whether germs can retain their *vitality* for the same lengthened periods; as he himself says, the proof of the theory ought to rest on direct evidence: "It must be confessed that the crucial observation has yet to be made; if vegetable germs exist in the drift, they can be discovered beforehand. I am not aware that any thorough search has ever been made for them."

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

MR. Wallace's "Reply" has disappointed me. From his unrivalled knowledge of the forms of animal life in those countries where nature is the most luxuriant, and from the extraordinary interest with which he invests every subject that he handles, I had expected from him something more conclusive than that he should charge his opponent with errors which he has not committed, and should reply to his arguments by a simple begging of the question.

The first "important error" with which Mr. Wallace charges me is, that "I lead my readers to understand that there is only one completely mimicking species of *Leptalis*." Where I have done so, I am unable to discover. I have, it is true, adduced one particular and striking instance as a sample of the rest, but distinctly say that "in a comparatively small area, several distinct instances of such perfect mimicry occur;" and point out how strongly, in my view, this tells against the theory of Natural Selection. In the next paragraph, "three great oversights" are alleged. Firstly, "that each *Leptalis* produces not one only, but perhaps twenty or fifty offspring." Mr. Wallace can hardly have supposed that I imagined each butterfly laid only a single egg, like the rok. The argument, however, is unaffected. In a species the numbers of which do not materially vary from year to year, it is obvious that, whatever the number of eggs laid, only one offspring from each individual, or rather two from each pair, survive to the period at which they themselves produce offspring. The "second oversight" is "that the right variation has, *by the hypothesis*, a greater chance of surviving than the rest; and the third, that at each succeeding generation the influence of heredity becomes more and more powerful." By what hypothesis? The hypothesis that these small variations in the right direction are useful to the individual—the very hypothesis against which I am contending as unproved; as neat a case of *petitio principii* as one often meets with. My "errors" in fact, amount to a non-admission of my opponent's premisses, who then naively adds, "with these three modifications the weight of the argument is entirely destroyed!" Of course it is. The "new factor of which I take no account" in the next paragraph, is again entirely dependent on the admission of the natural selectionist premisses.

With regard to the distinction between man and other animals, I much regret if I have unwittingly misrepresented Mr. Wallace's view; but if I have done so, I think it is owing to that view not having yet been clearly pronounced. Mr. Wallace distinctly states his opinion that "a superior intelligence has guided the development of man in a definite direction." ("Contributions," p. 359.) I have Mr. Wallace's own authority for saying that M. Claparède has misinterpreted him in referring this superior intelligence to a "Force supérieure," a direct action of the Creator; what alternative is there left but to suppose that it was man's own intelligence that he had in view? Whenever Mr. Wallace more clearly enunciates this portion of his theory, I think there will be no difficulty in showing that the same principle, whatever it may be, is operative in the lower creation as well as in man.

Having disposed, as I think, of Mr. Wallace's chief points of reply, I may be permitted to point out one or two errors into which he has himself, it seems to me, fallen. The changes of mimicry are, he says, "wholly superficial, and are almost entirely confined to colour." I was certainly surprised to read this, recollecting so many instances to the contrary, not only among tropical insects, but in the close approximation in form of some of our own Diptera to certain genera of Hymenoptera; and recollecting also the numerous illustrations of protective form and habit which Mr. Wallace himself gives, not only describing them but having also drawn them with such exquisite fidelity. (See "Malayan Archipelago.") In the *Kallima paralekta* of Sumatra, for instance, he says, "we thus have size, colour, form, markings, and habits, all *combining together* to produce a disguise which may be said to be absolutely perfect." ("Contributions," p. 61). Another sentence I had to read three or four times before I could believe that Mr. Wallace had penned it. In objecting to my parallelism between the development of protective resemblance and of instinct, he says, "in birds mimicry is very rare, only two or three cases being known." I do not know whether Mr. Wallace draws any subtle distinction between "mimicry" and "protective resemblance;" but if so, he should have noticed that it is the latter which I speak of as "being strongly developed in birds." I had, on reading the above sentence, to turn again to my "Contributions," to see whether I was correct in my impression that we find there the statement that "in the desert the upper plumage of *every bird* without exception is of one uniform isabelline or sand colour;" that "the ptarmigan is a fine example of protective colouring" ("Contributions," pp. 50, 51), and that two whole chapters are devoted to the wonderful protective instinct of birds in the matter of their nests.

On one point raised in my paper I am disposed somewhat to modify my views, and I do so with the greatest pleasure, in my objection, namely, to the title of Mr. Darwin's great work. Taking the origin of *species* as distinct from the origin of mere *varieties*, there is undoubtedly a sense, as Mr. Wallace points out, in which natural selection may be considered a prime factor. The law of variation is a centrifugal, the law of natural selection a centripetal force; the one acting by itself would produce a wild chaos, the other a barren uniformity: equilibrium can only be the result of their joint co-operation.

Whatever may be my "inability to grasp the theory," I hope I have shown that I have not fallen into the errors with which Mr. Wallace charges me. All the main points of the argument seem to me to be left untouched by him. He has brought forward no evidence that extremely small variations do afford any immunity from the attacks of enemies. He gives no explanation of the tendency of the *Leptalis* referred to by Mr. Bates "to produce *naturally* varieties of a nature to resemble *Ithomiæ*." He does not attempt to account for the parallelism of the development of protective resemblance and of instinct in the animal world. He fails to explain the nature of the intelligence which was operative in the creation of man, and which is a principle unknown in the rest of the organic world. Students of Nature who have spent their lives in their own country must always yield in point of experience to those who have had the advantage of comparing the faunæ and floræ of other climates, and can only arrive at their conclusions from the facts brought to their notice by travellers; these, I think, I have not misrepresented. Appeal to authority, as authority, is always to be deprecated in Science. I may, however, perhaps be permitted to strengthen my position by a quotation from a work, which I had not read at the time of writing my paper, by one who will be acknowledged to have some knowledge of the ways of Nature (Huxley's Lay Sermons, p. 323):—"After much consideration, and with assuredly no bias against Mr. Darwin's views, it is our clear conviction that, as the evidence stands, it is not absolutely proven that a group of animals, having all the characters exhibited by a species in Nature, has ever been originated by selection, whether artificial or natural."

ALFRED W. BENNETT

Westminster Hospital, Nov. 19

P.S.—Since writing the above, Mr. Jenner Weir has kindly called my attention to two papers read by him before the Entomological Society, "On the Relation between the Colour and the Edibility of Lepidoptera and their Larvæ." In one of these I find the following remarkable statement:—"Insectivorous birds, as a general rule, refuse to eat hairy larvæ, spinous larvæ, and all those whose colours are very gay, and which rarely, or only accidentally conceal themselves. On the other hand, they eat with great relish all smooth-skinned larvæ of a green or dull brown colour, which are nearly always nocturnal in their habits *or mimic the colour or appearance of the plant they frequent.*" 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?

THE soul of many an anti-Darwinian will have been cheered by Mr. A. W. Bennett's paper on "The Theory of Natural Selection from a Mathematical Point of View." It is, in fact, a very