We may now proceed to consider certain misconceptions of the Darwinian theory which are largely, not to say generally, prevalent among supporters of the theory. These misconceptions, therefore, differ from those which fall to be considered in the next chapter, i. e. misconceptions which constitute grounds of objection to the theory.

Of all the errors connccted with the theory of natural selection, perhaps the one most frequently met with—especially among supporters of the theory—is that of employing the theory to explain all cases of phyletic modification (or inherited change of type) indiscriminately, without waiting to consider whether in particular cases its application is so much as logically possible. The term "natural selection" thus becomes a magic word, or Sesame, at the utterance of which every closed door is supposed to be immediately opened. Be it observed, I am not here alluding to that merely blind faith in natural selection, which of late years has begun dogmatically to force this principle as the sole cause of organic evolution in every case where it is *logically possible* that the principle can have come into play. Such a blind faith, indeed, I hold to be highly inimical, not only to the progress of biological science, but even to the true interests of the natural selection theory itself. As to this I shall have a good deal to say in the next volume. Here, however, the point is, that the theory in question is often invoked in cases where it is not even logically possible that it can apply, and therefore in cases where its application betokens, not merely an error of judgment or extravagance of dogmatism, but a fallacy of reasoning in the nature of a logical contradiction. Almost any number of examples might be given; but one will suffice to illustrate what is meant. And I choose it from the writings of one of the authors of the selection theory itself, in order to show how easy it is to be cheated by this mere juggling with a phrase—for of course I do not doubt that a moment's thought would have shown the writer the untenability of his statement.

In his most recent work Mr. Wallace advances an interesting hypothesis to the effect that differences of colour between allied species, which are apparently too slight to serve any other purpose, may act as "recognition marks," whereby the opposite sexes are enabled at once to distinguish between members of their own and of closely resembling species. Of course this hypothesis can only apply to the higher animals; but the point here is that, supposing it to hold for them, Mr. Wallace proceeds to argue thus:—Recognition marks "have in all probability been acquired in the process of differentiation for the purpose of checking the intercrossing of allied forms," because "one of the first needs of a new species would be to keep separate from its nearest allies, and this could be more readily done by some easily seen external mark[1]." Now, it is clearly not so much as logically possible that these recognition-marks (supposing them to be such) can have been acquired by natural selection, "for the purpose of checking intercrossing of allied forms." For the theory of natural selection, from its own essential nature as a theory, is logically exclusive of the supposition that survival of the fittest ever provides changes in anticipation of future uses. Or, otherwise stated, it involves a contradiction of the theory itself to say that the colour-changes in question were originated by natural selection, in order to meet "one of the *first* needs of a *new* species," or for the purpose of *subsequently* preventing intercrossing with allied forms. If it had been said that these colour-differentiations were originated by some cause other than natural selection (or, if by natural selection, still with regard to some *previous*, instead of *prophetic*, "purpose"), and, when so "acquired," *then* began to serve the "purpose" assigned,

[1] *Darwinism*, pp. 218 and 227.

the argument would not have involved the fallacy which we are now considering. But, as it stands, the argument reverts to the teleology of pre-Darwinian days—or the hypothesis of a "purpose" in the literal sense which sees the end from the beginning, instead of a "purpose" in the metaphorical sense of an adaptation that is evolved by the very modifications which subserve it [1].

Another very prevalent, and more deliberate, fallacy connected with the theory of natural selection is, *that it follows deductively from the theory itself* that the principle of natural selection must be the sole means of modification in all cases where modification is of an *adaptive* kind,—with the consequence that no other principle can ever have been concerned in the production of structures or instincts which are of any use to their possessors. Whether or not natural selection actually has been the sole means of adaptive modification in the race, as distinguished from the individual, is a question of biological fact [2]; but it

[1] Since the above was written Prof. Lloyd Morgan has published a closely similar notice of the passage in question. "This language," he says, "seems to savour of teleology (that pitfall of the evolutionist). The cart is put before the horse. The recognition-marks were, I believe, not produced to prevent intercrossing, but intercrossing has been prevented because of preferential mating between individuals possessing special recognition-marks. To miss this point is to miss an important segregation-factor."—(*Animal Life and Intelligence*, p. 103.) Again, on pp. 184-9, he furnishes an excellent discussion on the whole subject of the fallacy alluded to in the text, and gives illustrative quotations from other prominent Darwinians. I should like to add that Darwin himself has nowhere fallen into this, or any of the other fallacies, which are mentioned in the text.

[2] Of course adaptive modifications produced in the individual lifetime, and not *inherited*, do not concern the question at all. In this and

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involves a grave error of reasoning to suppose that this question can be answered deductively from the theory of natural selection itself, as I shall show at some length in the next volume.

A still more extravagant, and a still more unaccountable fallacy is the one which represents it as following deductively from the theory of natural selection itself, that all *hereditary* characters are "necessarily" due to natural selection. In other words, not only all adaptive, but likewise all non-adaptive hereditary characters, it is said, *must* be due to natural selection. For non-adaptive characters are taken to be due to "correlation of growth," in connexion with some of the adaptive ones—natural selection being thus the *indirect* means of producing the former *wherever* they may occur, on account of its being the *direct* and the *only* means of producing the latter. Thus it is deduced from the theory of natural selection itself,—1st, that the principle of natural selection is the only possible cause of adaptive modification: 2nd, that non-adaptive modifications can only occur in the race as correlated appendages to the adaptive: 3rd, that, consequently, natural selection is the only possible cause of modification, whether adaptive or non-adaptive. Here again, therefore, we must observe that none of these sweeping generalizations can possibly be justified by deductive reasoning from the theory of natural selection itself. Any attempt at such deductive reasoning must necessarily end in circular reasoning, as I shall likewise show in the

the following paragraphs, therefore, "adaptations," "adaptive modifications," &c., refer exclusively to such as are hereditary, i.e. phyletic.

second volume, where this whole "question of utility" will be thoroughly dealt with.

