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The following works were presented to the Library :—

- “Natural Theology, being the Gifford Lectures for 1891.” By Sir George
G. Stokes, Bart., V.P.R.S. *From the Author.*
- “The Hebrew Bible and Science.” By the Rev. W. C. Badger, M.A.
From the Author.
- “Eclipses, Past and Future.” By Rev. S. J. Johnson, M.A. Oxon.
F.R.A.S. *From the Author*
- “Hindu Literature.” E. A. Reed. ”
- “Life and Times of Joseph.” By Rev. H. G. Tomkins. ”
- “The London Quarterly Review.” *A. McArthur, Esq.*
- “Natural Theology and Modern Thought.” Rev. J. H. Kennedy, B.D.
From the Author.
- “The Testimony of History to the Truth of Scripture.” By Rev.
H. L. Hastings. *From the Author.*
- “Bulletins of the Museum of Comparative Zoology at Harvard College.”
From Professor Alexander Agassiz.

The following Paper was then read by the Author :—

THE WEAK SIDES OF NATURAL SELECTION.

By J. W. SLATER, Esq., F.C.S., F.E.S.

IT may seem, perhaps, strange that in these days any sober-minded naturalist, especially if a believer in Organic Evolution, should venture to call in question the theory of “Natural Selection.” Yet it may be worth while to look closely into this process and to ask whether it can really do all that has been so freely ascribed to it. I shall not attempt to describe or to define Natural Selection, since that task has been performed in several works which are easily accessible. I will merely say that it amounts to nearly the same thing, though seen from another point of view, as the “struggle for existence,” or the “survival of the fittest,” and that it is the very essence of that form of Evolutionism which is mainly due to Charles Darwin. Now I am by no means seeking to deny that creatures out of harmony with their surroundings are ill-calculated to survive. Still less can I doubt that there is a struggle for existence raging in the world around us. But I ask if this struggle is not more likely to blot out existing forms of life than to bring new forms into being, or to raise them to a higher stage of existence?

In the first place we find that among those who accept Natural Selection as the main agent in the Genesis of Species there have sprung up wide differences of opinion both as to its scope and its modes of operation. Whilst some naturalists regard it as the main, if not the sole factor in phyllogeny, Charles Darwin himself in his later writings owns that in the earlier editions of his "Origin of Species," he "probably attributed too much to the action of Natural Selection and the Survival of the Fittest." More decisive is the language of Dr. A. R. Wallace: "Natural Selection is not the all-powerful, all-sufficient and only cause of the development of organic forms." Candour, however, compels me to admit that Dr. Wallace now appears to have swung round to a belief in Natural Selection more sweeping than that at first entertained by Darwin. Professor St. George Mivart also considers that the Survival of the Fittest "plays merely a subordinate part." Very similar is the contention of Mr. Herbert Spencer. Mr. S. Butler rejects Natural Selection entirely.

Mr. J. Huddart cannot realize that such haphazard means as Natural Selection can have wrought out such marvels as are exhibited throughout creation. He insists that "were Natural Selection permitted to mould the forms of life around us, uncontrolled and undirected by any Supreme power, shapes the most grotesque and monstrous would inevitably inhabit the globe."

That Natural Selection has not been thus uncontrolled may be gathered from the limits which seem to have been set to the development and the modification of species. Why do we never see in any vertebrate animal more than two pairs of limbs or their rudiments? Why are parts which have lost their function, such as the external ear in mankind, or the vermiform appendage to the cæcum, still produced in generation after generation? Why is the secretion of silk confined to invertebrate animals, and the production of physiological venoms to cold-blooded groups? To such questions and to many more the believer in Natural Selection is so far less able to reply than is the naturalist of the Old School. The latter could solve all problems by an appeal to the *sic volo, sic jubeo* of the Creator. The Natural Selectionist refers us instead, substantially to chance. Can such an exchange satisfy our reason?

We may thus venture to say that there prevails a very wide-spread feeling of the insufficiency of the Darwinian

explanation of the Origin of Species. Authorities are by no means agreed as to its bearings and its efficiency.

But there is further divergence of opinion. One of the authorised expounders of Darwinism tells us that most people misunderstand the meaning of the phrase, "struggle for existence." "They imagine that the struggle is chiefly waged between different species, whilst it is chiefly conducted between members of the same species." But what says the co-discoverer of the theory of Natural Selection? In his "Island Life" he admits that "The most effective agent in the extinction of species is the pressure of other species, whether as enemies, or simply as competitors," a distinction, I must remark, without a difference. We cannot, indeed, conceive of a species extinguishing itself, the case of the Kilkenny cats, of course, being always excepted. But let us turn from authorities, even the most eminent, to actual facts.

The native flora and fauna of St. Helena have been practically extirpated by the goat. The young seedlings were browsed down as fast as they sprung up, and when the old giants of the forest decayed there were no successors to take their place. As a necessary consequence the insects and the birds disappeared in turn. The same "horned wretch"—fit type of evil—which as Sir Joseph Hooker shows, has ravaged the earth even to a greater extent than man has done by war, is now, in the very same manner, laying waste South Africa; to such an extent has the mischief already been carried that a troop of the Colonial cavalry on the march actually gave three cheers on meeting with a tree.

Vile European weeds, devoid alike of use and beauty, are fast extirpating the lovely and interesting flora of West Australia and of California. To give a catalogue of the instances where some plant or animal is being extinguished, or has already disappeared under the pressure of some other species, would fill a goodly volume. But almost every observer or even reader will himself have met with such instances. We may, therefore, I think, venture to reject Mr. Grant Allen's contention, and to conclude that though much suffering has been occasioned to individuals by struggles within the boundaries of the same species, for the causes of the great changes in either the animal or the vegetable world, we must look elsewhere, *i.e.*, to attacks from without.

I fear it must be owned that Natural Selection supplies too easy a solution for many difficulties. Thus we are asked

why are many insects, known to be venomous or offensive, clad with remarkably gay colours. Mr. Wallace and my late friend Thomas Belt held that these colours are a danger-signal, and have observed that such creatures are in fact shunned. I have found that some of the most strikingly coloured caterpillars feed on deadly plants and retain poisonous principles in their bodies. Yet Mr. Grant Allen, on the other hand, does not believe in warning colours, but asserts that poisonous plants, such as the arum, have, by a process of Natural Selection, developed intensely brilliant colours so as to allure birds to eat them. The seeds are then supposed to vegetate more luxuriantly in the decaying body of the poisoned bird. I cannot learn that Mr. Allen has ever met with a dead bird with arum-berries in its crop.

It would surely be a boon to the scientific world if the leading Darwinians would come to some understanding concerning natural selection and tell us what we are expected to believe.

