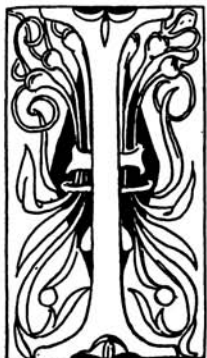


## The Deadlock in Darwinism

### PART I



T will be readily admitted that of all living writers Mr. Wallace is the one the peculiar turn of whose mind best fits him to write on the subject of natural selection, or the accumulation of fortunate but accidental variations through descent and struggle for existence. His mind in all its more essential characteristics closely resembles that of the late Mr. Charles Darwin himself, and it is no doubt due to this fact that he and Mr. Darwin elaborated their famous theory at the same time and independently of one another. I shall have occasion in the course of the following article to show how misled and misleading both these distinguished men have been, in spite of their unquestionable familiarity with the whole range of animal and vegetable phenomena. I believe it more respectful to both of them to do this in the most outspoken way. I believe their work to have been as mischievous as it has been valuable, and as valuable as it has been mischievous; and higher, whether praise or blame, I know not how to give. Nevertheless I would in the outset, and with the utmost sincerity, admit concerning Messrs. Wallace and Darwin that neither can be held as the more profound and conscientious thinker; neither can be put forward as the more ready to acknowledge obligation to the great writers on evolution who had preceded him, or to place his own developments in closer and more conspicuous historical connection

with earlier thought upon the subject ; neither is the more ready to welcome criticism and to state his opponent's case in the most pointed and telling way in which it can be put ; neither is the more quick to encourage new truth ; neither is the more genial generous adversary, or has the profounder horror of anything even approaching literary or scientific want of candour ; both display the same inimitable power of putting their opinions forward in the way that shall best ensure their acceptance ; both are equally unrivalled in the tact that tells them when silence will be golden, and when on the other hand a whole volume of facts may be advantageously brought forward. Less than the foregoing tribute both to Messrs. Darwin and Wallace I will not, and more I cannot pay.

Let us now turn to the most authoritative exponent of latter-day evolution. I mean to Mr. A. R. Wallace, whose work, entitled 'Darwinism,' though it should have been entitled 'Wallaceism,' is still so far Darwinistic that it develops the teaching of Mr. Darwin in the direction given to it by Mr. Darwin himself, so far, indeed, as this can be ascertained at all—and not in that of Lamarck. Mr. Wallace tells us, on the first page of his preface, that he has no intention of dealing even in outline with the vast subject of evolution in general, and has only tried to give such an account of the theory of natural selection as may facilitate a clear conception of Darwin's work. How far he has succeeded is a point on which opinion will probably be divided. Those who find Mr. Darwin's works clear will also find no difficulty in understanding Mr. Wallace ; those, on the other hand, who find Mr. Darwin puzzling are little likely to be less puzzled by Mr. Wallace. He continues :—

'The objections now made to Darwin's theory apply solely to the particular means by which the change of species has been brought about not to the fact of that change.'

But 'Darwin's theory'—as Mr. Wallace has elsewhere proved that he understands—has no reference 'to the fact of that change'—that is to say, to the fact that species have been modified in course of descent from other species. This is no more Mr. Darwin's theory than it is the reader's or my own. Darwin's theory is concerned only with 'the particular means by which change of species has been brought about ;' his contention being that this is mainly due to the natural survival of those individuals that have happened by some accident to be born most favourably adapted to their surroundings, or, in other words, through accumulation in the common course of nature of the more lucky,

variations that chance occasionally purveys. Mr. Wallace's words, then, in reality amount to this, that the objections now made to Darwin's theory apply solely to Darwin's theory, which is all very well as far as it goes, but might have been more easily apprehended if he had simply said, 'there are several objections now made to Mr. Darwin's theory.'

It must be remembered that the passage quoted above occurs on the first page of a preface dated March, 1889, when the writer had completed his task, and was most fully conversant with his subject. Nevertheless, it seems indisputable either that he is still confusing evolution with Mr. Darwin's theory, or that he does not know when his sentences have point and when they have none.

I should perhaps explain to some readers that Mr. Darwin did not modify the main theory put forward, first by Buffon, to whom it indisputably belongs, and adopted from him by Erasmus Darwin, Lamarck, and many other writers in the latter half of the last century and the earlier years of the present. The early evolutionists maintained that all existing forms of animal and vegetable life, including man, were derived in course of descent with modification from forms resembling the lowest now known.

