of vitreous reflection and refraction and of reflection at metallic surfaces.

The book contains a clear account of the theoretical aspects of the above questions, the mathematical treatment being as elementary as is consistent with the nature of the subject.

LETTERS TO THE EDITOR.

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The Non-Inheritance of Acquired Characters.

I WISH to call the attention and elicit the opinion of naturalists as to the interpretation of certain facts bearing upon this question.

In my article in the *Fortnightly Review* of May last, p. 664, I give what appears to be a new interpretation of facts which have been often quoted, as to the change in the external characters of a Texan species of Saturnia when the larvæ were fed upon *Juglans regia*, its native food-plant being *Juglans nigra;* and the somewhat analogous facts as to *Artemia salina* being changed into *A. Milhausenii* (the former living in brackish, the latter in salt water) when the water became gradually more salt; the change in this case being progressive, year by year, and proportionate to the change in the saltness of the water. The reverse change was also effected by gradually reducing the salinity of the water inhabited by *A. Milhausenii.*

As regards the former case I remarked in my article as follows:—

"Prof. Lloyd Morgan (in his 'Animal Life and Intelligence,' pp. 163–166) clearly sees that this and other cases do not prove more than a modification of the individual; but it seems to me to go further than this. For here we have a species the larvæ of which for thousands, perhaps millions, of generations have fed upon one species of plant, and the perfect insect has a definite set of characters. But when the larvæ are fed on a distinct but allied species of plant, the resulting perfect insect differs both in colouration and form. We may conclude from this fact that some portion of the characters of the species are dependent on the native food-plant, *Juglans nigra*, and that this portion changed under the influence of the new food-plant. Yet the influence of the native food-plant had been acting uninterruptedly for unknown ages. Why then had the resulting characters not become fixed and hereditary? The obvious conclusion is, that being a change produced in the body only by the environment, it is not hereditary, no matter for how many generations the agent continues at work; in Weismann's phraseology it is a somatic variation, not a germ variation."

I then referred to the marked difference between somatic and germ variations in plants, the former disappearing at once, the latter persisting, when cultivated under abnormal conditions; and also to the cases of many closely allied species of animals and of the races of mankind, which preserve their distinctive characteristics when living and breeding under very different conditions.

The above seems to me a perfectly valid and logical argument, and I was interested to see how it would be met by Lamarckians, who have frequently referred to the same facts as being obviously in their favour, though without any attempt to show how and why they are in their favour. I was therefore rather surprised to read, in the July issue of the *Contemporary Review*, a paper by Prof. Marcus Hartog, in which he characterises my argument as a very bad kind of special pleading, and adds that it amounts to this: "Any change in the offspring produced by altered conditions in the parent is limited to characters that are 'not fixed and inherited'; for fixed and inherited characters cannot be altered by changed conditions in the parent; therefore no experimental proof can be given of the transmission of acquired characters."

The above is of course simple reasoning in a circle, and I cannot recognise it as my reasoning. I have made no general proposition that "fixed and inherited characters cannot be altered by changed conditions in the parent," or that "no experimental proof can be given of the transmission of acquired characters." But I argue that when a decided character is immediately changed by changed conditions of the individual, as in Saturnia, it is not "fixed and inherited." The experiment itself shows that it is not a fixed character, and there can be no proof that it is inherited so long as it only appears under the very same changed conditions that produced it in the parent.

As to experimental proof I believe it to be quite possible. There is one case, which I do not remember having seen referred to, in which nature has tried an experiment for us. I was informed by the President of the Deaf-Mute College at Washington that the male and female students frequently marry after leaving the college, and that their children are rarely deaf-mutes. But the point to which I wish to call attention is the admitted fact that there is usually no disease or malformation of the vocal organs in a deaf-mute. Now, before deaf-mutes were taught to talk as they are now, they passed their whole lives without using the complex muscles and motor-nerves by the accurate coordination of which speech is effected. Here is a case of complete disuse, and there must have been some consequent atrophy. Yet it has, I believe, never been alleged that the children of deaf-mutes exhibited any unusual difficulty in learning to speak, as they should do if the effects of disuse of the organs of speech in their parents were inherited. Here is at all events the material of an experiment ready to our hands. An experiment to show whether the effects of use and disuse were inherited might also be tried by bringing up a number of dove-cot pigeons in a large area covered in with wire netting so low as to prevent flight, at the same time encouraging running by placing food always at the two extremities of the enclosure only, or in some other way ensuring the greatest amount of use of the legs. After two or three generations had been brought up in this way, the latest might be turned out among other dove-cot pigeons, at the age when they would normally begin to fly, and it would then be seen if the diminished wing-power and increased leg-power of the parents were inherited.

No doubt many better experiments might be suggested; but these are sufficient to indicate the character of such as do not require that the offspring be submitted to the same conditions as those which produced the change in the parents, and which thus enable us to discriminate between effects due to inheritance and those due to a direct effect of the conditions upon the individual. The cases of the Saturnia and the shrimps are of the latter kind, and in their very nature can afford no proof of heredity.

ALFRED R. WALLACE.

The Conditions Determinative of Chemical Change: Some Comments on Prof. Armstrong's Remarks.

IN a paper (NATURE, vol. xlviii. p. 237, Proc. Chem. Soc. 1893, 145) bearing the above title, Prof. Armstrong discusses the phenomena of contact action, particularly those of the kind described by Mr. H. B. Baker. The whole discussion appears to us to be based on erroneous conceptions and to call for some criticism, first, on the general position assumed by him and, second, of the details which he brings forward to support that position.

Eight years ago Prof. Armstrong defined chemical action as "reversed electrolysis." It is not clear from his remarks whether this is one of the views which recent observations have led him to modify; but, assuming that he still holds that belief, it may be pointed out that it by no means follows that because an electric current can effect a chemical change, every chemical change is due to or accompanied by electric action. It might as well be argued that because a stone let fall on a glass plate can shiver it, a shivered plate glass always implies a falling stone as its cause—it could be broken by irregular rise of temperature, or by loading it with a too heavy weight, phenomena which imply no expenditure of kinetic energy. Yet the statement contains a germ of truth, but only when so qualified as to amount to something very different. Electrical energy may be absorbed in effecting chemical decomposition; when chemical combination occurs *some form of energy* is made manifest. The facts, apart from theory, as we know them, appear to be these. A certain fraction of some definite amount of electrical energy may be absorbed in producing chemical decomposition, and that fraction will be quantitatively converted into chemical energy; the electrical energy disappears as such, and elements may be liberated from a compound, containing, as elements, the equivalent quantity of chemical energy. These elements may part with their chemical energy, which will then cease to exist