PROCEEDINGS

OF THE

BIOLOGICAL SOCIETY OF WASHINGTON

VOLUME VI.

FEBRUARY 8, 1890, TO DECEMBER 26, 1891.

WASHINGTON, D. C.
PRINTED FOR THE SOCIETY.
1892

PUBLICATION COMMITTEE.

CHARLES D. WALCOTT, *Chairman*.

R. E. C. STEARNS,
F. H. KNOWLTON,
T. S. PALMER,
F. V. COVILLE.

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LIST OF THE OFFICERS AND COUNCIL

OF THE

BIOLOGICAL SOCIETY OF WASHINGTON.

ELECTED JANUARY 10, 1891.

OFFICERS.

PRESIDENT.

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STANDING COMMITTEES—1891.

Delegates to the Joint Commission of Scientific Societies of Washington.

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LESTER F. WARD, *Chairman.*

GEORGE VASEY,
F. H. KNOWLTON,
THEODORE HOLM,
F. V. COVILLE.

* Ex-Presidents of the Society.

PROCEEDINGS.

One Hundred and Fifty-fifth Meeting,
February 8, 1890.

The President in the chair, and thirty-eight members and guests present.

The following active members were elected on recommendation of the Council: T. W. Stanton, U. S. Geological Survey; Dr. E. Roome, Columbian University.

Dr. Frank Baker presented a communication upon An Undescribed Muscle from the Infraclavicular Region of Man. Discussed by Drs. Merriam and Riley.

Mr. C. D. Walcott presented a note upon A New Genus and Species of Ostracod Crustacean from the Lower Cambrian.*

Dr. Cooper Curtice read a paper on The Moultings of the Cattle Tick (*Ixodes bovis*).† Discussed by Drs. Riley and Theobald Smith.

Prof. Lester F. Ward spoke of Flowers that Bloom in the Winter Time.

One Hundred and Fifty-sixth Meeting,
February 22, 1890.

The President in the chair, and thirty members present.

Mr. F. V. Coville presented a paper on The New Arrangement of Genera in the Herbarium of the Department of Agriculture.‡ Discussed by Messrs. Riley and Ward.

* Tenth Annual Report of the Director U. S. Geol. Survey, Pt. I, 1890, pp. 625, 626.

† Journal Comp. Med. and Vet. Archives 1890, p. 313; reprinted in Veterinarian 1891, p. 680.

‡ Bot. Gazette, xv, 1890, p. 68.

Dr. T. H. Bean presented NOTES ON SOME FISHES FROM BRITISH COLUMBIA.*

Dr. Merriam read a paper on the EVIDENCE OF SONORAN ORIGIN OF THE FLORA AND FAUNA OF THE GULF STATES. Discussed by Messrs. Ward, Gilbert, Coville, and Dall.

ONE HUNDRED AND FIFTY-SEVENTH MEETING, March 8, 1890.

The President in the chair, and forty-one members present.

Mr. B. T. Galloway read a paper giving the results of some observations on an apple disease caused by the fungus *Gymnosporangium macropus*. Discussed by Messrs. Van Deman and Marlatt.

Mr. C. L. Hopkins presented some NOTES ON THE ANIMAL LIFE ABOVE THE SNOW LINE ON MT. SHASTA.† Discussed by Messrs. Mann, Riley, Howard, Diller and Hasbrouck.

Dr. W. H. Dall read a paper entitled ON DYNAMIC INFLUENCES IN EVOLUTION.‡ Discussed by Messrs. Coville, Mann, Fernow and Ward.

ONE HUNDRED AND FIFTY-EIGHTH MEETING, March 22, 1890.

The President in the chair, and thirty-nine members present.

Dr. C. H. Stowell was elected an active member.

Dr. D. W. Prentiss read a paper entitled CHANGE IN THE COLOR OF HUMAN HAIR,§ CHANGE IN THE COLOR OF PLUMAGE OF BIRDS, AND IN THE FUR OF MAMMALS.||

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* Proceedings U. S. Nat. Mus., Vol. xii, pp. 641, 642.

† Insect Life, Vol. 2, p. 355.

‡ Proc. Biological Society, Vol. vi, pp. 1–10.

§ Phila. Med. Times, xi, 1880–81; also, Therap Gazette, Detroit, Mich., 1889, viii.

|| Journal Amer. Med. Assoc. xiii, 1889.

Prof G. B. Goode read a paper on the COLORS OF FISHES.*

Prof. C. V. Riley spoke on the COLORS OF INSECTS.

The papers were briefly discussed by Messrs. Mann, Dall, Seaman and Ward.

ONE HUNDRED AND FIFTY-NINTH MEETING.
April 5, 1890.

The President in the chair, and twenty-five members present.

Dr. Theobald Smith read a paper entitled SOME ILLUSTRATIONS OF FERMENTS AND FERMENTATION AMONG BACTERIA.† Discussed by Professor Atwater.

Dr. R. R. Gurley presented a paper on the AMERICAN GRAPTOLITES.

ONE HUNDRED AND SIXTIETH MEETING,
April 19, 1890.

The President in the chair, and forty-five members present.

Dr. C. W. Richardson was elected an active member of the Society.

Dr. Merriam presented a paper entitled HISTORICAL REVIEW OF THE FAUNAL AND FLORAL DIVISIONS THAT HAVE BEEN PROPOSED FOR NORTH AMERICA. Discussed by Dr. Dall.

Prof. J. F. James read a paper entitled VARIATION WITH ESPECIAL REFERENCE TO CERTAIN PALEOZOIC GENERA. Discussed by Professor Ward.

Dr. Dall exhibited some ORIGINAL DRAWINGS OF THE FUR SEAL AND STELLERS' SEA COW,‡ and gave an account of them.

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* Trans. Am. Fisheries Society, 1890.

† Centralblatt für Bakteriologie u Parasitenkunde, vii, 1890, p. 502.

‡ Report U. S. Coast Survey and Geodetic Survey, 1890.

ONE HUNDRED AND SIXTY-FIRST MEETING,
May 3, 1890.

The President in the chair, and thirty-six members present.

Mr. J. R. Edson was elected an active member.

Dr. Robert Reyburn read a paper entitled THE LIFE HISTORY OF MICRO-ORGANISMS WITH ITS RELATION TO THE THEORY TO EVOLUTION.* Discussed by Dr. Theobald Smith, Dr. Salmon, Mr. True, Mr. Erwin Smith, Dr. Shaeffer, Mr. Seaman and Dr. Dall.

Dr. Vasey read a paper entitled A NEW GRASS GENUS.† Discussed by Messrs. Holm, Coville and Prof. Ward.

ONE HUNDRED AND SIXTY-SECOND MEETING,
May 17, 1890.

The President in the chair, and thirty-six members present.

Mr. T. S. Palmer read a paper entitled SOME EARLY VIEWS OF GEOGRAPHICAL DISTRIBUTION OF SPECIES. Discussed by Dr. Merriam.

Mr. F. W. True exhibited a specimen of *Lophiomys imhausii*. Discussed by Dr. Merriam.

Prof. W. H. Seaman read a paper entitled THE PLACE OF BIOLOGY IN THE PUBLIC SCHOOLS.‡ Discussed by Profs. Chickering and Ward, Dr. Baker and Messrs. Waite and Howard.

ONE HUNDRED AND SIXTY-THIRD MEETING,
May 31, 1890.

The President in the chair, and twenty-six members present.

Dr. C. H. Merriam exhibited specimens of sundry new species of NORTH AMERICAN MAMMALS. Discussed by Dr. Gill.

* American Monthly Microscopical Journal, June, 1890.

† Bot. Gazette, xv, 1890, p. 106.

‡ The American Anthropologist for October, 1891.

Dr. Theodore Gill presented a paper on the CHARACTERISTICS OF THE HALOSAUROID FISHES.*

Dr. J. N. Rose read a paper entitled *Coulterella*, A NEW GENUS OF COMPOSITÆ.† Discussed by Messrs. Seaman, Vasey and T. S. Palmer.

Prof. Jos. F. James spoke on ORGANISMS IN ST. PETER'S SANDSTONE.

Mr. B. P. Mann made some remarks upon the AUTHORSHIP OF THE BIBLIOGRAPHY OF ECONOMIC ENTOMOLOGY, published by Department of Agriculture.

Mr. Howard refuted Mr. Mann's statement.

ONE HUNDRED AND SIXTY-FOURTH MEETING, October 18, 1890.

The President in the chair, and twenty-eight members present.

Mr. H. E. Van Deman spoke of CULTIVATED FRUITS IN THE MOUNTAINS OF NORTH CAROLINA.‡ Discussed by Dr. Salmon.

Dr. Theo. N. Gill presented a communication on the SUPER FAMILY CYCLOPTEROIDEA.§

Prof. Lester F. Ward spoke on the subject of AMERICAN TRIASSIC FLORA.||

ONE HUNDRED AND SIXTY-FIFTH MEETING, November 1, 1890.

The President in the chair, and thirty-seven members present.

Mr. Nathan Banks was elected an active member.

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* American Naturalist, xxiii, 1889, pp. 1015, 1016. (Pub. May, 1890.)

† U. S. Cont. Nat. Herb. i, 1890, p. 71.

‡ Ann. Report U. S. Dept. Agriculture, 1890, pp. 410, 411.

§ Proc. U. S. N. M. xiii, 1890, pp. 361–376; pl. 28–30.

|| Geological Society of America, iii, 1891, pp. 23–31. Abstracts in Science, Vol. xviii, Nov. 20, 1891, pp. 287, 288, and Proc. A. A. A. S., Vol. xi.

Mr. F. V. Coville read a paper entitled FRUITING OF THE GINKGO AT THE DEPARTMENT OF AGRICULTURE. Discussed by Dr. Riley, Mr. Seaman and Mr. Fernow.

Dr. Marx spoke of his recent INVESTIGATIONS OF THE POISON GLANDS OF LATHRODECTUS. Discussed by Drs. Riley, Dall and Theobald Smith.

Prof. Jos. F. James read a paper called FUCOIDS AND OTHER PROBLEMATIC ORGANISMS. Discussed by Mr. Lucas, Dr. Dall, Prof. Ward and Mr. Mann.

ONE HUNDRED AND SIXTY-SIXTH MEETING,
November 15, 1890.

The President in the chair, and forty-three members present.

Dr. Merriam presented a communication entitled LIFE IN THE LAVA BEDS AND CAÑONS OF SNAKE RIVER, IDAHO, IN OCTOBER.* Discussed by Messrs. Walcott and Howard.

