

and central Europe seem to have been more or less connected during and since the Cambrian period, with intermigrations of life-forms. This connection, with probable interruptions, appears to have continued down to the early centuries of the Quarternary. Also, from what little we know of the extinct animals and of the present relations of the plants and animals of the continents south of latitude 20° north, several observers have been led to suppose that these continents were more or less intimately united, and that possibly there were land connections with a former Antarctic continent. From maps, though naturally very hypothetical, published by de Lapparent and Koken, showing the probable distribution of land during the Middle Devonian, South America, Africa, southern Asia and Australia were possibly connected. Towards the end of the Carboniferous period there was probably a more or less continuous extent of land over what is now South America, Africa and Australia. This land connection between what are now separate continents appears to have persisted through the early Mesozoic age (Trias and Jura), though towards the end of the Jurassic Australia became widely separated by the Indian Ocean from Africa, while South America and Africa remained united. Our studies on the distribution of Neogæic and African (Ethiopian) Ceratocampidæ and two related families point to a connection in Cretaceous or early Tertiary times between Brazil and western Africa, thus bearing out the views of Ihering, Gill, Ortmann and others. The former connection of these Antarctogæic continents (whatever may be said of their possible connection with Antarctica) is borne out by the well-known facts in the distribution of certain terrestrial worms, land and fresh-water mollusks, insects, fresh-water fish, Dipnoi, *Peripatus*, am-

phibians, reptiles, birds and mammals. Our results also suggest that Africa south of the Sahara should properly be regarded as a zoological realm (for which the word *Afrogæa* is proposed), and not a dependence or region of *Arctogæa*.

C. JUDSON HERRICK,
Secretary.

AMERICAN SOCIETY OF ZOOLOGISTS,
CENTRAL BRANCH.

THE first annual meeting under the present organization was held at St. Louis, December 29 and 30. The following officers were elected for the ensuing year:

President—Professor C. H. Eigenmann.

Vice-President—Dr. S. J. Holmes.

Secretary and Treasurer—Professor F. R. Lillie.

Additional Members of the Executive Committee—One year, Professor G. A. Lefevre; two years, Professor T. G. Lee; three years, Professor Herbert Osborn.

The titles and abstracts of the papers presented appear together with those presented by Section F of the American Association for the Advancement of Science, in Professor Herrick's report printed above.

FRANK SMITH,
Secretary.

SCIENTIFIC BOOKS.

Evolution and Adaptation. By THOMAS HUNT MORGAN. The Macmillan Company. 1903. Pp. 470.

The modern evolutionist is obliged to confess, and somewhat painfully, that the processes connected with 'Darwinism' continue to receive different and conflicting explanations—this, too, in the face of a mass of documents which an ever-increasing number of investigators have been bringing together during the past decades. In token of this lack of concord in interpretation witness two volumes, not mere tracts, which have lately appeared. In the first of these, Plate,* following Darwinian

* 'Ueber die Bedeutung des Darwin'schen Selektionsprinzips und Probleme der Artbildung,' Zweite, vermehrte Auflage, 1903, Engelmann, pp. 247.

lines, does not hesitate to declare that 'for the harmony which exists between the living conditions of organisms and their morphological and physiological characters there is at the present time no other scientific explanation than natural selection.' In the second book, on the other hand, Morgan is firmly convinced that as a means of accounting for adaptations the doctrine of selection is largely to be discarded. Morgan represents the newer school of evolutionists, and from this standpoint his book has a timely value, for it is the first non-technical work of its kind. Whether he proves his case satisfactorily—for the work is *parti pris* throughout—is a question which each critic must answer. But all will agree that his work will find its place on the general bookshelves side by side with the volumes of Romanes, Lloyd Morgan, Spencer and Wallace.

Morgan, as he states in his preface, was early led to a belief in the inadequacy of natural selection from his studies upon regeneration, for this process often concerns itself with structures which play no part in strict Darwinism. The author now aims to explain some of these difficulties by mutations.* Thus, keeping in mind especially the results of De Vries, Morgan emphasizes the probability of mutations having occurred broadcast under infinite conditions of manner, degree, place and time. Such mutations produced series of creatures which were suited or unsuited to their particular locality, or were perhaps indifferent. Some, like *Kallima*, were even better adapted to their neighborhood than necessity for survival demanded; others were imperfectly adapted, but surviving in spite, *e. g.*, of bright colors, asymmetry or complicated reproductive processes. According to such a doctrine of mutations organic forms succeed one another kaleidoscopically, their variability becoming largely arrested between the definitely marked periods of change. Thus, if a species be compared to a facet upon which a polyhedron is balancing, the species, like the facet, may oscillate within definite limits.

