viewed from the upper surface. The cerebrum presents the following parts in successive order:—1, the medulla oblongata; 2, the cerebellum; 3, the lobus opticus, with its median furrow; 4, the lobus ventriculi tertii (thalami optici of authors); 5, the lobi hemisphærici, each of which terminates anteriorly in a knob constituting the tuberculum olfactorium. On the under surface of the brain there appear successively from before backwards:—(1) the bases of the lobi hemisphærici; (2) the chiasma of the optic nerves, which last proceed from (3) the lobus opticus, and between which is situated (4) the hypophysis cerebri, and behind this (5) the base of the medulla oblongata. M. Stieda then gives a full description of these parts, and of the various cerebral nerves in the frog. To this succeeds a very good general view or *résumé* of the anatomy of the brain in mammals. We may draw attention to some remarks made in the section where a comparison is made between the brain of man and that of the several classes of Vertebrata. It may be premised that little difficulty is experienced in discovering the homologous parts of the central nervous system of man and the more highly organised mammals. In the birds, however, there are several parts that are difficult to decipher; whilst in Amphibia, and still more in fishes, the nature of the several parts has given rise to much discrepancy of opinion between different observers. Dr. Stieda refers to his former work for the brain of fishes. In regard to Amphibia and reptiles, he considers that the lobi hemisphærici, or anterior lobes, being hollow, and containing a ventricle, are clearly the analogues of the cerebral hemispheres of man. The azygous portion of the central cavity, between the posterior parts of the hemispherical lobes (or ventriculus communis) in the frog, is the indication of the primordial single cavity of the first cerebral vesicle, and consequently establishes the transitional stage between the osseous fishes and the higher Vertebrata. The succeeding segment constituting the lobus ventriculi tertii, (or Zwischenhirn) corresponds in its upper part to the thalami optici; in its lower to the tuber cinereum and lamina cinerea. The third segment, or lobus opticus, agrees exactly with that of fishes, both in its external and internal relations, whilst reptiles exhibit the intermediate type between fishes and birds. Of the nature of the cerebellum there can be no doubt. In regard to birds, he observes, that the great club-like segment of the cerebrum of birds corresponds to the hemisphere of man, the bodies enclosed in them to the corpora striata, the radiated septum to the septum pellucidum. He considers the existence of parts analogous to the corpus callosum and fornix of man to be doubtful. The succeeding segment corresponds to the optic thalami; the large spheroidal body of the lobus opticus to the corpora quadrigemina. Two plates accompany the treatise, which are devoted to the histology of the parts described. H. P.

## LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

### The Difficulties of Natural Selection

As Mr. Bennett complains that I have charged him with errors he has not committed (which I should much regret to have done), I must ask permission to justify my statements by a reference to his own words.

1. Mr. Bennett says that he is unable to discover where he has led his readers to understand that there is only one completely mimicking species of *Leptalis*. I will therefore show him where he has done so. In the third column of his article (p. 31) he says: "Another South American genus of Lepidoptera, the *Leptalis*, belongs structurally to an entirely different class, the *Pieridæ*, and the majority of its species differ correspondingly from the *Heliconidæ* in their size, shape, colour, and manner of flying, being nearly pure white. There is, however, *one particular species* of *Leptalis*, which departs widely in external facies from *all its allies*, and so closely resembles a species of *Ithomia* as to deceive," &c. &c. Then comes the argument and the mathematical calculations always referring to "the *Leptalis*," and it is at the end of this, at the bottom of the next column, that we have the following passage (of which Mr. Bennett in his reply has only quoted a line and a half): "For supposing the chance is reduced from one in ten million to one in ten thousand, and it is said that the world has existed quite long enough to give a fair chance of this having occurred once, it is not a solitary instance that we have. Mr. Bates states that in a comparatively small area several distinct instances of such perfect mimicry occur, Mr. Wallace has a store in the Malay Archipelago, Mr. Trimen records several of wonderful completeness in South Africa," &c. Now, as there is not a word here about other species of *Leptalis*, but only about other cases of mimicry, as *Leptalis* is unknown in Africa or the East, as mimicry occurs in other genera and families of Lepidoptera, and other orders of insects, and as Mr. Bennett has himself stated, that the "*one particular species of Leptalis* departs widely in external facies from *all* its allies," I think it will be admitted that I was justified in asserting that Mr. Bennett's readers would be "led to understand," that there was only one species of completely mimicking *Leptalis*. If I was not so justified I confess my ignorance of the English language, and beg Mr. Bennett's pardon.