Mr. Theodore Holm spoke of the VEGETATIVE PROPAGATION OF DICENTRA CUCULLARIA.†

Dr. Dall presented some PALEONTOLOGICAL NOTES FROM THE NORTHWEST COAST.‡

Mr. Lucas described a FOOT DISEASE OF CAPTIVE BIRDS.

ONE HUNDRED AND SIXTY-SEVENTH MEETING,
November 29, 1890.

The President in the chair, and forty-seven members present.

The following new active members were elected: Jno M. Stedman, Merwin M. Snell and Rev. Alexis Orban.

Dr. T. H. Bean presented a paper on THE DEATH OF SALMON AFTER SPAWNING.§ Discussed by Dr. Gill, Prof. Goode, Mr. Stejneger, Drs. Dall and Merriam.

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* (In part.) North American Fauna No. 5, July, 1891, pp. 6–7.

† Bull. Torrey Botanical Club, Vol. xviii, 1891, pp. 1–11, pl. cxi-cxiii.

‡ Nautilus, Philadelphia, Vol. iv, 1890, No. 8, pp. 87–89, December.

§ Forest and Stream, November 27, 1890.

Dr. Theobald Smith spoke ON SPECIES AMONG BACTERIA. Discussed by Mr. True, Dr. Gill, Drs. Riley, Curtice, Mr. Banks.

Mr. Sudworth presented a communication entitled NOTES ON NOMENCLATURE. Discussed by Dr. Merriam.

ONE HUNDRED AND SIXTY-EIGHTH MEETING.
December 13, 1890.

The President in the chair, and twenty-five members present.

Mr. A. B. Baker was elected an active member.

Mr. Wm. Palmer read a paper on THE OCCURRENCE OF THE ASIATIC CUCKOO ON THE PRIBYLOV ISLANDS. Discussed by Dr. Dall.

Dr. Riley presented some NEW NOTES ON THE GENUS PHYLLOXERA.

Mr. True spoke on THE TEETH OF THE MUSKRAT.

Mr. Lucas read a paper on THE WING OF METOPIDIUS. Discussed by Mr. True.

ONE HUNDRED AND SIXTY-NINTH MEETING,
December 27, 1890.

The President in the chair, and twenty-six members present.

The following active members were elected: J. M. Holzinger, Frederick C. Test.

Dr. Cooper Curtice presented a communication entitled A PRELIMINARY STUDY OF TICKS IN THE UNITED STATES. Discussed by Dr. Smith.

Dr. C. Hart Merriam exhibited A NEW RABBIT FROM THE SNAKE PLAINS OF IDAHO.*

Dr. W. H. Dall read a paper entitled ON THE TOPOGRAPHY OF FLORIDA WITH REFERENCE TO ITS BEARING ON FOSSIL FAUNAS.†

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* (*Lepus idahoensis*) North American Fauna No. 5, July, 1891, pp. 76–78.
† Bulletin 84, U. S. Geol. Survey. (In press.)

One Hundred and Seventieth Meeting. (Tenth Annual.)

The President in the chair, and forty-two members present.

The annual reports of the Secretary and Treasurer were read and accepted.

The following board of officers was elected for the ensuing year:

President—C. Hart Merriam.

Vice-Presidents—C. V. Riley, Frank Baker, Richard Rathbun, and C. D. Walcott.

Secretaries—L. O. Howard and F. A. Lucas.

Treasurer—F. H. Knowlton.

Members of Council—F. W. True, T. H. Bean, R. E. C. Stearns, Theobald Smith, and Geo. Vasey.

One Hundred and Seventy-first Meeting, January 26, 1891.

(Eleventh Anniversary Meeting.)

The eleventh anniversary meeting was held in the law lecture-room of Columbian University, January 26, 1891, in the presence of members and invited guests.

The President, Lester F. Ward, delivered his annual address on the subject Neo-Darwinism and Neo-Lamarckism.*

One Hundred and Seventy-second Meeting, February 7, 1891.

The President in the chair, and twenty-five members present.

The President announced the following committees for the ensuing year:

Joint Commission—C. Hart Merriam, Lester F. Ward, Richard Rathbun.

* Published in this volume. See pp. 11–71.

Committee on Communications—Richard Rathbun, Walter B. Barrows, John Murdoch.

Committee on Publications—C. D. Walcott, R. E. C. Stearns, F. H. Knowlton, T. S. Palmer, F. V. Coville.

Committee on Trees and Shrubs—Lester F. Ward, Geo. Vasey, F. H. Knowlton, Th. Holm, F. V. Coville.

Mr. C. D. Walcott presented a paper on the DISCOVERY OF VERTEBRATE LIFE IN LOWER SILURIAN (Ordovician) STRATA.* Discussed by Dr. Gill.

Prof. Henry F. Osborn gave a REVIEW OF THE DISCOVERY OF CRETACEOUS MAMMALS.† Discussed by Dr. Gill.

ONE HUNDRED AND SEVENTY-THIRD MEETING,
February 21, 1891.

The President in the chair, and twelve members present.

Dr. Cooper Curtice read a paper entitled SOME LITTLE KNOWN WORMS IN CATTLE. Discussed by Mr. Holzinger.

ONE HUNDRED AND SEVENTY-FOURTH MEETING,
March 7, 1891.

The President in the chair, and twenty-five members present.

Mr. F. A. Lucas exhibited and described some young HOATZINS. Discussed by Dr. Dall.

Mr. Lucas also drew attention to a SPECIMEN OF BISON LATIFRONS FROM PEACE CREEK, FLORIDA.

Dr. Bean spoke of the FISHES OF GREAT SOUTH BAY.‡

Mr Rose spoke of A NEW ASTER FROM SOUTHERN CALIFORNIA.§ Discussed by Mr. Holzinger and Mr. Waite.

*Bull. Geol. Soc. Amer. Vol. iii, 1891 (in press). See also Proc. Phila. Acad.

† American Naturalist, xxv, July, 1891, pp. 295-611. See also Proc. Phila. Acad.

‡ Nineteenth Report of the Commission of Fisheries of New York, 1891, pp. 237–281.

§ Bot. Gazette, Vol. xvi, 1891, p. 113.

Mr. Sudworth presented a communication on the COLOR AND ODOR OF FLOWERS IN ATTRACTING INSECTS. Discussed by Messrs. Howard and Marlatt.

Mr. Stedman exhibited and described a fine specimen illustrating the EMBYRO OF A CHICK WITH TWO PROTOVERTEBRÆ.

Dr. Merriam spoke of DISTRIBUTION OF ANIMAL AND VEGETABLE LIFE. Discussed by Dr. Curtice and Messrs. Waite, Test and Holzinger.

ONE HUNDRED AND SEVENTY-FIFTH MEETING,
March 21, 1891.

Ex-President Ward in the chair, and twenty-eight members present.

Mr. W. A. Taylor was elected an active member.

Dr. Dall presented a paper on the AGE OF THE PEACE CREEK BONE BEDS IN FLORIDA.*

Dr. Shufeldt read a communication on A COLLECTION OF FOSSIL BIRDS FROM THE EQUUS BEDS OF OREGON.†

Mr. F. A. Lucas spoke of A POINT IN THE ANATOMY OF HESPERORNIS.

Mr. F. H. Knowlton presented a communication entitled WHAT ARE CYPRESS KNEES? Discussed by Prof. Ward and Mr. R. L. Garner.

ONE HUNDRED AND SEVENTY-SIXTH MEETING.
April 4, 1891.

Vice-President Walcott in the chair, and thirty-two members present.

Dr. Cornelius B. Boyle was elected an active member of the Society.

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* Bulletin 84, U. S. Geol. Survey. (In press).

† To be published in the Acad. of Nat. Sciences of Phila. Abstracts <Amer. Nat. Sept. 1891, pp. 818–821. The Auk, Vol. viii, Oct., 1891, pp. 365–368.

Dr. Bean read a paper on KENNERLY'S SALMON.* Discussed by Dr. Dall, Mr. C. D. Walcott and Mr. Waite.

Dr. Theobald Smith made some REMARKS ON RECENT BACTERIOLOGICAL PROGRESS IN THE PREVENTION AND CURE OF DISEASE.

Dr. V. A. Moore spoke of the PRODUCTION OF IMMUNITY IN GUINEA PIGS WITH STERILIZED CULTURES OF HOG CHOLERA BACILLUS. Discussed by Drs. Curtice, Schaeffer and Salmon, and Mr. Walcott.

Mr. E. M. Hasbrouck read a MONOGRAPH OF THE CAROLINA PARRAKEET.†

ONE HUNDRED AND SEVENTY-SEVENTH MEETING,
April 18, 1891.

Vice-President Riley in the chair, and twenty members present.

Prof. L. H. Dewey, was elected an active member of the Society.

Mr. Van Deman presented a communication on THE RECENT INTRODUCTION OF DATE PALMS.‡

Dr. Curtice read a paper entitled PRACTICAL VALUE OF INVESTIGATING PARASITES OF LIVE STOCK.

Prof. Lester F. Ward spoke of SOME FLORIDA PLANTS.

ONE HUNDRED AND SEVENTY-EIGHTH MEETING,
May 2, 1891.

Vice-President Riley in the chair, and twenty-eight members present.

Dr. Gill spoke of the CLASSIFICATION OF THE APODAL FISHES.§ Discussed by Mr. Lucas.

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* Forest and Stream, July 9, 1891.

† The Auk, Oct., 1891, pp. 369–379.

‡ Ann. Report, U. S. Dept. of Agriculture, 1890, p. 415.

§ Proc. U. S. Nat. Museum, Vol. xiii, 1890, pp. 157-242.

Mr. Galloway read a paper entitled RECENT PROGRESS IN THE STUDY OF PLANT DISEASES. Discussed by Dr. Erwin F. Smith.

Dr. Frank Baker presented some NOTES ON DWARFS. Discussed by Messrs. Ward, Gill and Riley.

Mr. F. A. Lucas read a paper, by Mr. Chas. Hallock, entitled DISTRIBUTION OF FISHES BY UNDERGROUND WATER COURSES.* Discussed by Dr. Gill.

Mr. F. C. Test presented some NOTES ON THE DENTITION OF DESMOGNATHUS.

ONE HUNDRED AND SEVENTY-NINTH MEETING,
May 16, 1891.

Vice-President Riley in the chair, and twenty-two members present.

