

### Are Specific Characters useful?

WITH the above title Mr. Alfred Russel Wallace brought before the Linnean Society on June 18 an important communication, which derived additional interest from the fact that he himself was present in full health and vigour, as well as from the presence of a large number of naturalists who have given attention to the questions arising from the consideration of the theory of the origin of species by natural selection.

In the course of the remarks which were offered by his audience at the conclusion of Mr. Wallace's paper, I ventured to point out that the consideration of the class of phenomena which Mr. Darwin had described under the title "correlation of variation," seemed to me to lead necessarily to the conclusion that very often characters which are obvious and distinctive marks of species may be not useful but useless, since such obvious species marks may be only superficial and non-significant phenomena "correlated" (as Mr. Darwin used that term) with other less obvious but really important life-saving peculiarities, which might quite well escape the observation of the describer of "specific characters." As instances of the phenomenon of "correlation," I referred to those cited by Mr. Darwin, such as the concomitance of a development of feathers on the feet with the webbing of the toes in certain breeds of pigeons, the concomitance of abnormal dentition with hairlessness of the body-surface in Chinese dogs, the concomitance of deafness with blue eyes in male white cats. A case which seemed to me most striking and suggestive in connection with the utility of specific characters was cited by me. It was that which had led Wells to propound a doctrine of "natural selection" many years before Darwin and Wallace had placed their views in 1858 before the Linnean Society—a case which Mr. Darwin cited in later editions of the "Origin of Species," and is familiar enough. Wells pointed out in a memoir communicated to the Royal Society in 1813, that persons with dark pigment in the skin are relatively immune to tropical fevers, as compared with fair-complexioned

individuals. He argued that owing to this property of dark-skinned varieties of men, there would be a survival selection in tropical regions of such varieties, and that probably, or at any rate possibly, in this manner the black colour of tropical races might be accounted for. I mention this more or less hypothetical case as showing that an obvious and striking character, namely, that of black pigment in the skin, might become predominant, and conceivably might become a "specific character," although the blackness was not in itself a "useful," that is, a "life-preserving or progeny-ensuring" character, but merely the *accompaniment* of a power of resisting malarial germs, which we now have reason to believe consists in a special chemical activity of the leucocytes (phagocytes) of the blood and other tissues. From the consideration of this and other similar cases, I argued that many "specific characters" (that is to say, as defined by Mr. Wallace, characters which individually or in definite association with other characters constantly occur in one species and not in the other species of a genus) must be devoid of utility themselves, and appear merely as the "correlatives" or "concomitants" of really effective life-preserving or progeny-ensuring characters. I insisted, finally, on the very great importance of the correlation of parts in animal organisms, and the necessity of regarding animals (and presumably also plants) as most highly-wrought mechanisms in which no part can vary without the accompaniment of variation in some remote and (in our present state of knowledge) unexpectedly correlated part, and to a degree often excessive and (in our present state of knowledge) unaccountable. Thus, as Mr. Darwin himself pointed out, the selection of a given favourable variation may lead to excessive variation in a remote region of the organism, which in its turn will very often (but not necessarily always or at once) become the subject of further selection. Mr. Darwin appears to have deprecated, in conversation with Mr. Thiselton-Dyer (according to the latter's interesting statement in the debate on Mr. Wallace's paper), the invocation of this theory of "correlation" as an explanation of cases of apparently useless parts in animals or plants when under investigation, holding that our ignorance of the modes in which parts may be serviceable to an organism is so great that we should rather experiment and observe as to their possible utility than advance a theory which dismisses further inquiry. Whilst agreeing with Mr. Thiselton-Dyer as to the "immorality" (as he termed it) of a naturalist who favours theories which paralyse his activity as an observer and experimentalist (on which subject see the last paragraph of this letter), I yet think that, as seekers after true knowledge, we are bound to face the complex problem in all its aspects. The obvious character, as well as many less obvious characters, which we note as distinguishing one species from another, are not improbably, it must be admitted, in many cases concomitant phenomena of some other phenomenon which alone among them is effective in determining the preservation of the life, or the production of progeny in the case of the individuals so characterised.