Mr. Darwin went as far as this, and farther no one can go. The point at issue between him and his predecessors involves neither the main fact of evolution, nor yet the geometrical ratio of increase and the struggle for existence consequent thereon. Messrs. Darwin and Wallace have each thrown invaluable light upon these last two points, but Buffon, as early as 1756, had made them the keystone of his system. 'The movement of nature,' he then wrote, 'turns on two immovable pivots: one, the illimitable fecundity which she has given to all species: the other, the innumerable difficulties which reduce the results of that fecundity.' Erasmus Darwin and Lamarck followed in the same sense. They thus admit the survival of the fittest as fully as Mr. Darwin himself, though they do not make use of this particular expression. The dispute turns not upon natural selection, which is common to all writers on evolution, but upon the nature and causes of the variations that are supposed to be selected from and thus accumulated. Are these mainly attributable to the inherited effects of use and disuse, supplemented by occasional sports and happy accidents? Or are they mainly due to sports and happy accidents, supplemented by occasional inherited effects of use and disuse?

The Lamarckian system has all along been maintained by Mr. Herbert Spencer, who, in his 'Principles of Biology,' published in 1865, showed how impossible it was that accidental variations should accumulate at all. I am not sure how far Mr. Spencer would consent to being called a Lamarckian pure and simple, nor yet how far it is strictly accurate to call him one; nevertheless, I can see no important difference in the main positions taken by him and by Lamarck.

The question at issue between the Lamarckians, supported by Mr. Spencer and a growing band of those who have risen in rebellion against the Charles-Darwinian system, on the one hand, and Messrs. Darwin and Wallace with the greater number of our more prominent biologists, on the other, involves the very existence of evolution as a workable theory. For it is plain that what Nature can be supposed able to do by way of choice must depend on the supply of the variations from which she is supposed to choose; she cannot take what is not offered to her; and so, again, she cannot be supposed able to accumulate unless what is gained in one direction in one generation, or series of generations, is little likely to be lost in those that presently succeed. Now variations ascribed mainly to use and disuse can be supposed capable of being accumulated, for use and disuse are fairly constant for long periods among the individuals of the same species, and often over large areas; moreover, conditions of existence involving changes of habit, and thus of organisation, come for the most part gradually; so that time is given during which the organism can endeavour to adapt itself in the requisite respects, instead of being shocked out of existence by too sudden change. Variations, on the other hand, that are ascribed to mere chance cannot be supposed as likely to be accumulated, for chance is notoriously inconstant, and would not purvey the variations in sufficiently unbroken succession or in a sufficient number of individuals modified similarly in all the necessary correlations at the same time and place to admit of their being accumulated. It is vital therefore to the theory of evolution, as was early pointed out by the late Professor Fleeming Jenkin and by Mr. Herbert Spencer, that variations should be supposed to have a definite and persistent principle underlying them, which shall tend to engender similar and simultaneous modification, however small, in the vast majority of individuals composing any species. The existence of such a principle and its permanence is the only thing that can be supposed capable of acting as rudder and compass to the accumulation of variations, and of making it hold steadily on one course

for each species, till eventually many havens far remote from one another are safely reached.

It is obvious that the having fatally impaired the theory of his predecessors could not warrant Mr. Darwin in claiming, as he most fatuously did, the theory of evolution. That he is still generally believed to have been the originator of this theory is due to the fact that he claimed it, and that a powerful literary backing at once came forward to support him. It seems at first sight improbable that those who too zealously urged his claims were unaware that so much had been written on the subject, but when we find even Mr. Wallace himself as profoundly ignorant on this subject as he still either is, or affects to be, there is no limit assignable to the ignorance or affected ignorance of the kind of biologists who would write reviews in leading journals thirty years ago. Mr. Wallace writes :—

A few great naturalists, struck by the very slight difference between many of these species, and the numerous links that exist between the most different forms of animals and plants, and also observing that a great many species do vary considerably in their forms, colours, and habits, conceived the idea that they might be all produced one from the other. The most eminent of these writers was a great French naturalist, Lamarck, who published an elaborate work, the 'Philosophie Zoologique,' in which he endeavoured to prove that all animals whatever are descended from other species of animals. He attributed the change of species chiefly to the effect of changes in the conditions of life—such as climate, food, &c. ; and especially to the desires and efforts of the animals themselves to improve their condition, leading to a modification of form or size in certain parts, owing to the well-known physiological law that all organs are strengthened by constant use, while they are weakened or even completely lost by disuse. . . .

