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.

As regards the other matter referred to, Mr. Tittmann does not mention the publication in which "Mr. Chaney states that the standard air to which his result is reduced weighs 0.3077 grains per cubic inch." The only publication known to me is Mr. Chaney's paper in the Proceedings of the Royal Society, and it does not contain any such statement.

I have always been taught to regard a standard weight as a standard of mass, and therefore independent of such conditions as temperature, pressure, and the material in the other scale pan; whereas, it appears that Mr. Chaney, by direction of the Board of Trade, has made a determination which is only true for a particular density of the surrounding air, and a particular density of the weights in the other pan.

For scientific purposes a standard of reference should be free from variable elements, and should be of the utmost attainable simplicity. For commercial purposes determinations to six figures are frivolous.

Mr. Tittmann's reductions appear to contain two errors. Instead of adding the weight of a cubic inch of air, he ought to have added the difference between this and the weight of the air displaced by the weights in the opposite pan. Again, he takes the metre as 39.3700 inches, whereas Clarke's value is 39.370432, and Kater's 39.37079.

I have had some correspondence with Mr. Chaney since the publication of my new edition, and have had an erratum slip printed, which I trust you will allow me to subjoin, as it may be useful to several of your readers J. D. EVERETT.

5 Princess Gardens, Belfast, March 28.

Addenda and Corrigenda.

Page 63. In reducing Cailletet's experiments, '0000026 should have been added instead of '0000039.

Page 77. *Add*—Violle's determination of velocity of sound is 331.10 ± 0.1 . *Ann. de Chim. XIX. March, 1890.*

Page 176, line 10. *For* Willeumier, 1890, *read* Willeumier, 1890, Lippmann method.

At end of page 164, *add*—Expressing C in amperes, R in ohms, and T in seconds, the heating effect in gramme-degrees is $C^2RT/4.2 = .24C^2RT$.

Page 35. Mr. Chaney's determination here quoted was not intended as a determination of the density of water, but of the apparent weight of water when weighed in air of density .00121684 against brass weights of density 8.143. The correcting factor for deducing the weight *in vacuo* or true density is 1.0010687, which will change the value .998752 obtained in the text into .99982, to compare with Tralles' .99988.

Mr. Chaney's result is for distilled water deprived of air, and Tralles' appears to be for ordinary distilled water. According to results recently obtained by the Vienna Standards Commission (*Wied. Ann.*, 1891, Part 9, p. 171), water deprived of air has the greater density, the difference being .0000032 at 0° C., and .0000017 at 62° F. These differences are too small to affect the above comparison.

Influenza in America.

IN my copy of "Johnson's Dictionary of the English Language in Miniature, to which are added an alphabetical account of the heathen deities and a copious chronological table of remarkable events, discoveries, and inventions, by the Rev. Joseph Hamilton, M.A., second American edition, Boston, 1806" (12mo, pp. 276), I find on p. 275, "Influenza in North America, 1647, 1655, 1697-98, 1732, 1737, 1747, 1756-57, 1761, 1772, 1781, 1789-90, 1802."

It is quite possible that these dates are well known, but they are new to me, and may be of interest in connection with the recent epidemic. EDWARD S. HOLDEN.

Mount Hamilton, March 29.

DUST COUNTING ON BEN NEVIS.

WITHIN the last few years quite a new factor has been introduced into the study of meteorology—namely, that which treats of the dust particles in the atmosphere, of the number of dust particles present in the air at any time, and the effect of dust in the air on climate and weather changes. It is now beginning to be recognized that the study of dust and its behaviour in the air forms the stepping-stone to the study of almost all

meteorological problems which deal with clouds and precipitation, solar and terrestrial radiation, and in a general way with the diurnal and annual variations in the temperature and pressure of the atmosphere. Mr. Aitken's work in originating this branch of science, and in making and discussing numerous observations of the number of dust particles in the air of various places in this country, as well as on the Continent, at various altitudes, is pretty well known already (see *NATURE*, vol. xli. p. 394). Mr. Aitken's results and conclusions were looked upon as being of such importance as to warrant some of our leading meteorologists to apply to the Research Fund of the Royal Society for a grant to enable them to equip the Ben Nevis Observatory with Aitken's dust-counting apparatus. The application was successful, and instruments were at once ordered, and in due time erected at the Observatory.

The apparatus consists of two dust counters, one a portable form for use in the open air, and the other a laboratory form for use inside the Observatory. The latter is fixed in the middle room of the tower, and has pipes leading out to the free air, so that it is possible to observe with it in almost all sorts of weather and at any hour day or night. The principle on which these instruments are constructed, so as to make the tiny invisible particles of dust visible and easily countable, is pretty well known already. Briefly it is this. To make the particles visible, the air containing them is saturated with water vapour, and by a stroke of an air-pump it is thereafter cooled so much as to cause a condensation of the vapour on the particles, whereby these are thus made visible. Ordinary air is so dusty that if the receiver were full of such air it would be impossible to count the particles, and to make them easily countable the following process is resorted to. First, the chamber or receiver, whose capacity is accurately known, is filled with pure dustless air by means of an air-pump and filter. Then a fifth, a tenth, or any other fractional part of the amount of pure air inside is taken out, and the same amount of dusty air allowed in. In this way the density of the shower caused by condensation is completely under the observer's control. A small graduated stage is placed one centimetre from the top of the receiver, so that all the dust above this falls on to it, and by means of a magnifying glass all the particles on one or more of the small squares of the stage are easily counted. Then, by multiplying by the reciprocals of the various fractions used we arrive at the number of dust particles in a unit of the free dusty air. In making an observation, the mean of ten such tests is taken as the number of particles present for that time.

Observations were begun at the Ben Nevis Observatory with the portable instrument in February 1890, and with the other instrument in the following summer. During the whole of that year the work done was mainly preliminary, as great difficulty was experienced in getting the dust work to fit into the general routine of Observatory work. The dust inquiry is not like some other special inquiries, that can be prosecuted for a certain time, and then discontinued after definite positive or negative conclusions thereanent have been arrived at, but must, on the other hand, be carried on side by side with the other observations of meteorological phenomena, as pressure, temperature, humidity, &c., with any of which it is of equal importance, and having once been admitted into the general routine of meteorological observations it must be kept on. This fact was soon seen on Ben Nevis from the extraordinary variations that were observed in the dustiness of the air with changes of weather; and it was attempted to make continuous hourly observations of the dust as of the other elements. It was found, however, that this could not be done without crippling the general routine, this being as much as the two observers at the Observatory could well cope with. In February 1891