Once more, there is an important oversight very generally committed by the followers of Darwin. For even those who avoid the fallacies above mentioned often fail to perceive, that natural selection can only begin to operate if the *degree* of adaptation is already given as sufficiently high to count for something in the struggle for *existence*. Any adaptations which fall below this level of importance cannot possibly have been produced by survival of the fittest. Yet the followers of Darwin habitually speak of adaptative characters, which *in their own opinion* are subservient merely to comfort or convenience, as having been produced by such means. Clearly this is illogical; for it belongs to the essence of Darwin's theory to suppose, that natural selection can have no jurisdiction beyond the line where structures or instincts already present a sufficient degree of adaptational value to increase, in some measure, the expectation of life on the part of their possessors. We cannot speak of adaptations as due to natural selection, without thereby affirming that they present what I have elsewhere termed a "selection value."

Lastly, as a mere matter of logical definition, it is well-nigh self-evident that the theory of natural selection is a theory of the origin, and cumulative development, of *adaptations*, whether these be distinctive of species, or of genera, orders, families, classes, and sub-kingdoms. It is only when the adaptations happen to be distinctive of the first (or lowest) of these taxonomic divisions, that the theory which accounts for *these* adaptations accounts also for the forms which present them,—i. e. becomes *also* a theory of the origin of *species*. This, however, is clearly but an accident of particular cases; and, therefore, even in them the theory is *primarily* a theory of adaptations, while it is but *secondarily* a theory of the species which present them. Or, otherwise stated, the theory is no more a theory of the origin of species than it is of the origin of genera, families, and the rest; while, on the other hand, it is *everywhere* a theory of the adaptive modifications whereby each of these taxonomic divisions has been differentiated as such. Yet, sufficiently obvious as the accuracy of this definition must appear to any one who dispassionately considers it, several naturalists of high standing have denounced it in violent terms. I shall therefore have to recur to the subject at somewhat greater length hereafter. At present it is enough merely to mention the matter, as furnishing another and a curious illustration of the not infrequent weakness of logical perception on the part of minds well gifted with the faculty of observation. It may be added, however, that the definition in question is in no way hostile to the one which is virtually given by Darwin in the title of his great work. *The Origin of Species by means of Natural Selection* is beyond doubt the best title that could have been given, because at the time when the work was published the *fact*, no less than the *method*, of organic evolution had to be established; and hence the most important thing to be done at that time was to prove the transmutation of species. But now that this has been done to the satisfaction of naturalists in general, it is, as I have said, curious to find some of them denouncing a wider definition of the principle of natural selection, merely because the narrower (or included) definition is invested with the charm of verbal associations[1].

So much for fallacies and misconceptions touching Darwin's theory, which are but too frequently met with in the writings of its supporters. We must now pass on to mention some of the still greater fallacies and misconceptions which are prevalent in the writings of its opponents.

[1] The question as to whether natural selection has been the only principle concerned in the origination of species, is quite distinct from that as to the accuracy of the above definition.

because it is the one which has been published most recently, and partly because it is of particular interest as occurring so low down in the zoological scale. I am indebted to the kindness of Mr. and Mrs. Peckham for permission to reproduce these few selected drawings from their very admirable work, which is published by the Natural History Society of Wisconsin, U.S. It is evident at a glance that all these elaborate, and to our eyes ludicrous, performances are more suggestive of incitation than of any other imaginable purpose. And this view of the matter is strongly corroborated by the fact that it is the most brightly coloured parts of the male spiders which are most obtruded upon the notice of the female by these peculiar attitudes--in just the same way as is invariably the case in the analogous phenomena of courtship among birds, insects, &c.

But so great is the mass of material which Darwin has collected in proof of all the points mentioned in the foregoing paragraph, that to attempt anything in the way of an epitome would really be to damage its evidential force. Therefore I deem it best simply to refer to it as it stands in his *Descent of Man*, concluding, as he concludes,—"This surprising uniformity in the laws regulating the differences between the sexes in so many and such widely separated classes is intelligible if we admit the action throughout all the higher divisions of the animal kingdom of one common cause, namely, sexual selection"; while, as he might well have added, it is difficult to imagine that all the large classes of facts which an admission of this common cause serves to explain, can ever admit of being rendered intelligible by any other theory.

We may next proceed to consider the objections which have been brought against the theory of sexual selection. And this is virtually the same thing as saying that we may now consider Mr. Wallace's views upon the subject.

Reserving for subsequent consideration the most general of these objections—namely, that at best the theory can only apply to the more intelligent animals, and so must necessarily fail to explain the phenomena of beauty in the less intelligent, or in the non-intelligent, as well as in all species of plants—we may take *seriatim* the other objections which, in the opinion of Mr. Wallace, are sufficient to dispose of the theory even as regards the higher animals.

In the first place, he argues that the principal cause of the greater brilliancy of male animals in general, and of male birds in particular, is that they do not so much stand in need of protection arising from concealment as is the case with their respective females. Consequently natural selection is not so active in repressing brilliancy of colour in the males, or, which amounts to the same thing, is more active in "repressing in the female those bright colours which are normally produced in both sexes by general laws."

