LETTERS TO THE EDITOR.

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Ornithology of the Sandwich Isles.

IT is easy to make assertions which, however improbable, it is not easy to disprove. I would therefore invite Mr. Albert F. Calvert to furnish documentary evidence of those he has advanced (*suprà*, p. 485). They do not indeed materially affect what I had said; yet, for the sake of accuracy, it might be as well to know on what foundation they rest. Those who are interested in the growth of ideas will be pleased to find that Sir Joseph Banks was so far in advance of his time as, on his return from his voyage with Captain Cook, which ended in 1771, to have "several cases of birds carefully mounted and arranged according to the localities in which they were collected"; and, among them, a "group of land birds from Owhyhee"—an island which Cook did not discover until 1778—or seven years later. As these assertions alone concern the subject on which I wrote, I refrain at present from offering any remarks on the others; but your correspondent seems to have been the victim of a delusion or something worse.

ALFRED NEWTON.

I OBSERVE in NATURE (p. 485) a letter from Mr. Albert F. Calvert, in which he states that certain cases of birds, which were collected by Sir Joseph Banks during his voyage in the *Endeavour* with Captain Cook, "were in the custody of the Linnean Society of London until 1863, when they formed part of their natural history sale."

This is not the fact. The birds belonging to Sir Joseph Banks were never in the possession of this Society. It is true that the Society at one time possessed the insects and shells which formed part of the Banksian collections, and these in 1863 were presented, not sold, to the Trustees of the British Museum. Had there been any birds, they would doubtless also have been presented at the same time.

Where, then, did the cases, which Mr. Albert F. Calvert says are still "carefully preserved in the museum of his ancestor Mr. John Calvert," come from? Certainly not from the Linnean Society. In view of Prof. Newton's valuable communication to NATURE (March 17, pp. 465-69), it is of importance that this inquiry should be answered, and a list furnished of the species contained in these cases, the reported existence of which will agreeably surprise ornithologists.

As Mr. A. F. Calvert has disclosed a source of information likely to be useful, perhaps he may be able to answer another question.

In 1860 the late Mr. J. D. Salmon, a well-known oologist, bequeathed to the Linnean Society a valuable collection of birds' eggs. This collection was known to contain the eggs of many British birds which were then becoming scarce, and have since become still rarer, if not quite extinct as breeding species; such, for example, as the golden eagle, osprey, kite, buzzard, honey buzzard, raven, chough, dotterel; and amongst some of the rarer species not found breeding in Great Britain, the spotted eagle, gerfalcon, black kite, rough-legged buzzard, Lapp owl, &c., and, above all, an egg of the great auk (*Alca impennis*), the value of which alone would almost equal that of all the other eggs in the collection.

I am informed that on the death of Mr. Salmon this collection was intrusted by his executors, for the purpose of being catalogued, to someone known to Mr. A. F. Calvert's ancestor; and by some accident, when it came to be handed over to the Linnean Society, the egg of the great auk was (some time afterwards) found to be missing, as also the eggs of certain species above mentioned, with several others that might be named.

Possibly they may have been removed at the time for safe custody, and were forgotten to be returned when the collection was deposited in its present resting-place.

Perhaps Mr. A. F. Calvert will say whether by chance these eggs (like the Banksian birds) have been "carefully preserved in the museum of his ancestor." If so, I presume that, on proof of the bequest of the collection to the Linnean Society, and proper identification of the eggs by their numbers, initials, or other

marks, Mr. A. F. Calvert would be willing (on behalf of his ancestor) to restore them to the cabinet from which they have been so long missing.

J. E. HARTING.

Linnean Society, Burlington House, March 28.

Poincaré's Thermodynamics.

IT is clear that I was justified in attributing the gist of M. Poincaré's first letter to his not having sufficiently read my notice of his book. He has not even yet fully apprehended the bearings of that notice, as a few words will show. Far from being unable to uphold any one of my critical remarks, as M. Poincaré is pleased to hint may be the case, I reassert every one of them, and could easily add to their number.

Let us begin with the particular item of my criticism which M. Poincaré persists in regarding as the most important. My words were:—"in his assumed capacity [of pure analyst] he quite naturally looks with indifference, if not with absolute contempt, on the work of the lowly experimenter." As an illustration of this I instanced M. Poincaré's ignorance of the thermo-electric researches of Sir W. Thomson, Magnus, &c. Then I quoted (in full) two of his remarks on the "Thomson effect." In the first of these he used the very peculiar phrase

"Sir W. Thomson *admet* qu'il existe une force &c."—and in the second he said

"si l'effet Thomson a pu être mis en évidence par l'expérience, on n'a pu jusqu'ici constater l'existence des forces électromotrices qui lui donnent naissance."

To these he has, in his recent letters, added other like statements. Now, as I understand the matter, Lord Kelvin *proved* (which, as I take it, means a good deal more than might be implied by "constater") the existence of the electromotive force which depends on the so-called "Thomson effect" (giving also thereby the means of measuring its amount) by showing that the Peltier electromotive force does not in general fully account for the observed current in a thermo-electric circuit, and may even be directly opposed to it; while no other source of electromotive force can exist save the gradation of temperature in one or both of the metals. He then proceeded, by experiment, to measure the amount of the "Thomson effect" for unit current in various metals, unequally heated. When the passages above quoted from M. Poincaré's work are compared with the facts just stated, my comments on them will be seen to be fully justified.

It is necessary to add that I made no reference whatever to M. Poincaré's distinctions between "true" and "apparent" electromotive force:—simply because I regard these, along with many other celebrated terms such as "disgregation" &c., as mere empty names employed to conceal our present ignorance.

As to the three chief objections I made to the work of M. Poincaré, every one (author, critic, or onlooker) is entitled to form and express his opinion. I need not restate mine, though I continue to adhere to every word of it:—but I may make the following additional remarks on these objections severally.

1. What sort of title to completeness can be claimed for a Treatise on Thermodynamics, in which no mention is made of the grand principle of Dissipation of Energy; nor of Thermodynamic Motivity, "that possession the waste of which is called Dissipation"?

2. With regard to the measurement of Absolute Temperature, what I *did* say was that the experiments of Joule and Thomson, which justified them in basing it on Carnot's Function, were not mentioned by M. Poincaré in this connection. The omission by M. Poincaré of the italicized words makes an absolutely vital change in the meaning of my statement; and enables him to make what, (at first sight only), appears to be an answer to it.

3. As regards the foundation of the Second Law, it is unfortunately clear that M. Poincaré and I must continue to differ:—so that further discussion of this point would be unprofitable. For I presume that M. Poincaré has not formed his opinion without careful study of all that Clerk-Maxwell said on the point:—so that even a perusal of Lord Kelvin's latest paper (*Fortnightly Review*, March 1892) is not likely to induce him to change it.

P. G. T.

26/3/92.

M. Poincaré and Maxwell.

In his recent treatise on "Électricité et Optique," M. Poincaré professes to give a description of Maxwell's theories of electro-