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 saltiness 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

as such, but will appear in various forms: some of it may be evolved as heat, some as volume energy, some as kinetic energy, and it is even possible by an appropriate contrivance to obtain a large portion of the chemical as electrical energy. But to state that the energy always passes through the electric stage on its way to other forms in which it manifests itself to us is something altogether different.

The question that Prof. Armstrong tries to answer by the supposition that the presence of an electrolyte is required in order to bring about chemical change admits of a very different reply. We conceive it to be this: In most exothermic combinations the heat evolved is sufficient, provided the change were to proceed adiabatically, to resolve the compound into its constituents. Why, then, should they react? To take a concrete instance:—Why should ammonia and hydrochloric acid combine at ordinary temperatures when the heat evolved by their union is sufficient (*provided none escape*) to raise the reacting molecules to the temperature at which they refuse to combine? For convenience sake the question is stated in terms of heat, since that is the usual form in which the loss of chemical energy manifests itself to us; but it is advisable to keep the statement of the question quite general. It appears to us that the answer is:—because the reaction is not adiabatic. Some substances must be present—the walls of the containing vessel, some compound capable of dissociation, some solid body, such as spongy platinum, which will absorb a portion, perhaps an exceedingly small portion, of energy, and so give the bodies present a chance of interacting without liberating so much energy by their interaction as would decompose the prospective compound. These views, it may be contended, are speculative. It is true: but we venture to think that they are legitimate speculations, involving a complete survey of the circumstances, and not one-sided and partial like those of the paper we are criticising.

Assuming the correctness of Prof. Armstrong's main idea, there are still one or two matters of detail where the assumption scarcely seems in harmony with known facts. He assumes that because hydrogen chloride when dissolved in water forms a composite electrolyte, a gaseous mixture of hydrogen chloride and water will also be an electrolyte. This by no means follows, and indeed experiments which have been made in this direction point to the contrary conclusion. The same holds good of his argument as to the combination of nitric oxide and oxygen—water vapour is not known to form a composite electrolyte with gaseous nitric acid.

With regard to the regularity displayed by iodine and hydrogen compared with the irregularity of the results obtained by Victor Meyer with chlorine and hydrogen, it is altogether impossible to understand Prof. Armstrong's attitude. In one sentence he assures us that "this is not surprising," and in the next that "there is a significant [of what?] difference in the behaviour of the two mixtures, as hydrogen iodide should behave as hydrogen chloride." He suggests that some special electrolyte may be active in the case of chlorine and hydrogen; but he is inclined to account for the difference observed from the fact that only one of the reactions is reversible under the conditions of experiment. We quite fail to understand the influence which the reversibility of the reaction would exert on its regularity.

In fine, still assuming for the sake of argument the notion of "reversed electrolysis," we would ask:—In a mixture of hydrogen and oxygen, are the ions there, or are they not there? If not there, will the presence of a vapour bring them into existence? If there, what is the need of a so-called impurity? Is it supposed that the impurity will discharge them? Why, then, does not the presence of one or of two conducting wires of the same metal in an electrolyte cause combination of the ions?

University College, London, July 8. W. RAMSAY.
JAMES WALKER.

The Corona Spectrum.

In the preliminary account by M. Deslandres of the main results of the eclipse photographs obtained by the French astronomers at Fundium, as reported in this journal on May 25 (vol. xlviii. p. 81), it is stated that many new coronal lines have been photographed, and that a displacement of the lines in the light from opposite points of the corona in the solar equatorial plane proves a rotational movement nearly corresponding with that of the surface of the sun itself.

In the absence of fuller details it is perhaps a little difficult

to accept without reserve these interesting statements, particularly when one considers the somewhat unfavourable conditions under which the photographs were obtained. In the first place, one would like to ask by what means have these new bright lines been identified as belonging to the corona, seeing that, owing to the hazy condition of the air at the above station, the brilliant chromospheric radiations were apparently reflected from a considerable area of the sky in the sun's neighbourhood, forming, as it were, a kind of false corona with a bright line spectrum. So obvious, indeed, is this atmospheric spreading of the chromosphere lines in the spectrum photographs obtained by the English astronomers at the same station, that many lines are shown as clearly on the moon's disk as in the coronal regions; the calcium lines "H" and "K," which are very brilliant chromosphere lines, are in these found to extend considerably above the limits of the true corona, as defined by its continuous spectrum, and are also found equally bright across the dark moon.

From the above considerations one is inclined almost to doubt whether, after all, any true corona lines have ever been proved to exist, excepting perhaps the line 1474 (K), which is not ordinarily a brilliant line in the chromosphere, and would therefore not be easily seen by atmospheric reflexion;¹ and it would seem possible, if not probable, that this beautiful solar appendage, with its dark rifts and curving streamers, shines simply by continuous light.

Definite information on this point would, however, be gladly welcomed by those who are endeavouring to photograph the corona without an eclipse. We would, in fact, clutch at any straw, in the shape of a bright line, in the hope of its yielding a true image of the coronal forms, and it was hoped that the recent eclipse would furnish evidence which would settle this question.

With regard to the second point, namely, the displacement of lines in the coronal spectrum. This is said to be equal to a velocity in the line of sight of 5 to 7 kilometres per second (I presume for the total difference of position of the line), say 3 kilometres for the speed of approach or recession at a distance from the solar limb equal to two-thirds of the diameter.

This is certainly a very striking result, and if confirmed by further study would in itself go far to prove the true coronal nature of the line measured. A displacement is conceivable, it is true, under certain conditions, on the assumption that the light is reflected chromospheric light, but this would not exceed a velocity of 1.87 kilometres, whilst the above result comes not far short of an angular rotation equal to that of the disk itself. A point at the distance named would, if rigidly connected with the sun, alternately approach and recede at a speed of about 4.35 kilometres per second.

It would be interesting to know, however, what are the limits of error in these measurements. I gather that a high dispersion was not employed, and it would seem, therefore, that a large uncertainty may be expected; supposing, for instance, that in the original negative the lines H and K are depicted 25 mm. apart, the total displacement corresponding to 7 kilometres per second will only amount to .09 mm.; an error, therefore, of $\frac{3}{32}$ mm., or $\frac{1}{128}$ of an inch (corresponding to over $1\frac{1}{2}$ kilometres) would materially affect the result; and to come within this limit would require unusually fine definition in the line measured.

In view of the novelty and great importance of the conclusions arrived at by the leader of the French eclipse expedition to Senegal, students of solar physics will await with keen interest, not to say impatience, the publication of a full detailed discussion of the results obtained.

J. EVERSHED.
Kenley, Surrey, July 2.

Lord Coleridge and Vivisection.

My attention has been called to a letter which the Lord Chief Justice has written in support of an endeavour which is being made by a section of the Society for Promoting Christian Knowledge to withdraw from circulation my little work "Our Secret Friends and Foes," recently published in their "Romance of Science Series." Until the Publication Committee of the

¹ It seems pretty certain, however, from the clearly-defined coronal "rings" seen by Prof. Lockyer and others at former eclipses by means of an objective prism, that a more or less uniform gaseous extension must exist far above the chromosphere and prominences; but is this the corona proper?