

from physical data, and that we, the geologists and zoologists, "are free to proceed, and to look for the conclusions warranted by our own evidence."

Lord Kelvin's experiments on thermal conductivity of rock at various temperatures are of the highest interest. I did not allude to them because it seemed to me unnecessary to point out that, until we know the nature of the material which forms the deeper parts of the earth, any attempt to generalise from the results of experiments on the material of the surface must be inconclusive.

Mr. Chree thinks that I might have made something of the uncertainty as to the true mean temperature gradient at the surface. The reports of the British Association on underground temperature afford abundant support for a temperature gradient in the northern hemisphere, which cannot be very different from that selected by Lord Kelvin.

I fail to grasp the object of Mr. Chree's letter, unless it be to proclaim that he never accepted Lord Kelvin's conclusions, and I cannot see that any great object is gained by even this statement.

As I am writing on the subject, I should wish to point out that the address, as printed in the Report of the Liverpool meeting, will be slightly different from that which has appeared in the columns of NATURE. Lord Kelvin kindly drew my attention to one or two errors which will be corrected in the Report.

Oxford, November 20. EDWARD B. POULTON.

Measurements of Crabs.

I AM much obliged to Prof. Weldon and Mr. H. Thompson for the careful consideration they have given to the doubt which I raised concerning the validity of the comparison made by the latter of measurements of crabs collected from the same locality in different years. Prof. Weldon offers some evidence to show that immersion in spirit does not affect the relative dimensions of parts of the carapace. The evidence is not direct, nor perhaps is it complete. It refers to female crabs, and not to the male specimens with which Mr. Thompson was dealing. But I notice that according to Prof. Weldon's measurements the spirit specimens of 1895 differed more than the fresh specimens of 1895, from the spirit specimens of 1893, whereas if the spirit were the cause, the difference would be less between spirit specimens and spirit specimens than between spirit specimens and fresh specimens. I admit then that there is little possibility of the observed difference being due to the action of spirit.

But Mr. Thompson's letter suggests other reflections. He draws my attention to the fact that the difference which he observed between crabs of the one year and those of the other, is of precisely the same kind as the difference between an older crab and a younger crab, or rather between a larger crab and a smaller crab. As the male crab increases in size, its frontal breadth is continually becoming less in proportion to its carapace length, while its dentary margin is becoming greater. What Mr. Thompson found, therefore, was simply that in one sample the individuals of a given size were more advanced in development than those of the same size in the other sample. The development or law of growth remaining the same, the size of one sample had been reduced in comparison with that of the other. But this is not, I think, correctly described as a change in the character of the species. I find that the crabs of the same stage from the two samples differed in average length by about 5 mm., which is a very small difference. Such a difference might well be caused in the young crabs of two different seasons by a difference in abundance of food, due to meteorological differences; the crop of young crabs was finer in one season than in the other. I believe that if the lambs of two seasons were compared in the same way, differences in the rate of growth, or in the size of the individuals which had reached the same stage of development, would be found. We may say, indeed, that such differences are known to be of general occurrence. Mr. Thompson's paper suggests that a considerable change in the proportions of parts characteristic of the species had been observed, whereas, in point of fact, no new proportions were observed at all, but the old were found to be present in individuals of different sizes. J. T. CUNNINGHAM.

College of Surgeons, November 20.

Suggested Reef Boring at the Bermudas—and elsewhere.

MR. W. K. MORRISON's suggestion (NATURE, November 5) of the Bermudas as a site for renewed reef-boring experiments, and for the establishment of a permanent biological

observatory is well worthy of consideration. It is, at the same time, desirable to remark that the Bermuda reefs scarcely appear to possess the most favourable conditions for boring operations. As long since recorded by Dana ("Corals and Coral Islands," p. 361), the Bermuda coral-rock abounds with caverns and fissures, and there would consequently be an imminent risk of negative results being obtained there, as has happened at Funafuti, through the uncontrollable infiltration of sea-water. The circumstance, also attested to by Dana, that the reef-making species of corals at the Bermudas are but few in number, and are constantly submerged to a depth of from, at least, one to four fathoms, places this island group, as a station for the especial investigation of coral-growths, at a disadvantage in comparison with many others that might be mentioned.

Much might be written in favour of the many locations on the Australian Great Barrier Reef that would be particularly suited for the prosecution of the borings and biological investigations proposed. My later sphere of investigation has brought within my notice another area which, in certain respects, offers advantages and facilities possessed by no other spot throughout the coral regions with which I am acquainted. I refer to Houtman's Abrolhos, lying thirty miles to the westward of the port of Geraldton, within a day's journey from Perth, on the coast of Western Australia. A paper indicating certain of the more remarkable marine biological features of these islands was communicated by me to last year's Ipswich meeting of the British Association, and the subject is dealt with in further detail in a chapter of my book, "The Naturalist in Australia," now on the eve of publication. The data recorded that are most pertinent to the present issue are as follows.

Notwithstanding the position of the Abrolhos islands so far south, 28° to 29° , as to be outside the limits of the tropics, the marine fauna—owing to a warm southerly drifting equatorial current—corresponds more nearly in character with that of Torres Straits than with that of the adjacent mainland coastline as far north at least as King's Sound, in latitudes 16° to 17° . The western is a perfect archipelago of coral islands, including lagoon, barrier, and fringing reefs, and abounds with readily accessible coral growths in infinite variety.

An abundant illustration of what Houtman's Abrolhos can produce is afforded among the series of coral specimens from the Western Australian reefs, recently contributed by me to the British Museum coral galleries.

Any practical scheme initiated in this country having as its object experimental reef-boring, or the establishment of a marine biological observatory on Houtman's Abrolhos, would undoubtedly receive substantial sympathy and support at the hands of the Western Australian Government collectively, and yet more definitely through the medium of its far-seeing and scientific-minded Premier, Sir John Forrest.

Any assistance towards the furtherance of this suggested scheme, should it merit consideration, that may lay within my power, would be most willingly contributed.

London, November 9.

W. SAVILLE-KENT.

The Structure of Nerve Cells.

THE Spanish histologist, Dr. S. R. Cajal, on p. 9 of his book "Les nouvelles idées sur la structure du Système nerveux," translated into French by Dr. L. Azoulay, makes the following assertion:—"Les cellules nerveuses sont des unités indépendantes, ne s'anastomosant *jamais* ni par leurs rameaux protoplasmiques, ni par leurs expansions nerveuses ou cylindres axes." (The italics are mine.) I venture to bring this statement forward because recently I have discovered that it is not universally correct; in sections of the medulla oblongata of a young snake, *Tropidonotus natrix*, prepared according to Cox's modification of Golgi's corrosive sublimate process, I have found a pair of cells on the ventral edge distinctly united together by a protoplasmic process, or, as I would propose to term it, a dendrite. Cox's mercurial method is so far better than the chrom-osmium-silver method, inasmuch as the preparations made by it keep much longer.

ALFRED SANDERS.

The Hawthorns, Caterham Valley, November 26.

Snow Buntings.

IT may interest some of your readers to learn that on November 17 I saw near the top of Snowdon a flock of snow buntings. The mountain was snow-clad, and had been so for several weeks.

J. R. DAKYNS.

Penn-y-ywyl, November 23.