On Frozen Fish.

MR. F. H. P. COSTE (*ante*, p. 516) supplies the reference to the statement I had in memory as to "one of the Arctic voyages" (*ante*, p. 440), and my necessarily imperfect quotation does not materially differ from the actual statement.

With regard to Mr. Turle's letter (*ante*, p. 464), I would remark that my words, "comparatively shallow" waters, were not intended to apply to "two feet of water," and I would suggest that the innumerable sticklebacks embedded in the ice, and which did not revive, were probably dead (from suffocation) before they became so embedded.

It is well known that insects which habitually hibernate as larvæ or pupæ do not suffer from being frozen for a lengthened period. On the other hand, they suffer greatly in "open" winters with frequent alternations of wet, warmth, and cold. Therefore, from an entomological point of view, the season of 1891 promises to be an unusually productive one.

It is not my intention to return to this subject.

Lewisham, April 3.

R. McLACHLAN.

In a letter appearing under the above heading in your last issue, the writer asks for well authenticated instances of the recovery of frozen insects. All climbers have, at one time or another, met with butterflies, lying frozen on the snow, on Alpine passes, and many persons have brought down some of these insects, which, on reaching the warmth of the lower regions, invariably recover animation, though when picked up they are so completely frozen, and consequently so brittle, that they break to pieces unless carefully handled. I have frequently, when climbing, placed these frozen butterflies on my hat, and, on descending, have noticed them always fly away. It must often have been a considerable time from the frozen stage till recovery.

E. MAIN.

Grand Hotel, Montreux-Territet, April 4.

Quaternions and the Algebra of Vectors.

MY remark about Prof. Willard Gibbs was meant in all courtesy, and I am happy to find it so taken by him. The question between us, being thus a scientific one only, can afford to wait for a fortnight or so:—until my present examination season is past.

P. G. TAIT.

THE MULTIPLE ORIGIN OF RACES.

IN NATURE of March 5 (p. 415), the Duke of Argyll has printed a very interesting letter of Mr. Darwin's, from which he drew the inference that the writer "assumed mankind to have arisen . . . in a single pair." I do not think myself that the letter bears this interpretation. But the point in its most general aspect is a very important one, and is often found to present some difficulty to students of Mr. Darwin's writings.

Quite recently I have found by accident, amongst the papers of the late Mr. Bentham at Kew, a letter of friendly criticism from Mr. Darwin upon the presidential address which Mr. Bentham delivered to the Linnean Society on May 24, 1869. This letter, I think, has been overlooked and not published previously. In it Mr. Darwin expresses himself with regard to the multiple origin of races and some other points in very explicit language. Prof. Meldola, to whom I mentioned in conversation the existence of the letter, urged me strongly to print it. This, therefore, I now do, with the addition of a few explanatory notes.

W. T. THISELTON DYER.

Royal Gardens, Kew, March 27.

Down, Beckenham, Kent, S.E.,
November 25, 1869.

MY DEAR MR. BENTHAM,—I was greatly interested by your address, which I have now read thrice, and which I believe will have much influence on all who read it. But you are mistaken in thinking that I ever said

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you were wrong on any point. All that I meant was that on certain points, and these very doubtful points, I was inclined to differ from you. And now, on further considering the point on which some two or three months ago I felt most inclined to differ, viz. on isolation, I find I differ very little. What I have to say is *really* not worth saying, but as I should be very sorry not to do whatever you asked, I will scribble down the slightly dissentient thoughts which have occurred to me. It would be an endless job to specify the points in which you have interested me; but I may just mention the relation of the extreme western flora of Europe (some such very vague thoughts have crossed my mind, relating to glacial period) with South Africa, and your remarks on the contrast of passive and active distribution.

P. lxx.—I think the contingency of a rising island, not as yet fully stocked with plants, ought always to be kept in mind when speaking of colonization.

P. lxxiv.—I have met with nothing which makes me in the least doubt that large genera present a greater number of varieties relatively to their size than do small genera.¹ Hooker was convinced by my data, never as yet published in full, only abstracted in the "Origin."

P. lxxviii.—I dispute whether a new race or species is necessarily, or even generally, descended from a single or pair of parents. The whole body of individuals, I believe, become altered together—like our race-horses, and like all domestic breeds which are changed through "unconscious selection" by man.²

P. do.—When such great lengths of time are considered as are necessary to change a specific form, I greatly doubt whether more or less rapid powers of multiplication have more than the most insignificant weight. These powers, I think, are related to greater or less destruction in early life.

P. lxxix.—I still think you rather under-rate the importance of isolation. I have come to think it very important from various grounds; the anomalous and quasi-extinct forms on islands, &c., &c., &c.

With respect to areas with numerous "individually durable" forms, can it be said that they generally present a "broken" surface with "impassable barriers"? This, no doubt, is true in certain cases, as Teneriffe. But does this hold with South-West Australia or the Cape? I much doubt. I have been accustomed to look at the cause of so many forms as being partly an arid or dry climate (as De Candolle insists) which indirectly leads to diversified (?) conditions; and secondly, to isolation from the rest of the world during a very long period, so that other more dominant forms have not entered, and there has been ample time for much specification and adaptation of character.

P. lxxx.—I suppose you think that the *Restiaceæ*, *Proteaceæ*,³ &c., &c., once extended over the world, leaving fragments in the south.

You in several places speak of distribution of plants as if exclusively governed by soil and climate: I know that you do not mean this, but I regret whenever a chance is omitted of pointing out that the struggle with other plants (and hostile animals) is far more important.

I told you that I had nothing worth saying, but I have given you my THOUGHTS.

How detestable are the Roman numerals; why

¹ Bentham thought "degree of variability . . . like other constitutional characters, in the first place an individual one, which . . . may become more or less hereditary, and therefore specific; and thence, but in a very faint degree, generic." He seems to mean to argue against the conclusion which Sir Joseph Hooker had quoted from Mr. Darwin that "species of large genera are more variable than those of small."

² Bentham had said: "We must also admit that every race has probably been the offspring of one parent or pair of parents and consequently originated in one spot." The Duke of Argyll converts the proposition.

³ It is doubtful whether Bentham did think so. In his 1870 address he says: "I cannot resist the opinion that all presumptive evidence is against European *Proteaceæ*, and that all direct evidence in their favour has broken down upon cross-examination."