Next, he argues that not only does natural selection thus exercise a negative influence in passively permitting more heightened colour to appear in the males, but even exercises a positive influence in actively promoting its development in the males, while, at the same time, actively repressing its appearance in the females. For heightened colour, he says, is correlated with health and vigour; and as there can be no doubt that healthy and vigorous birds best provide for their young, natural selection, by always placing its premium on health and vigour in the males, thus also incidentally promotes, through correlated growth, their superior coloration.

Again, with regard to the display which is practised by male birds, and which constitutes the strongest of all Mr. Darwin's arguments in favour of sexual selection, Mr. Wallace points out that there is no evidence of the females being in any way affected thereby. On the other hand, he argues that this display may be due merely to general excitement; and he lays stress upon the more special fact that moveable feathers are habitually erected under the influence of anger and rivalry, in order to make the bird look more formidable in the eyes of antagonists.

Furthermore, he adduces the consideration that, even if the females are in any way affected by colour and its display on the part of the males, and if, therefore, sexual selection be conceded a true principle in theory, still we must remember that, as a matter of fact, it can only operate in so far as it is allowed to operate by natural selection. Now, according to Mr. Wallace, natural selection must wholly neutralize any such supposed influence of sexual selection. For, unless the survivors in the general struggle for existence happen to be those which are also the most highly ornamented, natural selection must neutralize and destroy any influence that may be exerted by female selection. But obviously the chances against the otherwise best fitted males happening to be likewise the most highly ornamented must be many to one, unless, as Wallace supposes, there is some correlation between embellishment and general perfection, in which case, as he points out, the theory of sexual selection lapses altogether, and becomes but a special case of natural selection.

Once more, Mr. Wallace argues that the evidence collected by Mr. Darwin himself proves that each bird finds a mate under any circumstances—a general fact which in itself must quite neutralize any effect of sexual selection of colour or ornament, since the less highly coloured birds would be at no disadvantage as regards the leaving of healthy progeny.

Lastly, he urges the high improbability that through thousands of generations all the females of any particular species—possibly spread over an enormous area—should uniformly and always have displayed exactly the same taste with respect to every detail of colour to be presented by the males.

Now, without any question, we have here a most powerful array of objections against the theory of sexual selection. Each of them is ably developed by Mr. Wallace himself in his work on *Tropical Nature*; and although I have here space only to state them in the most abbreviated of possible forms, I think it will be apparent how formidable these objections appear. Unfortunately the work in which they are mainly presented was published several years after the second edition of the *Descent of Man*, so that Mr. Darwin never had a suitable opportunity of replying. But, if he had had such an opportunity, as far as I can judge it seems that his reply would have been more or less as follows.

In the first place, Mr. Wallace fails to distinguish between brilliancy and ornamentation—or between colour as merely "heightened," and as distinctively decorative. Yet there is obviously the greatest possible difference between these two things. We may readily enough admit that a mere heightening of already existing coloration is likely enough—at all events in many cases—to accompany a general increase of vigour, and therefore that natural selection, by promoting the latter, may also incidentally promote the former, in cases where brilliancy is not a source of danger. But clearly this is a widely different thing from showing that not only *a general brilliancy of colour*, but also *the particular disposition of colours*, in the form of ornamental patterns, can thus be accounted for by natural selection. Indeed, it is expressly in order to account for the occurrence of such ornamental patterns that Mr. Darwin constructed his theory of sexual selection; and therefore, by thus virtually ignoring the only facts which that theory endeavours to explain, Mr. Wallace is not really criticizing the theory at all. By representing that the theory has to do only with brilliancy of colour, as distinguished from disposition of colours, he is going off upon a false issue which has never really been raised [1]. Look, for example, at a peacock's tail. No doubt it is sufficiently brilliant; but far more remarkable than its brilliancy is its elaborate pattern on the one hand, and its enormous size on the other. There is no conceivable reason why mere *brilliancy of colour*, as an accidental concomitant of general vigour, should have run into so extraordinary, so elaborate, and so beau-

[1] Note C.

tiful a *design of colours.* Moreover, this design is only unfolded when the tail is erected, and the tail is not erected in battle (as Mr. Wallace's theory of the erectile function in feathers would require), but in courtship; obviously, therefore, the purpose of the pattern, so to speak, is correlated with the act of courtship—it being only then, in fact, that the general purpose of the whole structure, as well as the more special purpose of the pattern, becomes revealed. Lastly, the fact of this whole structure being so large, entailing not only a great amount of physiological material in its production, but also of physiological energy in carrying about such a weight, as well as of increased danger from impeding locomotion and inviting capture—all this is obviously incompatible with the supposition of the peacock's tail having been produced by natural selection. And such a case does not stand alone. There are multitudes of other instances of ornamental structures imposing a drain upon the vital energies of their possessors, without conferring any compensating benefit from a utilitarian point of view. Now, in all these cases, without any exception, such structures are ornamental structures which present a plain and obvious reference to the relationship of the sexes. Therefore it becomes almost impossible to doubt—first, that they exist for the sake of ornament; and next, that the ornament exists on account of that relationship. If such structures were due merely to a superabundance of energy, as Mr. Wallace supposes, not only ought they to have been kept down by the economizing influence of natural selection; but we can see no reason, either why they should be so highly ornamental on the one hand, or so exclusively related to the sexual relationship on the other.

