

*REPLY TO MR WALLACE'S THEORY OF  
BIRDS' NESTS.*

MR WALLACE'S paper on birds' nests, which appeared in last number of this Journal, was a welcome contribution, not only from its intrinsic excellence, but for its bearing on the great questions of the origin of species and development of form, which now occupy men's minds. It is unnecessary to say, however, that it only expressed his own views, and that our publication of them by no means implied their adoption by us, or any recantation of our own opinions, elsewhere promulgated on similar subjects.

The pages of this Journal will always be open to both sides of any question falling within its province. *Audi alteram partem* is one of the principles which we lay down for our guidance in conducting it. On that principle we now propose, as we have placed Mr Wallace's arguments before the reader, to say a few words in reply to them, or rather to those parts of them from which we dissent. On the greater part of the paper we have no remark to make, except to express the pleasure and interest which we felt in perusing it. We think he has completely made out his case, that in the main it is female birds with gaudy plumage which construct their nests under cover, and those with dull plumage which make them open. No doubt there are exceptions to the rule; for example, the hedge sparrow is not more gaudy than the house sparrow, and yet the one makes its nest open, the other usually under cover. Our little favourite Jenny Wren is not a whit more gay than the wife of her friend Cock Robin, and yet the female robin makes an open nest, and the wren a covered one. Still, looked at as a whole, and disregarding exceptions (which may be capable of individual explanation), the rule is nearly universal, that wherever the plumage is so bright as to attract attention the bird makes its nest under cover, and that where it is not, this precaution is dispensed with.

All will admit that this phenomenon, from whatever cause proceeding, is a means of preserving the bird and eggs from discovery, marvellously well-fitted to attain its object, and calling for our admiration as a work of design, whatever may be the laws or the process by which it is brought about. What that

process has been is the point on which we differ from Mr Wallace. He says it is "natural selection," and thoroughly convinced, as he is, of the soundness of his theory, he shuts his eyes to its imperfections and unwarranted assumptions, and with pardonable complacency demands that, "until at least an equally wide range of facts can be shewn to be in harmony with any other theory, he should not be expected to abandon that which has already done such good service, and which has led us to the discovery of so many interesting and unexpected harmonies among the common (but hitherto most neglected and least understood) of the phenomena presented by organised beings."

This can only be meant as a fashion of speech. He cannot mean that, until a better theory be produced, he will adhere to his present faith, even if proved to be wrong, merely because it has done good service. No one seeks to deny that it has done good service, but it is neither its confirmation, as Mr Wallace thinks, nor its refutation, as we regard it, but its ventilation which has done the service. Thousands of exploded fallacies have in their time done good service. Whatever tends to make men think and search after truth does good service. To the theory which does so we owe our obligations, whether it prove true or false. But our gratitude stops there. As soon as it is shewn to be wrong we give it up, whether we have a better theory to put in its place or not; and so here we have no doubt Mr Wallace would do the same, could we only convince him that he *is* wrong. *That* we despair of doing. We are afraid that he has too long and ardently laboured to build up and strengthen the theory of natural selection to be able to see any defect in it now, or rather to attach any importance to those flaws which he cannot but see, but which appear to him as mere motes in the sun. We may, however, be more fortunate with others who have not yet such confirmed conviction upon the matter; and shall at any rate make the attempt.

The present theory is the natural and legitimate sequence of the Darwinian theory of natural selection carried out to its full extent, and although not more startling than Mr Darwin's original position in regard to design, gives better means of testing it as being carried to a greater extreme. Natural selection—the belief in constant, unceasing, slow, and gradual change—and, to a certain extent, the influence attributed to the struggle for life, are the three points of the Darwinian creed from which we dissent. These are the points which are usually regarded as specially Mr Darwin's

own. Others, such as "derivation," the "homologies," &c., are regarded as less original, because first suggested by older authors; but, putting aside the abstract reflection, that there is no such thing as originality (everything that enters the mind of man being suggested by something else), we have always considered that in this estimate an injustice is done to Mr Darwin. According to our conception science owes vastly more to him for the way in which he has established the principle of derivation on a firm basis than for his other speculations, which have not brought the same conviction to our mind. Our objections to the latter may be briefly indicated in a few words—we have not space for more than an indication.