The only other important work dealing with the question was the celebrated 'Vestiges of Creation,' published anonymously, but now acknowledged to have been written by the late Robert Chambers.

None are so blind as those who will not see, and it would be waste of time to argue with the invincible ignorance of one who thinks Lamarck and Buffon conceived that all species were produced from one another, more especially as I have already dealt at some length with the early evolutionists in my work 'Evolution, Old, and New,' published ten years ago, and not, so far as I am aware, detected in serious error or omission. If, however, Mr. Wallace still thinks it safe to presume so far on the ignorance of his readers as to say that the only two important works on evolution before Mr. Darwin's were Lamarck's 'Philosophie Zoologique' and the 'Vestiges of Creation,' how fathomable is the

ignorance of the average reviewer likely to have been thirty years ago when the 'Origin of Species' was first published? Mr. Darwin claimed evolution as his own theory. Of course he would not claim it if he had no right to it. Then by all means give him the credit of it. This was the most natural view to take, and it was generally taken. It was not, moreover, surprising that people failed to appreciate all the niceties of Mr. Darwin's 'distinctive feature,' which, whether distinctive or no, was assuredly not distinct, and was never frankly contrasted with the older view, as it would have been by one who wished it to be understood and judged upon its merits. It was in consequence of this omission that people failed to note how fast and loose Mr. Darwin played with his distinctive feature, and how readily he dropped it on occasion.

It may be said that the question of what was thought by the predecessors of Mr. Darwin is, after all, personal and of no interest to the general public comparable to that of the main issue—whether we are to accept evolution or not. Granted that Buffon, Erasmus Darwin, and Lamarck bore the burden and heat of the day before Mr. Charles Darwin was born, they did not bring people round to their opinion, whereas Mr. Darwin and Mr. Wallace did, and the public cannot be expected to look beyond this broad and indisputable fact.

The answer to this is that the theory which Messrs. Darwin and Wallace have persuaded the public to accept is demonstrably false, and that the opponents of evolution are certain in the end to triumph over it. Paley, in his 'Natural Theology,' long since brought forward far too much evidence of design in animal organization to allow of our setting down its marvels to the accumulations of fortunate accident, undirected by will, effort, and intelligence. Those who examine the main facts of animal and vegetable organization without bias will, no doubt, ere long conclude that all animals and vegetables are derived ultimately from unicellular organisms, but they will not less readily perceive that the evolution of species without the concomitance and direction of mind and effort is as inconceivable as is the independent creation of every individual species. The two facts, Evolution and Design, are equally patent to plain people. There is no escaping from either. According to Messrs. Darwin and Wallace, we may have evolution, but are on no account to have it as mainly due to intelligent effort, guided by ever higher and higher range of sensations, perceptions, and ideas. We are to set it down to the shuffling of cards, or the throwing of dice without the play, and this will never stand.

According to the older men, cards did indeed count for much but play counted for more. They denied the teleology of the time—that is to say, the teleology that saw all adaptation to surroundings as part of a plan devised long ages since by a quasi-anthropomorphic being who schemed everything out much as a man would do, but on an infinitely vaster scale. This conception they found repugnant alike to intelligence and conscience, but, though they do not seem to have perceived it, they left the door open for a design more true and more demonstrable than that which they excluded. By making their variations mainly due to effort and intelligence, they made organic development run on all fours with human progress, and with inventions which we have watched to grow up from small beginnings. They made the development of man from the amoeba part and parcel of the story that may be read, though on an infinitely smaller scale, in the development of our most powerful marine engines from the common kettle, or of our finest microscopes from the dew-drop.

The development of the steam-engine and the microscope are due to intelligence and design, which did indeed utilize chance suggestions, but which improved on these, and directed each step of their accumulation, though never foreseeing more than a step or two ahead, and often not so much as this. The fact, as I have elsewhere urged, that the man who made the first kettle did not foresee the engines of the *Great Eastern*, or that he who first noted the magnifying power of the dew-drop had no conception of our now present microscopes—the very limited amount, in fact, of design and intelligence that was called into play at any one point—this does not make us deny that the steam-engine and microscope owe their development to design. If each step of the road was designed, the whole journey was designed, though the particular end was not designed when the journey was begun. And so is it, according to the older view of evolution, with the development of those living organs, or machines, that are born with us, as part of the perambulating carpenter's chest we call our bodies. The older view gives us our design, and gives us our evolution too. If it refuses to see a quasi-anthropomorphic God modelling each species from without as a potter models clay, it gives us God as vivifying and indwelling in all his creatures—He in them, and they in Him. If it refuses to see God outside the universe, it equally refuses to see any part of the universe as outside God. If it makes the universe the body of God, it also makes God the soul of the universe. The question at issue, then, between the Darwinism of Erasmus Darwin, and the nec-Darwinism of his grandson, is not a personal one, nor any-

