

volume of the "Principles," Lyell not only rejected the theory of Lamarck, but went far towards abandoning, for the time, any idea of "the transmutation of species."

It is scarcely necessary here to recall the fact that this second volume of the "Principles," so full of discussions bearing on the changes in organic life, reached Darwin in South America, just at the time when he was startled by discovering the relations between the living and recently extinct mammals of that continent. From that time forth Darwin no longer regarded the question of evolution with indifference. In the critical period between the return of the *Beagle*, in 1836, and the writing of the first sketch of the theory, in 1842, constant intercourse took place between the two friends: "I saw more of Lyell," says Darwin in his autobiography, "than of any other man, both before and after my marriage" (in 1839). In their frequent discussions, Darwin would become fully acquainted with the arguments of Cuvier and his school, which are, indeed, very clearly and trenchantly reproduced in the first three chapters of the second volume of the "Principles," which Darwin called his "own true love."

These facts borne in mind, I think we can have no difficulty in realising the source of the statements made by Darwin. I think the sentences may be paraphrased as follows:—

"Anti-evolutionists admit the inheritance of small variations. Well, the inheritance of such small variations is all I require for my theory of Natural Selection. I can afford to concede the non-inheritance of the greater variations."

But it is interesting to notice that in the sentence about plants and sea-shells following the passage in question, and in his discussion of the appearance and inheritance of a *sixth digit* in man, &c., Darwin was not satisfied that only small variations were transmitted.

It was the remembrance of facts like these that led me to suggest that the subject was "constantly present" in Darwin's mind. Prof. Meldola, thinking of the more acute discussion of the question aroused in 1885 by Weismann's declaration that no acquired characters are inherited, naturally expressed doubt on the subject, and I, of course, admit that this phase of the question, in all probability, never presented itself to Darwin, or at least never demanded his serious consideration.

Kew.

JOHN W. JUDD.

The Transference of Names in Zoology.

As the preparation of an official list of *Nomina conservanda* is now under consideration by the International Commission on Zoological Nomenclature, it may not be out of place to direct attention to a point that seems to me of prime importance in this connection, although it has received little notice from recent writers on nomenclatural reform.

It is simply this—while the rejection and replacement of familiar names for well-known animals is, of course, an inconvenience to zoologists, it is a trivial matter in comparison with the grave possibility of confusion that arises when the names are used in an altered sense. In the former case we merely multiply synonyms, and, unfortunately, they are so numerous already that a few more hardly matter; in the latter case there is a real and serious danger of ambiguity. Thus, at present, a writer who mentions *Trichechus* may be referring either to the walrus or the manatee, *Simia* may mean either the orang or the chimpanzee, *Cynocephalus* may be either a "flying lemur" or a baboon, and so on through all the great groups of the animal kingdom until we come to *Holothuria*, which may refer either to a sea-cucumber or to a Portuguese man-of-war. Cases like these seem to me to be on an entirely different plane as regards practical importance, from those in which an old name is simply rejected; even if the shore-crab is to be called *Carcinides* for the future, we have only the additional burden of remembering that it was once called *Carcinus*.

A striking (if somewhat exceptional) instance of the pitfalls that are in preparation for future students is found in the section on Crustacea in Bronn's "Thierreich" (Bd. v., Abth. ii.). On p. 1056 there is an allusion to "*Astacus*," and on the following page to "*Astacus*

(=*Homarus*)."

In the bound volume (unless the part-wrappers have been kept in place) there is nothing to show that a change of authorship intervened between those two pages, and that, while the second "*Astacus*" refers to the lobster, the first indicates the crayfish.

If the International Commission could be persuaded to consider first those names that are threatened with *transference*, before proceeding to deal with those that are merely in danger of *replacement*, they would, I believe, secure the support and cooperation of many zoologists who have doubts as to the practicability of the schemes lately put forward.

W. T. CALMAN.

British Museum (Nat. Hist.), Cromwell Road,
London, S.W., January 23.

Sex Relationship.

It seems a pity that writers should allow their political bias to influence their work, and especially that they should not at least ascertain the facts of a case before writing about it.

In his article on "Sex Relationship" in *NATURE* of January 5, Dr. R. J. Ewart said, in commenting on the present excess of females over males:—"The result of this is to produce in a community a section of women who cannot possibly perform that function for which they were fashioned. Their energies are naturally directed into other spheres, as evidence of which we see the revival of the movement for political recognition. The agitation is no new one, and apparently is dependent for its strength and virility on the position of the sex pendulum," &c.

Now, first, it may be observed that women are no more fashioned to perform a single function than men are; their natures are as complex, their brains as varied as men's—in fact, "God Almighty made 'em to match the men."

Secondly, the excess of females of all ages over males in this country is between one and two millions, while five million women earn their own livelihood. Thus a large number even of those who perform "the function for which they were fashioned" are obliged to "direct their energies to other spheres," quite irrespective of any excess of females.

Thirdly, there is no *revival* of the movement for political recognition—it has culminated. Since it first began with any vigour, in 1867, it has gone steadily on, and its greater activity during the last five years has been due to the genius and courage of two women, who had the political insight to realise that, by some curious quality in the psychology of men, the only tactics that are successful in obtaining a reform of the franchise are militant tactics.

Fourthly, the countries in which English-speaking women have already gained their political freedom are *not* those in which there is an excess of women over men, but are the comparatively new countries—New Zealand, Australia, and some of the western States of America.

Dr. Ewart errs in attributing to a purely physical cause a movement which really arises from a mental and moral awakening—and, indeed, his whole article is full of unsupported assertions and loose reasoning; but I should not have ventured to criticise it had he not so clearly allowed his judgment to be warped by his political bias.

HERTHA AYRTON.

41 Norfolk Square, Hyde Park, W., January 9.

I AM sorry that my little paper should have been taken as prompted by political bias. I am sure that its possible influence on the Suffragette question never entered my head. I should be quite willing to answer any question Mrs. Ayrton may care to put to me should she care to write me privately. I am not willing to enter upon a public correspondence.

R. J. EWART.

The Health Department, Municipal Buildings,
Middlesbrough, January 12.

The Origin of Man.

THE reference in "Dodsley's Annual Register for 1767," mentioned in *NATURE* of January 12 (p. 336), is to James Burnett, Lord Monboddo, whose speculations as to the simian origin of man excited so much ridicule amongst