But I must now ask if this process can produce new species? It has been admitted that before natural selection can come into play, variation must have already set in. Suppose a pair of animals existing in the primeval world had produced a hundred fertile ova. There are then only two possible cases: the young animals springing from these ova must either be one and all exactly alike, or they must exhibit certain differences. In the former alternative there is absolutely no ground for natural selection to work upon; the very idea of selection implying differences in the objects among which a selection is to be made. In the second alternative the varieties being, by hypothesis, antecedent to the action of natural selection, cannot be its effects. Hence in either case we have something which the Darwinian theory is quite unable to account for. We want a law which shall go deeper than Natural Selection, before we can understand the origin of species. At present we are merely offered, as it were, a rope ladder with no point from which it may be suspended.

We may ask how can Natural Selection have developed in any animal a power far beyond its utmost need? There is a small black spider in Southern Russia, that lurks among grass. Horses and cattle are often bitten upon the lips whilst grazing, and sometimes die in consequence. How can such a poison have been developed? What benefit can it confer upon the spider? It is, of course, unable to eat the dead

horse or cow. It cannot act defensively, since any animal which might crush the spider will not even be aware of its presence. And in order to overpower the creatures upon which the spider feeds, a venom incomparably less intense would suffice. A spider of similar properties is found in Queensland, and its bite, if not fatal to man, causes intense suffering. This species is black, with a red spot.

There is another consideration which seems to me not devoid of weight. Believers in Organic Evolution consider that all the species of mammalia found, *e.g.*, in Asia and Africa, have been derived from one—or a few—pristine placental forms. They suppose that in a similar manner the mammalia of Australia have been derived from one—or a few—pristine marsupial forms. But if we examine the Australian species we find them analogues, or it might perhaps be said parodies of the placental mammalian forms existing in the rest of the world. Thus the extinct *Thylacoleo carnifex* was in habits, form, and size, a lion, to be distinguished from the true lion merely by its marsupial bones. *Diprotodon* and *Nototherium*, also extinct, seem to have approximated to the elephant. The tiger wolf, or zebra wolf of Tasmania, is always, excepting its marsupial features, an excellent imitation of a wolf. In like manner various other Australian forms mimic the species of the rest of the world. This seems to show that Natural Selection is not supreme, but that its operation is over-ruled by some unknown agency which keeps it within certain limits.

We come now to another consideration. It is admitted that most animals and plants produce so numerous a progeny that were all to survive they could not find food. Hence the destruction of a large portion is imperative. But this process is not, as Darwinism supposes, a methodical weeding out of the unfit, whilst the healthiest and strongest are selected for preservation. As far as we can see it is a perfectly random operation. Mr. Wallace admits that the "weeding out" takes place among insects to a great extent in the egg and larva states, to which we may safely add in the pupa state. Of the eggs laid by a female butterfly many perish as such without ever seeing the light at all. But how is this effected? Every egg of the whole brood is equally helpless on the approach of a devourer or a parasite. For one that escapes in virtue of any superiority on its own part ten will owe their survival to what—humanly speaking—must be pronounced mere chance. One egg, without any peculiar fitness on its

part, may survive, because it has been deposited by the mother in a less conspicuous place than the rest. One egg may have perished, not from want of fitness, but because some ovivorous or parasitical insect visited the particular leaf to which it was attached. Other causes might be mentioned—accidental as far as man can judge—upon which the quickening, or the death of an egg, may depend. Here, then, there is no selection, no weeding out, but a destruction of a number of individuals with as little reference to their properties as if the question had been decided by lot.

From the egg we pass to the larva. Here there are doubtless greater individual differences. It may be at once admitted that one caterpillar may have keener senses to perceive the approach of danger, greater agility in escaping, more cunning in concealment, or an odour less attractive to enemies than have others, and that it may thus have a greater prospect of survival. But every observer knows that a vast number of cases must occur in which chance alone can decide. The quite accidental matter of position at some moment may be of far greater consequence for the life of a larva than a slight variation in any of the points just mentioned.

No small proportion of the premature deaths occur also in the pupa state, and here we have a return to the conditions of the egg. Without any reference to attributes of their own some pupæ may have been discovered by birds, by moles, hedgehogs, or the like, while others may by pure accident have escaped. The condition of a lepidopterous insect from the egg to its emergence from the chrysalis seems very much like that of the inmates of a town under the infliction of a heavy bombardment. It may perish or it may survive, neither alternative depending so much on its peculiar attributes as on the position which it occupies at some given moment.

From butterflies we pass to birds. In a work containing much with which I am unable to agree, the author argues that it is not the weaker and slower grouse on the Scottish moors which chiefly fall victims to the falcon. The swiftness of this destroyer is so vastly in excess of that of the fleetest grouse, that all relative differences in speed among the latter birds utterly vanish. The strongest winged and most vigorous moorcock, if once espied on the wing by the enemy, has practically no greater chance of escape than a feeble, sickly bird. On the very contrary, the boldest and most energetic grouse, which will be as a rule the healthiest, will fall victims

more frequently than their weaker brethren, from the mere fact that they are more venturesome, and hence more likely to be on the wing. The effects of the co-existence of grouse and falcons in any country, will, therefore, not be so much the development of a strain of the former better adapted for rapid flight, ultimately in the course of many generations endowed with longer or more pointed wings, but merely a thinning of numbers which will tell equally upon the strong and upon the weak, and which in some instances may even give an advantage to the latter.

The argument of the influence of the falcon upon the development of the grouse seems applicable not merely to this individual instance, but to every case where a bird or a beast has to struggle for existence against enemies greatly its superiors in speed, in strength, or in cunning. Slight increments in swiftness or force, trifling improvements in offensive or defensive arms or in means of concealment must, under many circumstances, be absolutely thrown away. Thus there are numbers of cases where preservation and destruction are not necessarily selective.

Nor can we admit that existing species are universally and necessarily ennobled by the "Struggle for Existence." It is well known that when a man is seeking to improve any cultivated plant or domestic animal, his first step is to suppress all struggle for existence, whether with other species or among co-existing individuals of the same species. The gardener plants on a given plot of ground only so many trees, etc., as may find a superabundance of nutrient matter, of air and light. As far as it lies in his power he eliminates all struggle with weeds, or animal competitors. And his results, gathered not by theory, but purely by experience, prove that he is right. Imagine a competitive turnip-field where the plants are left, in vulgar phrase, to "fight it out." The experience of slovenly farmers has proved that such a field will produce neither any fine roots, nor a total average crop equal to that of a field where the struggle for existence has been suppressed. If the weaker individuals finally go to the wall in this struggle, it has first called them into existence.

There is yet a further general consideration to be weighed. Mr. A. R. Wallace in his "Island Life" (p. 55) admits that "new species can only be formed when and where there is room for them." Hence the less severe the struggle for existence, or in other words the less Natural Selection is

brought into play, the more likely are new forms of plants and animals to be evolved.

