

Mr. Howorth on Darwinism

WILL you allow me to reply to the various letters which appeared in your last number in answer to one from me? I gratefully welcome their general courteousness. Postponing the consideration of Mr. Wallace's letter, I come to Dr. Lionel Beale, the relevancy of whose arguments, and especially of the lugubrious moral attached to them, I fail to understand. It seems to me to be so incoherent and rhetorical that it is far beyond the reach of reply.

Mr. Tylor refers to the last census as disproving my position. He says the population has increased enormously, and yet our age is characterised by its luxury. These statements are correct. But the argument deduced from them has a missing link. The luxury of the upper strata of society has increased with its wealth, but the numbers of the pauper class have been increased in the same rate. In considering the published returns of the Poor Law Board, I am compelled to admit that the increased luxury has been limited to the surface of society, and that its lowest ranks have been correspondingly recruited, and to admit the force of Mr. Doubleday's argument, that the population of England under the Tudors was stationary because of the generally diffused wealth, while that of Ireland in the last century was increasing at an enormous rate, because it was steeped in poverty and want. I am not arguing about individual cases, but about general laws. Now, in Lancashire, where the increase has been so marked, I have it on the authority of owners of mills that the indigenous stock of the county, which is thrifty and well off, is not an increasing element, but is being replaced by the children of the Irish, or semi-Irish blood, from the poorer quarters of the large towns, among whom prudential restraint (which is surely a very visionary *causa causans* in any event) cannot be said to have much influence. At Rome, Venice, Basle, and in France, where the aristocratic class was not limited by primogeniture, it was always dying out, and was only recruited by fresh creations (see the details in Doubleday, chapter iv. *passim*). In all these cases we can appeal to figures, and not to a superficial survey of a Peerage, or the limited area of our own acquaintance.

The particular passage quoted by Mr. Tyler from Malthus has been conclusively answered by Doubleday (chapter vi.), and it is useless to repeat his arguments, which on this point I consider to be unanswerable.

Mr. Lownes repeats the odd charge of Mr. Tait against me, that I put the cart before the horse. The latter gentleman, whom

I have not yet answered, cited against me the elementary case of cypriots and other creatures of that ilk. They are entirely beside the question. It is as reasonable to quote them in this discussion as to conclude that all chaste people must be cowardly and effeminate because mutilated animals are so. He also said that I mistook the whole rationale of the question, and that it is infertile creatures that grow fat, and not fatness that causes sterility. The only test of the question is the one I have not shrunk from applying in this argument (which, by the way, has not to do so much with the fat as the hearty and strong). This test is that in a great number of cases we can make strong and vigorous but sterile plants and animals fertile by starving or bleeding them, which proves that it is not the organs that are defective, but that the creatures are too hearty.

The experience of Mr. Lownes on the fecundity of consumptive patients, and of the poorest classes as compared with the richest, is at issue with that of the doctors and midwives whom I have access to, and of all the authorities I know whose opinions are based upon statistics.

I am not sure that I understand the second and third paragraphs of his letter. Whichever way the problem is put, I am satisfied if it be admitted that in the more crowded and squalid portions of our towns, the population as a rule is more fertile than in the less crowded neighbourhoods. The case he cites of poor women losing their children early and ceasing to give milk, and, in consequence, soon becoming pregnant again, is counterbalanced by the fact that among the richest the proportion of those who suckle their children is small, and this not because of fastidiousness, but because they secrete little milk. Mr. Lownes once more drags out the Indian and the backwoodsman, but he has overlooked the answer I gave to Mr. Wallace in my former letter, which needs no alteration to meet the case as he has put it. It is the case of the meat-eaters against the vegetable-feeders, the strong and hearty and active against the comparatively stolid and low-conditional, and as in such cases all the world over the former are not so fertile as the latter. Mr. Lownes objects to savages being cited, because of qualifying circumstances; he may as well say that it is not fair to test natural selection by wild animals, but only by domesticated ones. His treatment of the case of the Patagonian women is convenient but flippant. Mr. Lownes' experience in breeding both cattle and sheep and fowls and in rearing plants must be extremely limited, or he would hardly have made so rash an assertion as that contained in his last sentence. The starving of plants and animals to induce them to breed is one of the elementary axioms of both gardeners and stockkeepers.

