the meaning" of what I wrote, I must leave it to others to settle whether this be in a favourable or an unfavourable sense.

Turning to another matter: in reference to an interesting paragraph on p. 110 concerning the excavation of rock by snails, a subject on which I once wrote (*Geol. Mag.*, 1869, 1870), may I ask whether any of the readers of NATURE are acquainted with instances of these burrows occurring in non-calcareous rocks? All which I have seen were in limestone, and, as I believe, always in a pure variety. Hence, in the case of snails, one would suspect that the excavation was mainly due to chemical action.

T. G. BONNEY.

Coral Reefs, Fossil and Recent.

I SUPPOSE it will be expected of me that I should answer the two objections raised by Captain Wharton (May 22, p. 81), viz. (1) that he knows of no steep submarine reef-slopes exceeding 4000 feet in height; and (2) that the lagoons could not be so shallow as they are if we assumed any extensive positive shifting of the coast-line.

From the statements in the literature on the subject, concerning point (1), I select the following three:—

Captain Fitzroy found at the Keeling no bottom 2200 yards from the breakers with a line 7200 feet long (Darwin, "Coral Reefs").

Bourne says in his account of Diego Garcia (Proc. Roy. Soc., vol. xliii.), that the Maldives, Laccadives, and the Chagos rise from a bank 1000 fathoms below the surface very abruptly.

Heilprin ("Bermudas") states that the Bermudas rise abruptly out of a depth of 12,000 to 13,000 feet.

Concerning point (2), I cannot see why the gentle inward slopes of atolls should not be in harmony with the subsidence theory. It must be borne in mind that the shifting of the coast-line is both slow and oscillating. Positive and negative shiftings alternate. The latter predominates on the whole. Dr. Murray says that in shallow water the accumulation of material exceeds the removal by solution. I have professed my accordance with this view in my previous letter. Particularly in an inclosed or partially inclosed lagoon, sheltered from ocean currents, this filling-up process will be a rapid one. We can easily conceive that it will balance the subsidence until the lagoon becomes so shallow as to impede the life of those organisms whose skeletons form the raising-up deposit. If there is any oscillatory negative shifting of coast-line, the dry rim will rise, and extend horizontally, and afford to the atmospheric agencies a larger surface wherefrom material can be washed into the lagoon.

On the whole, if there is anything difficult to explain, it is that the lagoons are as deep as they are. Deep lagoons are, however, not common, and are generally only met with in large and interrupted atolls. Perfectly dry central depressions (with deposits of gypsum and the like) are by no means infrequent in very small atolls. The general proportionality of the depth and the horizontal extent of the lagoons is perfectly in accordance with the subsidence theory. It supports no other theory better than this one.

R. VON LENDENFELD.

Photographs of Water Drops.

IN NATURE of May 22 (p. 95) there is an account given of the discussion following Mr. C. V. Boys's demonstration of his photographs of falling water drops at the meeting of the Physical Society. In the course of this discussion, Lord Rayleigh, who was naturally much interested in the subject, remarked that it had never occurred to him that it would be possible to get enough light from a single spark to photograph the drops as Mr. Boys had done. And Lord Rayleigh believed Mr. Boys's success was owing to the fact of his using no lenses, which would absorb the ultra-violet rays.

With reference to this, it might, perhaps, be interesting to mention that I succeeded very well, some years ago, in photographing water drops, falling through air, with single sparks, the light of the spark passing two glass lenses and the objective of a camera which gave magnified images. My photographs (copies of which appeared in the *Annalen der Physik und Chemie*, vol. xxx., 1887) show all the forms obtained so very beautifully by Mr. Boys. From photographs taken at different depths below the orifice of the tube I could measure the periodic time of the elliptical vibrations and of the vibrations according to the next higher spherical harmonic, and show that the ratio of these two periodic times agreed very closely with the formulæ given by Lord Rayleigh in the Proc. Roy. Soc., 1879. The amplitudes had no influence upon the periodic times.

Richmond, Surrey, June 6.

P. LENARD.

THE CLIMATES OF PAST AGES.[1]

I.

IT happens sometimes in the history of science that a few striking facts lead to the building up of a far-reaching theory, which at first satisfies us, and with which, without being rigorously critical, we endeavour to bring the further results of experience into conformity. But contradictions and difficulties gradually manifest themselves, and go on accumulating, until at last we are convinced that we have built on an unsure foundation, and that the edifice that we have raised upon it must be utterly pulled down. Then follows a period of discussion and collection of further evidence, during which we abstain from any attempt to substitute new and more correct explanation for that which we have abandoned, until by assiduous labour we shall have prepared a broader and more stable basis for the superstructure.

In such a stage of transition, the old ground abandoned, the new not yet won, is our knowledge of the climatic conditions of our earth in bygone ages. In the far north a rich mass of fossilized plants and coal-beds had been found in the Carboniferous formation. Reef-building corals, such as to-day live only in tropical seas, were yielded by the Carboniferous limestone and the Silurian formation up to 80° of northern latitude; and many of the species were found to range, without any essential change of form, from arctic to temperate, nay in some cases even to equatorial regions. From a small number of data such as these it was hastily concluded that, under the influence of the internal heat of the earth, a warm uniform climate must have prevailed generally from the pole to the equator, while a sultry atmosphere, heavily charged with water vapour and carbonic acid, prevented the sun's rays from reaching the earth or in any case from exercising any considerable influence on it. As a consequence, the existence of climatic zones or of a distribution of the fauna and flora in such zones was denied. It was held that with the beginning of the Tertiary era a polar cooling first set in, and that it increased during its passage, until the present distribution of heat was brought about as the final result of this long-continued process.

The falsity of these assumptions is now pretty generally recognized, and the number of their adherents diminishes daily. It would lead us too far afield were we to follow out the hypothesis into all the details of its ofttimes fantastic errors, and to note their individual failure. It will be more to the purpose if, in the first place, we test the methods by which we arrive at conclusions on the temperature conditions of past ages, in order that we may thus gain a knowledge of what these really were and of the better-grounded attempts to explain them.

Among the more important data for judging of the climate of a past epoch, is the character of its plants and animals, on the assumption that these various organisms must have lived under nearly the same conditions of temperature as their nearest relatives now existing. This kind of reasoning has been very extensively applied, and within certain limits its validity cannot be gainsaid. If, for instance, in a comparatively recent deposit of the Pleistocene period in Central Europe, we find remains of the arctic willow, the dwarf birch, the white dryas, together with such mammals as the lemming, the musk-ox, the

[1] Translation of a Lecture delivered by the late Dr. M. Neumayr before the Society for the Dissemination of Natural Science, at Vienna, on January 2, 1889.