Prof. Riley read a paper on THE MEXICAN ARROW WEED AND JUMPING BEAN.† Discussed by Dr. J. N. Rose, Prof. Ward, and Dr. Vasey.

Mr. J. M. Holzinger read a paper entitled INCENTIVES TO NATURAL HISTORY STUDY.

Mr. Wm. Palmer described THE DISTRIBUTION OF CERTAIN MAMMALS, BIRDS AND PLANTS ON THE PRIBYLOV ISLANDS.

Dr. Vasey read some NOTES ON THE RECENT FIELD WORK OF THE BOTANICAL DIVISION OF THE DEPARTMENT OF AGRICULTURE.

Mr. Lucas presented some REMARKS ON A NEW TORTOISE FROM THE GALAPAGOS ISLANDS.‡

ONE HUNDRED AND EIGHTIETH MEETING,
October 17, 1891.

The President in the chair, and fifty-three members present.

Mr. W. T. Swingle was elected an active member of the Society.

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* Amer. Angler.

† Scient. Amer., June 13, 1891; Amer. Nat., July, 1891, pp. 673–675.

‡ Nature, June 4, 1891, p. 113.

Mr. F. V. Coville read a paper entitled FOOD PLANTS OF THE INDIANS OF THE DEATH VALLEY REGION. Discussed by Mr. Van Deman, Dr. Merriam, and Dr. Vasey.

Dr. R. W. Shufeldt presented some NOTES ON PALEOPATHOLOGY.* Discussed by Mr. Lucas and Mr. Gilbert.

Mr. Wm. Palmer read a paper on THE FATE OF THE FUR SEAL IN AMERICAN WATERS.† Discussed by Dr. Dall, Dr. Merriam, Mr. Lucas, Dr. Bean, and Dr. Schaeffer.

ONE HUNDRED AND EIGHTY-FIRST MEETING,
October 31, 1891.

The President in the chair, and thirty members present.

The following were elected active members of the Society: Mr. A. G. Masius, Prof. B. W. Everman, Dr. W. F. Morsell, Dr. C. W. Stiles.

Dr. Theo. Gill presented a communication on THE CLASSIFICATION OF THE TETRAODONTOIDEA.‡

Dr. T. H. Bean made some remarks on SOME FISHES NEW TO NEW ENGLAND WATERS.§ Discussed by Dr. Gill and Mr. Barrows.

Mr. W. B. Barrows read a paper on CUCKOO STOMACHS AND THEIR CONTENTS.

Dr. N. H. Egleston read a paper on THE TEMPERATURE OF TREES.

Dr. C. W. Stiles presented some NOTES ON PARASITES—THE DEVELOPMENT OF ECHINORHYNCHUS GIGAS.‖ Discussed by Mr. Howard and Dr. Merriam.

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* The Popular Science Monthly, 1892.

† Forest and Stream, Oct. 29, 1891.

‡ Proc. U. S. Nat. Museum, xiv, 1891.

§ Forest and Stream, Dec. 17, 1890.

‖ American Journal for Comp. Med. and Vet. Archives, Dec., 1891. French translation in Comp. rend. de la Soc. de Biologie, Paris, 1891, pp. 764-766.

One Hundred and Eighty-second Meeting, November 14, 1891.

The President in the chair, and thirty-one members present.

Mr. T. S. Palmer read a paper entitled The Winter Aspects of the Mojave Desert Region.

Dr. V. A. Moore spoke of a case of Echinococcus in Swine.* Discussed by Drs. Smith and Stiles.

Dr. C. W. Stiles presented a paper entitled Notes on Parasites, describing *Coccidium bigeminum* and *Filaria gasterostei*.†

Prof. Lester F. Ward spoke of Haeckel's Radiolaria of the Challenger Expedition, and presented a communication entitled Three Days in the Tropics. The latter was discussed by Dr. Merriam and Mr. Coville.

One Hundred and Eighty-third Meeting, November 28, 1891.

The President in the chair, and thirty-six persons present.

The following active members were elected on recommendation of the Council:

Mr. W. E. Clyde Todd and Mr. J. D. Figgins.

Dr. George Marx read a paper on The Structure and Construction of the Geometric Spider Web. Discussed by Dr. Th. Gill.

Mr. David White presented a paper entitled Some Peculiar Forms in an Upland Carboniferous Flora.‡ It was discussed by Prof. Lester F. Ward.

Prof. F. H. Knowlton presented a paper on Fruiting Ferns from the Laramie Group. Discussed by Professor Ward, Mr. C. D. Walcott and Dr. Cooper Curtice.

* Amer. Journal of Comp. Med. and Vet. Archives and Annual Report of the Dept. of Agriculture, 1891. Report on Animal Parasites for 1891, C. W. Stiles.

† Proc. U. S. Nat. Museum, 1892. Note preliminarie sur quelques Parasites, Bull. d. l. Soc. Zool. d France, 1891, pp. 163-165.

‡ Bulletin 98, U. S. Geological Survey. (In press.)

One Hundred and Eighty-fourth Meeting,
December 12, 1891.

The President in the chair. and thirty-eight members present.

Mr. F. V. Coville read a paper entitled A Review of Kuntze's Revisio Generum Plantarum. Discussed by Mr. J. N. Rose and Dr. C. Hart Merriam.

Mr. E. M. Hasbrouck presented some Remarks on Dichromatism. Discussed by Dr. W. H. Dall, Dr. Erwin F. Smith, Dr. C. Hart Merriam, and Mr. F. W. True.

Prof. Lester F. Ward spoke of Recent Discoveries of Potomac Fossil Plants near Washington.

One Hundred and Eighty-fifth Meeting,
December 26, 1891.

The President in the chair, and twenty-six members present.

Mr. F. H. Knowlton presented a paper entitled A Fossil Bread-Fruit Tree from the Sierras of California. Discussed by Prof. Ward.

Prof. Lester F. Ward presented a communication entitled Alphonse de Candolle on the Transmission of Acquired Characters.

Dr. C. Hart Merriam made extended Remarks on the Affinities of the North American Squirrels, Chipmunks, Spermophiles, Prairie Dogs, and Marmots. Discussed by Dr. Erwin F. Smith, Dr. Stiles, Dr. Dall, and Professor Ward.

ON DYNAMIC INFLUENCES IN EVOLUTION.

By Wm. H. Dall.*

It is generally admitted that in the doctrine of Natural Selection we have a theory which accounts for the perpetuation of favorable variations in organic beings and their progeny, and for the elimination in the long run of those which vary in unfavorable directions. It is equally admitted that the origin of variation is not accounted for by this theory. In order to round out our conception of the mode of evolution of the organic universe it is necessary that this deficiency should be supplied, and that to it should be added some conception of the mode by which variation is sustained in any given direction until it has reached a point where its usefulness is sufficiently marked to enable the selective process to operate. Besides this it is hardly doubtful that there are many characters developed in organisms, especially those of the lower rank, in which selection of any sort is but little concerned.

It is not necessary to recapitulate the names of those who have turned to the relations between the organism and its environment as the only nidus of the influences sought. Such an enumeration would comprise nearly all American biologists of prominence and many foreign naturalists.

On the other side of the Atlantic a small but not unimportant number of biologists, of whom Weismann and Laukester may be taken as spokesmen, have recently endeavored to show

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*Read before the Biological Society of Washington, Mar. 8, 1890.

that the current of hypothesis most favored in America, though not confined to our naturalists, is running in a wrong direction, although they do not seem to have any satisfactory alternative to offer.

For convenience in discussion those who accept the ideas referred to, in greater or less degree, may be termed Dynamic Evolutionists. Their position has been very fairly and temperately stated by Osborne in his article on the paleontological evidence of the transmission of acquired characters.* Without attempting to speak for others I have felt that a statement of the position to which I have been led by my own studies might not be without use in the present status of the question.

In the first place, in opposition to the notion that characters acquired in other than the embryonic or larval condition are not transmitted to the progeny ;—I maintain that a direct or indirect transmission of acquired characters is absolutely essential to any theory of evolution and that, speaking broadly, the whole system of Darwinism must stand or fall with this hypothesis. It is as axiomatic as the "survival of the fittest" itself.

It therefore becomes necessary to define what is meant by "acquired characters" and their "transmission."

The environment stands in a relation to the individual such as the hammer and anvil bear to the blacksmith's hot iron. The organism suffers during its entire existence a continuous series of mechanical impacts, none the less real because invisible, or disguised by the fact that some of them are precipitated by voluntary effort of the individual itself. So far as re-

*Nature Jan. 9, 1890, p. 227; Science, 1890, p. 110. The name Neo-Lamarckian is objectionable, as it tends to connect with the modern hypothesis the different and obsolete theory of the distinguished French naturalist.

sults are concerned, for the ground to strike the horse's hoof would be the same as for the horse to strike the ground with his hoof; direction and dynamic value of shock being assumed to be equal in the two cases. Since individual organisms usually appear free to wander about or remain quiescent, the idea that they are under constant stress does not ordinarily suggest itself. To this habit of superficial observation I ascribe the slowness with which the dynamic element in evolution has received recognition, though pointed out clearly so long ago, by Herbert Spencer.

That which distinguishes the organic individual from the inorganic fragment of matter is the complexity of its reäction to these impacts, which reäction we term physiological in contra-distinction to the simply mechanical, though both, at bottom are doubtless similar.

The characters which develop in an organism in response to these impacts are acquired, but that which is transmitted is a facility of response in the same line, which may, under favorable circumstances, lead to a similar response in the progeny, and, in the course of time with a continuation of similar impacts through successive generations, promote and establish the physiological habit which is the directive influence toward the regular development of the characters in question.

It is, I believe, generally admitted that such is the case in relation to mental stimuli and reäctions in man and some of the higher animals and that the growth of intellectual life in the human race depends upon it.

It is a matter of indifference, dynamically, whether the particular series of impacts concerned in developing a special physiological response is the result of conscious effort by the organism or not; but, as it is highly unlikely that any voluntary effort, no matter how seconded by habit, should be as constant and unceasing as the impacts due to ordinary mechanical forces, we should expect the responses due to conscious effort to be feeble in intensity and numerically few in comparison with those arising from the dynamic forces undirected by consciousness.

The dynamics of the environment, so far as we are able to understand them, in their principal features must be remarkably constant. The weight and consistency of the water or air which forms the surrounding medium, the character of the supporting surface, the range of temperature, the supply of light, the friction of adjacent bodies, the attraction of gravitation, vary within comparatively narrow limits, when consistent with organic existence. We should therefore expect that their influence would on the whole be conservative and tend toward the preservation of the main characteristics of organisms once brought into substantial equilibrium with their surroundings.

