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 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."

should not the President's addresses, which are often, and I am sure in this case, worth more than all the rest of the number, be paged with Christian figures?

My dear Mr. Bentham,

Yours very sincerely,

CH. DARWIN.

HERTZ'S EXPERIMENTS.

I.

"OH! yes; I understand it all now. Electricity is the ether;" or, "Yes; it's just like everything else: electricity is a vibration." These are the remarks one hears made by those who think that a few scattered words picked up at a popular lecture make things quite clear. It is no doubt unfortunate that repeating a form of words is a different matter from understanding them, and still more different from understanding the subject they are intended to explain. In this case there is the added misfortune that the form of words is not accurately repeated, and in its inaccurate form does not mean what is true. It is often hardly worth while remarking this to those who make these statements, because the words convey to them little or no signification, and are to them as true as any other unmeaning sentence. The connection between electricity and the ether is certainly not, as far as is known, well described by saying that "electricity is the ether," and we cannot say with any certainty that electricity is or is not a vibration. Hertz's experiments have given an experimental proof of Maxwell's theory that electrical phenomena are due to the ether, and Hertz's experiments deal with vibrations. One cannot, however, say, because the pressure of 15 pounds per square inch exerted by the atmosphere is due to the air, that therefore "pressure is the air": nor even, because a person who studied the properties of the air had studied them by means of sounds propagated through it, can one assert that "pressure is a vibration." It is to be hoped no one will now assert that "electricity is pressure." The example is given to illustrate the absurdity of the statements made as deductions from recent experiments, and not to teach any new theory. And yet one comes across people who, after listening to an interesting lecture Lord Rayleigh might give, illustrated by Mr. Boys's sound-pressure-meter, would make the above statements, and really think they understood them. That blessed word "Mesopotamia" comforted the soul of an old lady with some reason, for religion is to some extent a question of feeling; but in science it is high treason to truth to be comforted by unmeaning sounds—they should produce despair.

It is to be hoped after this tirade that any reader of these articles who comes across statements he cannot understand will not tell himself the lie that he does understand them, nor pretend to others that he does. The subject is very difficult: one that has engaged the attention of thoughtful and clever men for many years, and is still in many parts, even to the most acute, shrouded with difficulties, uncertainties, and things unknown, so that nobody need be the least ashamed of not following even as far as others can go into this wonderful region. If the articles can give to most who read them glimpses which unfold intelligible ideas of even the outskirts of this region, it is all that any writer can reasonably expect who is not one of those masters of exposition who combine the highest scientific and literary abilities.

Consider for a minute the question at issue. That electric and magnetic phenomena are due to the same medium by which light is propagated—that all-pervading medium by whose assistance we receive all the energy on this earth that makes life here possible, by which we learn the existence of other worlds and suns, and analyze their structures and read their histories; that medium which certainly pervades all transparent bodies, and probably all matter, and extends as far as we know of anything existing: this wonderful all-pervading medium is the one we use to push and pull with when we act by means of electric and magnetic forces; and remember that we can pull molecules asunder by this means, as well as propel trains and light our houses. The forces between atoms are controlled by this all-pervading medium, which directs the compass of the mariner, signals round the globe in times that shame e'en Shakespeare's fancy, rends the oak and terrifies creation's lords in the lightning flash. It was a great discovery that proved all concord of sweet sounds was due to the medium that supplies the means of growth to animals and plants, and deals destruction in the whirlwind; and yet the 80 miles depth of our air is but a puny thing compared with the all-pervading illimitable ether.

That there is a medium by which light is transmitted in a manner somewhat analogous to that by which the air transmits sound has been long held proved. Even those who held that light was due to little particles shot out by luminous bodies were yet constrained to superpose a medium to account for the many strange actions of these particles. Now, no one thinks that light is due to such particles, and only a very few of those who have really considered the matter think that it can be due to air, or other matter such as we know. How does light exist for those eight minutes after it has left the sun and before it reaches the earth? Between the sun and earth there is some matter, no doubt, but it is in far-separated parts. There are Mercury and Venus, and some meteors and some dust no doubt, and wandering molecules of various gases, many yards apart, that meet one another every few days, perhaps, but no matter that could pass on an action from point to point at a rate of thousands of miles each second. Some other medium must be there than ordinary gross matter. Something so suble that the planets, meteors, and even comets—those wondrous fleecy fiery clouds rushing a hundred times more quickly than a cannon-ball around the sun—are imperceptibly impeded by its presence, and yet so constituted as to take up the vibrations of the atoms in these fiery clouds and send them on to us a thousand times more rapidly again than the comet moves to tell us there is a comet toward, and teach us what kinds of atoms vibrate in its tail. How can a medium have these contrary properties? How can it offer an imperceptible resistance to the comet, and yet take up the vibrations of the atoms? These are hard questions, and science has as yet but dim answers to them, hardly to be dignified by the name of answers—rather dim analogies to show that the properties supposed to coexist, though seeming contradictory, are not so in reality. One of the most beautiful experiments man knows—one fraught with more suggestions than almost any hundred others—is that by which a ring of air may be thrown through the air for many yards, and two such rings may hit, and, shivering, rebound. These rings move in curved paths past one another with almost no resistance to their motion, urged by an action not transmitted in time from one ring to another, but, like gravitation, acting wherever a ring may be, and yet the air through which they move *can* take up vibrations from the rings, showing thus that there is no real contradiction between the properties of things moving through a medium unresistedly in certain paths round one another, and yet transmitting other motions to the medium. This same air can push and pull, as when it sucks up waterspouts and deals destruction in tornadoes. Hence there seems no real contradiction between a medium that can push and pull and transmit vibrations, and yet offer no resistance to such fragile, light, and large-extended things as rings of air.