* The Lamarckian hypothesis is given little importance: 'I am not sure that we should not be justified at present in claiming that the theory is unnecessary and even improbable.'

But if the limits of variation are transcended, the entire polyhedron changes its position and comes to equilibrium on a new facet. This plan of variation forms the undercurrent of Morgan's philosophy; through it he sees appearing structures, forms and habits, which have no 'selective value,' their purpose or utility may be partly or wholly *nil*, 'for we can not measure the organic world by measure of utility alone,' yet they appear as perfectly and as plentifully as the crystal-forms of snow. Granted then a galaxy of mutations some of them will fit their surroundings with marvelous accuracy. And with this in mind Morgan develops what he believes is a probable answer to many of the puzzles of shape and symmetry, the mutual adaptation of colonial forms, degeneration (those mutations only surviving which 'we may almost say, have been forced' into a parasitic environment, 'for these degenerate forms can only exist under such conditions'), coloration, life-length, regeneration, individual adjustments, secondary sexual characters and even of sex itself.

It is obviously impracticable for a reviewer to consider more than the barest outlines of a work which touches many fundamental viewpoints. Each of the dozen chapters of the present book contains enough to warrant separate reviews and reviewers. And each critic will find little trouble in pointing out some of the many 'pitfalls' referred to in Morgan's preface. For discussions in evolution have long since shown that facts may be read in different ways.

There are general features in which the present volume deserves warm praise, as in providing a mass of helpful examples, and in urging attractive arguments against many 'purposeful' or 'useful' variations, and above all things in considering critically the doctrine of sexual selection, furnishing against the latter—in spite of lack of reference to Cunningham's work—the most serious objections hitherto given. There are other features, however, in which improvement might well have been made. Regrettable is a general dearth of exact references—there are, in fact, not a dozen citations in all. And especially regrettable, in view of the scope of the

book, is the total absence of treatment of vitalism, which, as all admit, has become too troublesome a specter to be ignored in problems of adaptation. Regrettable is this omission, none the less, since there are few authors in a better position than Morgan to summarize the strongest arguments of the neovitalists. Altogether the book would have been more valuable, it seems to me, if the author had readjusted somewhat his themes; he might thus have included a discussion of vitalism, amplified his section on Naegeli's 'perfecting principle,' and given a more adequate account of orthogenesis, respects in which the work is not to be mentioned in terms of Plate's, and by the same token abridged his discussion of natural and artificial selection. As it stands, the work gives page after page quoted from Darwin—indeed, throughout the entire book there are over a hundred pages in quotation marks. This extended treatment of the classic aspects of selection, we freely admit, led us to the false hope that the newer theme of organic selection, as set forth by Osborn, Lloyd Morgan and Baldwin, would at least be given definite reference.

As already remarked, each reviewer of a work of the present broad scope will find abundant ground for criticism. The most formidable and most pertinent discussion might easily arise over mutations themselves. Morgan, like Korschinsky and De Vries, holds that mutations are saltations by which new 'modes' are established in organisms, while variations, in the sense of the usual selectionist, are changes occurring about the same 'mode.' But we find that Morgan is willing to go further, and embrace under the term-mutation all discontinuous variations. And if this point is granted, I confess that I can not see that his standpoint is widely different *in esse* from that of the rank and file of latter-day selectionists. For after all discontinuity in variations is a question of degree, and it would not be a serious matter to show that transitions occurred between discontinuous to continuous variations. A case, one which I happen at the present moment to be interested in, occurs in the development of *Chimæra*. In the fertilization of this form supplemented

