

question here. For ten years I have been engaged in dredging and trawling about the coasts of Scotland, chiefly as captain of Dr. Murray's yacht *Medusa*, and my experience does not coincide with that of Messrs. Brady, Scott, Thompson, and Herdman. For instance, in Loch Fyne I have always been able at any time of the year to collect in half an hour enormous numbers of *Euchæta*, *Calanus*, and *Nyctiphanes* over the mud in depths of about 70 fathoms or greater. Any person can see at the Millport Biological Station large bottles filled with these Crustaceans taken in a single haul. The stomachs of the herrings are frequently crammed with these Crustaceans, and the herrings certainly never got this food about the Laminarian zone, as suggested in Prof. Herdman's address.

Dr. David Robertson, who is one of the best-known collectors in the country, pointed out years ago that the Loch Fyne herrings got their food in the deep water, and attributed their fine quality to this fact. Dr. Robertson authorises me to say that, though there may be more species of Copepods in the Laminarian zone than in the deep water, still the number of individuals is very much greater in the deep water over the mud, as is conclusively proved by the *Medusa's* work.

Proper methods must, of course, be used, for I know of at least one instance in which a gentleman of considerable scientific repute was prepared to say that the free-swimming Crustaceans over the mud had completely left Loch Fyne; he communicated his opinion to Dr. Murray, with the result that the *Medusa* was ordered to Loch Fyne to investigate the matter. As was expected, the result was that these Crustaceans were found in as great profusion as on any previous occasion.

The result of my experience in Loch Fyne is that the nearer the nets are dragged to the mud in the deep water the greater will be the number of *Euchæta*, *Calanus*, and *Nyctiphanes* captured. I have taken, hundreds of times, in 70 fathoms, in a single haul lasting from twenty minutes to half an hour, more Copepods than can be collected in the Laminarian zone in eight or ten days. I have also captured herrings by means of drift nets sunk to the bottom, in depths of 70 and 80 fathoms, and their stomachs were filled with Crustaceans of the same species as we captured by the nets just over the mud at these depths.

As to the deep mud in Loch Fyne not producing a rich fauna, I may state that in the deepest water the trawl could not be kept down for a longer time than about half an hour; otherwise the deck engine and all other appliances on board would have failed to bring the net to the surface through sheer weight, chiefly of organisms. There was generally a certain percentage of mud present, but the bag of the net was generally crammed with thousands of *Actinia*, which live there, along with *Pecten septemradiatus*, *Hippolyte*, *Pandalus*, *Crangon*, *Ascidians*, and many other invertebrates and fishes.

I have dredged Loch Fyne systematically for months, and examined its fauna from the littoral zone to the greatest depth; the specimens collected are now beside me, and all the journals with the notes are in Dr. Murray's possession. But I think enough has been said to show that the greatest abundance of Copepods is not to be found in the Laminarian or other shallow zones, but in the deep water over the mud; also that the deep mud does possess a very rich fauna. I speak only of the abundance of the above-mentioned organisms, with which I am well acquainted. I am not a specialist nor a scientific man, but I have had a great deal to do with the practical part of the investigations which have assisted Dr. Murray in drawing his conclusions.

ALEXANDER TURBYNE.

Millport, Cumbrae, N.B., October 5.

The Toronto Meeting of the British Association.

AN effort will be made to have the meeting of the American Association for the Advancement of Science held at San Francisco in 1897, so that the members of the British Association may cross the continent, and join us there, either before or after their own meeting at Toronto, which many of us hope to attend.

A suggestion of great importance, and deserving immediate consideration, seems to me that the Australasian Association should try to arrange a meeting for the same year on the Pacific coast of America, so that we may all join in the meeting of the American Association at San Francisco. This will be the first meeting of any of these Associations on that coast, and hence a momentous occasion.

I do not know how to reach the officers of the Australasian Association; but think that an insertion of this letter in NATURE

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will find them. I have already sent a letter at a venture to the President by his official title, as I do not know his name, in care of the Post-master of Melbourne, to be forwarded; but perhaps the Post-master may not know where to send it.

I have also written to Mayor Sutro of San Francisco, calling his attention to it.

Brooklyn, October 9.

WM. H. HALE.

The Theory of Magnetic Action upon Light.

IN the British Association Reports for 1893, Mr. Larmor has attempted to show that a satisfactory theory of magnetic action upon light can be constructed by means of a modification of Maxwell's theory which was proposed by Prof. Fitzgerald in 1879; and he alleges, with special emphasis (see p. 349), that his theory furnishes "a consistent scheme of equations of reflection and refraction, without the necessity of condoning any dynamical difficulties in the process." And on p. 359, after raising objections against a theory originally suggested by Prof. Rowland, and afterwards fully developed by myself, he says:—"But against this procedure," that is my own, "there stands the pure assumption as regards discontinuity of electric force at an interface."

To fully discuss the defects of Larmor's resuscitation of Fitzgerald's theory would occupy too much space, and would necessitate the introduction of a considerable amount of mathematical analysis. I shall, therefore, confine myself to pointing out that his theory is open to exactly the same objections as my own, viz. *discontinuity of the tangential component of electromotive force at an interface.*

One of Larmor's boundary conditions (see p. 349) is equivalent to the condition that the expression

$$\frac{4\pi g}{K} + 4\pi \frac{CdB}{d\theta} - 16\pi^2 C\gamma_0 \frac{df}{dt}$$

should be continuous. Now $4\pi g/K = Q$, where Q is one of the tangential components of the E.M.F. at an interface; also in unmagnetised media $C = 0$. Consequently, if accented letters refer to the latter medium, the condition becomes

$$Q + 4\pi CdB/d\theta - 16\pi^2 C\gamma_0 df/dt = Q';$$

in other words, *the tangential component of the E.M.F. is discontinuous.*

A. B. BASSET.

Holyport, Berks, October 9.

The Society of Chemical Industry and Abstracts.

AT the recent annual meeting of the Society of Chemical Industry, the retiring President and the new President each made some remarks concerning the cost of the journal of the Society, and the necessity of curtailing expenses by dealing more strictly with the abstracts. I suppose hardly any two of us would quite agree as to what is the rubbish, Teutonic or otherwise, which ought to be left out, and what is good matter, which ought to be abstracted at greater or less length. No matter who is editor, all of us would abide as firmly as ever in the belief that we could have made a better selection of articles for abstraction. Before, however, we set about any further movement in the direction of cutting down abstracts to a mere useless list of titles, I would like to point out one direction in which expense might safely be curtailed without fear of objection from any quarter. All will agree, I am sure, that it is a waste of money to abstract the same article twice. I am sure other members besides myself must have noticed that this blemish is not entirely absent from the Society's journal. It should be known to every chemical babe and suckling, that even very unimportant papers are sometimes published more than once. Yet this seems to have escaped the notice of whoever is responsible for the editing of the abstracts. Witness the following from this year's journal:—P. 389, "Sulphides of Cobalt and Nickel, A. Villiers (*Bull. Soc. Chim.*, 1895, 13 [4])," and "Qualitative Separation of Nickel from Cobalt, A. Villiers, *Bull. Soc. Chim.*, 1895, 13 [4]." Now let us turn to p. 524, where we find, "Sulphides of Nickel and Cobalt, A. Villiers, *Comptes rend.*, 1894, 119," and on p. 509, "Qualitative Separation of Nickel and Cobalt, A. Villiers, *Comptes rend.*, 1895, 120." We have cobalt and nickel in one case, and nickel and cobalt in the other; but the articles from the *Bull. Soc. Chim.* are the same as those from the *Comptes rend.*, and by the same author. A still more incomprehensible example will