On the other hand, owing to the very narrowness of the limits within which life is possible, the dynamic variations, within those limits, to which organic forms are subjected become relatively more important. It is probable that since the initiation of life upon the planet no two organisms have ever been subjected to exactly the same dynamic influences during their development. Differences of impact necessarily imply differences in response, hence a certain amount of variation is the inevitable result. It is absolutely impossible that any two individuals can be or ever have been strictly similar and the application of a conception of exact similarity to any two actual beings becomes more and more difficult as the complexity of their organization is increased.

The origin of variation therefore presents no difficulties ;

rather the presence of two strictly similar beings, could it be shown, would border on the miraculous.

The question which demands an answer is, how are the small necessary and admitted differences stimulated to develop into the obvious differences which are recognized by systematic biologists?

To this I would answer that the reäctions of the organism against the physical forces and mechanical properties of its environment are abundantly sufficient, if we are granted a simple organism, with a tendency to grow, to begin with; time for the operation of the forces; and the principle of the survival of the fittest.

It is often assumed in discussing variation that the possibility of variation is equal in every direction. A consideration of the dynamic conditions of life show that this is not the case, or at least, if we grant its theoretic truth, in practice it never can be true. Under any conditions which would permit it, the resulting organic forms would all be sub-spherical, and would have to pass their existence in constant rotation.

The moment that any one of them came to rest it would begin to be subjected to unequal stresses relatively to its different parts. Light, gravity, friction, opportunities for nutrition, would be unequally distributed, with the result of forcing an unequal growth, development, and specialization of its regions. Inequality of form once established, if it were a moving organism, friction and resistence of the circumambient medium would confirm the inequality and put individuals of its kind at a disadvantage when they varied toward the original shape. Flexure of an elongated body would mechanically institute changes analogous to segmentation, as pointed out by Spencer. Any organic mass possessed of mechanical continuity must develop surface tension and initiate a superficial film.

The fact that these portions of matter are organic, in no respect releases them from the common servitude of matter to the laws of mechanics through the operation of physical forces.

If then development of structure is constrained to operate within a limited field, which can hardly be denied, all those calculations based upon the assumption that the field is unlimited fall to the ground and may be safely disregarded as irrelevant.

The operations of biologic selection may be divided into two categories, 1st. those in which fitness and unfitness are determined by the perfection in adjustment of the individual to the mechanics of the environment, which will include the great mass of the lower organisms; and 2nd., those in which intelligence becomes a factor. The latter will include all forms of sexual selection, mimicry, protective coloration, and every case in which discrimination on the part of pursuer or pursued may come into play. It is by no means necessary that the organism which becomes modified should possess even consciousness, but one of the two parties to the modification, at least, must possess intelligence of a certain grade. The mental qualities of the insect are necessary to the modification of the colors of the orchid, as far as they serve to attract its attention or direct its movements, while the modifications of the stigma or pollen mass to facilitate cross fertilization, fall into the other category.

While the operations of the first category must always have been active, and probably were not supplemented by those of the second category for an immense period of time, yet I believe the latter also to be very ancient. It is probable, however, that influences of the second category operate more rapidly and are productive of much greater diversity in development than could ever have been expected from the unassisted working of the physical forces.

Passing from these general considerations to those of a more special character, the contention of Weismann that "not a single fact hitherto brought forward can be accepted as proof" of the transmission of acquired characters demands attention. This reminds one of the familiar statement of twenty years ago that the Darwinians had not brought forward a single instance of the conversion of one species into another species. If the Dynamic Evolutionist brings forward an hypothesis which explains the facts of nature without violence to sound reasoning, that hypothesis is entitled to respect and consideration until some better one is proposed or some vitiating error is detected in it. No one has yet "proved" that one species is developed out of another species in the sense in which Weismann uses the word proof in his criticism. But plenty of facts which support the hypothesis that acquired characters are transmitted in the sense hereinbefore explained have been accumulated, of which Osborne's paper, above cited, affords evidence in one direction. Can anyone believe that the permanent limb-callosities of the horse and deer, for instance, are selective developments of fortuitous larval corns? Our knowledge of the physiology of any animal, except too or three which have been domesticated for ages, and excepting man, is so contemptibly meagre that it cannot be quoted as evidence on either side.

The question has been much obscured by the attempt to quote the effect or non-effect of mutilations upon progeny, on one side or the other.

For the Dynamic hypothesis only those characters can be considered which arise from permanent physiological reactions due to the impact of external forces. Mutilations rarely fall into this category and are essentially sporadic. In the case of circumcision, so often cited, they affect, at most, half the individuals of a race and only half of any one generation.

There is not a particle of reason to believe that the excision of a trifling scrap of cuticle from an infant would lead to any physiological reäction worthy of attention. One might with greater warrant seek such an effect in the growth of hair and of the nails in civilized races accustomed to trim them. Neither case has been shown to afford valuable evidence.

There is no reason to deny that a pathologic incident of sufficiently fundamental character may effect the progeny of an individual, but it is of no consequence to the Dynamic hypothesis whether it can be proven or not.

Experience shows that it is not single mutilation or loss of substance which results in permanent physiological reäctions so much as continued impacts which lead to locally increased nutrition or local anaemia.

The objection to reasoning drawn from pathologic cases is not that it is not or may not be true, but that the cause affects only individuals in trifling numbers.

The forces invoked by Dynamic hypothesis, on the other hand, affect every individual of a race and every generation as long as the environment continues unchanged. Sporadic modifications must always be finally swallowed up in the general average of the organic type, unless carefully selected by intelligent agencies. The steady pressure of telluric forces lets no individual escape.

On the coast of California the soft tertiary sandstones are drilled by several species of boring mollusks, *Pholas*, *Lithophagus* and *Petricola*. In the course of time the borers die and leave their closely fitting cells untenanted. Into these safe retreats the young of several non-boring bivalves are in the habit of retiring.

As they grow they become too large to escape by the hole through which they entered. Grow they must but the stone walls of their dwelling permit growth only in certain directions. The collector breaks the rock and finds *Kellia*, *Tapes* or *Rupellaria* with the outward conformation of the antecedent borer. Those which refused to conform, if any, have died. Here we have a case where characters have been assumed under an abnormal stress analogous to a pathologic or traumatic mutilation. The progeny of these nestlers would probably exhibit no traces of their parents' deformity. But the pressure of the physical forces on this progeny would be, though invisible. as constant and effective in its results as the rock seemed to be with the nestlers. These results in proportion to their harmony with the environment produce upon the observer the impression which is implied when he speaks of the appearance of such species as "normal."

In my paper on the hinge of Pelecypods and its development,* I have pointed out a number of the particular ways in which the dynamics of the environment may act on the characters of the hinge and shell of bivalve mollusks.

In a paper now in preparation for publication I have shown how the initiation and development of the columellar plaits in *Voluta*, *Mitra* and other Gastropods, is the necessary mechanical result of certain comparatively simple physical conditions; and that the variations and peculiarities connected with these plaits perfectly harmonize with the results which follow with inorganic material subjected to analogous stresses.

Attention once directed to this class of influences and their effects and it is certain facts will accumulate not less numerous

*Am. Journ. of Science, Dec., 1889, p. 445.

and convincing in their establishment of the theory than those which have been taken as "proof" of the survival of the fittest.

NOTE. Since this paper was delivered before the society the discussion of the subject has been continued in the pages of *Nature*. I have been interested to note that Prof. Lankester (in the issue for Mar. 6, 1890, page 414) like the skilled tactician he is, has begun building bridges in his rear which may serve as a means of retreat from his present untenable position. He now explains that by the "transmission of acquired characters" he means the obsolete theory of Lamarck in its purity, which, so far as I have followed the discussion, nobody has proposed to uphold. Why he has continued to oppose the Dynamical theory by arguments intended to demolish a totally different hypothesis, he does not explain.

Mr. Romanes has also pointed out that recent admissions of Dr. Weismann are fatal to the ingenious hypothesis and assumptions with which that gentleman's name has been chiefly connected (*Nature*, Mar. 13, 1890, p. 429.)

In fact these and other signs indicate that the most able of those who have through haste or conservatism been disposed to ignore dynamical influences in evolution, will before long join in the procession, and lend their undoubted abilities to the perfection and elaboration of the only theory yet propounded which fully and efficiently supplements that of Natural Selection and closes the too obvious gaps which have hitherto existed in the intellectual structure of the modern theory of organic evolution.

NEO-DARWINISM AND NEO-LAMARCKISM.*

By Lester F. Ward.

INTRODUCTORY.

In casting about for a subject on which to address this Society I have encountered serious difficulties. A presidential address to a Biological Society should, as it seems to me, follow one of two courses. It should either relate in its general aspects to the subject with which its author is most familiar, and so coordinate the facts within his specialty as to correlate them with the sum-total of biological science ; or else, it should be an exposition of and commentary upon the most prominent problem of biology which at the time of its presentation, is engrossing the attention of the scientific world. One year ago I realized these two alternatives as clearly as I do now, and I felt then that while the second of them was not more appropriate than the first, the overwhelming prominence of one great biological question almost demanded that I should sink my individual preferences and, as a matter of sheer duty, undertake to grapple with that question. But I have always believed and often said that the Biological Society should choose as its president one who represents the whole science of biology, and that it made a mistake in selecting a narrow specialist, and a specialist in a department which has the reputation of not keeping

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*Annual Presidential Address delivered at the Eleventh Anniversary Meeting of the Biological Society of Washington, January 24, 1891, in the Law Lecture Room of the Columbian University.

pace with the rest of the science, and I felt that if compelled to listen to views growing chiefly out of that narrow specialty, it was in some degree its own fault. But in view of the fact that the Society saw fit to repeat its mistake, and that I last year presented the problems of botany and its geologic history, there seems no escape from that duty which still confronts me of closing with the great problem of heredity which continues to occupy the foreground of all biological discussion.

There is strictly speaking only one prominent question before the biological world and that is the question whether qualities that are acquired after birth are capable of being transmitted to descendants. Darwinism in its original entirety, as expounded by Darwin himself, admits such transmission. But by the new school of Neo-Darwinists this is denied. On the other hand Lamarckism, as expounded by Lamarck, explains all change through the transmission of functionally acquired characters, the law of natural selection not having been perceived by Lamarck. But Neo-Lamarckism, as I understand it, while recognizing natural selection as the more potent of the two agencies, also recognizes that the increments of change impressed upon individuals during their lifetime or brought about by individual efforts or habits are also perpetuated in some measure through heredity and form an important factor in the general process of organic development.

