

Hungary, with existing Australian genera. Baron Ettingshausen himself is largely responsible for these identifications, which have been questioned "by certain critics insufficiently acquainted with the subject." He claims that he was supported in his views by such eminent palæontologists as Franz Unger and Oswald Heer. It is now some years since Unger published his sensational "Neuholland in Europa." In this little work almost every one of a set of Eocene fossil plants is identified with some essentially Australian genus, and often, we should add, on the very slenderest of material. The late Mr. G. Bentham, who, as is well known, handled and described every Australian plant of which specimens had been collected up to his time, disputed the correctness of the identifications, and endeavoured to prove that the remains might well be those of genera still found in the northern hemisphere; yet Baron Ettingshausen gives us to understand that Mr. Bentham confirmed his determination of a European fossil leaf as belonging to the genus *Dryandra*.

Quite recently the Marquis de Saporta has attacked Baron Ettingshausen's position, and the present pamphlet may be regarded as a reply. The author concludes with the statement that, to prevent misunderstanding, he wishes it to be known that any objections or criticisms will meet with no response from him, because he is convinced of the accuracy of his "facts," and his time is too valuable to enter upon superfluous discussion. Without discussing his "facts" one by one, and without actually denying their accuracy, we may say that the illustrations given are by no means convincing, as most botanists who have worked many years in herbaria on plants from all parts of the world, we believe, will agree. Few persons probably have paid so much attention to the venation and forms of leaves as Baron Ettingshausen, yet we find none of his determinations absolutely beyond doubt. So far as we are aware, not a single fruit of *Eucalyptus* or of the assumed *Proteacea* has been discovered in the European Tertiary formations. As to his leaves of *Eucalyptus*, they might be matched in the genus *Eugenia*, and we see no reason why any of the others are necessarily remains of species of Australian genera. W. B. H.

Is the Copernican System of Astronomy True? By W. S. Cassedy. (Standard Publishing Co., Kittanning, Pa., 1888.)

AN astronomer nowadays would find it a hard task to bring forth any facts which would throw doubt upon the truth of the Copernican theory, but it appears that there are still people amongst us who are bold enough to attack the strongholds of astronomy. Such attempts are always hopeless failures, and the one under notice is no exception. It is, indeed, doubtful whether the author knows what is meant by the Copernican system, for he goes so far as to suggest that the known diameter of the earth's orbit (assuming that it exists) should be used as a base-line for determining the distance of the sun! He also states that he has "found by experiment" that similar right-angled triangles have sides proportionate in length, though it is only fair to say that he is aware of the existence of the first book of Euclid, if not of the sixth.

We have already said enough to show that the book need not be considered seriously; but we cannot refrain from stating that the author, by sighting the sun along straight-edges at the equinoxes, has found that "the distance of the sun from the surface of the earth, at 40° N., is one million miles (p. 49)." This result is about as near the mark as could be expected from the method employed.

Naturalistic Photography. By P. H. Emerson, B.A., M.B. (London: Sampson Low, Marston, Searle, and Rivington, 1890.)

THE quick call for a second edition of this work indicates the approval with which it has been received, and we may

safely say there is not a better or more instructive book on the art principles of photography than the one before us. Dr. Emerson is a photographer of the first rank, his artistic compositions are everywhere admired, and the energetic manner with which many of the old and cherished ideas of the ordinary photographer are attacked and others established makes it very manifest that he only writes what he knows to be true. The literary style of the book is excellent, and the exposition has the merit of being strikingly original; it should, therefore, be studied by every photographer, both amateur and professional, who desires to excel in his art.

LETTERS TO THE EDITOR.

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Acquired Characters and Congenital Variation.

BEYOND this letter I cannot pursue my interpolated adversary, Mr. Dyer.

The syllogisms which he attributes to me are entirely his own. I willingly admit, therefore, that they are as ingeniously bad as they can well be.

I will now state shortly what my position was, and is:—

(1) The assumed antithesis between "acquired characters" and "congenital variation" has arisen out of the cult of Darwin as opposed to Lamarck.

(2) The theory of Lamarck fails, in my opinion, as much as the theory of Darwin, to give any adequate or satisfying explanation either of the genesis, or of the development, of organic forms.

(3) But the theory of Lamarck is more philosophical than the theory of Darwin, in so far as it seeks for, and specifies, a definite natural cause for the phenomena of variation.

(4) The theory of Darwin is essentially unphilosophical in so far as it ascribes these phenomena to pure accident, or fortuity.

(5) That Darwin himself, at one time, if not always, admitted this idea of fortuity to be a mere provisional resort under the difficulties of ignorance.

(6) That the later worshippers of Darwin depart, in this respect, from their master, and making the weakest part of his system the special object of their worship, have set up Fortuity as their idol.

(7) That it is under the influence of this superstition that they now seek to deny altogether that acquired characters can become congenital.

(8) That this denial is against the most familiar experience of Nature, and especially of artificial selection, which is the ante-type and foundation of the whole theory of evolution.

(9) That in all domestic animals, and especially in dogs, we have constant proof that many acquired characters may become congenital.

(10) That it is no answer to this argument to demand proof that the babies of a blacksmith are ever born with the abnormal arm-muscle of their papa.

(11) That in order to avoid and evade the force of innumerable facts proving that many acquired characters may, and do, become hereditary, fortuitists have invented a new verbal definition of what they mean by "acquired."

(12) That this definition is full of ambiguities and assumptions, concealed under plausible words, but the object of which is to limit the meaning of "acquired characters" to gross, visible, palpable changes affecting single individuals, and which the analogies of Nature do not lead us to expect or to suppose can be repeated in a single generation, even if a tendency to their development is really implanted in the race.

(13) That, still farther to render impossible the proof they demand, our fortuitists affix to their definition of the word "acquired," conditions which beg the whole question in dispute. Not only must the new characters be gross, palpable, visible—cases of "hypertrophy," of "extension," or of "thickening,"—but also they must be "obviously due to the direct physical action of the environment on the body of the individual." This is a condition which is irrational. It excludes