At the same time I think that it may well be maintained that such secondary or concomitant characters are not long allowed to remain non-significant, and that sooner or later they fall under the moulding action of natural selection, becoming as they increase in volume either useful or injurious.

My chief object in writing this letter is to draw attention to the views of Prof. Weldon, who has for some time, as all zoologists know, been occupied in tabulating a very large series of measurements of growing crabs. When I had stated my views as to the importance of "correlation of variation," with which Mr. Meldola and Mr. Wallace subsequently expressed their complete agreement, Prof. Weldon declared, with some expressions of reluctance and regret—due, as he was good enough to say, from an old pupil to the teacher whom he is about to denounce and demolish—that to attempt to say which of two or more correlated growths is the cause of survival is unreasonable, and that when I suggested, even as a matter for consideration, that a certain germ-slaying quality in phagocytes accompanying a pigmented skin, rather than the pigment itself in the skin, is the cause of the survival of dark-skinned people in malarial regions, I was "absolutely illogical." "It is," said Prof. Weldon, "impossible logically to separate these two correlated phenomena. The coloured skin is as much a cause of the survival of the dark man as is the germ-destroying property of his blood."

I was at the time entirely unable to appreciate the drift of Prof. Weldon's thought. I was not prepared for an empty

wrangle in regard to the proper uses or improper uses of the word "cause." But I did remember that Mill says that the most vulgar form of "the fallacy of generalisation" is that which is expressed by the phrase "*post hoc or cum hoc, ergo propter hoc*." I could not imagine how or why my friend Prof. Weldon had been led to make himself the defiant, not to say jubilant, champion of this fallacy. I have, on reading Prof. Weldon's paper in the *Proceedings* of the Royal Society, vol. lvii. 1894-95, found matter which throws light on the problem. It would appear that Prof. Weldon, in discussing his measurements of crabs, had already publicly adopted the logical position which so much astonished those who heard him at the Linnean Society. It appears that the fallacious process, which consists in ignoring the possibility of two concomitant phenomena being two independent consequences of one set of antecedents, gives an apparent value to the laborious measurement of crabs which, it seems, they would not possess if treated in a rational way. Prof. Weldon says (*loc. cit.*, p. 380): "It is the object of the present remarks to discuss the *effect* of small variations, as it may be deduced from the study of two organs in a single species. The case chosen is the variation, during growth and in adult life, of the dimensions of female *Carcinus mænas*."

Further on he speaks of "the *effect* of small variations upon the chance of survival," and in close proximity occurs this passage: "The law of growth having been ascertained, the rate of destruction may be measured, and in this way an estimate of the *advantage or disadvantage* of a variation may be obtained." And again: "Knowing that a given deviation from the mean character is associated with a greater or less percentage death-rate in the animals possessing it, the *importance* of such a deviation can be estimated without the necessity of inquiring how that increase or decrease in the death-rate is brought about so that all ideas of functional adaptation become unnecessary." The title of the paper drawn up by a Committee, of which Prof. Weldon is a member, and in reference to which his own paper is written, stands: "An attempt to measure the death-rate due to the *selective* destruction of *Carcinus mænas* with respect to a particular dimension."

(The italics in these citations are mine.)

It appears to me that the language which I have italicised indicates that Prof. Weldon—in his interpretation of the fact ascertained by him, viz. that crabs with a particular proportion of frontal breadth are commoner in the adult condition than in younger stages—has deliberately departed from the simple statement which his observations warranted, viz. that such-and-such a proportion of frontal measurement accompanies survival, and has unwarrantably (that is to say unreasonably) proceeded to speak of the "effect" of this frontal proportion, to declare it to be a *cause* of survival, to estimate the "advantage" and "disadvantage" of this same proportion, and finally to maintain that its "*importance*" may be estimated without troubling ourselves to inquire how it operates, or whether indeed it is operative at all.

Such methods of attempting to penetrate the obscurity which veils the interactions of the immensely complex bundle of phenomena which we call a crab and its environment, appear to me not merely inadequate, but in so far as they involve perversion of the meaning of accepted terms and a deliberate rejection of the method of inquiry by hypothesis and verification, injurious to the progress of knowledge.

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Oxford, June 30.