

was made and decanted or transferred from the vessel in which it was liquefied to another by means of a valve, and thereby rendered capable of use as a cooling agent. In support of this assertion, I call as witness Prof. Charles Olszewski himself, who states in the *Philosophical Magazine*, February 1895, p. 189: "In 1883, and for several years following, I liquefied the gases in a strong glass tube." There is no suggestion made that a steel cylinder and valve was used by Olszewski till the year 1890. Whereas four years in advance I had used a much safer and better form of apparatus, practically identical in principle with that used in Cracow in the year 1890. Have I ever suggested that Prof. Olszewski was anticipated, or attempted to raise any question of priority? Perhaps the critic will have the audacity to say, in reply, this is no publication, the *Proceedings* of the Royal Institution, English and American science periodicals, not being amongst the class of recognised scientific journals. Well, if I am pleased to throw my bread upon the waters, adopting the view that every truthfully recorded experiment which appears in any journal associated with my name is publication, surely I should simply be conducting myself in the "too modest" way my critic commends.

As a specimen of the distortion of facts to prove another case of priority that is claimed, I find that MM. Charles Olszewski and Auguste Witkowski, Membres Correspondants, presenting their memoir "Propriétés optiques de l'oxygène liquide," on October 3, 1892, and, on referring to the paper, it is dated July 15, 1892, and the following footnote is added:

"Avant la publication de notre communication, MM. Liveing et Dewar ont fait connaître (*Phil. Mag.* Aout 1892), les résultats de leurs recherches sur la refraction des gaz liquéfiés."

Yet the critic says our experiments were "mainly repetitions of the work of Olszewski and Witkowski." The garbled extracts selected to make it appear that I have been guilty of misrepresentation are all of the same kind. . . . Thus I am taken to task for using the expression in the lecture on liquid air, of 1893: "Having no recorded experience to guide us in conducting such investigations, the best instruments and methods of working have to be discovered." The next sentence runs as follows: "The necessity of devising some new kind of vessel for storing and manipulating exceedingly volatile fluids like liquid oxygen and liquid air, became apparent when the optical properties of the bodies came under examination. Apart altogether from the rapid ebullition interfering with the experimental work, the fact that it did take place involved a great additional cost in the conduct of experiments on the properties of matter under such exceptional conditions of temperature." What can be said in defence of such glaring misrepresentation of the meaning of my words? Mr. M. M. Pattison Muir's demand for "*instant and serious consideration*" of his client's "*case*" has been quickly met. I trust the result will . . . fit in with his brief. JAMES DEWAR.

Royal Institution, February 12.

[A few personal remarks in Prof. Dewar's letter have been omitted, as they do not affect the points at issue.—Ed. NATURE.]

#### Vertebrate Segmentation.

MR. H. G. WELLS, in a recent number of *NATURE*, honours my little book by making it an example of a contravention of what he regards as a principle of education. With that I have no quarrel. But I must object to the instance he has chosen. The sentences from which he quotes refer to the phenomena of segmentation common to celomate tissues, and not to the derivation of vertebrates from any invertebrate group. So far from giving "the impression almost in so many words—'cut and dried,' and ready to be cast into the oven—the vertebrate type is merely a concentrated derivative (concertina fashion) of the chaetopod type," I devote the chapter (xv.) from which he has taken his quotations, to showing that the earthworm and the vertebrates merely belong to two out of the many isolated groups; and at the end of the chapter (though not in spaced type, as I did not consider the question of vertebrate descent congruous with the aims of an elementary textbook) I state that "the type common to the lowest members of the groups of which the earthworm on the one hand, and the vertebrates on the other, form the highest examples, is a simple unsegmented celomate animal."

P. CHALMERS MITCHELL.

#### The Black-veined White Butterfly.

MR. KIRBY, on p. 340 of your last issue, says (in criticism of Mr. Furneaux) that this insect "would not frequent open ground at a distance from trees." I suppose there are not now many Englishmen who have taken it in this country; and it may be worth while to record that the common on which my brother and I used to find it tolerably abundant in the years 1857-1859, was quite an open place, with no adjacent wood, and very little hedge timber. This common is about a mile and a half to the west of Cardiff; I passed it in the train a few weeks ago, and noted that it is being encroached on by suburbs. We had many a hot chase there over gorse and briar, and always considered this butterfly the most difficult of all to catch. I have never seen it in England since 1859 or 1860.

Oxford, February 11.

W. WARDE FOWLER.

#### Parrots in the Philippine Islands.

PRAY allow me space to acknowledge a bad mistake which I first made in the ninth edition of the "*Encyclopædia Britannica*" (xviii. p. 322), and have lately repeated in the "*Dictionary of Birds*" (p. 687), by asserting that parrots are "wanting in the Philippine Islands." Seeing that the article was written more than ten years ago, it is quite out of my power to account for the misstatement: my only wonder is that it has not been before challenged, since there is, and has been for some centuries, abundance of evidence to show that there are plenty of parrots in that group of islands, which, indeed, is as well furnished with them (as remarked by my friend Mr. L. W. Wignlesworth, who has kindly drawn my attention to my error) as is the island of Celebes, and I had already (p. 93) noticed the Philippine species of Cockatoo.

Cambridge, February 9.

ALFRED NEWTON.

#### TWENTY FIVE YEARS OF GEOLOGICAL PROGRESS IN BRITAIN.

LOOKING back across the fourth part of a century in the progress of any branch of science, we naturally turn first to the list of names of those to whose labours that progress has been due, and though many of these names may happily still be counted among the living, we note many a blank where the hand of death has thinned the ranks. Perhaps in this country no department of natural knowledge can boast a more illustrious bead-roll than that of Geology. The story of the earth had hardly begun to be scientifically studied until the first decades of the present century, and some of the early fathers of geology lived on until well within the life-time of the present generation. A curious transition has thus been going on during the last five-and-twenty years. On the one hand, there have been moving amongst us geological magnates who achieved their fame in the old days when it was still possible for a man to possess a tolerably full personal knowledge of almost every department of the science. On the other hand, around these few living memorialists of the heroic age, grew up hosts of younger men, who, finding the main lines already traced for them, have become in large measure specialists, devoting themselves with enthusiasm, but with more restricted vision, to one formation, or one group of rocks, or one tribe of fossils. The days of broad outlines and rapid generalisation have gone. No new systems remain to be added to the geological record of these islands. No new assemblages of extinct types of life now reward the sedulous collector. We have entered upon the era of minute detail and patient elaboration. The field-glass has given way to the microscope. The advance of the science must now be based on laborious research, less brilliant no doubt in its immediate effect, but probably not less lasting in its influence and its results.