Mr Darwin regards the struggle for life as a means by which the less perfect conceptions of nature, which he assumes to be constantly appearing, are wiped off; it is as a destructive agent that he chiefly regards it. We regard it from the opposite point of view—as a preservative rather than a destructive agent—as a means of strengthening and maintaining the life of *species*, in the same way that pain maintains and restores the life of individuals. Without the blessing of pain we should succumb to every accident and ailment—without sorrow, happiness would be impossible—without grief there could be no joy. In the same way, but for the struggle for life, general degeneracy would be the result, and the species would come to an end from the degradation of its constituent parts.

Next, as to the slow and gradual change which Mr Darwin maintains to be in constant operation in all organic beings, we maintain that if such a change really were constantly in progress without intermission, the inevitable result must have been the confusion of all species. It could not be otherwise. No mathematical problem can be more inexorable. If there were no pause or resting-place in the course of change there could be no distinction of species—the whole of organic life would be one confused mass of individuals. To mark off that mass into sections, there must be some pause in the process of change. We have elsewhere ("Geographical Distribution of Mammals") argued that the real explanation of what we see is, that organic life is endowed with a plasticity and readiness to change which only requires stimulus to force it into operation, and that the usual stimulus is change of conditions of life; without change of condition the species rests undisturbed; with it, it produces new forms. This would explain both the continued endurance of species and the appearance of

new species, a double phenomenon which appears to us inexplicable under Mr Darwin's theory.

The last of the Darwinian theories to which we demur, is that of natural selection. Our objection to it in wholesale is, that it implies constant succession of failures and a never-ending waste of power on the part of nature, both of which we believe to be inconsistent with her working; she never errs and never wastes. No instance has ever been pointed out of a failure by nature. No species has ever been discovered which is not the fittest for its place. Mr Darwin admits the absence of all evidence of the transitions which his theory requires, and endeavours to explain it away by the imperfection of the geological record and the shortness of our current living experience. Of course, if that is a sufficient apology for the absence of transitional forms, it is equally so for the absence of any unfit forms. We are of those who think that it is not sufficient. Our creed is that nature not only does everything well but everything best, and that it is an inherent part of the constitution of the laws of development that they must produce the fittest—that they have no power to produce anything but the fittest—just as in minerals, however various the forms of crystals may be, they are all crystals. In minerals nature never deviates off into vagaries, making them globes or ovals. Whatever may be their constituent elements, the inherent necessity of their constitution compels them to appear in angular forms. So the laws of development of organic beings leave only one course open to them, and that leading to the production of the fittest. This hypothesis is at least in accordance with known facts. The theory of natural selection has to assume its facts and apologise for their absence.

It is the attempt to explain the evidence of design by natural selection, which is the weak link in the theory. Had the origin of that been left unexplained, or simply assumed to be the product of laws bearing that result *in gremio*, and natural selection limited to dealing with the fate of species after their appearance under such laws, or applied only to explain or maintain a progressive advance in the scale of life in general, it would not have been open to the same objections, nor would it have encountered the same opposition.

The simple proposition that when two competing forms of life appear, the weakest will go to the wall, and disappear from the strife leaving the other as the fittest in possession of the field, is not cal

culated to draw forth much opposition. The failure lies in the attempt to refer to blind chance (the chance of the production of an accidental combination of design or an element which, by continued selection, shall ultimately assume the form of combined design) structures which all—Darwinites not less than their neighbours—admit to be examples of skilful and elaborate contrivance.