STATUS OF THE PROBLEM.

From the appearance of the Origin of Species in 1859 until within the past four or five years it had been the opinion of nearly all naturalists that the existing forms of animals and plants were the result mainly of two cooperating causes, one of which may be called *functional* and the other *selective*. The multitudinous infinitesimal effects wrought by both of these causes upon the form and character of organisms were believed to be cumulatively perpetuated by heredity in the modification of species and the production of new and altered forms of vegetable and animal life. Prior to the date named the few who conceived that existing forms might be modifications of ancestral ones ascribed the changes wholly to the first of these causes, the functional. Mr. Darwin showed that this could not account for all cases, and in pointing out, simultaneously with Mr. Wallace, the existence and mode of operation of the selective agency he made the most important contribution yet brought forward to the science of biology.

At the date of Darwin's death, 1882, the general doctrine of evolution and the theory of development in biology had been accepted by so nearly the entire body of scientific men that it was scarcely worth the effort to conciliate the small remnant who still adhered to the special creation hypothesis. The only question was : By what agency or agencies is evolution accomplished ?

It would carry me too far to attempt to pass in review the various theories of the pre-Darwinians—Treviranus, De Maillet, Goethe, Buffon, Geoffroy St. Hilaire, Erasmus Darwin, and the anonymous author of the Vestiges of Creation. This task has been admirably performed by Professor Haeckel in his History of Creation, and in the later editions of the Origin of Species Mr. Darwin has collected quite a number of sporadic adumbrations not only of the law of evolution itself but even of that of natural selection. I shall be obliged to confine myself almost exclusively to the one great mind, who far more than all others combined, paved the way for the new science of biology to be founded by Darwin, namely, Lamarck. His life was chiefly devoted to the systematic and structural investigation of animals and plants and his earlier works gave little indication of philosophical tendencies, but his Philosophie Zoologique,* which appeared in 1809, showed that he had reasoned deeply about the objects he had so long studied, and in this work is contained the whole of his celebrated system of the transmutation of species.

LAMARCKISM.

Although most of the members of this Society are doubtless familiar with the general character of the Lamarckian philosophy, and many have probably read this work, the nature of my subject seems to demand that some of the more general principles of Lamarckism be set forth. A few paragraphs from the work in question will accomplish this better than any attempt at exposition. The following quotations will serve to show the sweeping character of Lamarck's generalizations, and when we remember the time at which they were written it will not appear strange that his views attracted so few adherents and had to wait half a century for a respectful hearing.

"In order to judge" says Lamarck "whether the idea that has been formed of a *species* has any real foundation, let us return to the considerations which I have already set forth; they show:

1st. That all the organized bodies of our globe are true products of Nature, which she has successfully brought forth (*exécutés*) in the course of long periods of time;

2d. That in her march Nature has commenced, and is still daily commencing to form the most simple organized bodies,

* Philosophie Zoologique, etc. Par Jean-Baptiste-Pierre-Antoine de Monet, Chevalier de Lamarck. Nouvelle Edition, revue et précédée d'une introduction biographique par Charles Martins. Paris, 1873, 2 vols. 8°.

and that she only forms these latter directly, that is, only these first sketches of organization that have been designated by the expression *spontaneous generation;*

3d. That these first outlines of the animal and of the plant formed at suitable places and under appropriate circumstances, and possessing the attributes of incipient life and organic movement, have themselves little by little developed organs and in time multiplied these as well as parts;

4th. That the power of growth in each part of the organized body being inherent in the first effects of life, it has given rise to the different modes of multiplication and reproduction of individuals, and that in this way the progress acquired in organization and in the form and diversity of parts has been preserved;

5th. That by the aid of sufficient time, of circumstances which have been necessarily favorable, of the changes which all points on the surface of the globe have successively undergone in their condition, in a word, by the power which new situations and new habits possess to modify the organs of bodies and of life, all the organisms that now exist have been insensibly formed such as we see them;

6th. Finally, that under the influence of such an order of things, living bodies having undergone each of the changes, greater or less, in their structure and in their parts, what is called *species* thus insensibly and successively brought about among them has only a relative constancy in its character and cannot be as ancient as Nature herself." *

It will be seen that both the mutability and the transmutation of species are distinctly formulated. But in order to make this more clear he elsewhere says:

* Op. cit., Vol. I, pp. 81-83.

"In the same climate very different situations and exposures cause individuals thus placed at first simply to vary; but in the course of time the continual differences of situation of the individuals of which I speak, living and reproducing under the same conditions, bring about in them differences which become in some sort essential to their existence, so that at the end of many generations which succeed each other those individuals which belonged originally to another species find themselves transformed into a new species, distinct from the other." *

The two great causes to which he ascribes this transformation are: 1, what he calls the "circumstances," and 2, the "habits" of the creatures transformed, and to enforce this idea he lays down the following "zoological principle" the fundamental truth of which, he says, appeared to him incontestable:

"Progress in the structure (*composition*) of the organization undergoes here and there in the general series of animals anomalies brought about by the character of the habitat (*circonstances d'habitation*), and by that of habits contracted."†

Between the "circumstances" and the "habits," however, Lamarck perceived a casual relation, and this he expressed in the following argumentative form:

"The true order of things to be considered in all this consists in recognizing:

1st. That every permanent change of any consequence in the circumstances under which each race of animals finds itself effects within it a real change in their needs;

2d. That every change in the needs of animals necessitates for them other actions to satisfy the new needs, and hence, other habits;

* Op. cit., Vol. I, pp. 79-80.

† Op. cit., Vol. I, p. 145.

3d. That every new need necessitating new actions to satisfy it requires of the animal experiencing it either the more frequent employment of such of its organs of which it previously made less use, which develops and enlarges considerably, or else the employment of new parts to which the needs insensibly give rise within it through efforts of its internal sense."*

In all this Lamarck does not expressly say that these transformations are perpetuated by heredity, although this is clearly implied, otherwise they would not be permanent. But he now proceeds to embody the general principle in what he calls a *law*, the first law of organic life, to which he adds a second law in which the principle of heredity is distinctly formulated. Although these two great Lamarckian laws have been frequently quoted in biological discussions, especially within the past three or four years, it seems essential to the completeness of the present exposition to introduce them. They are as follows:

First Law: "In every animal which has not passed the limit of its development the more frequent and sustained exercise (*emploi*) of any organ gradually strengthens that organ, develops and enlarges it, and gives it a power proportioned to the duration of such exercise; while the continued lack of exercise (*usage*) of an organ gradually enfeebles it, deteriorates it, progressively diminishes its power, and finally causes it to disappear."

Second Law: "All that nature has caused individuals to acquire or to lose through the influence of the circumstances to which their race has found itself for a long time exposed, and consequently, through the predominant exercise of certain organs, or through a failure to exercise certain parts, it preserves through heredity (*génération*) to the new individuals that are

* Op. cit., Vol. I, pp. 234-235.

produced by them, provided the changes acquired are common to the two sexes, or to those that have produced these new individuals." *

These laws are enforced by considerable iteration and all the facts and illustrations that he could command. He condenses his first law into the following form :

"The frequent exercise of an organ which through habit has become permanent increases the capacity of such organ, develops it, and causes it to acquire dimensions and power of action which it does not possess in animals that exercise it less." †

The second law is re-stated in the following language :

"Every change acquired in an organ by an habitual exercise sufficient to have brought it about, is preserved thereafter through heredity (*génération*) if it is common to the individuals which, in fecundation, unite in the reproduction of their species." ‡

Such is Lamarckism pure and simple, which it seems necessary to set forth at first hand before approaching those modern phases of the problem which have grown out of it. It is obvious that it deals only with the first of the two agencies in biologic progress mentioned at the outset, namely the functional ; and Lamarck, although he clearly grasped the law of competition, or the struggle for existence, the law of adaptation, or the correspondence of the organism to the changing environment, the transmutation of species, and the genealogical descent of all organic beings, the more complex from the more simple ; he nevertheless failed to conceive the selective principle as formulated by Darwin and Wallace, which so admirably complemented these great laws.

* Op. cit., Vol. I, pp. 235-236.

† Op. cit., Vol. I, p. 247.

‡ Op. cit., Vol. I, pp. 258-259.

The cogency of Lamarck's reasoning, especially when we consider the time at which he wrote, is sufficiently apparent to all, but it may not be without interest to note the manner in which it struck so excellent a judge as Professor Huxley as late as 1876. In contrasting it with the views of Cuvier who maintained the fixity of species and their special creation, Professor Huxley says: "It is impossible to read the 'Discours sur les Revolutions' of Cuvier, and the 'Principes' of Lamarck without being struck with the superiority of the former in sobriety of thought, precision of statement, and coolness of judgment. But it is no less impossible to consider the present state of biological science without being impressed by the circumstance that it is the conception of Lamarck which has triumphed and that of Cuvier which has been vanquished . . . It is not too much to say that the facts of biology known at the present day are all consistent with and in favor of the view of species entertained by Lamarck, while they are unfavorable to, if not incompatible with, that advocated by Cuvier."*

DARWINISM.

Darwin was acquainted with Lamarck's views when he wrote the Origin of Species, and notwithstanding the fact that whenever he refers to Lamarck, as he does in several of his letters† he does so in a very disparaging way, he must have been greatly influenced by them, or at least by views of the same import expressed by others as well as by Lamarck, but especially those of his grandfather Erasmus Darwin, who anticipated, rather from the standpoint of the poet and seer, the truths to which Lamarck was led by a life-long study of living things.

* Am. Cycl., Art. Species.

† Life and Letters, Vol. I, p. 542, Vol. II, p. 198.

But Darwin, like most other thorough naturalists, was little satisfied with the Lamarckian theory, because it left, as all now admit, so much still unexplained, but instead of rejecting it in toto, as most other naturalists did, he sought, and happily succeeded in finding the principle on which the remainder of the facts could be accounted for; or, at least, the greater part of them, for it seems that however deeply we may probe the secrets of nature there will ever remain a few residual phenomena that refuse to submit to our canons.