thing like a personal one. It not only involves the existence of evolution, but it affects the view we take of life and things in an endless variety of most interesting and important ways. It is imperative, therefore, on those who take any interest in these matters, to place side by side in the clearest contrast the views of those who refer the evolution of species mainly to accumulation of variations that have no other inception than chance, and of that older school which makes design perceive and develop still further the goods that chance provides.

But over and above this, which would be in itself sufficient, the historical mode of studying any question is the only one which will enable us to comprehend it effectually. The personal element cannot be eliminated from the consideration of works written by living persons for living persons. We want to know who is who—whom we can depend upon to have no other end than the making things clear to himself and his readers, and whom we should mistrust as having an ulterior aim on which he is more intent than on the furthering of our better understanding. We want to know who is doing his best to help us, and who is only trying to make us help him, or to bolster up the system in which his interests are vested. There is nothing that will throw more light upon these points than the way in which a man behaves towards those who have worked in the same field with himself, and, again, than his style. A man's style, as Buffon long since said, is the man himself. By style, I do not, of course, mean grammar or rhetoric, but that style of which Buffon again said that it is like happiness, and *vient de la douceur de l'âme*. When we find a man concealing worse than nullity of meaning under sentences that sound plausibly enough, we should distrust him much as we should a fellow-traveller whom we caught trying to steal our watch. We often cannot judge of the truth or falsehood of facts for ourselves, but we most of us know enough of human nature to be able to tell a good witness from a bad one.

However this may be, and whatever we may think of judging systems by the directness or indirectness of those who advance them, biologists, having committed themselves too rashly, would have been more than human if they had not shown some pique towards those who dared to say, firstly, that the theory of Messrs. Darwin and Wallace was unworkable, and secondly, that even though it were workable it would not justify either of them in claiming evolution. When biologists show pique at all they generally show a good deal of pique, but pique or no pique, they shunned Mr. Spencer's objection above referred to



with a persistency more unanimous and obstinate than I ever remember to have seen displayed even by professional truth-seekers. I find no rejoinder to it from Mr. Darwin himself, between 1865 when it was first put forward, and 1882 when Mr. Darwin died. It has been similarly ostracized by all the leading apologists of Darwinism, so far at least as I have been able to observe, and I have followed the matter closely for many years. Mr. Spencer has repeated and amplified it in his recent work, 'The Factors of Organic Evolution,' but it still remains without so much as an attempt at serious answer, for the perfunctory and illusory remarks of Mr. Wallace at the end of his 'Darwinism' cannot be counted as such. The best proof of its irresistible weight is that Mr. Darwin, though maintaining silence in respect to it, retreated from his original position in the direction that would most obviate Mr. Spencer's objection.

Yet this objection has been repeatedly urged by the more prominent anti-Charles-Darwinian authorities, and there is no sign that the British public is becoming less rigorous in requiring people either to reply to objections repeatedly urged by men of even moderate weight, or to let judgment go by default. As regards Mr. Darwin's claim to the theory of evolution generally, Darwinians are beginning now to perceive that this cannot be admitted, and either say with some hardihood that Mr. Darwin never claimed it, or after a few saving clauses to the effect that his theory refers only to the particular means by which evolution has been brought about, imply forthwith thereafter none the less that evolution is Mr. Darwin's theory. Mr. Wallace has done this repeatedly in his recent 'Darwinism.' Indeed, I should be by no means sure that on the first page of his preface, in the passage about 'Darwin's theory,' which I have already somewhat severely criticized, he was not intending evolution by 'Darwin's theory,' if in his preceding paragraph he had not so clearly shown that he knew evolution to be a theory of greatly older date than Mr. Darwin's.