Finally, we must take notice of the fact that where peculiar *structures* are concerned for purposes of display in courtship, the *elaboration* of these structures is often no less remarkable than that of patterns where

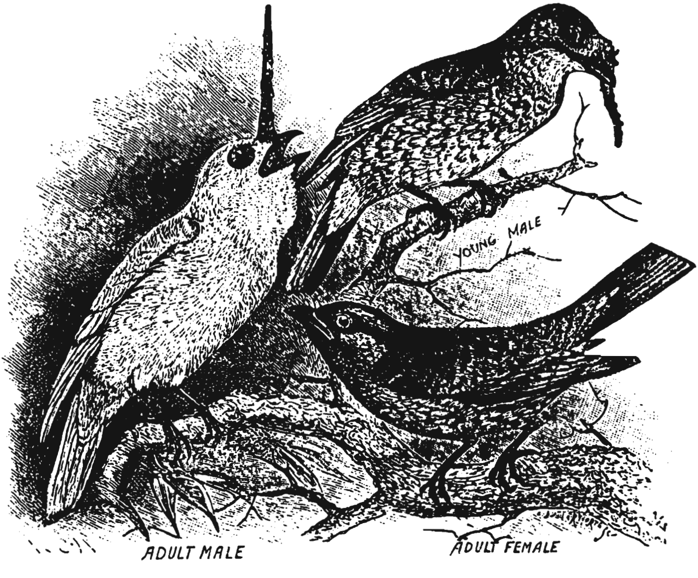

FIG. 124—The Bell-bird (*Chasmorhynchus niveus*, $\frac{1}{4}$ natural size). Drawn from nature (*R. Coll. Surg. Mus.*). In the drawing of the adult male the ornamental appendage is represented in its inflated condition, during courtship; in the drawing of the young male it is shown in its flaccid condition.

colours are thus concerned. Take, for example, the case of the Bell-bird, which I select from an innumerable number of instances that might be mentioned because, while giving a verbal description of this animal, Darwin does not supply a pictorial representation thereof. The bird, which lives in South America, has a very loud and peculiar call, that can be heard at a distance of two or three miles. The female is dusky-green; but the adult male is a beautiful white, excepting the extraordinary structure with which we are at present concerned. This is a tube about three

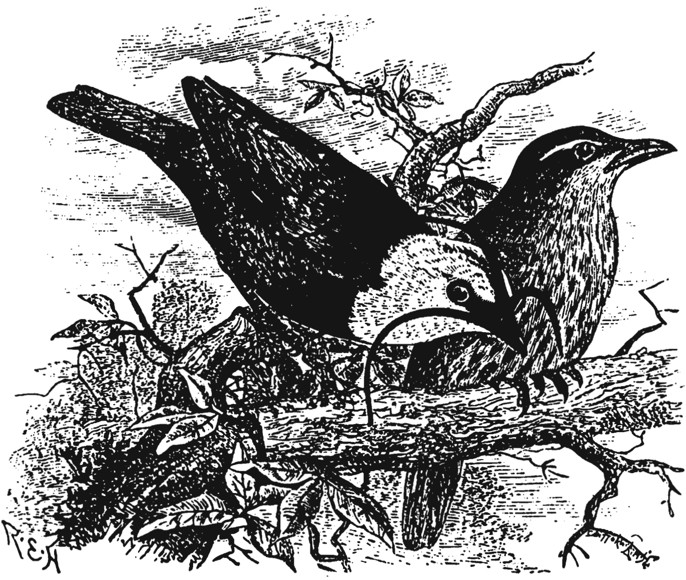

FIG. 125.—*C. tricarunculatus*, ¼ natural size. Copied from the *Ibis*. The ornamental appendages of the male are represented in a partly inflated condition.

inches long, which rises from the base of the beak. It is jet black, and dotted over with small downy feathers. The tube is closed at the top, but its cavity communicates with the palate, and thus the whole admits of being inflated from within, when, of course, it stands erect as represented in one of the two drawings. When not thus inflated, it hangs down, as shown in the second figure, which represents the plumage of a young male. (Fig. 124.)

In another species of the genus there are three of these appendages—the two additional ones being mounted on the corners of the mouth. (Fig. 125.) In all species of the genus (four in number) the tubes are inflated during courtship, and therefore perform the function of sexual embellishments. Now the point to which I wish to draw attention is, that so specialized and morphologically elaborate a structure cannot be regarded as merely adventitious. It must have been developed by some definite cause, acting through a long series of generations. And as no other function can be assigned to it than that of charming the female when it is erected in courtship, the peculiarity of form and mechanism which it presents—like the elaboration of patterns in cases where colour only is concerned—virtually compels us to recognise in sexual selection the only conceivable cause of its production.

For these reasons I think that Mr. Wallace's main objection falls to the ground. Passing on to his subsidiary objections, I do not see much weight in his merely negative difficulty as to there being an absence of evidence upon hen birds being charmed by the plumage, or the voice, of their consorts. For, on the one hand, it is not very safe to infer what sentiments may be in the mind of a hen; and, on the other hand, it is impossible to conceive what motive can be in the mind of a cock, other than that of making himself attractive, when he performs his various antics, displays his ornamental plumes, or sings his melodious songs. Considerations somewhat analogous apply to the difficulty of supposing so much similarity and constancy of taste on the part of female animals as Mr. Darwin's theory undoubtedly requires. Although we know very little about the psychology of the lower animals, we do observe in many cases that small details of mental organization are often wonderfully constant and uniform throughout all members of a species, even where it is impossible to suggest any utility as a cause.