I now come to Dr. Ross's letter, which, although somewhat patronising in parts, is altogether more to my taste than some others. He has properly referred me to Mr. Herbert Spencer, but I am afraid of venturing into his book, for fear that I should open upon myself the floodgates of Evolution. It is not the general problem of Evolution about which we are now at issue, but that limited form of it called Natural Selection. It is satisfactory, however, to find that, according to Dr. Ross, Mr. Herbert Spencer admits the main facts upon which my argument is founded. His doing so is quite a relief after the jaunty manner in which some of your correspondents have spoken about the matter. To speak of its being late in the day to be now defending Mr. Doubleday, to tell one that "what one says is ludicrous," "a monstrous error," &c., &c., is surely a sign that the crowing of the Gallic cock has been mistaken for more substantial arguments. I am very sorry that Mr. Spencer's book is not in my library, and that I cannot meet with it at the Manchester Free Library or Mudie's, so that until I am aware of Mr. Spencer's arguments I cannot say how far they affect the position I maintain. If the facts are admitted, as Dr. Ross says they are, I confess that I cannot see any other interpretation of them than the one given by Mr. Doubleday. Will Mr. Ross do me the favour of pointing out what other explanation they are capable of?

Mr. Wallace has misunderstood me if he thinks me capable of sneering at the good and sound work that has been done by himself for many years, the value of which I am as conscious of as I am of the worthlessness of mere Olympian dogmatism. Sneers are only justifiable in answer to contempt, and if he feels aggrieved with any of my words I withdraw them.

Mr. Wallace says my criticism of the phrase Survival of the Fittest is satisfactory. In regard to the phrase I used, and for which I was severely flouted by Mr. Wallace, he says it is unknown to Darwinians; that may be, but it can hardly be said to be unknown to Mr. Darwin himself. Speaking of the problem

of the conversion of varieties into species, the latter says: "The inevitable result is an ever recurrent struggle for existence. It has been truly said that all nature is at war, the strongest ultimately prevail, the weakest fail, and we well know that myriad forms have disappeared from the face of the earth" ("Variation of Animals and Plants under Domestication," i. 5). Let me especially commend this extract to Dr. Lionel Beale, for whom I entertain the profoundest respect, notwithstanding his vituperation of myself.

I find a difficulty in meeting Mr. Wallace's latest arguments, because they are entirely *a priori*, and Mr. Wallace asks me to admit as premisses the very thing I dispute, namely, the relative sterility of strong and hearty animals and plants. I cannot see the relevancy of his quotation of the effects of cross-breeding to the present argument, unless he means to infer that crosses are more vigorous and stronger than pure bred animals, on which position I should like to be furnished with a little evidence. Again, I cannot test the supposititious problem put by Mr. Wallace as to the strongest individual of an animal's progeny eventually being the stem-father of the race. He takes for granted that it is, and in doing so begs the question. I can only say the only experiments I know do not favour Mr. Wallace's *a priori* view, and that in the cases we can experiment upon, not the least satisfactory of which is the case of man himself, the condition most favourable to fertility, as I have quoted many examples to show, is that of comparative depletion.

Mr. Wallace, as before, is spare of instances. I can only extract two *bona fide* ones from his letter. He tells us the strongest bull leads the herd; this proves nothing, unless we are to infer from it that his progeny is the most numerous, and that the biggest and strongest therefore survive. I prefer to quote Mr. Darwin himself where I can. If Mr. Wallace's instance be worth anything, how does he account for the following: "The decrease in size of the Chillingham and Hamilton cattle must have been prodigious, for Prof. Rutimeyer has shown that they are almost certainly the descendants of the gigantic *Bos primigenius*. No doubt this decrease in size may be largely attributed to less favourable circumstances. Yet animals roaming over large parks and fed during severe winters can hardly be considered as placed under very unfavourable conditions" ("Variation of Animals and Plants under Domestication" ii. 119). What Mr. Darwin says of the wild cattle is equally true of the reindeer kept by the Laplanders compared with the wild ones on the Samoyede tundras, of the red deer of our larger forests compared with the skeletons of red deer from the turbaries, and is, perhaps, generally true of semi-wild races where man has not intervened with the special object of increasing the size by breeding from the largest individuals only.