But formidable as the objection is when stated merely to the production of *species*, or instances of design in *species*, it becomes infinitely more so when we are asked to extend its application to sexual differences and periodical changes. See what Mr Wallace's theory of birds' nests requires. Take his explanation of the origination of dull-coloured female mates to bright-coloured male birds. He starts with the assumption that originally both male and female had bright plumage. There seems no reason why he should begin with the assumption that they were both bright rather than both dull-coloured; but his argument is only directed to that side of the question, and it would obviously have required a different line of reasoning to convert a dull male into a bright male from that which would convert a bright female into a dull female. In the case of both being originally bright-coloured, there is a *quasi* necessity for the female to become dull-coloured. There is the supposed compulsion of the struggle for life. But in the case of both being dull at first, that compulsion would not apply to the male bird. There is no controuling necessity for him to put on a finer coat than he had before. However, pass that, and let us accept the question from the side which Mr Wallace presents to us—both male and female bright; female sitting on an open nest; species like to be extinguished from the exposure; all enemies at once see her, and seize upon the eggs; ruin stares the species in the face. It has two modes of escape: one, an easy and natural one, no ways beyond the instinct of a species or the intellect of an individual, by building the nest in a more concealed position; the other, a more difficult one, wholly beyond its own control, the female becoming converted, by Darwinian process of natural selection, from a bright-coloured bird into a dull-coloured bird. According to Mr Wallace, some adopted the easy remedy; others did not, and in them a change in the colour of the plumage was effected by natural selection. All the females of the same species were, of course, not equally bright in plumage; and as the least brilliant females escaped observation

better than the gaudier ones, natural selection perpetuated their offspring in greater numbers than that of the latter, and so ended in producing a race of duller-coloured females. But is it so? Had it been a *species*, and not merely the *half* of a species, the argument would at least be consequent. But a female produces males as well as females, and the result of breeding in-and-in from dull-coloured females would be to dilute the colour of the whole breed, both male and female—not preserve the male bright and turn the female dull. The hereditary qualities of father and mother are no ways special to the respective sexes of the offspring. In our own species it is very commonly said that the sons take after the mother, and the daughters after the father, which would go against Mr Wallace's theory, if true; but we believe it is not true, and that we have no reason to suppose that one parent has on an average a greater share in producing the physiognomy of their offspring than another.\*

The explanation of the phenomenon seems to us of a totally different nature. Although the plumage of such males and females as we have been speaking of is often apparently very different, there are grounds for believing that they are both the same, only developed to different degrees, according to the amount of vital action operating on each. Thus we see, in the pheasant and common fowl, old hens assuming more or less of the plumage of the males. In them it is obviously not a different livery, but the same livery at different stages of its production. It is the same with young birds, their plumage is different from that of their parents, and we do not suppose that Mr Wallace would refer that to natural selection. It is a parallel case to the down on the chin of the boy and the beard on that of the man. Why this immaturity of plumage (if we may so call it) exists in some female birds and not in others we do not pretend to explain; but it appears most probable that the building of open nests was at first the normal habit with all birds, and that the instinct of building them under cover, and in concealed places by those whose bright plumage would betray them in open nests was acquired by experience. We all know that in

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\* On this subject see the evidence collected by Mr Darwin in his new work, "Animals and Plants under Domestication," vol. ii., p. 72, which seems to us to confirm the above conclusion, although Mr Darwin regards the facts as shewing that peculiarities appearing in either sex "strongly tend to be inherited by the offspring of the same sex, but are often transmitted in a latent state through the opposite sex."

birds, intelligent modification of the ordinary construction of their nests is by no means rare under exceptional circumstances.

Mr Wallace and Mr Bates have, with their usual ability, argued also for natural selection as the explanation of the instances of mimicry or disguise which are to be met with in many animals, such as moths pretending to be bees, flies to be wasps, and generally the resemblance which exists between the colour of animated beings and that of the scenery in which they live—white in snowy regions, sand-coloured in sandy-deserts, heath-coloured in moors, and so on. These, however, do not appear to belong to the same category of phenomena as the sexual differences in plumage above referred to. In sexual differences the theory relates to modification of colour previously existing; in mimicry to the causes by which it is originally determined. It is part of the Darwinian theory that surrounding objects and extraneous circumstances are without influence on the development of structure, or on the appearance of species produced among them. We do not see that such a belief is essential to that hypothesis; for although, if it were once admitted that such causes had some influence, it would not be easy to say where it stopped, still it would always leave natural selection and the struggle for life full scope for work. The fact is, however, that unless he has changed his mind, Mr Darwin holds that surrounding objects or conditions *have* no determinate influence on the formation of species. The wonderful resemblance between the colour of the ground and that of the animals which inhabit it, seems to us to furnish at least a *prima facie* case to the contrary; and seeing that neither view has any direct evidence to offer in its favour, it seems to us more reasonable to suppose that an adjustment of the particles of colour in harmony with surrounding colours should be inherent in the laws regulating its development, than that all animals have come to wear the colour of the scenery in which they live, by those which were produced of any other colour having been wiped off as less fitted for surrounding conditions. It is a strong call on our faith to admit that power of natural selection to extend not only to making the colour once for all, but to varying it regularly twice every year, as in the case of many animals inhabiting arctic regions.