Again, as regards the objection that each bird finds a mate under any circumstances, we have here an obvious begging of the whole question. That every feathered Jack should find a feathered Jill is perhaps what we might have antecedently expected; but when we meet with innumerable instances of ornamental plumes, melodious songs, and the rest, as so many witnesses to a process of sexual selection having always been in operation, it becomes irrational to exclude such evidence on account of our antecedent prepossessions.

There remains the objection that the principles of natural selection must necessarily swallow up those of sexual selection. And this consideration, I doubt not, lies at the root of all Mr. Wallace's opposition to the supplementary theory of sexual selection. He is self-consistent in refusing to entertain the evidence of sexual selection, on the ground of his antecedent persuasion that in the great drama of evolution there is no possible standing-ground for any other actor than that which appears in the person of natural selection. But here, again, we must refuse to allow any merely antecedent presumption to blind our eyes to the actual evidence of other agencies having co-operated with natural selection in producing the observed results. And, as regards the particular case now before us, I think I have shown, as far as space will permit, that in the phenomena of decorative colouring (as distinguished from merely brilliant colouring), of melodious song (as distinguished from merely tuneless cries), of enormous arborescent antlers (as distinguished from merely offensive weapons), and so forth—I say that in all these phenomena we have phenomena which cannot possibly be explained by the theory of natural selection; and, further, that if they are to be explained at all, this can only be done, so far as we can at present see, by Mr. Darwin's supplementary theory of sexual selection.

I have now briefly answered all Mr. Wallace's objections to this supplementary theory, and, as previously remarked, I feel pretty confident that, at all events in the main, the answer is such as Mr. Darwin would himself have supplied, had there been a third edition of his work upon the subject. At all events, be this as it may, we are happily in possession of unquestionable evidence that he believed all Mr. Wallace's objections to admit of fully satisfactory answers. For his very last words to science—read only a few hours before his death at a meeting of the Zoological Society—were:

> I may perhaps be here permitted to say that, after having carefully weighed, to the best of my ability, the various arguments which have been advanced against the principle of sexual selection, I remain firmly convinced of its truth [1].

[1] Since the above exposition of the theory of sexual selection was written, Mr. Poulton has published his work on the *Colours of Animals*. He there reproduces some of the illustrations which occur in Mr. and

Mrs. Peckham's work on *Sexual Selection in Spiders*, and furnishes appropriate descriptions. Therefore, while retaining the illustrations, I have withdrawn my own descriptions.

Mr. Poulton has also in his book supplied a *résumé* of the arguments for and against the theory of sexual selection in general. Of course in nearly all respects this corresponds with the *résumé* which is given in the foregoing pages; but I have left the latter as it was originally written, because all the critical part is reproduced *verbatim* from a review of Mr. Wallace's *Darwinism*, of a date still earlier than that of Mr. Poulton's book—viz. *Contemporary Review*, August, 1889.

If the advantage of freedom from competition in any given variation depends on the possession, in some degree, of new adaptations to unappropriated resources, there must be some cause that favours the breeding together of those thus specially endowed, and interferes in some degree with their crossing with other variations, or, failing this, the special advantage will in succeeding generations be lost. As some degree of Independent Generation is necessary for the continuance of the advantage, it is evident that the same condition is necessary for the accumulation through Natural Selection of the powers on which the advantage depends. The advantage of divergence of character cannot be retained by those that fail to retain the divergent character; and divergent character cannot be retained by those that are constantly crossing with other kinds; and the prevention of free crossing between those that are equally successful is in no way secured by Natura Selection.

So much, then, as expressive of Mr. Gulick's opinion upon this subject. To exactly the same effect Professor Lloyd Morgan has recently published his judgement upon it thus:—

That perfectly free intercrossing, between any or all of the individuals of a given group of animals, is, so long as the characters of the parents are blended in the offspring, fatal to divergence of character, is undeniable. Through the elimination of less favourable variations, the swiftness, strength, and cunning of a race may be gradually improved. But no form of elimination can possibly differentiate the group into swift, strong, and cunning varieties, distinct from each other, so long as all three varieties freely interbreed, and the characters of the parents blend in the offspring. Elimination may and does give rise to progress in any given group, *as a group*; it does not and cannot give rise to differentiation and divergence, so long as interbreeding with consequent interblending of characters be freely permitted. Whence it inevitably follows, as a matter of simple logic, that where divergence has occurred, intercrossing and interbreeding must in some way have been lessened or prevented. Thus a new factor is introduced, that of *isolation* or *segregation*. And there is no questioning the fact that it is of great importance. Its importance, indeed, can only be denied by denying the swamping effects of intercrossing, and such denial implies the tacit assumption that interbreeding and interblending are held in check by some form of segregation. The isolation explicitly denied is implicitly assumed [1].

Similarly, and still more recently, Professor Le Conte writes:—

It is evident, then, as Romanes claims, that natural selection alone tends to *monotypic* evolution. Isolation of some sort seems necessary to *polytypic* evolution. The tree of evolution under the influence of natural selection alone grows palm-like from its terminal bud. Isolation was necessary to the starting of lateral buds, and thus for the profuse ramification which is its most conspicuous character [2].

In order to complete this historical review, it only remains to consider Mr. Wallace's utterances upon the subject.