Dr. H. Behr, speaking of the aboriginal vegetation of California, says:—"Its very variation (*i.e.*, its richness in species) is a proof of a certain want of vitality, for any more vigorous organism by superseding the weaker ones would have produced originally the monotony developed at present by the immigration of alien plants." Here an intensified struggle for existence is held up not as a multiplier but as a reducer of the number of species, as a cause of monotony. If such is its function in our time we may surely demand very good evidence before we admit that it can ever have played the opposite part, and been chiefly or even largely instrumental in producing the present multitude of organic forms from a few original types. We often forget that out of the almost infinite array of animal and vegetable species, a multitude, perhaps the majority, are rare. Now, if it be true that a rare species is one that is verging towards extinction, what are we to infer?

Passing from these general considerations to more specific objections, we often find in animals organs removed from their normal position and placed elsewhere. We generally find the organs of hearing, like those of the other special senses, placed in the head. But in insects the ears, or what stands in their stead, are located differently in different groups. Thus the Orthoptera (locusts, cockroaches, etc.) seem to have ears on their fore-legs. In other groups these organs are supposed to be attached to the subcostal vein of the wings. In the two-winged flies, on the contrary, the power of hearing has been traced to the two little knobs, called by some "balancers" or poisers, which take the place of the hindwings.

We may therefore ask how can the organs of so important a sense have been gradually transported, by Natural Selection, from one of these positions to another? What could be the advantage gained at each successive step? For we must remember that the advocates of Natural Selection tell us that only advantageous changes are likely to be preserved or handed down to posterity.

A most familiar fact in the life-history of insects is the change which most of the so-called orders undergo. On being hatched out from the egg they appear in forms for the most part quite unlike their parents, and it is only by a series of metamorphoses (as they are usually termed), that they

assume the form of maturity. But in one of the most primitive groups, the Orthoptera, there are no such changes. The young cockroach issues from the egg not as a grub or a maggot, but a miniature of the adult insect, from which, indeed, it differs mainly by the absence of wings. But the Orthoptera, and in particular the cockroach group, seem to be among the most ancient forms of insects—indeed, according to some authorities the most ancient of all true insects. It is further supposed that all insects are ultimately descended from the Thysanura. These creatures also do not undergo a metamorphosis. Thus there arises the question how, on the principle of Natural Selection, the metamorphic character in the higher and more recent orders of insects can have arisen? Where has been the advantage, or in other words, how has this change contributed to the preservation and multiplication of the species? We all know that the caterpillar, the grub, or the maggot is more helpless than the insect in its mature form. Its organs of sensation are less developed and its locomotory apparatus is less efficient. We are then almost forced to conclude that insects cannot have become metamorphic by a process of Natural Selection.

Another difficulty is the disappearance of the hind-wings in the Diptera, such as the gnat, the house-fly and their kindred. We find the other orders, both earlier and more recent, provided with the normal four wings, and we do not readily see how, on the principle of Natural Selection, the Diptera should have lost the hinder pair.

Among the vertebrate animals we find similar questions suggested. We take the fore-leg of the lizard and the wing of the bird, and we find each of these limbs useful. But if Natural Selection has gradually modified the one into the other it is hard to conceive how the earliest steps towards developing the leg into the wing could have been of the slightest use to the creature in question. And unless useful, such variations should not, on Darwin's hypothesis, have been reproduced and continued.

Perhaps the most decisive case of the inability of Natural Selection to account for some particular structure is the position of the mouth of the shark. Everyone knows that in fishes or reptiles generally the mouth opens at or very near to the foremost extremity of the body. Take up a herring, a frog, a serpent, or a lizard and imagine how strangely the animal would be inconvenienced in attempting to seize its food, and at what a disadvantage it would be placed in

defending itself against any enemy, if the mouth were made to open not at the front of the head. This, of course, would be especially the case in fishes which do not possess any limbs capable of assisting the mouth. Yet such is the position of the mouth in the shark which, by the way, ranks among the most ancient fishes.

Now can the mouth have conceivably been brought into its present position by Natural Selection? This peculiarity of the mouth, and every step by which it can have been reached, must be and has been a constant disadvantage to the shark. By it he often loses an expected prey, as many a diver and many a sailor who has fallen overboard can testify. Any shark which should have its mouth in the normal position would have the advantage over its rivals in the struggle for existence. Surely, then, we may safely conclude that the peculiar position of the shark's mouth has been reached and is now maintained not in virtue of, but rather in defiance, of Natural Selection.

From the above considerations, and from many more which might be brought forward if time permitted, we may, I submit, venture to conclude that Natural Selection or the struggle for existence is by no means the prime agent in genesis of species. That it may have a subordinate and limited efficacy I am not prepared to deny.

The more we reflect on the subject the more shall we become convinced that the origin of species is a far more difficult and complicated question than it may seem on skimming the writings of Darwin and Wallace, or indulging in the whipped cream of their popular expounders.

We have certainly no proof that Natural Selection is at present multiplying species, or that in existing species it is leading to any higher development. Often, indeed, it seems to work rather in the opposite direction.

That it seems to furnish in many cases a happy explanation we must admit. But in others it leaves us so completely in the lurch that it must be supplemented if not over-ruled by some higher agency.

We must also remember that supposing all the above mentioned difficulties explained away, and the objections set aside, Natural Selection furnishes merely a final cause for the properties of animals and plants. But science is in general more concerned with the efficient causes. Natural Selection may tell us that the colours of an animal approximate to the colours of the objects by which it is surrounded ;

that the exquisite designs of the wings of a male butterfly are elaborated for the sake of attracting the female, etc. This is very well as far as it can be demonstrated. But we rather seek to know how, when, and where, and from what materials the colouring-matters are produced, and how they are conveyed to the parts where we find them deposited?

This, I submit, the doctrine of Natural Selection does not do—does not even attempt to do. Bacon told us that the study of final and ultimate causes corrupted philosophy. How much more must this be the case if everything in the organic world is substantially referred, not to the Divine will, but to accident!

The PRESIDENT (Sir G. G. STOKES, Bart., LL.D., D.Sc., V.P.R.S).—I will now ask you to accord your thanks to Mr. Slater who has favoured us with this Paper and invite your remarks upon it.