In regard to the carnivora, I know of no reliable facts. I am not proposing the monstrous paradox that those animals which are so weak, diseased, or decrepit that they cannot sustain life at all, are the only ones that keep up the succession of the animal world. The toothless tigress, who cannot kill her food and is starving, will most certainly not be the mother of a long race. She can do nothing but die. But I say that, judging from analogy, it is probable that the lean and comparatively ill-fed tigress will breed more freely than the man-eater supplied with regular and abundant food.

The banks of the Chinese rivers and the rough country in the south and south-west of Ireland are both inhabited by teeming populations, remarkable for their poverty and fertility, and remarkable further for sending out immense colonies, which supplant wherever they go, in Manchuria, in Songaria, in Glasgow, in Manchester, in New York, the strong hearty, indigenous races. This being so (and I only quote these two as examples of a whole class), when Mr. Wallace asks the question, "How can weak and sickly parents provide for and bring up to maturity their offspring, and how are the offspring themselves (undoubtedly less vigorous than the offspring of strong and healthy parents) to maintain themselves?" I can only reply that they actually do so: *Veni, vidi, et credi*.

I must correct a wrong impression that Mr. Wallace has got hold of. In this controversy I have no theory; my only theory is that Natural Selection is an ingenious but fallacious explanation of the varieties of life.

I cannot understand Mr. Wallace's last sentence if it be meant for an argument; while if it is only a *jeu d'esprit* and witticism, it requires a commentary to tell us where the point is.

Lastly, I will consider Mr. Wallace's reiterated complaint that I have only treated of what is in most cases the least important factor in determining the continuance of species. Let me turn

very briefly to another of these factors put prominently forward by both Mr. Wallace and Dr. Beale, namely, "Obscure Colour."

We are not arguing about exceptional and individual cases, we are dealing with a general law, applicable or supposed to be applicable to the great majority of cases. Can it be said gravely that obscure colour has tended to the preservation of particular forms of life to the exclusion of others, not in a few exceptions, but as a general biological law?

Daylight, it will be admitted, is more likely to disclose an object than darkness. If we compare diurnal forms of life with nocturnal ones, we ought to find, if I read the tendency of the Darwinian argument rightly, that in the daylight when a sombre, obscure, or indifferent colour, would be of great service to hide an object, that there are a much smaller proportion of conspicuous forms of life abroad than at night when there would be no such need for obscurity, and a bright colour might be worn with impunity. Is such the fact?

Again, if we compare the animals and plants that live in tropical climates, where the light is intense, with those found in temperate and severe ones where the light is not so great and objects are not so prominent, do we find that the former has a comparative monopoly of conspicuous objects, or do we find rather that the reverse is the case, and that all the brightest objects we know in nature—the parrots, macaws, humming birds, butterflies, orchids, &c.—are found in the greatest profusion in the tropics, while we proverbially console ourselves for the absence of colour in our birds by boasting of their singing, and hang the beetles of Brazil in necklaces round our sisters' and wives' necks, while we crush our sombre representatives of the same class under our heels? Is it not equally true of the sea? In the Mediterranean, for instance, do not the brightly decked out gurnards and mullets far outnumber the dingier fish, while on the banks of foggy Newfoundland the sober tinted cod and ling are the prevailing types? In the former we have the clear blue water that washes round Sorrento pierced through and through by the blazing sun, while in the latter we have everything gloomy except the fisherman.

If we separate the animal world into flesh eaters and vegetable eaters, we ought to find, if this theory be true, that the former (which as a rule are not themselves the prey of other animals) are more conspicuous than the latter, since they have less reason for adopting a secret costume. But is it so? Are the hawks and owls and carnivorous beetles as classes more conspicuous than their victims? Is it a not fact that the most beautifully coloured creatures are as a rule the most helpless, weak, and accessible; that those animals which are supplied by nature with weapons of defence or are strong and can defend themselves, are as classes more obscure in colouring than those not so protected, and that the same rule applies to plants which are poisonous, nauseous, or protected by thorns? If these facts be true in the great majority of cases, we have another factor in Mr. Darwin's theory which is not satisfactory, and the cases quoted to support it become mere exceptions, which, by being exceptions, disprove the particular law he is maintaining. This letter has already exceeded reasonable limits, and I must postpone a further consideration of this and other objections to another occasion.

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