It is needless to say that he stoutly resists the view of Weismann, Delbœuf, Gulick, and myself, that specific divergence can ever be due—or, as I understand him, even so much as assisted—by this principle of indiscriminate isolation (apogamy). It will be remembered, however, that Mr. Gulick has adduced certain general principles and certain special facts of geographical distribution, in order to prove that apogamy eventually leads to divergence of character, provided that the isolated section of the species does not contain any very large number of individuals. Now, Mr. Wallace, without making any reference to this argument of Mr. Gulick, simply states the reverse —namely that, as a matter of fact, indiscriminate

[1] *Animal Life and Intelligence*, pp. 98, 99 (1890–1891).

[2] *The Factors of Evolution* (1891).

isolation is not found to be associated with divergence of character. For, he says, "there is an entire absence of change, where, if this were a *vera causa*, we should expect to find it[1]." But the only case which he gives is that of Ireland.

This, he says, furnishes "an excellent test case, for we know that it [Ireland] has been separated from Britain since the end of the glacial epoch: . . . yet hardly one of its mammals, reptiles, or land molluscs has undergone the slightest change[2]." Here, however, Mr. Wallace shows that he has failed to understand "the views of those who, like Mr. Gulick, believe isolation itself to be a cause of modification of species"; for it belongs to the very essence of these views that the efficiency of indiscriminate isolation as a "*vera causa*" of organic evolution varies inversely with the number of individuals (i. e. the size of the species-section) exposed to its influence. Therefore, far from being "an excellent test case," the case of Ireland is unsatisfactory. If we are in search of excellent test cases, in the sense intended by Mr. Wallace, we ought not to choose a large island, which from the time of its isolation must have contained large bulks of each of the geographically separated species concerned: we ought to choose cases where as small a number as possible of the representatives of each species were in the first instance concerned. And, when we do this, the answer yielded by any really "excellent test case" is unequivocal.

No better test case of this kind has ever been furnished than that of Mr. Gulick's land-shells,

[1] *Darwinism*, p. 151. [2] *Ibid.*

which Mr. Wallace is specially considering in the part of his book where the sentence above quoted occurs. How, then, does he meet this case? He meets it by assuming that in all the numerous adjacent valleys of a small island there must be as many differences of environment, each of which is competent to induce slight varietal changes on the part of its occupants by way of natural selection, although in no one case can the utility of these slight changes be surmised. Now, against this explanation there are three overwhelming considerations. In the first place, it is purely gratuitous, or offered merely in order to save the hypothesis that there *can* be no other cause of even the most trivial change in species than that which is furnished by natural selection. In the second place, as Mr. Gulick writes to me in a private letter, "if the divergence of Sandwich Island land molluscs is wholly due to exposure to different environments, as Mr. Wallace argues on pages 147–150, then there must be completely occult influences in the environment that vary progressively with each successive mile. This is so violent an assumption that it throws doubt on any theory that requires such support." In the third place, the assumption that the changes in question must have been due to natural selection, is wholly incompatible with the facts of isolation elsewhere—namely, in those cases where (as in that of Ireland) a large section of species, instead of a small section, has been indiscriminately isolated. Mr. Wallace, as we have seen, inadvertently alludes to these "many other cases of isolation" as evidence against apogamy being *per se* a cause of specific change. But although, for the reason above stated, they are without relevancy in this respect, they appear to me fatal to the explanation which he gives of specific changes under apogamy where only small sections of species are concerned. For example, can it be rationally maintained that there are more differences of environment between every two of the many contiguous valleys of a small island, such as Mr. Gulick describes, than there are in the incomparably larger area of the whole of Ireland? But, if not, and if natural selection is able to work such "occult" wonders in each successive mile on the Sandwich Islands, why has it so entirely lost this magic power in the case of Ireland —or in the "many other cases of isolation" to which Mr. Wallace refers? On his theory there is no coherent answer to be given to this question, while on our theory the answer is given in the very terms of the theory itself. The facts are plainly just what the theory requires that they should be; and therefore, if they were not as they are, the theory would be deprived of that confirmation which it now derives from them.

Thus, in truth, though in an opposite way, the case of Ireland is, as Mr. Wallace says, "an excellent test case," when once the theory of apogamy as a "*vera causa*" of specific change is understood; and the effect of applying the test is fully to corroborate this theory, while at the same time it as fully negatives the other. For the consideration whereby Mr. Wallace seeks to explain the inactivity of natural selection in the case of Ireland is not "coherent." What he says is, "That changes have not occurred through natural selection, is perhaps due to the less severe struggle for existence, owing to the smaller number of competing species[1]." But even with regard to molluscs alone, there is a greatly larger number of species in Ireland than occurs in any one valley of the Sandwich Islands; while if we have regard to all the other classes of animal life, comparison entirely fails.

Much more to the point are certain cases which were adduced long ago by Weismann in his essay previously considered. Nevertheless, although this essay was published as far back as 1872, and, although it expressly deals with the question of divergence of character through the mere prevention of intercrossing (Amixia), Mr. Wallace nowhere alludes to these cases *per contra*, which are so much more weighty than his own "test case" of Ireland. Of such are four species of butterflies, belonging to three genera[2], which are identical in the polar regions and in the Alps, notwithstanding that the sparse Alpine populations have been presumably separated from their parent stocks since the glacial period; or of certain species of fresh water crustaceans (*Apus*), the representatives of which are compelled habitually to form small isolated colonies in widely separated ponds, and nevertheless exhibit no divergence of character, although apogamy has probably lasted for centuries. These cases are unquestionably of a very cogent nature, and appear of themselves to prove that apogamy alone is not invariably capable of

[1] *Loc. cit.*, p. 151.