Professor E. HULL, LL.D., F.R.S.—I think we are very much indebted to Mr. Slater for the able manner in which he has handled this question. He is one who is thoroughly competent to do so as an authority, and the facts and statements that he has placed before us speak for themselves. I do not pretend to be an authority on the subject; but as a geologist, it is one which I have been obliged to some extent to deal with; and even before this Society I have ventured to bring forward some arguments of a kind analogous to Mr. Slater's, although perhaps less formidable to the development theory than those he has produced this evening. There are many points in the paper which one would like to take up. For instance, I was much impressed with the question of the position of the shark's mouth, to which Mr. Slater has alluded. We might take that as a case of design in position, if so disposed; but I think, perhaps, a Darwinian advocate would reply to Mr. Slater on that point, that the shark is one of the oldest fishes, coming down from the Old Red Sandstone and Upper Silurian. They are heterocercal, and belong to an old type undoubtedly, and the position of the mouth may be due to descent from an ancient type. This only occurs to me by the way, and of course I am not

able to verify it.* Perhaps Mr. Slater will say if I am right or not. If I am right in this view, the Darwinian advocate would perhaps say that the position of the mouth in the shark was only a survival of its progenitors of the Old Red Sandstone order. Well, that is just one point out of many, but I confess I do not see how it is possible to answer some of the arguments that Mr. Slater has adduced.

Now, as regards the survival of the fittest—that is to say, the fittest for its environment;—it will occur to one at once, that one cannot see, on that hypothesis, why there should have been any inhabitant of the ocean of a higher type than, say, the sharks or Placoid or Ganoid fishes. What is the difference in the environment in the ocean of the present day and that of the Tertiary time; or in the character of the ocean now and in the Silurian time? I think it would be very difficult for geologists to assert that there was any difference whatever in the oceanic waters of those ancient geological periods and those of the present day, and we may say of those ancient times that the creatures of those periods were fully adapted to their environment, and there is no cause, as far as I can see, why they should have been modified into other forms in consequence of any change in the environment. The same argument might be adduced in reference to many land animals. Why should there have been any animal higher than, say, the primitive earliest marsupial? To all intents and purposes the surface of the ocean, the air, climate and productions, were as suitable to the animals of those days as they are now. What I mean to assert is, there is no physical reason, as far as one can see, why there should have been any modification in the animal structures to suit any altered conditions of the surface of the land or the atmosphere or waters of the ocean. We might take up many points of this inquiry, and I think we should probably find that we were just as much in the dark as regards the higher races of animals and plants, as time went on, as we were at the beginning.

It seems to me to be almost unreasonable for anyone to assert that the present races of animals and plants can have come into existence by any natural process without the superintending,

* Professor Seeley refers to this subject in his *Manual of Geology* part I, p. 501.

guiding, and controlling intervention of an Almighty Creator; that is a conclusion to which I came a good many years ago, and one which I have never seen any reason to alter to the present day

Professor H. LANGHORNE ORCHARD, M.A., B.Sc.—I think Mr. Slater has abundantly proved that natural selection, according to the Darwinian theory, is subordinate to what Mr. Darwin would call chance, *i.e.*, undesigned coincidence. It appears that natural selection could not work at all until variation is produced; but this variation is not supposed to owe its origin at all to natural or any other selection, but to chance. Then again, after this natural selection has worked, the results of its working will or will not endure according as chance (*i.e.*, according to the Darwinian hypothesis, undesigned coincidence) shall go on. That, I think, is shown fully on pages 62–3; so that really the whole fabric and emphasis of Darwinism reposes upon chance. I think Mr. Slater rather dwelt upon this, that natural selection, even according to its advocates, is subordinate to chance, and would never originate but for *chance* having set up variation, and, having originated, it will or will not endure according as *chance* determines the matter. That, I think, is well shown here. It is very interesting to see that the goat is such a good natural selector, and no one, I suppose, not even the most thorough-going Darwinian, would deny that the goat has a certain amount of will and purpose; now if, in its action of natural selection, the goat works by will, purpose, and intelligence, why should not natural selection work on *other* occasions and through other agents also by will and purpose? Here, in one case at all events, it has done so, and in no case can it be shown that it does not do so, if it exist at all. If, in the case of the goat, there is will, purpose, and intelligence, why should it not appear in other cases? In every case in which we trace the cause or origin it is found to be in design, and it is not philosophical or scientific to assume that in other cases the cause can be unconnected with design.

Rev. A. K. CHERILL, M.A.—Mr. Slater has brought a formidable attack against the theory of natural selection, and it would require a very careful consideration of all the points he has raised, one by one, to see if any answer can be found to them from the point of view of the advocate of natural selection. Perhaps one of his difficulties might be answered: I refer to the

one about the hearing apparatus of insects. It is no doubt difficult to suppose that the hearing apparatus has been turned from place to place in the course of development; but the hearing of insects is in a very rudimentary condition, and I imagine that when the sense of hearing first began to show itself, it would be by some parts of the body becoming more sensitive than other parts to sound waves; and it might be naturally supposed that this beginning would take place in various parts of the body, and that, afterwards, those rudimentary organs of hearing would be improved by natural selection which were in the most convenient place. But with regard to development generally, as has been pointed out this evening, the theory of natural selection seems to found itself on chance—the idea that chance variations take place and that then these chance variations are improved and confirmed when found to be useful. I think a very strong argument might be brought against this. A chance variation would be single, and there is no reason why, when a variation takes place by chance, two or three variations should take place at the same time, having a definite reference to each other. But it can almost always be shown that a mere single chance variation would be no advantage to the creature. Take such a case as the development of the power of flight in a bird. Supposing that a bird was born with a longer wing than the species usually had; if this were merely the result of chance there would be no reason why both wings should be longer—why not one only? But passing this by, and supposing they were both longer, still that would be of no advantage to the bird unless the muscles were also stronger in proportion, and then they would require a stronger attachment, so that it not only requires longer wings, but also a modification in the muscles and breast bone and possibly also in the breathing apparatus, all corresponding together in order that the bird may gain any advantage by it, and it is not credible that all these variations should occur together by mere chance. That opens up a further question upon which I should be glad if anyone would throw any light. It is often said by the advocates of natural selection that acquired variations are not perpetuated. I always ask everyone who knows anything about development, whenever I get the opportunity, what their opinion on that point is; I have asked a good many doctors and men who are expected to know something about it, and have very often received the answer from them that

they do not see any reason why an acquired peculiarity should not be inherited just as much as one that is accidental. Now if we could suppose that acquired faculties are inherited, it would throw a very different light on the theory of development. For example, in the development of the wings of birds; a bird that was very active in its habits would strengthen its own wings, and one might suppose that in the process of reproduction it would be likely that the nourishment should be specially directed towards that part which in the parent bird had been developed by practice, and that the wings of the young should be not only stronger, but also a little bigger. If anything of that kind could be maintained you would get something like a moral law brought into the theory of development—that when the parent exerts itself, and does its best, there might be some improvement in its offspring; and that seems to me to remove a good deal of the objection which is sometimes felt to Evolution as a theory which excludes moral government from the world, showing, perhaps, that it rather lays some sort of foundation for it.

