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 superieure," 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 admirable piece of special pleading, based on a skilful assumption of premisses which, to a careless or biassed observer, might seem indisputable.

The tendency to variation is spoken of as something very mysterious, of which no adequate account has ever yet been given. Yet the very simple explanation is no bad one, that where two parents are concerned in the production of any offspring, the product in part resembling each of the producers must of necessity also in part differ from each of them. Between the parents themselves, Mr. Herbert Spencer has shown that differences of age and external circumstances would ensure the requisite want of resemblance in the absence of any other cause.

"The rigid test of mathematical calculation" is then applied to the case of mimetic butterflies, with the view of showing that they could not have been produced simply according to the laws of variation, inheritance, and natural selection. In the application of this rigid test the very first step is a perfectly gratuitous assumption, "that it would require, at the very lowest calculation, 1,000 steps to enable the normal *Leptalis* to pass on its protective form." Who is to prove that fifty differences would be insufficient? An interval of a thousand years might be granted for establishing each one of these variations. Suppose even 50,000, instead of only 50 steps to be necessary, it is another gratuitous assumption that "the smallest change in the direction of the *Ithomia*, which we can conceive in any hypothesis to be beneficial to the *Leptalis*, is at the very lowest one-fiftieth of the change required to produce perfect resemblance." How small a difference must decide the choice made by a donkey placed equidistant between two bundles of hay! Certainly, then, a bird on the wing, having to choose amidst myriads of butterflies, may be determined by an almost infinitesimal distinction. Further, though the whole change may be produced by an immense number of small changes, it is not necessary to suppose that all the changes will be equally small. It is merely begging the question to assume that the first change could not possibly be large enough to be of any use. And if it may be of use, the whole mathematical calculation, based on its being useless, breaks down from the beginning. Again, since the *Leptalis* may have spent 1,000,000 years in arriving at its present likeness to the present *Ithomia*, it is impossible to assert that the normal forms of the two butterflies were as wide apart at the beginning of that period as they are at present. The mimicry having once set in, might be retained by parallel variations. This, indeed, cannot fail to be the case, if the protection is to be a lasting one; for when the *Ithomia* varies in outward appearance, unless the *Leptalis* varies in the same direction, the resemblance will be lost. This progressive mimicry would be more valuable than an imitation in which no changes occurred, since the enemies of a mimetic species would in time become aware of a fraud which had no variations at its command, as birds are said now-a-days to pounce without hesitation upon caterpillars which very much resemble twigs. Even "a rough imitation" may be useful in the first instance, and yet when hostile eyes have long been exercised, and have acquired greater and greater sharpness, finally nothing less than *absolute identity* of appearance may be thoroughly effective. Thus the perfecting of the resemblance will be no "mere freak of Nature," nor shall we be "landed in the dilemma that the *last* stages are comparatively useless" in this procedure.

The array of figures brought forward to prove that the *Leptalis* could not have made twenty steps of variation in the direction of the *Ithomia by chance*, would be much to the purpose if any exponent of the theory of Natural Selection had ever argued or supposed that it could. The calculation takes it for granted that the theory is erroneous, instead of proving it to be in error. Upon this assumption, it might have been put far more strongly, only that a stronger way of putting it would have borne on the face of it the suspicion of some inherent fallacy. It begins by supposing that there are "twenty different ways in which a *Leptalis* may vary, only one of these being in the direction ultimately required;" it might quite as truthfully, or even more so, have said a thousand instead of twenty, and then the second step would have given the chance as only one in a million, instead of one in four hundred. But while the theory of Natural Selection speaks of numerous minute useful variations, Mr. Bennett will not allow that combination of terms. Let them be numerous and minute, if you will, he says, but if small they cannot be useful, if useful they cannot be small. He claims to have Mr. Darwin's own word for it, that a large variation would not be permanent, as though Mr. Darwin had said, "living creatures have come to be what they are by successive useful deviations of structure permanently propagated, but no large deviations are permanent, and no small ones are useful." It is quite obvious that in the use of relative terms, such as great and small, Mr. Darwin neither intended to stultify himself nor has done so. A thing may be large enough to be useful without being large as compared with something twenty times its own size; and a man may be said to have a huge brain in a very small body, although the body in solid content far exceeds the brain. When Mr. Darwin says that "Natural Selection always acts with extreme slowness," he does not imply that its steps must therefore be so numerous as to be too small to confer any advantage. This would be a contradiction in terms. But the steps may be exceedingly small notwithstanding, and also sometimes separated by enormous intervals of time from one another.

In introducing his own explanation of things, Mr. Bennett affirms that "resemblances, and resemblances of the most wonderful and perfect kind" in the vegetable kingdom, "are in no sense mimetic or protective." This may be so, but it can hardly be said to be proved. When he speaks of "man's reason" having "assisted him so to modify his body as to adapt himself to the circumstances with which he is surrounded," and suggests that the instinct of animals may have assisted them also to modify their bodies by slow and gradual degrees to the same purpose, it is difficult to imagine the process intended, and still more difficult to see how "the slow and gradual degrees" will escape the rigid test of mathematical calculation which Mr. Bennett has elsewhere applied; for if the steps are great they ought not to be permanent, and if small they ought not to be useful. A theory which makes it possible for a bee to "modify its proboscis" by instinct, or for a man to treat his nose in the same manner by reason, seems harder of digestion than the Darwinian.

THOMAS R. R. STEBBING

Torquay, Nov. 12

MR. BENNETT, in his very able paper read before the British Association at Liverpool, and published in NATURE of the 10th November, calls in question the explanation given by the theory of Natural Selection of the various instances of mimicry found in the animal kingdom.

He bases his argument principally on the fact that the alterations in the early stages being useless to the animal would not be preserved, and that these changes must be very slow.

He assumes that to enable the normal *Leptalis* to imitate a species of *Ithomia*, it may be considered to have gone through at least 1,000 stages, and that no change less than one-fiftieth of the whole alteration effected would be of any use to the insect. He gives us no information as to how he arrives at these figures, and we are left with the idea that they are selected principally because they are what are called "round numbers," and are more easily dealt with in the calculation which he gives us.

Now I think that the number of stages which Mr. Bennett considers it necessary for a *Leptalis* to pass through so as to mimic an *Ithomia* is vastly too great: 1,000 stages means at least 1,000 years.

Let us look at the alteration which frequently takes place in the colouring of a butterfly, possibly in one generation, as shown by varieties of which sometimes only solitary specimens are known, figured in Newman's work on English Butterflies. I need only refer your readers to the figures of varieties of *Apatura iris*, *Epinephele janira*, *Limenitis sibylla*, *Melitæa athalia*. Now can it be contended that it required 1,000 of such stages to effect the alteration?

If any of these variations happened to be useful, there seems no reason for supposing that one stage might not make much more than $\frac{1}{50}$ of the alteration, which Mr. Bennett lays down as being the least which would be useful, and which I agree with him in considering much too small. Why might not one stage make one-fourth or one-sixth of the alteration required?

Mr. Darwin quotes a passage in his work on Natural Selection (page 32) from Sir John Sebright with regard to pigeons, in which he says that it takes three years to produce a given feather, but six years to make a head and beak. If the bony structure of an animal so far above a butterfly can be altered in six years, we surely do not require more than that time to effect an alteration in the colour of a butterfly's wing.

Mr. Bennett states that the early stages of the alteration would be useless to the insect; every one, I think, will grant this, when each stage is only one-thousandth of the whole, but not if it be