It is certainly unnecessary that I should occupy your time with any extended exposition of the law of selection, and I will content myself with the following bare definitions:

Natural selection is the general law that variations are constantly occurring in organized beings, and that such of these variations as prove advantageous to the species are preserved through heredity and transmitted to posterity while those which are not advantageous or are disadvantageous to the species are not preserved nor transmitted; the cause of such selection being the fact that advantageous variations tend to increase the chances that the individuals possessing them will reach the reproductive age and continue longer to reproduce, and will hence leave a larger number of offspring than those individuals which had not varied or had varied in an equivocal or unfavorable manner.

Artificial selection is the act of man in intelligently selecting the individuals that possess in the highest degree the particular qualities that he desires to produce as the parents of the animals or plants which he wishes to domesticate or cultivate. The eminent success obtained by man in this way is the certain proof that the qualities of the parents are transmitted to their offspring, and explains the efficacy of natural selection.

Sexual selection is the law that one sex, usually the female, exercises a choice between the individuals of the other, whereby those individuals possessing the selected qualities stand a much greater chance to have the opportunity of transmitting them to their offspring. This law explains the greater ornamentation of the males in so many species, since most such characters are peculiar to one sex and only appear at maturity. Sexual selection also checks the tendency of natural selection to extreme variation in certain directions, since the sexes are well known to prefer their opposites, which causes the offspring to occupy a mean between the extremes. This effect is very marked in the human race, but is doubtless operative among the lower animals. As I pointed out in last year's address, sexual selection has wrought a great revolution in the relative size, strength, and beauty of the two sexes, and reversed in birds and mammals the normal law of female superiority which prevails in most of the lower departments of life.

ACQUIRED CHARACTERS.

It will be readily perceived from what has been said of the two great principles of transformism, the functional, as set forth by Lamarck, and the selective, as elaborated by Darwin, that the fundamental distinction between them is that in the former the transforming qualities which are to be cumulatively transmitted through heredity to the descendants of a given ancestral pair are acquired during the lifetime of these individuals, whereas in the latter the transforming increment is a merely accidental modification arising from unknown causes and hence called spontaneous. The theory is that such spontaneous variations are constantly taking place in all individuals, some in one direction and some in another, and that all except the advantageous ones are immediately lost, while such as tend to increase the chances of survival in the struggle for existence are preserved. Nature has thus provided, through this survival of the fittest, for the maintenance of the equilibrium between the organism and the environment, and also for the increase of structural adaptation and vital power, independently both of the effort of the individual to conform more exactly to its surroundings and of the reaction of the organism upon the impinging environment.

There has never been any doubt of the perfect transmissibility of these spontaneous modifications, or, as they have been called, fortuitous variations. They belong to the essential nature of the organism, and have, as we shall see later on, been ingeniously explained as originating in the very germ itself.

But with regard to functional modifications, or as they are more commonly called, acquired characters, grave doubts have arisen in the minds of many naturalists as to whether they are capable of being inherited by the descendants of those in which they have been superinduced. They are in a certain sense foreign to the organism, external and superficial, and the great question has been how they can succeed in so affecting the reproductive germs of the parents as to reappear in the offspring. That Darwin believed in the transmission of functionally acquired characters is attested not only by many passages in which this belief is expressly stated but by the bringing together by him of more facts in support of it than have been given by all other writers combined either before or since. And although the greater part of his work was naturally directed to the establishment of the hitherto unknown, but as he believed, more important law of selection, nevertheless Darwinism proper must be made broad enough to embrace both of the great agencies of organic transformation, the functional and the selective.

It is hardly necessary to add that pure Lamarckism has nothing whatever to do with such a question as whether accidental modifications produced upon an organism, such as mutilations from whatever cause, are inheritable, since these are not due to continuous interaction between organism and environment, are not the objects of the creature's efforts, and are not acquired by any functional or habitual activities. And yet it is no exaggeration to say that at least one-half, probably much more, of the space devoted by the Neo-Darwinians to the supposed refutation of Lamarckism has been directed to proving that acccidental mutilations are not transmitted to offspring.

I do not deny that there is a doctrine of the transmissibility of mutilations, and Darwin and others have collected a large body of facts pointing strongly in that direction, while Brown-Séquard is believed by many to have demonstrated that hereditary epilepsy may be artificially superinduced in guinea-pigs by lesions of the brain. And it may be that Lamarck, coming upon similar facts, gave them a certain credence, but, as we have seen from typical passages quoted from his work, these cases are not capable of being used in support of his general philosophy, which rests entirely upon the effects of functional activities exerted in response to secular alterations in the surrounding conditions of existence.

Whatever of truth, therefore, there may be in the doctrine of the transmissibility of suddenly or accidentally acquired characters, it is clearly outside the present discussion and need not be further touched upon.

After the doctrine of natural selection had been clearly explained it was found to be so simple and at the same time so far-reaching that it began to be questioned whether much that had been formerly attributed to the other agency ought not to be credited to it instead ; and it cannot be denied that this inquiry tended to broaden the field of the selective at the expense of that of the functional principle. So clear and certain are the workings of the former that it is considered safe to credit it with every fact that can be explained by it, even though it be also explicable by the other law.

But it was not allowed to rest here. The difficulties in the way of accounting for the transmission of qualities originating after the birth of the parents appeared to some so great that they began to doubt whether in fact such a thing is really possible. Of course, there were many popular and superficial writers on evolution who fail to distinguish the two principles and talked as though all development was due to natural selection, so that to the unscientific and popular mind evolution and natural selection were largely synonymous and vaguely comprehended, as is, in fact, still to a large extent, the case. Other better informed people, including some naturalists of note, were so dazzled by the new idea that they lost sight of the old one, and habitually ignored the functional element without criticising it or taking any account of it. It appears to have been against this class that Mr. Herbert Spencer's brilliant exposition of the principle which, in characteristic language, he calls "direct equilibration" was directed. To this I shall have occasion to revert.

For the present I propose to confine myself to those writers who clearly comprehend the nature of the two principles, and who either gravely doubt for what seem to them sufficient reasons, or else deny altogether the efficacy of functional modification and the doctrine of the transmission of acquired characters. The limits of an address such as this preclude any effort to make the discussion historically exhaustive by enumerating all the investigators who from the first to last have taken this view, or some modified form of it, and I shall be content to name among Germans Du Boise Reymond, Pflüger, His, and Weismann, and among Englishmen Galton, Wallace, and Ray Lankester; while what I shall have time to say relative to the nature of the objections raised by these authors will be chiefly confined, for the present, to the views of Galton and Weismann.

THEORIES OF HEREDITY.

It must, however, be premised that inasmuch as the objections raised against the doctrine of the transmission of acquired characters are based upon the difficulties encountered in attempting to explain how such characters can impress themselves upon the germ, all those who have doubted or denied such transmission have approached the subject from the side of embryology, which makes their arguments difficult to explain to biologists in general and still more so to the general public. The laws and processes of heredity are still in the stage of mystery, and their mysterious character has led to many erroneous beliefs and popular superstitions. It is a significant fact that all the mysteries that have been thus far cleared up by science—astronomical, physical and chemical mysteries—have been shown to be the expressions of previously unknown laws of matter and force, and to rest upon a purely material and mechanical basis. The chief obstacle to their comprehension has been the minuteness of the material elements in action—a minuteness far beyond the capacity of the most powerful artificial aids to the senses—so that their secrets have had to be wrung from them by ingenious and multiplied experiments upon their effects. Now, the ultimate reproductive elements, though doubtless many times larger than any chemical molecule, even the most complex, such as those of protein and other organic compounds, are doubtless still far too minute to be observed by the highest powers of the microscope, and if the entire history of the formation of a new organic being is ever to be learned it must be by a successful study of the actions of such minute objects. But this is infinitely more difficult than the study of the actions of inorganic elements, since they take place within an organism whose destruction destroys their vital character.

In view of the history of the less complex sciences it is natural that biologists should insist that the phenomena of heredity are due to the activities of the ultimate material reproductive elements, and not to any vague and occult force or *deus ex machina*. Consequently we find that the only theories of heredity that have been put forth have been based on this assumption.

One of the earliest, and certainly the most celebrated of such theories is Darwin's *pangenesis*, published in 1867.* According to this theory, which is doubtless familiar to most of you, the ultimate reproductive elements, called *gemmules*, are given off from the cells of all parts of the body and collect in the germ-cells and sperm-cells, so that the fertilized ova contain literal representatives of every organ and every part of both parents, which in the new being return to their respective locations and cause the repetition in each of the exact qualities possessed by the parental organs or parts, subject, of course, to the modifications due to a conflict or cooperation between the gemmules of the two parents, equalizing a character where they are different, and emphasizing it where they are alike.

*Variations of Animals and Plants under Domestication. Vol. II, Chaps. XXXVII, XXXVIII.

It will be readily seen that this theory adapts itself to the broadest conception of heredity and, if true, accounts for the transmission of functionally produced modifications as well as the selection of such accidental ones as prove advantageous. But to the ordinary mind this strictly materialistic explanation of heredity seems crude and is to a large extent unintelligible, and the doctrine of pangenesis has gained few adherents among scientific men. They fail as a rule to comprehend Mr. Darwin's gemmules and to understand how they should behave in the manner required by the theory.

Very much of this difficulty, however, is cleared away by the admirable exposition of Mr. Herbert Spencer of the nature of what he calls physiological units. To the biologist the organic unit is the cell and when he has explained the nature and action of cells he thinks he has gone far enough. But the facts of heredity cannot be explained by any phenomena manifested by cells. Between the cell or morphological unit in biology and the molecule of a highly complex organic compound, such as albumen,—the highest class of chemical units—no intermediate element had hitherto been recognized. Mr. Darwin's gemmule is clearly such an intermediate element, and the question at once arose, is there any such? Mr. Spencer has, I think, shown beyond the possibility of doubt that there is and must necessarily be such an element—a unit which is not chemical, since it possesses life, and which is not the morphological unit or cell, but is that of which the active part of every cell consists, and is appropriately termed the physiological unit. I have elsewhere* undertaken to show that life may have resulted from a process of chemical recompounding,

*American Naturalist, Vol. XVI, December, 1882, p. 976. Dynamic Sociology, New York, 1883, Vol. I, p. 311.

and may actually constitute the leading property of the highest organic compound protoplasm, and I venture to suggest here that the gemmules of Darwin and the physiological units of Spencer may be nothing more than the molecules of protoplasm, which, as I have explained, are so immensely complex that any required degree of difference in their essential constitution may easily exist.