The history of science—well exemplified by that of the development theory—is the history of eminent men who have fought against light and have been worsted. The tenacity with which Darwinians stick to their accumulation of fortuitous variations is on a par with the like tenacity shown by the illustrious Cuvier, who did his best to crush evolution altogether. It always has been thus, and always will be; nor is it desirable in the interests of Truth herself that it should be otherwise. Truth is like money—lightly come, lightly go; and if she cannot

hold her own against even gross misrepresentation, she is herself not worth holding. Misrepresentation in the long run makes Truth as much as it mars her; hence our law courts do not think it desirable that pleaders should speak their *bonâ fide* opinions, much less that they should profess to do so. Rather let each side hoodwink judge and jury as best it can, and let truth flash out from collision of defence and accusation. When either side will not collide, it is an axiom of controversy that it desires to prevent the truth from being elicited.

Let us now note the courses forced upon biologists by the difficulties of Mr. Darwin's distinctive feature. Mr. Darwin and Mr. Wallace, as is well known, brought the feature forward simultaneously and independently of one another, but Mr. Wallace always believed in it more firmly than Mr. Darwin did. Mr. Darwin as a young man did not believe in it. He wrote before 1839, 'Nature, by making habit omnipotent and its effects hereditary, has fitted the Fuegian for the climate and productions of his country,'<sup>1</sup> a sentence than which nothing can coincide more fully with the older view that use and disuse were the main purveyors of variations, or conflict more fatally with his own subsequent distinctive feature. Moreover, as I showed in my last work on Evolution,<sup>2</sup> in the peroration to his 'Origin of Species,' he discarded his accidental variations altogether, and fell back on the older theory, so that the body of the 'Origin of Species' supports one theory, and the peroration another that differs from it *toto cælo*. Finally, in his later editions, he retreated indefinitely from his original position, edging always more and more continually towards the theory of his grandfather and Lamarck. These facts convince me that he was at no time a thorough-going Darwinian, but was throughout an unconscious Lamarckian, though ever anxious to conceal the fact alike from himself and from his readers.

Not so with Mr. Wallace, who was both more outspoken in the first instance, and who has persevered along the path of Wallaceism just as Mr. Darwin with greater sagacity was ever on the retreat from Darwinism. Mr. Wallace's profounder faith led him in the outset to place his theory in fuller daylight than Mr. Darwin was inclined to do. Mr. Darwin just waved Lamarck aside, and said as little about him as he could, while in his earlier editions Erasmus Darwin and Buffon were not so much as named. Mr. Wallace, on the contrary, at once raised the

<sup>1</sup> 'Voyages of the *Adventure* and *Beagle*,' iii. p. 237.

<sup>2</sup> 'Luck, or Cunning, as the main means of Organic Modification?' (Longmans), pp. 179, 180.

Lamarckian spectre, and declared it exorcised. He said the Lamarckian hypothesis was 'quite unnecessary.' The giraffe did not 'acquire its long neck by desiring to reach the foliage of the more lofty shrubs, and constantly stretching its neck for this purpose, but because any varieties which occurred among its antitypes with a longer neck than usual at once secured a fresh range of pasture over the same ground as their shorter-necked companions, and on the first scarcity of food were thus enabled to outlive them.'<sup>1</sup>

'Which occurred' is evidently 'which happened to occur' by some chance or accident unconnected with use and disuse. The word 'accident' is never used, but Mr. Wallace must be credited with this instance of a desire to give his readers a chance of perceiving that according to his distinctive feature evolution is an affair of luck, rather than of cunning. Whether his readers actually did understand this as clearly as Mr. Wallace doubtless desired that they should, and whether greater development at this point would not have helped them to fuller apprehension, we need not now inquire. What was gained in distinctness might have been lost in distinctiveness, and after all he did technically put us upon our guard.

Nevertheless he too on a pinch takes refuge in Lamarckism. In relation to the manner in which the eyes of soles, turbot, and other flatfish travel round the head so as to become in the end unsymmetrically placed, he says:—

'The eyes of these fish are curiously distorted in order that both eyes may be upon the upper side, where alone they would be of any use. . . . Now if we suppose this process, which in the young is completed in a few days or weeks, to have been spread over thousands of generations during the development of these fish, those usually surviving *whose eyes retained more and more of the position into which the young fish tried to twist them* [italics mine], the change becomes intelligible.'<sup>2</sup> When it was said by Prof. Ray Lankester—who knows as well as most people what Lamarck taught—that this was 'flat Lamarckism,' Mr. Wallace rejoined that it was the survival of the modified individuals that did it all, not the efforts of the young fish to twist their eyes, and the transmission to descendants of the effects of those efforts. But this, as I

<sup>1</sup> *Journals of the Proceedings of the Linnean Society* (Williams and Norgate), 1858, p. 61.

