

shown fail to recognize the bird at all. They speak also of another flightless bird by the name of *Mehunui*. This bird, one old and very intelligent Moriori informed me, was the same as the Maoris called *Kakapo*. Mr. Alexander Shand, an old resident in the Chatham Islands, and the sole European living versed in Moriori customs and traditions, and capable of speaking their language with fluency, also confirmed this information, and told me that the *Kakapo* (according to the Morioris) was very abundant in the islands prior to 1836. He himself in the early days had seen their burrows often. I had observed, while collecting, several Psittacine bones, and on learning this fact I felt sure that those I had picked up and packed away must belong to *Stringops*. On my arrival here, however, I find so far that there are no *Kakapo* bones in the collection, the Psittacine bones being the head and beaks of *Nestor notabilis*, the Kea. I have as yet had time to do no more than run through the collection I have brought back; but there appear to be in it several large Ralline tibiae of species unknown to me. I am looking forward to another opportunity of thoroughly exploring these interesting islands with more time at my disposal than I could afford on this occasion.

HENRY O. FORBES.

Canterbury Museum, February 23.

Pigments of Lepidoptera.

A LETTER of mine on the subject of butterfly pigments was published so recently in NATURE (December 31, 1891, p. 197) that I hesitate to ask for further space at the present time. But the appearance of Mr. Perry Coste's articles, together with the tone of some remarks made by him at the close of the last article, lead me to venture upon a few words, partly in criticism of a theory he advances, and partly (though this is less important) in claim of priority, since Mr. Coste does not do me the honour to refer to my work on the subject.

The chief generalization which Mr. Coste bases upon his experiments is that which he terms the "reversion effect,"—that is to say, the production of yellows from reds by the action of acids, and the restoration of the former by neutralization and other means. The theory that he advances to explain these phenomena—namely, that the red body acts as a base, and forms with acids salts which are yellow—is quite untenable. As I have shown (Proceedings of the Chemical Society, June 1889; vide NATURE, vol. xl. p. 335), the soluble yellows are themselves acid bodies of quite definite composition. Indeed, as far back as 1871, Prof. Meldola called attention to the fact that the pigment of *G. rhamni* was soluble in water, and showed that its aqueous solution had an acid reaction. Mr. Coste has worked with *D. eucharis*; if he will dissolve the red pigment from the border of the hind wing of this insect in pure water, he will find that a yellow solution is the result, but that, if the solution be evaporated to dryness, the solid residue of pigment is red once more; showing that there is either the question of hydration to consider, or a weak combination of the yellow acid body with a base, which is dissociated in aqueous solution. At any rate, I have obtained from this red pigment of *eucharis* a silver compound which contains a percentage of metal exactly equal to that from the pigment of *G. rhamni*.

In 1889 I was able to predict possible constitutional formulæ for the acid yellow pigments, and am happy to say that recent careful combustions of their silver salts to a large extent confirm my original ideas. My results will be shortly published *in extenso*.

Mr. Perry Coste's experiments are very useful as forming a method of classifying these lepidopterous pigments; but, if he will forgive me for saying so, they are of far too empirical a nature for any considerations as to the constitution of the bodies to be based upon them. As one who has for many years past spent a large portion of his time and no inconsiderable portion of his substance in obtaining a sufficiency of these pigments for analysis and investigation, Mr. Coste will forgive me if I do not respond to his invitation to leave him "to continue his researches alone." It is hardly well for one investigator to say "hands off" to another, and I shall not imitate Mr. Coste in this matter; but will only express a hope that in his future work he will not confine himself to the immersion of the wings of his insects in strong and destructive reagents.

I have lately been working at the genesis of these pigments in the pupæ, and might say something with regard to the nature of

the group which Mr. Coste labels as doubtfully pigmentary; but for the present I have sufficiently trespassed upon your space.

F. GOWLAND HOPKINS,

Guy's Hospital, S.E., April 9. Gull Research Student.

"C.G.S. System of Units."

THE new edition of Prof. Everett's "C.G.S. System of Units" contains, at the very beginning, two misleading statements, based seemingly on a misapprehension of facts. In so valuable a work, such errors are to be deplored.

Pp. xiii. and xiv. give an account of weighings made at the International Bureau of Weights and Measures between certain "standard pounds" and the international standard of mass.

From the statement as given, it would be inferred that there is room for doubt as to the relation between the British standard of mass and the international kilogramme.

The real facts are, that the standard pounds were only nominally "pounds"; they were standards with known corrections, which, however, have not been applied to the equivalents given on p. xiv.

The true relation of the Imperial pound to the international kilogramme is given in the Board of Trade Report of Proceedings under the Weights and Measures Act, 1884 (p. 4), according to which the Imperial pound = 453.5924277 grammes.

On p. 34 of "C.G.S. System of Units," Mr. Chaney's results of the weight of a cubic inch of water are discussed, and the conclusion is reached that Mr. Chaney's result differs by 0.00125 from the theoretical relation between volume and mass of water in the metric system.

This result is obtained by comparing Mr. Chaney's results, without reduction to *vacuo*, with the mass of a cubic centimetre of water.

Mr. Chaney states that the standard air to which his result is reduced weighs 0.3077 grains per cubic inch. Therefore his result reduced to *vacuo* is: one cubic inch of water at 62° F. weighs 252.286 + 0.308 = 252.594 grains.

If we take the value for the thermal expansion of water, in terms of the hydrogen thermometer scale, as determined at the International Bureau, we find the density of water at 16°.667 C., = 62° F., referred to its maximum density = 0.998861.

Using the equivalents 1 metre = 39.3700 inches, and 1 gramme = 15.43235639 grains, we obtain: one cubic inch of water at 62° F. weighs in *vacuo* 252.6045 grains; while Mr. Chaney found 252.594 as above given, a discrepancy of one part in 25,000 only, as compared with one part in 800, given by Prof. Everett. It is not clear from Mr. Chaney's statement whether his weight in air is against brass or other weights; therefore the vacuum reduction above applied is uncertain by a small amount.

O. H. TITTMANN.

Washington, D.C., March 10.

MR. TITTMANN thinks the true relation is, without doubt—

1 pound = 453.5924277 grammes.

Prof. W. H. Miller determined it to be—

1 pound = 453.59265 grammes,

which is the value given on the Card of Equivalents published by the Board of Trade. If the determination quoted by Mr. Tittmann from a Board of Trade Report of 1884 was made under such conditions as to render it authoritative, it is a pity it has been allowed to remain for eight years buried in a Blue-book. One would have expected some intimation of it to be given to scientific men through the Royal Society or in the pages of NATURE.

As regards the three "standard pounds" which were compared with standards at the Bureau International in 1883, Mr. Tittmann says they had known corrections. This is not stated in the *Travaux et Mémoires*, where the account of the comparison is given. There is, however, in the case of the two which are of gilded bronze, a reference to a description of them by Prof. Miller in his paper on the standard pound (Phil. Trans. 1856), and, on turning to it, I find that their errors, as stated by him, do not agree even approximately with the determinations made at the Bureau. They differ even in the first significant figure of the error, which is the sixth figure of the entire value; so that, as far as this evidence goes, the five figures 45359 are all that are certain.