The only other theory of heredity which time will warrant my mentioning now is that of Professor Haeckel, published in 1876 and known as "the perigenesis of the plastidule." To avoid the possibility of misstatement, I will give this theory in the words of its author, as epitomized in the latest (8th) edition (1889) of his Schöpfungsgeschichte (pp. 200–201): "The perigenesis-theory was founded by me in 1876 in a memoir 'on the wave-reproduction of vital particles or the perigenesis of plastidules,' and as a 'provisional attempt at a mechanical explanation of the elementary processes of development,' and especially of heredity. (In the second part of my collection of popular lectures, Bonn, 1879, pp. 25–80). The perigenesis-hypothesis seeks to explain heredity by a simple mechanical principle, namely, by the well-known principle of transmitted motion. I assume that in every process of reproduction not only is the special chemical composition of the plasson or plasma transmitted from the parent to the offspring, but also the special form of molecular motion which belongs to its physico-chemical nature. In harmony with the fundamental laws of modern histology and histogeny, I assume that this plasma (either the caryo-plasma of the cell-nucleus or the cytoplasma of the cell-body) is alone the original bearer of all vital activity, and hence also of heredity and reproduction. In all plastids (as well the anucleated cytodes as the genuine nucleated cells) this plasma or plasson is composed of plastidules or plasma-molecules, and these are 'probably surrounded by aqueous envelops; the greater or less thickness of these aqueous envelops, which at once separate and bind the neighboring plastidules, determines the softer or harder condition of the flowing plasson' (p. 48).

'Heredity is the transmission of plastidule motion, whereas adaptation is change of plastidule motion' (p. 55). This motion may in its general aspects be conceived as a ramified wave-motion. In all protists or unicellular organisms (protophytes and protozoans) this periodical movement of the mass goes on in a correspondingly simple manner while in all tissue-bearing or multicellular creatures (metaphytes and metazoans) it is combined with a mutual generation of the plastids and a division of labor of the plastidules."

It will be observed that although this theory of heredity lays special stress upon the idea of motion, thereby recognizing the element of force, it is nevertheless based like all others upon the existence of ultimate material elements different on the one hand from the chemical molecules and on the other from cells, and intermediate between these. The gemmule of Darwin, the physiological unit of Spencer, and the plastidule of Haeckel are the same in essence, and the study of the phenomena of these ultimate elements of biology open up a new and most promising field of research into which scarcely any investigator has as yet deliberately entered.

We are now prepared to consider the objections of Galton and Weismann to the doctrine of the transmission of functionally acquired characters.

VIEWS OF MR. GALTON.

The earliest expression of Mr. Galton's views, so far as I am aware, is contained in a paper "On Blood-Relationship" presented by him on June 13, 1872, to the Royal Society of London and published in its proceedings.* In this paper stress is laid upon the distinction in embryonic development between what he calls the "patent" and the "latent" elements, and he argues from the facts of reversion and atavism that the greater part of the parental elements in heredity are latent in the germ, but prepared to express themselves in more or less remote decendants. Although he addresses himself to the anthropologist rather than the biologist, and claims only to be making a contribution to the difficult subject of kinship, he nevertheless evinces a clear grasp of the embryonic conditions of the problem, and as we shall see, anticipates, some of the more exact conceptions of Weismann. He does not wholly deny the possibility of the transmission of acquired characters, but says that "the effects of use and disuse of limbs, and those of habit, are transmitted to posterity in only a very slight degree."

In this respect Mr. Galton makes only a slight advance toward the conclusions of Weismann in the much more elaborate paper which he read before the Anthropological Institute of Great Britain on November 9, 1875, and which appeared in the December number or the Contemporary Review for that year, and also in an expanded form in the Journal of the Institute (Vol. V, p. 329). In this paper which is entitled "A Theory of Heredity," he, however, approaches the main question with much greater directness. "The facts" he says "for which a complete theory of heredity must account may conveniently be divided into two groups; the one refers to those inborn or congenital peculiarities that were also congenital in one or more ancestors, the other to those that were not congenital in the ancestors, but were acquired for the first time by

*Vol. XX, p. 394.

one or more of them during their lifetime, owing to some change in the condition of their life. The first of these two groups is of predominant importance, in respect to the number of well-ascertained facts that it contains, many of which it is possible to explain in a broad and general way, by more than one theory based on the hypothesis of organic units. The second group includes much of which the evidence is questionable or difficult of verification and which, as I shall endeavor to show, does not for the most part, justify the conclusion commonly derived from it.'' He further says that his theory ''is largely based on the arguments and conditions brought forward by Mr. Darwin in support of pangenesis; nevertheless the conclusions in this paper will be seen to differ essentially from his own. Pangenesis appears more especially framed to account for the cases which fall in the second of the above-mentioned groups which are of a less striking and assured character than those of the first group, and it will be seen that I accept the theory of pangenesis with considerable modification, as a supplementary and subordinate part of a complete theory of heredity, but by no means for the primary and more important part.''

He employs the term *stirp* ''in a special sense—to express the sum-total of the germs, gemmules, or whatever they may be called, which are to be found, according to every theory of organic units, in the newly fertilized ovum—that is in its earliest pre-embryonic stage.'' In defending the theory of organic units he says: ''We must bear in mind that the alternative hypothesis of a general plastic force resembles that of other mystic conceptions current in the early stages of many branches of physical science, all of which yielded to molecular views, as knowledge increased.''

The paper is an exceedingly luminous contribution to the subject, and the theory advanced may be designated in general terms as the doctrine of natural selection or survival of the fittest among the organic units constituting the stirp, to determine which shall become manifest in the offspring and which shall lie latent to reappear or not in later generations. As the stirp contains organic units that have lain latent in previous generations and may become patent in the generation in question, the theory accounts for reversion, atavism, and the whole train of facts in heredity that have so long puzzled the scientific investigator. We are at present only concerned with so much of it as relates to the transmission of acquired characters. The following passage expresses his views on this point : "We have thus far dealt with three agents—(1) the stirp, which is an organized aggregate of a host of germs ; (2) the personal structure, developed out of a small portion of these germs ; and (3) the sexual elements, generated by the residuum of the stirp. The cases before us are those which are supposed to prove that 2 reacts on 3—that is, the personal structure upon the sexual elements. The first and largest class of these cases refer to adaptivity of race. It is said that the structure of an animal changes when he is placed under changed conditions ; that his offspring inherit some of his change ; and that they vary still further on their own account, in the same direction, and so on through successive generations, until a notable change in the congenital characteristics of the race has been effected. Hence it is concluded that a change in the personal structure has reacted on the sexual elements. For my part, I object to so general a conclusion." And he proceeds to elaborate his reasons for such objection. Passing over these for want of time I will conclude this exposition of Galton's views by quoting the following passage :

"The conclusion to be drawn from the foregoing arguments is, that we might almost reserve our belief that the structural cells can react on the sexual elements at all, and we may be confident that at the most they do so in a very faint degree; in other words that acquired modifications are barely, if at all, *inherited*, in the correct sense of that word. If they were not heritable, then the second group of cases would vanish, and we should be absolved from all further trouble about them; but if they exist, in however faint a degree, a complete theory of heredity must account for them. I propose, as already stated, to accept the supposition of their being faintly heritable, and to account for them by a modification of pangenesis."

I am not aware that Mr. Galton has modified the views here expressed since the date of that paper, but in all his subsequent ones, as well as in his work on "Hereditary Genius" (1879) he continues to emphasize the paramount importance of the latent elements in heredity, and the superiority, as he forcibly expresses it, of nature over nurture.

TEACHINGS OF PROFESSOR WEISMANN.

The vigorous onslaught which has been made upon the doctrine of the transmission of acquired characters, since the date of Mr. Galton's papers, and apparently without a knowledge of them, by Prof. August Weismann of the University of Freiburg has probably aroused a greater amount of interest among scientific men than any other event that has transpired since the appearance of Darwin's *Origin of Species*. Professor Weismann is an embryologist and histologist and has conducted a series of prolonged and successful investigations upon several groups of lowly organisms. But he has looked beyond the special facts which are immediately connected with his researches and has thought out for himself all the deeper problems of biology. Besides making himself complete master of the whole field of that science as generally accepted he has co-ordinated its facts and drawn from them a number of new and brilliant conclusions which have set the world to work on entirely new lines of investigation.

Professor Weismann was logically led to the conclusion that acquired characters cannot in any conceivable way be transmitted. The first of the series of essays which have produced such a sensation, that on the duration of life, was originally read before the Association of German Naturalists and Physicians at Salzburg in September, 1881, and a short abstract of it appeared in *Nature* for April 5, 1888 (Vol. XXXVII, p. 541). It was in this paper that he elaborated the theory that unicellular organisms are potentially immortal. The second of the series, that on heredity, was his inaugural address as Pro-rector of the University of Freiburg, delivered June 21, 1883. It was in this that he first attacked the doctrine of the transmission of acquired characters, and in it and the preceding essay may be found the germs of all his later theories. The remaining six essays appeared at intervals from 1883 to 1888. Abstracts and reviews of them occured in *Nature* and the English magazines, and long before the appearance in 1889 of the admirable work containing an English translation of the whole series with numerous additions and amendments by the author and notes by the translators,* the controversy had begun in which so many of the most eminent biologists of Europe and America have taken part.

Professor Weismann's general course of reasoning is somewhat as follows: It is universally admitted that all the higher organisms consist of tissues made up of cells and that these

* Essays upon Heredity and kindred biological problems. By August Weismann. Authorized translation edited by Edward B. Poulton, Selmar Schönland, and Arthur E. Shipley. Oxford, Clarendon Press, 1889.

cells do not differ essentially from those which are found leading an independent existence and are termed unicellular organisms. Many of these unicellular organisms reproduce by the process known as fission or division; that is, they split or divide into two equal parts each of which becomes a new organism exactly like the original. These halves exist for an appointed time, increase in size until they are each equal to the original cell before division, and then divide again, so that what was formerly one now becomes four. Each of these four repeats the process, and so on, thus multiplying in a geometrical ratio. But if we follow any one of these lines of descent we observe that the last of the line contains some of the same matter that was in the first, and none of the matter has ceased to live. Unless destroyed by some external cause all of the substance of the original cell will continue to live for any conceivable length of time. It is "potentially immortal." Now, the theory of descent as a universal organic principle, which Weismann fully accepts, explains all the life of the globe as resulting from previous life through some form of reproduction. Fission is the simplest form of reproduction, and it is found that it is the common form of cell-reproduction within the tissues of the higher animals. All growth is brought about by it or some modification of it. A study of the phenomena of reproduction in the lower organisms shows that it takes place ultimately through some similar process, which, however greatly modified in its details, consists essentially in the actual transmission of the reproductive cell-substance from parent to offspring, and Weismann maintains that the reproductive cells, like those of unicellular organisms, are immortal or perpetual, and that nothing can get into the body of the offspring except through that of one or other of its parents. This is his fundamental doctrine of the continuity of the germ-plasm. The impregnated ovum contains the germ-plasms of the two parents, and out of it the embryo is formed. The embryo develops independently of the mother by a circulation of its own, and no external influences can by any conceivable method affect or change the characters of the offspring.