[2] Namely, *Lycaena donzelii, L. pheretes, Argynnis pales, Erebia manto.*

inducing divergence—at any rate, so rapidly as we might expect. There appears, however, to be another factor, the presence or absence of which makes a great difference. This as stated in the text, is the degree in which a specific type is stable or unstable—liable or not liable to vary. Thus, for example, the Goose is what Darwin calls an "inflexible" type as compared with most other domesticated birds. Therefore, if a lot of geese were to be indiscriminately isolated from the rest of their species, the probability is that in a given time their descendants would not have diverged from the parent type to such an extent as would a similar lot of ducks under similar circumstances: the more stable specific type would require a longer time to change under the influence of apogamy alone. Now, the butterflies and crustaceans quoted by Weismann may be of a highly stable type, presenting but a small range of individual variability; and, if so, they would naturally require a long time to exhibit any change of type under the influence of apogamy alone. But, be this as it may, Weismann himself adduces these cases merely for the sake of showing that there are cases which seem to tell against the general principle of modification as due to apogamy alone—i.e. the general principle which, under the name amixia, he is engaged in defending. And the conclusion at which he himself arrives is, that while it would be wrong to affirm that apogamy *must* in all cases produce divergence, we are amply justified in affirming that in many cases it *may* have done so; while there is good evidence to prove that in not a few cases it *has* done so, and therefore should be accepted as one of the factors of organic evolution [1].

My view from the very first has been that variations in the way of cross-infertility are of frequent occurrence (how, indeed, can they be otherwise, looking to the complex conditions that have to be satisfied in every case of full fertility?); and, therefore, however many of such variations are destined to die out, whenever one arises, "under suitable conditions," "it must inevitably tend to be preserved as a new natural variety, or incipient species." Among the higher animals—which are "comparatively few in number"—I think it probable that some slight change of form, colour, habit, &c., must be usually needed either to "superinduce," or, which is quite a different thing, to *coincide* with the physiological change But in the case of plants and the lower invertebrata. I see no reason for any frequent concomitance of this kind; and therefore believe the physiological

[1] Since the above was written, I have heard of some cases which seem to present greater difficulties to our theory than those above quoted. These refer to some of the numerous species of land mollusca which inhabit the isolated rocks near Madeira (Dezertas). My informant is Dr. Grabham, who has himself investigated the matter, and reports as follows:—

"It is no uncommon thing to meet with examples of the same species, sub-fossil, recent, and living upon one spot, and presenting no variation in the long record of descent." Then, after naming these examples, he adds, "All seem to vary immediately on attaining new ground, assuming many aspects in different districts."

Unquestionably these statements support, in a very absolute manner, Mr. Wallace's opinion, while making directly against my own. It is but fair, however, to add that the cases are not numerous (some half-dozen at the most, and all within the limits of a single genus), and that, even in the opinion of my informant himself, the facts have not hitherto been sufficiently investigated for any decisive judgement to be formed upon them.

change to be, "as a general rule," the primordial change. At the same time, I have always been careful to insist that this opinion had nothing to do with "the essence of physiological selection"; seeing that "it was of no consequence" to the theory in what proportional number of cases the cross-sterility had begun *per se*, had been superinduced by morphological changes, or only enabled to survive by happening to coincide with any other form of homogamy. In short, "the essence of physiological selection" consists in *all* cases of the diversifying *effect* of cross-infertility, whensoever and howsoever it may happen in particular cases to have been *caused*.

Thus I emphatically reaffirm that "from the first I have always maintained that it makes no essential difference to the theory *in what proportional number of cases* they [the physiological variations] have arisen 'alone in an otherwise undifferentiated species'"; therefore, "even if I am wrong in supposing that physiological selection can *ever* act alone, the *principle* of physiological selection, as I have stated it, is not thereby affected. And this principle is, as Mr. Wallace has re-stated it, 'that some amount of infertility characterizes the distinct varieties which are in process of differentiation into species'—infertility whose absence, 'to obviate the effects of intercrossing, may be one of the *usual* causes of their failure to become developed into distinct species.'"

These last sentences are quoted from the correspondence in *Nature*[1], and to them Mr. Wallace replied by saying, "if this is not an absolute change of front,

[1] Vol. xliii. p. 127.

words have no meaning"; that "if this is 'the whole essence of physiological selection,' then physiological selection is but a re-statement and amplification of Darwin's views"; that such a "change of front" is incompatible, not only with my term "physiological selection," but also with my having "acknowledged that Mr. Catchpool had 'very clearly put forward the theory of physiological selection'"; and much more to the same effect.

Now, to begin with, it is due to Mr. Catchpool to state that his only publication upon this subject is much too brief to justify Mr. Wallace's inference, that he supposes variations in the way of cross-infertility always to arise "alone in an otherwise undifferentiated species." What Mr. Catchpool's opinion on this point may be, I have no knowledge; but, whatever it is, he was unquestionably the first writer who "clearly stated the leading principles" of physiological selection, and this fact I am very glad to have "acknowledged." In my correspondence with Mr. Wallace, however, I not only named Mr. Catchpool: I also named—and much more prominently—Mr. Gulick. For even if I were to grant (which I am far indeed from doing) that there was any want of clearness in my own paper touching the point in question, I have now repeatedly shown that it is simply impossible for any reader of Mr. Gulick's papers to misunderstand *his* views with regard to it. Accordingly, I replied to Mr. Wallace in *Nature* by saying:—

Not only have I thus from the first fully recognized the sundry other causes of specific change with which the physiological variations may be associated; but Mr. Gulick has gone into this side of our common theory much more fully, and elaborately calculated out the high ratio in which the differentiating agency of any of these other causes must be increased when assisted by—i. e. associated with —even a moderate degree of the selective fertility, and vice versa. Therefore, it is simply impossible for Mr. Wallace to show that "our theory" differs from his in this respect. Yet it is the only respect in which his reply alleges any difference. (Vol. xliii. p. 127.)