spermheads, as they pass into the egg, divide at once amitotically. And we have thus what appears at first sight a distinct saltation from the usual conditions in polyspermy. Comparing, however, *Chimæra* with the conditions in the allied sharks, we find that this peculiar behavior of the sperm nuclei is not a feature which has arisen as a discontinuous variation; it represents nothing more nor less than an abbreviated process of what occurs in the more primitive sharks; in these forms amitosis in sperm nuclei appears at the end of a graded series of nuclear divisions, a series at one end of which mitosis occurs, and at the other amitosis. In the development of *Chimæra* the earliest stage of the sperm nucleus thus corresponds to a late stage in the sperm nucleus of sharks, and thus we conclude that the discontinuous character in *Chimæra* is not a new 'mode,' but a modified phase of a simple continuous process. This example, be it understood, occurs not immediately between offspring and parent, but, like 'mutations' in paleontology, between offspring and early ancestor, but it seems to me that a typical mutation differs from it in degree rather than in substance, and that similar processes may combine to form the complex of wonderfully adjusted discontinuous variations which we call a typical mutation. I incline to the belief that, in the elaboration of the doctrine of mutations, Morgan, like some other transmutationists,* keeps too prominently in the foreground not the strands which make up the complicated web of adaptation, but the mutant as a whole, picturing not the few details of structure which our present knowledge enables us to grasp, but a progression of brilliant, perfectly formed organisms, delicate in internal adjustments and *new*, different from their parents, indeed, even from their earliest stages, a picture which, as Morgan says of Naegeli's progression 'has a grandeur that appeals directly to the imagination.' On the other hand, admitting that discontinuous vari-

* Morgan substitutes 'transmutation' for 'evolution,' adjusting the term more closely, therefore, to his interpretation of the process. If terms are to be shifted, why should not the Lamarckian 'transformism' be revived?

ations ('mutations') are in themselves but adaptations of continuous variations, and bearing always in mind that extreme mutations are numerically rare, and, in our present knowledge, obscure as to fate, I take it that we are hardly in a position to give them supreme importance in the economy of species-building. Of extreme interest they are, none the less, and from theoretical standpoints, they are worthy of the most painstaking research, and are not to be discredited as mere 'freaks' or 'sports' important only in the praxis of gardening, as Plate concludes. There is, furthermore, a feature of mutations which has, it seems to me, never been adequately considered, *i. e.*, the definiteness of their characters, a feature which tells rather in favor of orthogenesis than the less definite interpretation which Morgan supports. For in all mutations—we refer to typical cases—the creature which appears is constant to a remarkable degree; the peacock mutant is the black-japanned, never the yellow- nor the red-, and even when many mutants appear 'simultaneously' they are surprisingly constant in characters. The word orthogenesis, by the way, does not occur in the index, and in similar instances we find that the index is troublesomely brief.

It is, I conclude, as a postulate of his doctrine of mutations that Morgan attacks our venerable recapitulation (or, as he prefers to call it, *repetition*) theory. For when a mutation does occur it appears literally *ab ovo*, although the author does not commit himself as to the exact point at which the '*presto change*' occurred, whether in fertilization or segmentation, but, if I understand him aright, it occurred during earliest development; nor does he say concretely whether all mutants date from an identical stage. But the drift of his remarks on recapitulation leads us to infer that they do, for otherwise this chapter seems to lose its point. For, outgrowing the days of Oken, no author of good repute has maintained that the *adult* structures of ancestors are still present in embryos, nor that the embryo chick resembles an *adult* reptile, nor even that the embryo of one species is ever exactly identical with the embryo of the corresponding stage

of the nearest species. We might even go further and assert that no modern zoologist has maintained that even within the same species any individual is absolutely identical with any other individual at any corresponding stage, early or late, in growth. Even the enthusiast who substituted a cut of an embryonic dog for one of man, and cheerfully admitted the imposture on the ground that the figures were equivalent, has never been charged with believing that the stages *were* equivalent, but only with crudely illustrating an elementary text-book, and with unscientific levity. Morgan's therefore must imply something more than the doctrine of von Baer—which was to the effect that chick embryos resemble (he does not say are absolutely identical with) embryos of lizards, and that the stages of chick and lizard correspond during a longer period of development than do embryos of chick and fish. And I have gathered what I believe is his meaning only piecemeal, through his references to larval forms, his assertion that 'jumps, or short cuts, of the developmental process are unknown in the physiological process of development,' and his quotations regarding the early appearance of mutational characters in the development of birds and dogs. He entirely fails to appreciate, it seems to me, the part played by adaptation at all stages of growth.

