of the tectonics of the earlier Palæozoic rocks describe folding and shearing which approach this average, though, of course, there are some extensive areas where the disturbance has been much less. When we come to the Archæan rocks the case is even stronger.

A very simple calculation shows that the amount of contraction suggested above implies only a rate of shrinkage so slow that if going on at present it would fail to be detected by the most refined geodetic measurements repeated after a century. A contraction of the earth's radius of only three inches a year, which would cause a 20 per cent. diminution of the radius in about twenty million years (and this is probably a low estimate for the age of the Cambrian strata) would only change the absolute length of geodetic lines by less than one part in a million in a hundred years. Observations of the lunar parallax, which afford another theoretical means of detecting changes of the earth's radius, would have to be repeated at intervals of twenty-five thousand years in order to detect a single second of diminution, even if we could assume the moon's mean distance to be constant throughout this period.

If the hypothesis of a continuous contraction of the earth's radius at an average rate of somewhere about three inches a year throughout geological time is entertained, it not only furnishes a plausible explanation of the prevalent folding of the ancient rocks, but also tempts one to indulge in a number of other speculations, such as, for instance, how far the flying-powers of the great Mesozoic reptiles may have been influenced by the lesser value of gravity at the earth's surface with a larger terrestrial radius and a greater speed of rotation. But collateral issues may well rest until the main point has been argued, and as to this I should like the opinion of geologists whose experience of the older formations has been greater than my own. I am at present surveying in a disturbed country where I should certainly have had to correct my base-line very considerably for dislevelment if I had measured it along the planes of stratification of the rocks, and this circumstance has suggested the question to my mind on reading Mr. Fisher's letter.

JOHN BALL.

Wadi Shellal, Sinai, March 31.

Zoological Classification.

THE complaint, thus entitled, of Mr. H. C. Williamson in *NATURE* for April 9, p. 135, is a fairly common one just now, and his way of putting it suggests some remarks from the other side.

The zoological classification of this or any other day is "satisfactory" in proportion, first, as the principles of that classification commend themselves to the general intelligence of zoologists; secondly, as the classification is in agreement with those principles. The multiplication of the units to be classified, whether subspecies, species, or genera, can have no bearing on the validity of the classification.

The object of classification is the arrangement, but not the nomenclature, of the units.

The older genera have been subdivided, not merely because the number of species has proved unwieldy (indeed, if the necessary bases be absent, such genera cannot be split; however much it be desired), nor merely because it has proved possible to group the species (for in this case subgeneric divisions are adequate), but because study has shown that the species formerly included in a genus have not really the close interrelationship which such inclusion implies, but are of very diverse descent. In nomenclature as affected by classification most of the serious difficulties which happen to have come under my own

notice are due to our increased, but still imperfect, knowledge of origins.

The real trouble and source of disagreement between zoologists is that some (and especially the "applied" workers) look at taxonomy with the eyes of the old school, which regarded only the amount of resemblance in structure, while others follow the new school which seeks to express history and relationship. In a word, there are two sets of principles, and our classification is, if not halting between the two, at least passing rather lamely from one to the other. And as for nomenclature, the trouble is that the Linnean system was devised for a classification on the old principles, and one often doubts how far it is applicable to the new method.

When the nature of our difficulties is clearly understood, nobody will propose to effect progress by such retracing of our steps as "the extinction of half at least of the genera." In ordinary writing one practical way of getting round the difficulty is half hinted at in Mr. Williamson's last sentence; it is to use the family name. Thus we can speak of a Cidarid, a Terebratulid, a Phacopid, a Pentacrinid. Or the transition may be eased by using the old name with some such caveat as *sensu lato*, e.g. Rhynchonella (s. l.), Antedon (s. l.). In writing the "Guide to the Fossil Invertebrate Animals in the British Museum" the conflict between accuracy and intelligibility was settled by adding the older better-known name within square brackets after the correct or modern generic name, e.g. *Dalmanites* [*Phacops*] *caudatus*.

F. A. BATHER.

Natural History Museum, April 11.

Electric Emissivity at High Temperatures.

WE have recently conducted at the National Physical Laboratory some experiments on the emission of electricity from a number of substances at very high temperatures—between 2000° C. and 2500° C. The experiments were carried out in a carbon-tube resistance furnace at atmospheric pressure. Among the substances tried were the alkaline earths and a number of metals. In every instance the temperature was sufficient to vaporise rapidly the substance under test, and, under these conditions, very large amounts of electricity were emitted. For example, barium oxide emitted negative currents of the order of 4 amperes per sq. cm., while boiling tin gave currents of about 2 amperes per sq. cm. No external potential was applied in any of the experiments.

We hope to publish very shortly a full discussion of the results.

G. W. C. KAYE.

W. F. HIGGINS.

National Physical Laboratory, Teddington,
Middlesex, April 22.

An Optical Illusion.

SOME days ago I was reading, the sunrays falling aslant on my forehead, it being about five o'clock p.m. After having read awhile, the letters showed a vivid red, the paper itself retaining its white colour. The rays did not fall on either of the two sides of the page.

When I used my hand to shade the eyes, the letters immediately became black again. On removing my hand, it took them some seconds to change from black through dark red to bright red. I may add that I am short-sighted, but was reading without spectacles.

I should feel obliged if some reader of *NATURE* would give me an explanation of this.

J. W. GILTAY.

Delft, Holland, April 11.