

and that it is not difficult to explain, yet I do not feel bound to explain every supposed fact as if it were a well-established one. As to the "parallelism of the development of protective resemblance and of instinct in the animal world," which I am also asked to explain, I deny that it has been proved to exist.

In conclusion, I will observe that the theory of Natural Selection, and its subordinate theory, Mimicry—have now been so fully developed by Mr. Darwin, Mr. Bates, Mr. Trimen, and myself, that I conceive it to be a full and sufficient answer to any opponent if we can show that his particular objections are unsound. This, I believe, I have done in the case of Mr. Bennett, although I am sorry to find that he cannot see it, and it is therefore unnecessary to go fully into the collateral points on which he has touched, and which have already been sufficiently explained by Mr. Darwin or myself.

ALFRED R. WALLACE

I AM forcibly reminded of Pope's lines,

A little knowledge is a dangerous thing;  
Drink deep, or taste not, the Pierian spring,

by the argument used by Mr. Bennett in the P.S. to his letter in NATURE, of the 24th November, in which he says, after quoting a passage from a paper by Mr. Jenner Weir: "Here at least it would seem as if *imperfect* mimicry was anything but beneficial to the individual; how can the principle of natural selection account for its propagation in these instances?" He considers that a little mimicry is a dangerous thing. I would rather agree with Lord Brougham in his remark on the above lines, that as a little knowledge is better than great ignorance, so a little mimicry is better than great dissemblance.

But the case referred to by Mr. Jenner Weir is plain, and the argument, instead of being against the theory of natural selection, is really in its favour.

Some of the larvæ in question, for some reason of which we are unaware, are not so palatable to birds, and they, therefore, are not eaten by them to the same extent. These larvæ have not so much need of the aid of protective resemblance, and indeed their hair, spines, and gay colouring are advantageous to them instead of a drawback. The smooth-skinned larvæ require the aid of protective resemblance for their preservation, but no one would for a moment expect that because an insect has a protective resemblance to the place on which it rests, that every individual is to escape destruction by its enemies.

Mr. Bennett again asks for an explanation of the tendency of the South American *Leptaliidæ* to resemble *Ithomiæ*. I think the reason is clear. Mr. Bates, in his paper, read before the Linnean Society in 1862 (Trans., vol. 23), states that the *Leptaliidæ* are exceedingly rare compared to the *Heliconiæ*, and that the proportion is about 1 to 1,000, and also that none of the *Leptaliidæ* are found in any other locality than those of the *Heliconiæ* they mimic. From this I should judge that the *Leptaliidæ* cannot make head against their enemies, and require the assistance of mimicking some better protected species to be able to maintain itself.

November 25

S. N. CARVALHO, JUN.

PROFESSOR HUXLEY has referred Mr. Bennett to the highest authority for an answer to his reasoning on a difficulty in the theory of natural selection. Meanwhile, Mr. Wallace has replied on his own account. Upon the biological question I do not presume to touch, but I wish to say a word upon the mathematical one, especially as I cannot think Mr. Wallace has really met this part of the argument.

Mr. Bennett's argument is shortly this. A modification must be advantageous before natural selection can take hold of it. In order to be advantageous, it must not be too small; it must be so great as to be attainable only in the course of many generations, during which, in the absence of natural selection, we must see whether chance will carry us over the ground. As an extreme concession, he supposes that an advantageous amount of change might be accumulated in twenty steps; and, assuming that the required *direction* of change is only one out of twenty directions equally probable, he easily shows it to be violently improbable that a stationary population of one million should produce a single instance of even ten such steps in successive generations.

But why is it necessary to suppose the steps made in successive generations? Provided that the required number are made

within reasonable time, it may surely be immaterial what intervals of merely unprogressive variation may elapse between them. In 200 generations, the first, fifteenth, fiftieth, for instance, and seventeen more, might make steps in the right direction, and all the rest might make steps in some or all of the other nineteen possible directions. Ten would in fact be the most probable number of steps in the right direction, and it would be about an even chance that there were ten at least.

However, as soon as we suppose steps in other directions, we must allow for the possibility of steps which shall actually reverse such progress as might be made in the right direction. If one change out of twenty equally likely is in the right direction, there will be on an average one in the opposite direction, and eighteen in indifferent directions. If we assumed that, in 200 generations, 180 were neutral, while twenty made steps forward or steps backward, these twenty *might* be all forward, and the chance that they were so would be one in 2<sup>20</sup>, or one in little more than a million. Generally, the number of neutral steps would be a little more or a little less than 180, and if we allow for this the resulting chance will be considerably increased. Several instances would probably be produced by a population of a million; and I presume it is easy to allow much more than 200 generations of butterflies.

Nov. 23

C. J. MONRO

#### Dr. Nicholson's "Zoology"

I NOTICE in NATURE for Oct. 20, a review by Mr. E. Ray Lankester, of a Manual of Zoology recently published by me, and I crave a small portion of your space to say a few words thereon. Upon Mr. Lankester's zoological strictures on my work I will not enter, partly because the public verdict on the merits of my work has already been very emphatically and decisively expressed; partly because the sins laid to my charge are chiefly of *omission* and not of *commission*, and are, therefore, more or less inevitable in a work of such limited compass; and partly because it must be patent to everyone how much more admirably the work, unfortunately left to me, would have been discharged by Mr. Lankester himself.

In the matter of *Greek*, however, Mr. Lankester really must excuse me if I decline to bow to his superior knowledge. I am well aware that he probably entertains a fresher recollection of his school days than I can boast of, and I might, therefore, without shame, have pleaded guilty to some obliviousness of Greek roots. Mr. Lankester, however, has been singularly unlucky in the point of attack chosen by him. He takes upon himself to condemn the whole of the glossary to my work, because he finds the *twelfth* word of the same ("actinomerēs") derived from the Greek word *aktin*, and he is good enough to add the information that "there is no such Greek word as *aktin*." Now, any decent lexicon would have informed Mr. Lankester that *aktin* is not only good Greek, but that it is the original form of the word, and that *aktis* was employed for the first time by Pindar, not, therefore, till about 450 B.C.

In conclusion, if I may be permitted to make a suggestion, I would recommend Mr. Lankester, in his capacity as critic and appraiser of the work of other men, not to judge in future of the value of a haystack by the first straw that he may happen to pull out of it; or, if he must do this, to be very sure before giving his opinion to the public, that it is a straw that he has succeeded in laying hold of.

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DR. NICHOLSON'S extraordinary assertions as to the supposed word "*aktin*" really demand no serious discussion, which, indeed, would be out of place in NATURE. A reference to Liddell and Scott's Lexicon will conclusively demonstrate to any person interested in the matter that he is entirely wrong. The following additional blunders in Dr. Nicholson's glossary will enable your readers more fully to judge of his accuracy, and it will require considerable boldness to attempt to justify them by reference to imaginary archaic forms:—1. In several places we find Dr. Nicholson giving "*poda*" as the Greek for "feet," a gross grammatical fault. 2. "*Pseudos*" is given as the adjective corresponding to the English word "false." 3. "*Enchuma*" is said to be a Greek word meaning "tissue." It has not this meaning. Dr. Nicholson's mistake arises from ignorance of the origin of the signification of the word "*parenchyma*." 4. "*Laima*" is given in several places in the glossary for "throat," in place of "*laimos*."