A final word regarding useful variations. Morgan maintains (and we believe that the majority of zoologists are in sympathy with such a view as opposed to Wallace's) that many 'useful' characters in organisms are useless and accidental, even although, *a priori*, the case appears adapted to purpose in marvelous detail. We smile at the silhouetted skull on the back of a moth, or the head of the drowned Taira warrior on the carapace of a Japanese crab, or the profile of a Scandinavian face on the 'earbone' of a Norwegian whale, but are these complicated coincidences more accidental than some of the 'purposeful' variations which we accept rather on the faith of coincidence than upon actual proof of utility? Facts are clearly what we need before we can assert that definite characters or variations are useful—but it is equally true

that facts must be cited before many of the stock examples of 'useful adaptations' can be cast out. And from this standpoint a number of Morgan's examples of non-useful adaptation fail to convince. Thus, why may not the different colors in the mouths of the male and female hornbills be useful in sexual selection? Morgan assures us that these differences in the colors are useless since they can not be seen, but on the other hand, from what we know of the habits of huge-billed birds, it is quite possible that during nuptial antics the bills are widely open. Again, Morgan admits that the green color of a frog is probably useful, but believes useless the black pigment lining the body cavity. He does not show that it is useless, in spite of our lingering suspicion that a black screen behind a thin body wall might well be useful in collecting warmth, or even in protecting from light delicate viscera. So, also, we are not convinced that gray hair and retreating chin are altogether useless organs, for it is quite possible that there is some foundation for the popular belief that they are adverse to sexual sentiment, and may thus, after all, play a useful part in selection.

BASHFORD DEAN.

SCIENTIFIC JOURNALS AND ARTICLES.

The Journal of Physical Chemistry, November. 'History of the Water Problem' (Mrs. Fulhame's theory of catalysis), by J. W. Mellor. A sketch of Mrs. Fulhame's 'Essay on Combustion,' published in 1794, in which appears the first clear statement of the influence of water on chemical transformations. 'An Apparatus for the Electrolytic Determination of Metals, Using a Rotating Cathode,' by E. S. Shepherd. By this means the copper in chalcopyrite was determined electrolytically in from twenty-five to forty minutes. 'Solubility of Calcium Sulfate in Aqueous Solutions of Sulfuric Acid,' by F. K. Cameron and J. F. Breazeale; 'The Solubility of Magnesium Carbonate in Aqueous Solutions of Certain Electrolytes,' by F. K. Cameron and A. Seidell. *December.* 'Action of Sodium and Potassium Amalgams on Various Aqueous Solutions,' by Gustave Férenkes; 'The Rate of Formation of Iodates

in Alkaline Solutions of Iodin,' by E. L. C. Forster; 'Iron Salts in Voltameter Solutions,' by J. M. Bell.

SOCIETIES AND ACADEMIES.

THE WASHINGTON ACADEMY OF SCIENCES.

THE annual meeting of the Washington Academy of Sciences was held on Wednesday evening at the Cosmos Club and the following officers were elected for the ensuing year:

President—Charles D. Walcott.

Vice-Presidents—From the Anthropological Society, D. S. Lamb; Archeological Society, J. W. Foster; Biological Society, B. W. Evermann; Botanical Society, F. V. Coville; Chemical Society, F. W. Clarke; Entomological Society, W. H. Ashmead; Geographic Society, A. Graham Bell; Geological Society, G. K. Gilbert; Historical Society, W. J. McGee; Medical Society, C. W. Richardson; Philosophical Society, C. F. Marvin.

Secretary—Frank Baker.

Treasurer—Bernard R. Green.

Managers: 1905—L. O. Howard, O. H. Tittmann, Carroll D. Wright; 1906—C. W. Hayes, G. W. Littlehales, C. Hart Merriam; 1907—Geo. M. Kober, Gifford Pinchot, F. A. Lucas.

PHILOSOPHICAL SOCIETY OF WASHINGTON.

At the 33d annual meeting, December 19, 1903, Professor C. F. Marvin, of the Weather Bureau, was elected president; Messrs. Abbe, Hagen, Littlehales and Day, vice-presidents; Mr. B. R. Green, treasurer; Messrs. Hayford and Wead, secretaries, and Messrs. De Caindry, Paul, Winston, Bauer, Briggs, Fischer, Harris, Rosa and Abbot as members of the general committee; on this committee are also *ex officio* Past Presidents Dall, Walcott, Rathbun and Gore.

The secretaries' and treasurer's reports showed the society to be in a prosperous condition.

THE 577th regular meeting was held January 2, 1904, President Marvin in the chair.

Mr. F. G. Nutting presented by invitation a paper on 'The Electron Theory of the Radiation of Gases,' pointing out how this theory explains various peculiarities in the spectra of gases.

Mr. C. G. Abbot then described work of the past two years at the Smithsonian Astrophys-