2. I leave your readers to judge for themselves whether the fact of a *Leptalis* having twenty offspring does or does not affect the mathematical argument as set forth by Mr. Bennett; but when, in answer to my statement, that the right variation has, by the hypothesis, a greater chance of surviving than the rest, he asks: "By what hypothesis? The hypothesis that these small variations are useful to the individual, the very hypothesis against which I am contending as unproved,"—I must protest against his denying his own words. For, at p. 31, col. 1, he says: "The next step in *my argument* is, that the smallest change in the direction of the *Ithomia* which we can conceive, on any hypothesis, to be beneficial to the *Leptalis* is, at the very lowest, one-fiftieth of the change required to produce perfect resemblance;" and six lines farther on, "For the sake of argument, however, *I will suppose* that a change to the extent of one-fiftieth *is beneficial*," and then comes the calculation. Again, I must acknowledge my ignorance of the meaning of words if Mr. Bennett does not here directly contradict himself. I never said the hypothesis was proved, but only that Mr. Bennett's argument, founded on it, was unsound, and for the sake of the argument he had admitted the hypothesis.

Mr. Bennett goes on to say: "The new factor, of which I take no account, is, again, entirely dependent on the admission of the natural selectionist premiss." This new factor is the principle of *heredity*. As he acknowledges that he takes no account of it, we must presume that he denies its existence; and as the whole of Mr. Darwin's theories and my own fall to the ground without it, he might have spared himself the trouble of his "mathematical demonstration."

3. I do not consider, as Mr. Bennett seems to do, that the distinction between "protective resemblance" and "mimicry" is a subtle one. Anyone who reads his paragraph on this subject (p. 32, col. 2) will, I think, be under the impression, as I was, that he alluded to mimicry, or mimetism, properly so called, as being strongly developed in birds. It seems, however, that he means only protective resemblance; but this, I believe, to be equally common among the very lowest forms of life. Transparency, for example, is a great protection to aquatic animals, and it is very prevalent in low organisms. Fishes are all, or almost all, protectively coloured, by the back being dark and the belly light, so that, whether looked at from above on the dark background, or from below on the light one, they are equally difficult to see. In many fishes, too, we have a specific protective resemblance as perfect as in any birds (see "Contributions to the Theory of Natural Selection," p. 55), and this is as much opposed to Mr. Bennett's theory as the absence of true mimicry in birds and mammals.

4. Mr. Bennett says, I have "brought no evidence to show that *extremely small variations* afford any immunity from the attacks of enemies,"—but this was quite unnecessary, because I show that the variations which continually occur in insects are by no means "extremely small." He also says that I "give no explanation of the tendency of the *Leptalis*, referred to by Mr. Bates, to produce naturally varieties of a nature to resemble *Ithomiæ*." But Mr. Bates introduces this remark with—"It would seem as if;" and though I think that the fact may be so,

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

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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 *Leptalidæ* 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 *Leptalidæ* are exceedingly rare compared to the *Heliconidæ*, and that the proportion is about 1 to 1,000, and also that none of the *Leptalidæ* are found in any other locality than those of the *Heliconidæ* they mimic. From this I should judge that the *Leptalidæ* 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.

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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 $2^{20}$, 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

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## 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 ("actinomeres") 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.

Newhaven, Edinburgh H. ALLEYNE NICHOLSON

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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. "Pseud*os*" 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."