The AUTHOR.—I was glad to hear what Professor Hull said in regard to the peculiar position of the shark's mouth. I have frequently heard it asserted that it was a providential provision to restrain its ravages, but to such assertions I have simply given Darwin's declaration:—"If one instance can be found that any property or peculiarity of an animal is not for its own advantage but for the advantage of its species, I throw my theory up entirely."

A remark was made by Mr. Cherrill which agrees very much with a point that was raised by Professor Fleeming Jenkin, I think it was in the *North British Quarterly*. The article made a considerable sensation and rather staggered Darwin himself. The point was to this effect:—"Suppose a male bird of any species possesses a rather better power of flight than the average of its contemporaries, the probability is that unless it mate with a female bird which always possesses some exceptional advantage, the advantage of the male will, in the course of a couple of generations, be bred out." We must have at least two individuals possessing a variation in some favourable direction, if we are to have a new and improved breed. Permit me in conclusion to express my thanks for the kind manner in which my paper has been received.

The meeting then adjourned.

REMARKS ON THE FOREGOING PAPER.

Dr. D. BIDDLE, M.R.C.S., writes :—

In regard to the compatibility of (what looks like) chance with design, some very able remarks have been made by the author of *Ednor Whitlock*, Mr. Hugh MacColl. He shows by a mathematically conducted process of dotting paper within prescribed limits, that pre-ordained patterns can be produced with unerring precision, and even the shading be arranged, although the utmost licence be allowed to "chance"—within those limits.

But it has always appeared to me that the weak point in the theory of evolution is the making time a cause of change. Natural selection is admitted to be unavailing to produce new species within the period allowed to any single observer, and some go so far as to admit that the formation of new species by evolutionary methods must be regarded as pre-historical. But everything is possible, say they, if time be given. This is a delusion. It has lately been asserted that Sir G. B. Airy tossed pennies with a friend for a week, in order to find the longest run of heads (or tails) obtainable in that period, and 28 was the longest. But by the generally-accepted laws of probability, if time were allowed, a run of a million would occur, and there is nothing to prevent its occurrence early in the tossing. Common-sense, however, avers that a run of one hundred would make us doubt whether a fair penny was being fairly tossed. The law is said to be that, however often one face has turned up in succession, the chance is half, or absolutely equal, for the next toss; and yet it is affirmed that there is a constant tendency to equalisation, which should make the chance favour the other face, after a run on the former. This only shows how careful we should be in accepting the dicta of theorisers.

A theory which depends on chance-variations, occurring at stupendous intervals of time, and of which no trustworthy instance can be produced before our eyes, is doomed to failure, and must ere long be laughed out of court. It is eminently unscientific, for it believes in the production of an effect without the prior action of any proper cause.

Dr. H. B. GUPPY writes :—

Mr. Slater makes several very good points in his criticism of the theory of natural selection, and I think most people nowadays

are beginning to perceive that this is too difficult and complicated a question to be decided either in this generation, or, in fact, in any future generation without a far greater use of the methods of observation and experiment than has hitherto been made. As he remarks, the theory does not touch some of the simplest of phenomena in the world around us. Granted for the moment that in natural selection we have an explanation of the origin of a species, I cannot see that that lands us very much on our way; the practical knowledge of the cattle-breeder, the pigeon-fancier, and the horticulturist, in pre-Darwinian times carried them nearly as far, only that they did not formulate a theory of the universe on those grounds.

Some reference is made by Mr. Slater to the extent in which indigenous plants have been often exterminated by introduced species, but we must also remember the destruction, far more extensive, as I think, of would-be intruders into the domains of previously established species, and the modification of others. For nearly two years I have been making observations on the dispersal of water-plants and marsh-plants, and for a long time I imagined that the problem to be solved might be briefly thus stated:—"Given the distribution and capacity for dispersal of a plant, to explain its distribution," but I gradually came to see that another postulate was required. Take, for instance, the case of our common marsh-plant, *Bidens cernua*. It is rarely that one finds in the same plant to the same degree equal capacities for dispersal by the different agencies of the currents, birds, etc., etc. The achenes can float for months in sea-water and yet germinate; they float all the winter through in our rivers, such as the Lea, and must be transported in great numbers annually to the sea, when they commence their ocean voyage. The reflexed prickles of the achenes eminently fit them also for transportal in birds' plumage, for which they are as well adapted as the fruits of *Galium aparine*, and I cannot doubt but that birds such as duck, teal, &c., are very important agents in the dispersal of this plant. Yet with all these means of dispersal, this plant, though diffused widely in the temperate and northern regions of Europe, Asia, and America, is not to be found in the tropics. I do not doubt for a moment but that the achenes of this plant have been transported to almost every corner of the globe a thousand times over, and yet the species is not to be found in the tropics. And why not? Either the conditions there are antagonistic, or else it has sported in its new home into varieties that owe their permanence to their surroundings, and so we call them "species." The genus, as we learn from Bentham's and Hooker's handbook, is not very numerous in species and is diffused over the whole globe, occurring even in the Arctic Circle.

We thus perceive that the absence of a plant in a particular region may be by no means due to its inability to get there. We

must know "how it behaves under its new conditions" when it is there, and this is the postulate required for the complete statement of the problem. In this manner we open up *an illimitable field for experiment and observation*. A *Bidens cernua* cultivated for a series of generations in the tropics might tell a rather strange story concerning the antecedents of the species already established in that region.

Mr. JOSEPH JOHN MURPHY writes:—

I wish to offer a few observations on Mr. Slater's paper on "The weak sides of Natural Selection."

I agree with his main conclusion, which I understand to be that although natural selection is an agent in the origin of species, it is by no means the sole or the chief agent; but in some ways he seems unjust to the theory of natural selection, by demanding that it should explain what in the nature of things it cannot explain.

He says:—"Suppose a pair of animals in the primeval world had produced a hundred fertile ova. The young animals springing from these ova must either be one and all exactly alike, or they must exhibit certain differences. In the former alternative there is no ground for natural selection to work upon; the very idea of selection implying differences in the objects among which a selection is to be made. In the second alternative, the varieties (he means *variations*) being, by hypothesis, antecedent to selection, cannot be its effects. Hence, in either case, we have something which the Darwinian theory is quite unable to account for." This is perfectly true, and perfectly irrelevant. It is like objecting to the Newtonian theory of the planetary motions that it does not account for gravitation; an objection which, I believe, was actually made in Newton's time. Every theory, except in pure logic and mathematics (and I am not sure that geometry ought to be excepted) must postulate facts—and not only particular facts but general truths—without being able to account for them. The next observation, that "before we can understand the origin of species, we want a law which shall go deeper than natural selection," is as true and as luminous as if he had said "we want a law which shall go deeper than gravitation before we can understand the motions of the planets." To such objections it is enough to reply that gravitation is ultimate in astronomy, and spontaneous variation ultimate in morphology and evolution.