<sup>2</sup> 'Darwinism' (Macmillan, 1889), p. 130.

said in my book 'Evolution, Old and New,'<sup>1</sup> is like saying that horses are swift runners, not by reason of the causes, whatever they were, that occasioned the direct line of their progenitors to vary towards ever greater and greater swiftness, but because their more slow-going uncles and aunts go away. Plain people will prefer to say that the main cause of any accumulation of favourable modifications consists rather in that which brings about the initial variations, and in the fact that these can be inherited at all, than in the fact that the unmodified individuals were not successful. People do not become rich because the poor in large numbers go away, but because they have been lucky, or provident, or more commonly both. If they would keep their wealth when they have made it they must exclude luck thenceforth to the utmost of their power, and their children must follow their example, or they will soon lose their money. The fact that the weaker go to the wall does not bring about the greater strength of the stronger; it is the consequence of this last and not the cause—unless, indeed, it be contended that a knowledge that the weak go to the wall stimulates the strong to exertions which they would not otherwise so make, and that these exertions produce inheritable modifications. Even in this case, however, it would be the exertions, or use and disuse, that would be the main agents in the modification. But it is not often that Mr. Wallace thus backslides. His present position is that acquired (as distinguished from congenital) modifications are not inherited at all. He does not indeed put his faith prominently forward and pin himself to it as plainly as could be wished, but under the heading, 'The Non-Hereditability of Acquired Characters,' he writes as follows on p. 440 of his recent work in reference to Professor Weismann's Theory of Heredity:—

Certain observations on the embryology of the lower animals are held to afford direct proof of this theory of heredity, but they are too technical to be made clear to ordinary readers. A logical result of the theory is the impossibility of the transmission of acquired characters, since the molecular structure of the germ-plasm is already determined within the embryo; and Weismann holds that there are no facts which really prove that acquired characters can be inherited, although their inheritance has, by most writers, been considered so probable as hardly to stand in need of direct proof.

We have already seen in the earlier part of this chapter that many instances of change, imputed to the inheritance of acquired variations, are really cases of selection; \* \* \*

And the rest of the remarks tend to convey the impression that Mr.

<sup>1</sup> Longmans, 1890, p. 376.

Wallace adopts Professor Weismann's view, but, curiously enough, though I have gone through Mr. Wallace's book with a special view to this particular point, I have not been able to find him definitely committing himself either to the assertion that acquired modifications never are inherited, or that they sometimes are so. It is abundantly laid down that Mr. Darwin laid too much stress on use and disuse, and a residuary impression is left that Mr. Wallace is endorsing Professor Weismann's view, but I have found it impossible to collect anything that enables me to define his position confidently in this respect.

This is natural enough, for Mr. Wallace has entitled his book 'Darwinism,' and a work denying that use and disuse produced any effect could not conceivably be called Darwinism. Mr. Herbert Spencer has recently collected many passages from 'The Origin of Species,' and from 'Animals and Plants under Domestication,'<sup>1</sup> which show how largely, after all, use and disuse entered into Mr. Darwin's system, and we know that in his later years he attached still more importance to them. It was out of the question, therefore, that Mr. Wallace should categorically deny that their effects were inheritable. On the other hand the temptation to adopt Professor Weismann's view must have been overwhelming to one who had been already inclined to minimize the effects of use and disuse. On the whole one does not see what Mr. Wallace could do, other than what he has done—unless, of course, he changed his title, or, had been no longer Mr. Wallace.

Besides, thanks to the works of Mr. Spencer, Professor Mivart, Professor Semper, and very many others, there has for some time been a growing perception that the Darwinism of Charles Darwin was doomed. Use and disuse must either do even more than is officially recognized in Mr. Darwin's later concessions, or they must do a great deal less. If they can do as much as Mr. Darwin himself said they did, why should they not do more? Why stop where Mr. Darwin did? And again, where in the name of all that is reasonable did he really stop? He drew no line, and on what principle can we say that so much is possible as effect of use and disuse, but so much more impossible? If, as Mr. Darwin contended, disuse can so far reduce an organ as to render it rudimentary, and in many cases get rid of it altogether, why cannot use create as much as disuse can destroy, provided it has anything, no matter how low in structure, to begin with? Let us know where we stand. If it is admitted

<sup>1</sup> See *Nature*, March 6, 1890.

that use and disuse can do a good deal, what does a good deal mean? and what is the proportion between the shares attributable to use and disuse and to natural selection respectively? If we cannot be told with absolute precision, let us at any rate have something more definite than the statement that natural selection is 'the most important means of modification.'

