For, as these lofty regions must, even within the tropics, be far above the plane constituting the lower boundary of the term of perpetual congelation, the condensed vapour must necessarily assume the form of aggregations of ice around these nuclei. Hence the diffractive coronæ may be associated with imperfectly developed ice-crystal halos.

It seems to me scarcely necessary to invoke—as Mr. Rowell has done (NATURE, vol. xxix. p. 251)—the repulsive agency of electricity to account for the persistent suspension of the volcanic dust, even in these regions of rarefied air. If the attenuation be sufficiently great, there will be no sensible subsidence of the dust-particles. Faraday found that even metallic gold, when minutely divided, required months to subside when suspended in water; and some forms of insoluble mineral matter remain suspended in water for an almost indefinite period. Now, the dust-particles constituting the nuclei of condensation for fogs and clouds are absolutely *ultra-microscopic* in smallness; hence their suspension, even in rarefied air, may be prolonged almost indefinitely. Moreover, it is possible that air may possess some degree of *viscosity;* in which case the indefinitely attenuated dust-particles might have no tendency to subside, and could only be removed from the atmosphere by those meteorological agencies,—such as the condensation of vapour,—which tend to augment their size.

Mr. D. Wetterhan (NATURE, vol. xxix. p. 250) refers to Mr. Kesselmeyer's hypothesis of the atmospheric origin of meteorites put forth some twenty years ago, which ascribes them to the condensation of metallic and other vapours issued from volcanoes. If I am not mistaken this hypothesis was advanced by Biot near the beginning of the present century. The *high velocities* of meteorites is overwhelmingly *fatal* to their *terrestrial* origin.

JOHN LE CONTE

Berkeley, California, February 1

THE recent sunsets were nearly or quite as remarkable in the Rocky Mountain region as they were in Europe, and the phenomena were very similar. There was the same peculiar fire-red after-glow continuing for two hours after sunset, &c. These unusual appearances began to attract attention soon after the middle of November. They were most brilliant during the last week of November, but continued at intervals until early in January. The carefully kept meteorological record of Prof. F. H. Loud, of Colorado College, shows that the atmospheric pressure varied considerably during the latter part of November, but there was no apparent accompanying change in the after-glow. The sunrises were also quite brilliant, but less so than the sunsets. Late in November I began to observe the wide chromatic belt which surrounded the sun, and at midday usually reached from near the sun to the horizon. Somewhat similar appearances and chromatic halos are not uncommon here, and it was not until after several weeks of comparison of colours that I became convinced that the tints seen around the sun during the time of the remarkable sunsets were somewhat different from those ordinarily seen. By degrees the brick, or fire-red, and other abnormal tints of the twilight hours have given place to the ordinary prismatic colours, and a similar but less marked change could be seen in the colours observed near the sun during the daytime. These day colours were brightest when the sky was overcast with thin clouds or filmy cirri, though plainly visible when there was no cloud to be seen. The prevailing day tint is usually a peculiar dull purple, but during the time of the red after-glow the common colour was duller, more like a yellowish brick-dust.

Colorado College, February 8 G. H. STONE

"Probable Nature of the Internal Symmetry of Crystals"

IN reply to the important criticisms offered by Herr L. Sohncke on my new theory published in NATURE of December 20 and 27, 1883 (pp. 186 and 205)—

Taking first those relating to the geometry of the subject; the following explains why only the five symmetrical arrangements of points in space described in my paper are taken as the basis of the theory.

If it is the case that, prior to the act of crystallisation, the chemical atoms of a body fall into some symmetrical arrangement, it is natural to suppose that they do so through some influence they exert on one another—such, for example, as mutual repulsion—and that a similar influence is exerted by each atom of the same kind on atoms around it. And if this be so, there will be no *stable equilibrium* of the forces thus exerted *until the atoms are very evenly distributed throughout the space allotted to them.*

Now although, as Herr Sohncke has shown, there is a large variety of symmetrical arrangements of points in space in which the points are disposed around every one point of the system in precisely the same manner as around every other, it would appear that only four of these regular systems, the first four described in my paper, signally fulfil the requirement of even distribution, these four systems being distinguished from all the rest by the property that, *if the nearest points grouped around any point of either of these four systems are joined, the solid thus outlined has its edges all equal.*

And further, although the fifth system described in my paper is not one of Herr Sohncke's *regular* systems, its points are more evenly distributed through space than those of any of these systems except the four just referred to. In this system the property is found that either lines joining the nearest points around any point of the system, or lines joining the next nearest, in all cases outline a solid whose edges are all equal.

As the five systems I have in my paper too vaguely distinguished as "very symmetrical" thus stand alone, and moreover, if my views are adopted, they appear to be adequate to all cases of crystallisation, I still incline to think that the chemical atoms of bodies about to crystallise always have one or other of these five kinds of symmetrical arrangement. If I am wrong in this, and some other symmetrical arrangements are admissible, the general lines of the new theory will not however be affected.

Next, as to the bearing of the theory on chemical valency and the usual conception of a chemical molecule, it may be remarked that, while there is no clear knowledge of the nature of the union between the different sorts of atoms in a compound by which to test the new theory, this theory appears to receive support from the phenomenon of electrolysis. For the fact that one *ion* is liberated at one pole, the other at the other, while *no apparent alteration takes place in the fluid between the poles,* goes to show that any particular atom can change its partners without dissolving the chemical ties subsisting between the several atoms of the compound, and thus favours the view that similar atoms equally near to a particular atom are similarly related to it.

As to my supposition that the expansion, or contraction, occurring in the act of crystallisation, is due to the increased or diminished repulsion exerted by *some only* of the atoms of a body on surrounding atoms, it is, perhaps, interesting to notice that if this conception could be extended to the gaseous state, and the expansion to the state of gas of any compound attributed to the agency of certain atoms in each molecule, or ideal unit, to the exclusion of the rest, the simple relations found subsisting between the volumes of compounds and the volumes of their uncombined constituents might in this way be accounted for:—Thus the fact that aqueous vapour has a volume two-thirds that of the added volumes of the hydrogen and oxygen of which it is composed would be explained if all the gaseous expansion of this compound is due to the hydrogen atoms only.

Muswell Hill WM. BARLOW

"Mental Evolution in Animals"

MR. FARADAY does not seem to have quite understood one point in my comment on his letter. I said that whether the action of the skate was accidental or designed, "in either case, under the conditions, and more especially the 'attitude' described, seizure of the food at the proper moment can only be ascribed to the sense of smell." When we remember the form of a skate, it is certain that, under the conditions described, the animal could not *see* the approaching food, and therefore Mr. Faraday's illustration from the cricketer would only hold if the cricketer continued to hit the ball after he had been blindfolded.

I do not care to continue this discussion; but I may say that as the glass wall of a tank is not an object upon the solidity of which a skate would be likely to calculate, and as the sense of smell in this animal is so highly developed that it might easily give rise to "the appearance of co-ordination" described, I still think that the incident was probably accidental. Any other piece of food happening to approach the mouth would no doubt have been seized in just the same way.

GEORGE J. ROMANES