In another passage, Mr. Slater appears to have not only mistaken the logic, but the meaning, of the question under discussion. He says Mr. Wallace "admits that the most effective agent in the extinction of species is the pressure of other species, whether as enemies or simply as competitors—a distinction, I must remark, without a difference." No difference between enemies and competitors! If sheep were exterminated in one country through

being devoured by wolves, and in another through being deprived of pasture by the competition of goats, the difference would be important from a naturalist's point of view.

When the Darwinian (I say this without being myself a Darwinian) is assailed with a volley of questions, "Can your theory account for this, or for that?" it is generally wisest to reply "No, we are human, and do not profess to account for everything." When Mr. Slater asks "why do we never see in any vertebrate animal more than two pairs of limbs, or their rudiments? Why are parts that have lost their function, such as the external ear in mankind, or the vermiform appendage to the cæcum, still produced in generation after generation?" it is a sufficient answer to say that we have no means of measuring the force of heredity, which tends to the preservation of such organs, against the forces which tend to their disappearance; but the Darwinian, or any other, theory of evolution must take account of the existence of both. And when he goes on to ask "why is the secretion of silk confined to invertebrate animals, and the production of physiological venoms to cold-blooded ones?" he is propounding questions far more difficult than if he were to ask why certain crystalline forms are correlated with certain chemical properties; yet, so far as I am aware, the first step has not yet been taken in the explanation of such correlations in the inorganic world. The same applies to his concluding difficulty. "Natural selection may tell us that the colours of an animal approximate to the colours of the objects by which it is surrounded. This is very well as far as it can be demonstrated, but we rather seek to know how, when, where, and from what materials the colouring matters are produced." This is as reasonable as if he were to see a shipyard with machines of magnificent power and precision for forging steel, and then complain because he was informed by his guide that the chemistry of steel is very imperfectly understood.

I will conclude my reply to Mr. Slater with the consideration of what appears to me a purely imaginary difficulty, though I am aware that it has been strongly insisted on. I mean the position of the mouth in the shark, which is on the under side of the fish, some way back from the snout, instead of at the snout, as in some allied fishes. Mr. Slater says, "This peculiarity of the mouth must be a constant disadvantage to the shark. By it he often loses an expected prey, as many a diver and many a sailor who has fallen overboard can testify." This has been constantly repeated, and yet a little reflection will show its untenability. If a diver or a half-drowned sailor seriously asserts that he was saved from being devoured by a shark because the shark lost the imperceptible fraction of a second which he required to swim through the distance between his snout and his mouth, I cannot credit it.*

* The position of the mouth may be fitted for his usual prey, but the shark having to turn to seize *a man*, gives the latter an advantage.—Ed.

Another form of the statement is, if possible, yet more untenable. It has been said that the shark loses time in seizing his prey through the necessity of turning on his side. I think this must depend on the position of the prey; but if it is always necessary, what fraction of a second will this movement require? and cannot one of the swiftest swimmers in the sea turn his body half way round while swimming, so as to lose no time at all?

I have endeavoured to reply to Mr. Slater where I think him wrong, and especially to demolish his shark; but in many things I agree with him, especially as to the inadequacy of natural selection to account for the metamorphoses of insects, which appear to point to some unexplained law of life; and also its inadequacy to explain the very remarkable fact of the existence of closely parallel, though but distantly related, forms in the placental and the marsupial sub-classes of the mammalia. I believe that no theory of evolution can explain away the necessity of a Guiding Intelligence. My work on *Habit and Intelligence* contains my detailed views on this subject.

Mr. F. P. PASCOE, F.L.S., ex-President of the Entomological Society, writes:—

Many thanks for the proof copy of Mr. Slater's paper.

"Natural selection" is such a convenient phrase for our real ignorance that it will probably be long before it is discarded.

A power "picking out with unerring skill" seems to me to be utterly inadequate to account for the formation of new organs—some apparently useless as, for example, the comb-like organs of the scorpions. It makes no attempt to account for the numerous forms of the Protozoa—perhaps the most extraordinary beings in all organic nature.

"The proof that there is a selective agency at work is," Mr. Wallace thinks, "to be found in the stability of species." (*Nature*, Oct. 1, 1891.)

I have elsewhere remarked (in my *Summary of the Darwinian Theory*) that Darwin, with the conspicuous candour that distinguished him, was ever ready to admit—and in the strongest terms—what he considered were objections to his theory. Some he thought at first were "insuperable," such as the absence of the infinitely many fine transitional forms which must have existed; others—as the neuter ants—"fatal to the whole theory." That the eye could have been formed by natural selection "seemed absurd in the highest degree." Instincts, too, were so wonderful that they might appear sufficient "to overthrow the whole theory."

Some of these difficulties were "so serious that to this day he could hardly reflect on them without being in some degree staggered." But he says the more important of the objections to his theory "relate to questions on which we are confessedly ignorant; nor do we know how ignorant we are."

Darwin adds that the absence of "the infinitely many fine gradations between past and present species required on the 'theory,' is the most obvious of the many objections which may be urged against it." This he attributes to the imperfection of the geological record. Perhaps he has relied too much on the dogma, *natura non facit saltum*.

Dr. GERARD SMITH, M.R.C.S., writes:—

The Paper is a very important one, it is very desirable that biologists should be very accurate in their formulation of the facts of "natural selection," at present it is spoken of as if it were a *cause*, whereas the expression "natural selection" only really forms a convenient heading under which to group the results of observations upon the gradual modification of organisms; the way in which *variation* is used. So far as we have gone, variation is creation, for the power of variation must either be a production *de novo* of organs; or it must be the results of originally implanted potentialities in the protoplasm; I have heard and seen much of *rudimentary*, *i.e.*, degenerated organs, but fail to learn much about *nascent* organs; everywhere there are structures which must be *complete*, or *nothing*, that is, if the theory of natural selection as a *cause* is to hold; a nascent and as yet useless organ has a meaning if one believes in an *implanted* potentiality towards a certain grade of perfection or differentiation. So far as I can learn, on the purely materialistic conception, I am expected to put my faith in a process which is the result of a *previously existing* (but not *foreseen* or *implanted*) potentiality for variation in a useful direction producing variations having at first no relation to their environment, but subsequently made *useful*, though *useless* at first, by *use*; in preparation for a future more complete utility, which is not foreseen or expected! This is rather a hard creed I find.