Mr. Darwin gave us no help in this respect: and worse than this, he contradicted himself so flatly as to show that he had very little definite idea upon the subject at all. Thus in respect to the winglessness of the Madeira beetles he wrote:

In some cases we might easily put down to disuse modifications of structure, which are wholly or mainly due to natural selection. Mr. Wollaston has discovered the remarkable fact that 200 beetles out of the 550 species (but more are now known) inhabiting Madeira are so far deficient in wings that they cannot fly; and that of the 29 endemic genera no less than 23 have all their species in this condition! Several facts,—namely, that beetles in many parts of the world are frequently blown out to sea and perish; that the beetles in Madeira, as observed by Mr. Wollaston, lie much concealed until the wind lulls and the sun shines; that the proportion of wingless beetles is larger on the exposed Desertas than in Madeira itself; and especially the extraordinary fact so strongly insisted on by Mr. Wollaston that certain large groups of beetles, elsewhere excessively numerous, which absolutely require the use of their wings are here almost entirely absent;—these several considerations make me believe that the wingless condition of so many Madeira beetles is mainly due to the action of natural selection, *combined probably with disuse* [italics mine]. For during many successive generations each individual beetle which flew least, either from its wings having been ever so little less perfectly developed or from indolent habit, will have the best chance from not being blown out to sea; and on the other hand, those beetles which most readily took to flight would oftenest have been blown to sea and thus destroyed.<sup>1</sup>

We should like to know firstly somewhere about how much disuse was able to do after all, and moreover why, if it can do anything at all, it should not be able to do all. Mr. Darwin says: 'Any change in structure and function which can be effected by small stages is within the power of natural selection.' 'And why not,' we ask, 'within the power of use and disuse?' Moreover on a later page we find Mr. Darwin saying:

*It appears probable that disuse has been the main agent in rendering organs rudimentary* [italics mine]. It would lead by slow steps to the more and more

<sup>1</sup> 'Origin of Species,' Sixth Edition, 1876, p. 401.

complete reduction of a part, until at last it has become rudimentary—as in the case of the eyes of animals inhabiting dark caverns, and of the wings of birds inhabiting oceanic islands, which have seldom been forced by beasts of prey to take flight, and have ultimately lost the power of flying. Again, an organ, useful under certain conditions, might become injurious to others, *as with the wings of beetles living on small and exposed islands*; and in this case natural selection will have aided in reducing the organ, until it was rendered harmless and rudimentary [italics mine].<sup>1</sup>

So that just as an undefined amount of use and disuse was introduced on the earlier page to supplement the effects of natural selection in respect of the wings of beetles on small and exposed islands, we have here an undefined amount of natural selection introduced to supplement the effects of use and disuse in respect of the identical, phenomena. In the one passage we find that natural selection has been the main agent in reducing the wings, though use and disuse have had an appreciable share in the result; in the other, it is use and disuse that have been the main agents, though an appreciable share in the result must be ascribed to natural selection.

Besides, who has seen the uncles and aunts going away with the uniformity that is necessary for Mr. Darwin's contention? We know that birds and insects do often get blown out to sea and perish, but in order to establish Mr. Darwin's position we want the evidence of those who watched the reduction of the wings during the many generations in the course of which it was being effected, and who can testify that all, or the overwhelming majority of the beetles born with fairly well-developed wings got blown out to sea, while those alone survived whose wings were congenitally degenerate. Who saw them go, or can point to analogous cases so conclusive as to compel assent from any equitable thinker?