I think it is to be regretted that, in his answer to this, Mr. Wallace alludes only to Mr. Catchpool, and entirely ignores Mr. Gulick—whose elaborate calculations above alluded to were communicated to the Linnaean Society by Mr. Wallace himself in 1887.

The time has now come to prove, by means of quotations, that I have from the first represented the "principle," or "essence," of physiological selection to consist in selective fertility furnishing a needful condition to specific differentiation, in at least a large proportional number of allied species which afterwards present the reciprocal character of cross-sterility; that I have never represented variations in the way of this selective fertility as necessarily constituting the initial variations, or as always arising "alone, in an otherwise undifferentiated species"; and that, although I have uniformly given it as my opinion that these variations do *in some cases* thus arise (especially among plants and lower invertebrata), I have as uniformly stated "that it makes no difference to the theory in what proportional number of cases they have done so"—or even if, as Mr. Wallace supposes, they have never done so in any case at all [1].

[1] This refers to what I understand Mr. Wallace to say in the *Nature* correspondence is the supposition on which his own theory of the origin of species by cross-infertility is founded. But in the original statement of that theory itself, it is everywhere "supposed" that when species are

These statements (all of which are contradictory of the only points of difference alleged) have already been published in my article in the *Monist* of October, 1890. And although Mr. Wallace, in his reply to that article, ignores my references to the "original paper," it is scarcely necessary to quote the actual words of the paper itself, since the reader who is further interested in this controversy can readily refer to it in the *Journal of the Linnaean Society* (vol. xix. pp. 337–411).

Having arrived at these results with regard to the theory of Isolation in general and of Physiological Isolation in particular, I arrive also at the end of this work. And if, while dealing with the post-Darwinian period, I have imparted to any general reader the impression that there is still a great diversity of expert opinion: I must ask him to note that points with reference to which disagreement still exists are but very subordinate to those with regard to which complete agreement now prevails. The noise of wrangling disputations which has so filled the camp of evolutionists since the death of their captain, is apt to hide from the outside world the solid unanimity that prevails with regard to all the larger and more fundamental questions, which were similarly the subjects of warfare in the past generation. Indeed, if we take a fair and general

originated by cross-infertility, the *initial* change *is* the physiological change. In his original statement of that theory, therefore, he literally went further than I had gone in my "original paper," with reference to supposing the physiological change to be the initial change. I do not doubt that this is due to some oversight of expression; but it is curious that, having made it, he should still continue his endeavour to fix exactly the same oversight upon me.

view of the whole history of Darwinism, what must strike us as the really significant fact is the astonishing unanimity which has been so rapidly attained with regard to matters of such immeasurable importance. It is now but little more than thirty years since the publication of the *Origin of Species*; and in that period not only have all naturalists unequivocally embraced the doctrine of descent considered as a fact; but, in one degree or another, they have all as unequivocally embraced the theory of natural selection considered as a method. The only points with regard to which any difference of opinion still exist, have reference to the precise causation of that mighty stream of events which, under the name of organic evolution, we have now all learnt to accept as scientifically demonstrated. But it belongs to the very nature of scientific demonstration that, where matters of great intricacy as well as of high generality are concerned, the process of demonstration must be gradual, even if it be not always slow. It is only by the labours of many minds working in many directions that, in such cases, truth admits of being eventually displayed. Line upon line, precept upon precept, here a little and there a little—such is the course of a scientific revelation; and the larger the subject-matter, the more subtle and the more complex the causes, the greater must be the room for individual differences in our reading of the book of Nature. Now, if all this be true, must we not feel that in the matter of organic evolution the measure of agreement which has been attained is out of all proportion to the differences which still remain—differences which, although of importance in themselves, are insignificant when compared with those which once divided the opinions of not a few still living men? And if we are bound to feel this, are we not bound further to feel that the very intensity of our disputations over these residual matters of comparative detail, is really the best earnest that can be given of the determination of our quest—determination which, like that of our fathers, cannot fail to be speedily rewarded by the discovery of truth?

Nevertheless, so long as this noise of conflict is in the Senate, we cannot wonder if the people are perplexed. Therefore, in conclusion, I may ask it to be remembered exactly what are the questions—and the only questions—which still divide the parties.

Having unanimously agreed that organic evolution is a fact and that natural selection is a cause, or a factor in the process, the primary question in debate is whether natural selection is the only cause, or whether it has been assisted by the co-operation of other causes. The school of Weismann maintain that it is the only cause; and therefore deem it worse than useless to search for further causes. With this doctrine Wallace in effect agrees, excepting as regards the particular case of the human mind. The school of Darwin, on the other hand—to which I myself claim to belong—believe that natural selection has been to a considerable extent supplemented by other factors; and, therefore, although we further believe that it has been the "main" factor, we agree with Darwin himself in strongly reprobating all attempts to bar *a priori* the progress of scientific investigation touching what, if any, these other factors may be.