The Rev. F. A. WALKER, D.D., F.L.S., writes:—

All arguments respecting the "struggle for existence" should, I venture to think, be stated in reference to some particular climate and country, and to its Fauna, which, whether consisting of insects, or other forms of organic life as well, happen to serve as the subject under discussion. It is obviously impossible to arrive at any world-wide generalization on this topic, because climatic influences which in the steaming tropics act with astonishing rapidity and productive power on all forms of living beings, prove actually the retarding, not to say destructive agents in respect of all except the very hardiest species in the frozen north. While *vice versâ*, arctic regions are singularly free from, and in many instances, altogether without the noxious creatures, and animals of prey that are constantly occupied in diminishing the numbers of their weaker and more defenceless brethren. By far the most manifold forms of life

have their beginning in regions of tropical heat which generate alike multitudes of harmless creatures, and at the same time give birth and development to their numerous natural destroyers, a counterpoise to the too rapid or excessive preponderance of any individual species being thereby effected. The parasitic lianas, and other creepers which surround with their deadly embrace the towering forest tree, and by degrees strangling all vitality in their supporter, hasten on its decay, and ultimately themselves come to an end together with the fall of the dead trunk, giant serpents, huge and venomous spiders, centipedes, and scorpions, etc. These are altogether wanting in temperate regions of our globe; and in Iceland no reptile of any description is to be met with, the most common of our small British centipedes occurs very rarely, while the circumstance of the Arachnida only comprising ground spiders, and very few (and I am not certain that there are any at all there) that construct webs, tends to numbers of flies and moths that would otherwise come to an end, being preserved.

Climate and isolation are the two factors we have to take account of in a review of the "struggle for existence" in "Ultima Thule." To take the second of these two circumstances first, its isolation at a distance of 500 miles from the north coast of Scotland, renders the chance of any new species of insect visiting its lonely wastes, almost, if not altogether, an impossibility. Supposing, for argument's sake that during the short island summer of 10 or 12 weeks an insect was imported by the periodical voyage of the Danish steamer, having settled on the vessel before it left the port of Copenhagen (as a solitary Painted Lady *V. Cardui*) was reported on reliable authority to have been seen in Shore Street, Reyhjavik, in the summer of 1888) the chances of its perpetuation and continuance are even more infinitesimal than those of its arrival. The food plants of the larvæ of most of our common butterflies either do not occur at all in Iceland, as for example the oak and the elm, or are very rare and local as the nettle and thistle, or are very scantily cultivated, as the cabbage and turnip. Any English species of butterfly moreover would be seriously, if not altogether handicapped in the struggle for existence in consequence of the fact that as all the so-called Icelandic forest consists of dwarf scrub, willow and birch, there is no hollow tree trunk wherein the imago can safely hibernate, or sheltered place whereon the pupa can hang up during the inclement weather. The actual severity of the climate, which, by the way, varies considerably in different parts of the Island, is not the only enemy to be reckoned with, but the fact of unavoidable exposure to its storms of wind, rain, and snow as well. The larvæ of several moths on the contrary, which occur in Iceland seek a refuge under ground preparatory to undergoing their change into the pupa state, and are thereby preserved from any ill-effects consequent on

"The dreadful pother o'er their heads."

The isolation above mentioned has not, as might at first be supposed, tended to produce a genesis of species peculiar to the island, so far as I have been able to ascertain after a thrice repeated visit to the greater number of the Icelandic fjords. Interesting local types and varieties of certain of the Icelandic moths beyond all question do exist, but they are either such as are also found at Rannoch or elsewhere in Scotland, or where not occurring in Scotland, are at all events represented by precisely the same forms in other parts of Scandinavia, in Finland, for example. Whether the Flora and Fauna of Iceland be compared with those of the Faroes and of Scotland on the one hand, or with those of Norway, Sweden, Lapland etc., on the other, Iceland in either case will be found to possess quite the lowest number of species of any of the aforesaid regions. The great scarcity of land birds as contrasted with aquatic ditto in Iceland *may* serve to account for the astonishing number of individuals of certain species of geometridæ which are thus marvellously aided in their struggle for existence. The vast quantities of offal and refuse of fish that lie scattered on the shores of every fjord beyond all doubt tend to the perpetuation in portentous numbers of such species of Diptera as habitually derive their subsistence from garbage, while the prevalence of the Arctic Tern, as delighting in similar food, is referable to the same cause. On the contrary, how are we to account for the fact that Ichneumonidæ are very few and far between, except by the circumstance that Diurnal Lepidoptera being wholly wanting, there are no chrysalids there for them to deposit their eggs in, as with ourselves? Or again, why is there only one species of humble bee in the whole of Iceland, and why is that so rare (for I believe I was the first to report it at all from the N. and E. sides of the island) except that some of the flowers in which the insect delights, as the blossom of the lime, are incapable of being cultivated in Iceland, and no pains whatever has been taken to plant others, as the broad bean and the clover, which last plant shows a straggling blossom here and there of both red and white varieties, solely from its seed having been accidentally introduced along with grass seed from another land. Here in the struggle for existence the perpetuation of the particular insect and plant is maintained indeed, but with difficulty, and in scanty proportions, and very locally. The utility of bees in hybridising clover is so well-known, that if a live batch were introduced into Iceland just as several have ere this into New Zealand, fragrant plant and winged bee might act and react on each other beneficially were it not for the utter want of enterprise and industry displayed by the Icelanders. In conclusion, with regard to Diptera once more, genus *Eristalis* occurs in the Faroes but not in Iceland, and I was told by a noted British entomologist, that if I wanted to find *Eristalis* in Iceland, I had only to run a drain there, but for all that one species of genus *Helophilus* is found in Iceland, and that genus both there and at home delights fully as much in the neighbourhood of drains

on a sunshiny day as that of *Eristalis* itself. May we not rather regard the absence of *Eristalis* to be consequent on the scarcity or total want in Iceland of the cabbage, on which plant I have captured that tribe in the Faroes, as well as of the thistle, on which blossom I ordinarily take it at home ?

“The colours of an animal approximate to the colours of the objects by which it is surrounded.” P. 66.