Darwinians of the stamp of Mr. Thiselton Dyer, Professor Ray Lankester, or Mr. Romanes, insist on their pound of flesh in the matter of irrefragable demonstration. They complain of us for not bringing forward some one who has been able to detect the movement of the hour-hand of a watch during a second of time, and when we fail to do so declare triumphantly that we have no evidence that there is any connection between the beating of a second and the movement of the hour-hand. When we say that rain comes from the condensation of

<sup>1</sup> 'Origin of Species,' Sixth Edition, 1876, p. 109.

moisture in the atmosphere, they demand of us a rain drop from moisture not yet condensed. If they stickle for proof and cavil on the ninth part of a hair, as they do when we bring forward what we deem excellent instances of the transmission of an acquired characteristic, why may not we, too, demand at any rate some evidence that the unmodified beetles actually did always, or nearly always, get blown out to sea, during the reduction above referred to, and that it is to this fact and not to the masterly inactivity of their fathers and mothers that the Madeira beetles owe their winglessness? If we began stickling for proof in this way our opponents would not be long in letting us know that absolute proof is unattainable on any subject, that reasonable presumption is our highest certainty, and that crying out for too much evidence is as bad as accepting too little. Truth is like a photographic sensitized plate, which is equally ruined by over and by under exposure, and the just exposure for which can never be absolutely determined.

Surely if disuse can be credited with the vast powers involved in Mr. Darwin's statement that it has probably 'been the main agent in rendering organs rudimentary,' no limits are assignable to the accumulated effects of habit, provided the effects of habit, or use and disuse, are supposed, as Mr. Darwin supposed them, to be inheritable at all. Darwinians have at length woke up to the dilemma in which they are placed by the manner in which Mr. Darwin tried to sit on the two stools of use and disuse, and natural selection of accidental variations, at the same time. The knell of Charles-Darwinism is rung in Mr. Wallace's present book, and in the general perception on the part of biologists that we must either assign to use and disuse such a predominant share in modification as to make it the feature most proper to be insisted on, or deny that the modifications, whether of mind or body, acquired during a single life-time, are ever transmitted at all. If they can be inherited at all, they can be accumulated. If they can be accumulated at all, they can be so, for anything that appears to the contrary, to the extent of the specific and generic differences with which we are surrounded. The only thing to do is to pluck them out root and branch: they are as a cancer which, if the smallest fibre be left unexcised, will grow again, and kill any system on to which it is allowed to fasten. Mr. Wallace, therefore, may well be excused if he casts longing eyes towards Weismannism.

And what was Mr. Darwin's system?—who can make head or tail of the inextricable muddle in which he left it? The 'Origin of Species' in



its latest shape is the reduction of hedging to an absurdity. How did Mr. Darwin himself leave it in the last chapter of the last edition of the 'Origin of Species'? He wrote:—

I have now recapitulated the facts and considerations that have thoroughly convinced me that species have been modified during a long course of descent. This has been effected chiefly through the natural selection of numerous, successive, slight, favourable variations; aided in an important manner by the inherited effects of the use and disuse of parts, and in an unimportant manner—that is in relation to adaptive structures whether past or present—by the direct action of external conditions, and by variations which seem to us in our ignorance to arise spontaneously. It appears that I formerly underrated the frequency and value of these latter forms of variation as leading to permanent modifications of structure independently of natural selection.

The 'numerous, successive, slight, favourable variations' above referred to are intended to be fortuitous, accidental, spontaneous. It is the essence of Mr. Darwin's theory that this should be so. Mr. Darwin's solemn statement, therefore, of his theory after he had done his best or his worst with it, is, when stripped of surplusage, as follows:—

The modification of species has been mainly effected by accumulation of spontaneous variations; it has been aided in an important manner by accumulation of variations due to use and disuse, and in an unimportant manner by spontaneous variations; I do not even now think that spontaneous variations have been very important, but I used once to think them less important than I do now.

It is a discouraging symptom of the age that such a system should have been so long belauded, and it is a sign of returning intelligence that even he who has been more especially the *alter ego* of Mr. Darwin should have felt constrained to close the chapter of Charles-Darwinism as a living theory, and relegate it to the important but not very creditable place in history which it must henceforth occupy. It is astonishing, however, that Mr. Wallace should have quoted the extract from the 'Origin of Species' just given, as he has done on p. 412 of his 'Darwinism' without betraying any sign that he has caught its driftlessness—for drift, other than a desire to hedge, it assuredly has not got. The battle now turns on the question whether modifications of either structure or instinct due to use or disuse are ever inherited, or whether they are not. Can the effects of habit be transmitted to progeny at all? We know that more usually they are not transmitted

to any perceptible extent, but we believe also that occasionally, and indeed not infrequently, they are inherited and even intensified. What are our grounds for this opinion? It will be my object to put these forward in the following number of THE UNIVERSAL REVIEW.

SAMUEL BUTLER.

*(To be continued.)*