Every artist, every admirer of scenery, is familiar with the harmony of colour in nature. We do not believe that this is fortuitous. It seems as if there were a polarization of colour as there is of magnetism and electricity, which reduces everything into harmony.

In last number of this Journal, in a notice of Dr Wyman's views of the symmetry and homology of limbs, the reader will find facts mentioned which go to prove the presence of a magnetic or electrical polarization in the development of the embryo of vertebrate animals. We are fearfully and wonderfully made, and there are many things in the making of which we have as yet no idea—the distribution of colour one of them. If the theory of natural selection does not give a satisfactory explanation, why the colour of animals corresponds with that of the district in which they live, then neither will it give one in the cases of mimicry by one animal of the pattern of colour or appearance of another. We believe that more of the instances of mimicry than is usually allowed are explicable on the ground of actual affinity between the imitator and the imitated; others, perhaps, by the colour of the species having been produced under similar original conditions. A gaudy butterfly, of a staring pattern, may not, when taken separately, look as if very much in harmony with the scenery where it was produced; but seen alive in its native woods it no longer appears so. There it is natural, and, if we give the fancy vein it would not be difficult to imagine a mixture of flower and foliage, dewdrop and sunshine, making a gorgeous effect, as bright and not unlike the most gaudy butterfly; and if to that influence the pattern is due, in one instance, similar causes might produce similar results in another, as in the case of the colour of the animals in the desert, &c. These resemblances, taken from tropical forests, may be mere harmonious representations of common general effect. Still, there remain many most remarkable imitations, both in form and colour, which are not so explicable; but in most of these we know of no habit of life or advantage derived from the similarity, which could be explained by referring the resemblance to natural selection. The cases where such a reason has been assigned are very few, and at least requiring more investigation before it can be said that it accounts for the resemblance on the ground of natural selection.

In examining Mr Wallace's able paper we have not put his cases under any critical examination. We have taken his facts as stated by himself. He is most careful and accurate in all matters falling under his own observation; but we think his fondness for generalizing sometimes leads him to accept as settled and admitted statements which are only conjectural hypothesis not yet generally allowed. For example, he adopts the statement as a proved fact



that the bright colour of flowers is confined to those which require insects to fecundate them, and that the object of their being provided with gay colours is to attract the attention of insects to them. Now, both of these assumptions are without warrant. As to the first, many bright flowers in the garden need no insects to fecundate them. For example, what colours can be brighter or more gaudy than those of our orange and tiger lilies, the campanulas, the poppies; almost every gay flower, indeed, which has a long flexible stalk, that will bend with the wind, fecundates itself without help of insects. The stalk and the wind do it between them. As to the second assumption, that the gayness of the colour attracts insects, we have only to observe that insects are provided with very imperfect and inferior means of vision. They have the power of smell very largely developed, but that of sight is very feeble. What is more, the defect is chiefly in the distance to which they can see; they are very near-sighted, as any one can convince himself, by watching a butterfly attempting to fly over a high wall. In approaching it, it obviously does not see it until it is close upon it; it retreats a little, and flies a little higher up and again approaches it: again finds the barrier, again repeats the process, and it is only after several attempts that it at last surmounts it. This defect is inherent in the structure of the insect's eye; and therefore any argument founded upon the analogy of our own vision would lead us wrong; and it is plain that if the insects cannot see the gay flowers, their bright colour cannot answer the purpose of attracting them.

For the above reasons, we cannot agree with Mr Wallace in the views he has arrived at; and we have been the more disposed to canvass them, because any hypothesis proposed by him deserves full consideration, not only from the ability with which it is sure to be advocated, but from the weight attached to whatever proceeds from his pen. If his views are erroneous, it is of the more importance that they should be questioned; if they are doubtful, that they should be sifted; and if correct, ventilation will make them only better known.