This is most true in respect of all organic nature and is corroborated by sundry instances in beasts, birds, reptiles, insects, etc. But when the lecturer proceeds to state “But we rather seek to know how, when, and where, and from what materials the colouring-matters are produced, and how they are conveyed to the parts where we find them deposited ?” It is difficult to give a satisfactory answer, various are the hypotheses adduced. The green of many kinds of caterpillar from its similarity to that of the stalk or leaf that holds the insect affords one of the commonest and best known examples of the approximation above mentioned. Some have it that the insect is indebted for its colour owing to its constantly eyeing the verdure by which it is surrounded. (1) But effects produced by eyeing external objects are confined to creatures in a state of parturition. (2) Also all insects in the larva stage are incapable of reproduction. (3) And the green colour is common to both sexes of the larva. Others hold that the green of the caterpillar is occasioned by the creature’s absorption of the colouring matter through its pores, and others that it is caused by the creature’s constantly devouring the “chlorophyll” or colouring matter of the leaf, which forms its habitual food. But these are serious, if not fatal objections to both of these last two theories. In the first place perfect insects (moths for example) as closely resemble surrounding rocks in many cases as caterpillars do leaves. Now the rocks obviously cannot afford them nutriment, and further, a moth could not possibly imbibe by means of the pores, nor is its proboscis capable of the same work as the jaws of the caterpillar. Also the effect produced by feeding different individuals of the same kind of caterpillar on two different kinds of leaves or on two differently coloured leaves (dark green and light green for example) respectively does not appear to have any result as regards the next generation of *caterpillars*, while on the other hand a corresponding variation has been noted in the *moths* which are yellowish or white respectively after an indefinite period—say three or four seasons of the larvæ being so fed. Then again, leaves such as the larvæ feed on, are not the only objects that the larvæ resemble. Other larvæ of a dark brown tint are quite as undistinguishable from a crooked stick or twig. It is hardly possible to tell the difference between a common oak moth (*Tortrix Viridana*) when settled on a tree trunk from a small patch of pale green lichen, or again, another common *Tortrix* in a similar situation, from bird lime. Probably no one besides myself has collected the mountain geometra (*larentia*

caesiata) alike in the western highlands, and also in the S.W. of Iceland. It is worth while to compare the two series in question. The marbled appearance of the Scotch specimens so closely resembling their limestone or schistose rocks, and the dingy or grimy appearance of the Icelandic ditto enabling them to lie *perdu* on their native lava. While fully conceding that these instances of similarity in insects (of which a hundred more examples might be given) to the vegetable and the mineral world are ordered by Providence as a safeguard against total or partial destruction by their natural foes, I think we must be content to suspend our judgment as to the particular agency by which this wondrous similarity is effected.

“Genesis of Species,” p. 58. I am not personally quite certain whether I thoroughly understand this term. By exposure of larva or pupa to greater heat or more cold, or by feeding the larva on a different food plant from that which it frequents in a state of nature, we may obtain moths of different colour and markings, and by breeding again from these and repeating the same experiments through several successive seasons, we may perpetuate these superficial distinctions, but can we so permanently perpetuate them during the time we keep and register our observations of each successive brood or during our own lifetime as to render it certain that the insects, if restored to liberty and to their original food plants, would not shortly or at any rate by degrees hark back to their former type. The ultimate test of two true species is inability to pair with one another, or at least of reproduction in a third generation, just as the ultimate test of two genera is diversity of structure. Difference of colour, size, markings, may frequently be noticed in the case of two really different species, but these are not invariable nor final tests, either of two different species, or two different genera of butterflies, and even the two sexes of the same insect are often far from presenting the same striking difference to those of another tribe.

THE AUTHOR'S FURTHER REPLY.

Dr. BIDDLE's remarks on the compatibility of chance (apparent) with design deserves serious attention. It may be mentioned that in the opinion of some authorities—undemonstrated, I must admit, and probably undemonstrable—the formation of new species or even varieties is at an end. We no longer witness the origin of new well-marked varieties of mankind, save by the mixture of races which already exist. Perhaps the isolation needed for this end is no longer existent. But the development of the European, the negro, and the Mongol from the original human stock—intermixture being impossible—seems to present a problem of the same nature as the origin of the tiger, the leopard and the jaguar from one common feline stock.

Mr. GUPPY's studies on the distribution of aquatic and marsh plants are of very high value as the type of a class of researches which ought to be extensively followed up. They are likely to throw useful cross-lights on all theories concerning the origin of species.

An interesting fact is the career of the Canadian water-weed (*Anacharis*, or *Elodea*?). Some years back it was spreading with alarming speed in our rivers and inland navigations. Suddenly it has ceased to multiply and has even died out in very many cases. No known cause has been ascertained.

Another interesting fact is the spread of the periwinkle. It is asserted by horticulturists and botanists not to ripen its seed in England. Yet we find it growing and spreading in woods where it cannot have straggled away from gardens, and where certainly no one can have taken the trouble to plant it.

Mr. J. J. MURPHY's remarks call for some reply, in fact they make me fear that I have not explained my views with sufficient distinctness.

The objection that Natural Selection cannot be accepted as the prime cause of the genesis of species seems to me, as to not a few abler men, simply fatal to Darwinism, and is not to be disposed of by the scarcely relevant illustration drawn from the Newtonian theory of planetary movements. I do not reject Natural Selection because I do not know its origin, but because it fails to account for the phenomena. Now, Mr. Murphy's objection to Newton merely raises the question of the *origin* of gravitation, not urging that it fails to account for the planetary movements. Hence between the cases there is no parallelism and Mr. Murphy's illustration does not apply.

As regards the "volley of questions" with which the Darwinian is assailed we all know that any theory must stand or fall according to the questions it can solve. If Nature—I dislike the term—has been for millions of years ever striving to improve plants and animals, preserving only modifications which are useful to them and cancelling every step in a different direction, it might be expected that the peculiarities which I pointed out would have been on the way to extinction. Darwinism says that every useless feature in an animal is a drain on its resources by which it must be *pro tanto* handicapped in the "struggle for existence." The illustration from the ship-building yard seems to me singularly unhappy. The uses to which iron and steel are there put are purely *mechanical*, and the question raised by the supposed visitor as to its *chemical* constitution is therefore irrelevant. But the difficulties which I have ventured to point out are of the very essence of the question.

The position of the shark's mouth is undeniable, and all evidence agrees to show that it is an inconvenience. Granted that the loss of time to the shark is small, yet the delay of a second may turn the scale between life or death. Unless the peculiarity of the shark's mouth is a gain to this fish it ought not, on the principle of Natural Selection, to have been preserved.

After careful inquiry made both before drawing up my paper and subsequently, all the observations I have been able to meet with agree with the view that the position of the shark's mouth is and must be a disadvantage.

The distinction between an enemy and a competitor is, in the instance given, purely *nil*, as far as the species attacked is concerned.

Mr. Murphy, in contending that this difference is something real, forgets that to the sheep it makes no ultimate difference whether it is devoured by wolves or starved from want of food. Nor does it differ substantially from the naturalist's point of view, since one and the same end is effected though in another manner.

The Rev. Dr. WALKER'S critique is most valuable. He regards the question—or questions—with the eye of a practical observant naturalist and points out some of the many difficulties to be encountered in explaining, *e.g.*, the colouration of insects.

It is, indeed, possible that the chlorophyll of green vegetables may take a part in the colouration of butterflies. But we have to ask why are green colours wanting in other species which select the same diet?

I have succeeded in detecting tannin in many insects—all plant-feeders—and I think this fact may explain the frequent occurrence of browns, russets, tans, &c., both in Lepidoptera and Coleoptera. But before we can generalise we must acquire a