But it is well known that variation takes place, that the offspring does not always resemble either parent, and that changes go on so great as to result in the creation of new species, new genera, and entirely new types of life. All this Weismann admits. How does he explain it? Primarily by natural selection, but he does not stop with that. It has always been admitted that natural selection did not explain the cause of variation. Weismann attempts to do this, and his reasoning is exceedingly ingenious.

The original reproductive cells are assumed by him to consist of an indefinite number of units which he calls germ-plasms, and their presence is explained on the assumption of their preservation from ancestral organisms. Asexual reproduction is of course incapable of producing variation, and he maintains that sexual reproduction has been developed and exists solely for the purpose of insuring variation.

Relative to the constitution of the germ-plasm he says: "Every detail in the whole organism must be represented in the germ-plasm by its own special and peculiar arrangement of the groups of molecules (micellæ of Nägeli) and the germ-plasm not only contains the whole of the quantitative and qualitative characters of the species, but also all individual variations as far as these are hereditary: for example the small depression in the center of the chin noticed in some families. The physical causes of all apparently unimportant hereditary habits or structures, of hereditary talents, and other mental peculiarities, must all be contained in the minute quantity of germ-plasm which is possessed by the nucleus of a germ-cell; not indeed, as the preformed germs of structure (the gemmules of pangenesis), but as variations in its molecular constitution; if this be impossible, such characters could not be inherited" (pp. 100, 101).

The union of two germ-cells from entirely different individuals always multiplies the number of ancestral germ-plasms by two. The excess is kept down by the removal of the second polar body, as he supposed was proved by its not taking place in parthenogenesis. But the part removed as well as the part retained contains germ-plasms from both parents alike and hence the offspring must partake of the nature of both.

These ancestral germ-plasms exist in the reproductive cells in vast numbers, and in the removal of half of them at each union of the sexes, there must remain not merely those of the immediate parents, but those of previous generations. If we were theoretically to conceive that at the outset only a single germ-plasm existed from each parent, then the second generation would transmit four, the third eight, and so on in a geometrical ratio, until they would become so numerous as to require the removal of a portion and ultimately always of half the ancestral germ-plasms at each act of reproduction. Says Professor Weismann: "These different qualities are what I have called the ancestral germ-plasms, i. e., the germ-plasms of the different ancestors, which must be contained in vast numbers, but in a very minute quantities, in the nuclear thread. The supposition of a vast number is not only required by the phenomena of heredity but also results from the comparatively great length of the nuclear thread; furthermore it implies that each of them is present in very small quantity. The vast number together with the minute quantity of the ancestral germ-plasms permit us to conclude that they are, upon the whole, arranged in a linear manner in the thin thread-like loops; in fact the longitudinal splitting of these loops appears to me to be almost a proof of the existence of such an arrangement, for without this supposition the process would cease to have any meaning" (pp. 359–360).

His general view of the origin of variation is thus given by him: "It is well known that this process [sexual or amphigonic reproduction] consists in the coalescence of two distinct germ-cells, or perhaps only of their nuclei. These germ-cells contain the germ-substance, the germ-plasm, and this again, owing to its specific molecular structure, is the bearer of the hereditary tendencies of the organism from which the germ-cell has been derived. Thus in amphigonic reproduction two groups of hereditary tendencies are as it were combined. I regard this combination as the cause of hereditary individual characters, and I believe that the production of such characters is the true significance of amphigonic reproduction. The object of this process is to create those individual differences which form the material out of which natural selection produces new species" (p. 272). "I do not know what meaning can be attributed to sexual reproduction other than the creation of hereditary individual characters to form the material upon which natural selection may work" (p. 281). "The most important duty of sexual reproduction is to preserve and continually call forth individual variability, the foundation upon which the transformation of species is built" (p. 373). "Sexual reproduction is to be explained as an arrangement which ensures an ever-varying supply of individual differences" (p. 384).

Weismann's classification of cells into somatic and reproductive is fundamental to his whole philosophy. On this point he says: "The first multicellular organism was probably a cluster of similar cells, but these units soon lost their original homogeneity. As the result of mere relative position, some of the cells were especially fitted to provide for the nutrition of the colony, while others undertook the work of reproduction. Hence the single group would come to be divided into two groups of cells, which may be called somatic and reproductive—the cells of the body as opposed to those which are concerned with reproduction (p. 27) . . . As the complexity of the metazoan body increased, the two groups of cells became more sharply separated from each other. Very soon the somatic cells surpassed the reproductive in number, and during the increase they became more and more broken up by the principle of the division of labor into sharply separated systems of tissues. As these changes took place the power of reproducing large parts of the organism was lost, while the power of reproducing the whole individual became concentrated in the reproductive cells alone" (p. 28). His theory further assumes that the germ-cells contain two kinds of plasm, which he calls respectively the *ovogenetic* and the *somatogenic*, i. e., the first capable only of producing germ-cells, the latter capable only of producing somatic cells. These exist together in the fertilized ovum, and if allowed to remain there would go on reproducing themselves in something like equal numbers. But the body consisting almost entirely of somatic cells, it is evident that such a multiplication of germ-cells would be only a hindrance to development. This, he claims, explains the mysterious phenomena so long observed by embryologists and called the removal of polar bodies. The polar body first removed is nothing more nor less than the ovogenetic nucleo-plasm, which is now in the way, and whose removal is necessary to the formation of the embryo. This is the work alone of the somatic cells, and these, consisting as they do of the germ-plasms of an indefinite series of ancestors, and containing representatives of every part of the parent organism, proceed to reproduce a new creature on the hereditary type of the parents with the modifications due to the commingling of many ancestral types.

Without dwelling longer upon these ultimate processes which constitute the premises of Weismann's argument, I will now proceed to state his conclusion. It is simply that he is utterly unable to see how the somatic cells of an adult individual can react upon or in any way affect its reproductive cells. If it cannot, the transmission through either parent to its offspring of any peculiarity acquired since the embryo of the parent began to form is impossible. Firmly believing in the truth of his theory he stoutly insists that no such thing can take place. Of course it needs to be clearly understood what he means by acquired characters, and here, it is claimed lies the chief point in dispute between the Neo-Darwinians and the Neo-Lamarckians. The former contend that the latter class as acquired characters those which are simply due to natural selection. It will therefore be profitable to dwell a moment upon this point.

"The tendencies of heredity", says Weismann, "of which the germ-plasm is the bearer, depend upon this very molecular structure, and hence only those characters can be transmitted through successive generations which have been previously inherited, viz., those characters which were potentially contained in the structure of the germ-plasm. It also follows that those other characters which have been acquired by the influence of special external conditions, during the lifetime of the parent, cannot be transmitted at all" (p. 267). "It is only by supposing that these changes arose from molecular alterations in the reproductive cell that we can understand how the reproductive cells of the next generation can originate the same changes in the cells which are developed from them; and it is impossible to imagine any way in which the transmission of changes produced by the direct action of external forces upon the somatic cells, can be brought about . . . To this class of phenomena of course belong those acts of will which call forth the functional activity of certain groups of cells" (p. 80). "Only those new characters can be called 'acquired' which owe their origin to external influences, and the term 'acquired' must be denied to those which wholly depend upon the mysterious relationship between the different hereditary tendencies which meet in the fertilized ovum. These latter are not 'acquired' but inherited, although the ancestors did not possess them as such, but only, as it were, the elements of which they are composed" (p. 252). "If acquired characters are brought forward in connexion with the question of the transformation of species, the term 'acquired' must only be applied to those characters which do not arise from within the organism, but which arise as the reaction of the organism under some external stimulus, most commonly as the consequence of the increased or diminished use of an organ or part" (p. 322).

That such characters cannot be inherited he asserts with the strongest emphasis and frequent iteration. His treatment of this point often borders on the dogmatic, as a few extracts will show.

"It has never been proved" he says, "that acquired characters are transmitted, and it has never been demonstrated, that, without the aid of such transmission, the evolution of the organic world becomes unintelligible. The inheritance of acquired characters has never been proved, either by means of direct observation or by experiment" (p. 81). "No single fact is known that really proves that acquired characters can be transmitted" (p. 267). "If acquired characters cannot be transmitted the Lamarckian theory completely collapses, and we must entirely abandon the principle on which alone Lamarck sought to explain the transformation of species,—a principle of which the application has been greatly restricted by Darwin in the discovery of natural selection, but which was still to a large extent retained by him. Even the apparently powerful factors in transformation—the use and disuse of organs, the results of practice or neglect—cannot now be regarded as possessing any direct transforming influence upon a species, and the same is true of all the other direct influences, such as nutrition, light, moisture, and that combination of different influences which we call climate. All these, with use and disuse, may perhaps produce great effects upon the body (soma) of the individual, but cannot produce any effect in the transformation of the species, simply because they can never reach the germ-cells from which the succeeding generation arises" (pp. 387–388). And much more in the same strain.

Weismann fully admits the influence of the environment upon the individual in producing marked changes. He also fully admits the facts of adaptation to environment and the transformation of species and development of organic beings. But he insists that natural selection is competent to explain all this, that it takes place through the selection of such accidental variations in the germ as prove advantageous, or, as he puts it, the selection from among an infinite number of ancestral germ-plasms in the fertilized ovum of such as will produce an individual most in harmony with its environment, leaving all others in the latent state. This, as we have seen, is pure Galtonism.

But this incapacity for the inheritance of acquired characters is confined to metazoans or multicellular organisms—organisms whose reproductive and somatic cells are differentiated. It does not apply to protozoans or unicellular organisms. These are greatly influenced by the environment, and, consisting entirely as it were of reproductive cells, naturally transmit their variations to their descendants directly. Only thus can variability be perpetuated, and whatever is true of them is true of all germ-cells. "The origin of hereditary individual variability," says Weismann, "cannot indeed be found in the higher organisms—the Metazoa and Metaphyta; but is to be sought for in the lowest—the unicellular organisms. In these latter the distinction between body-cell and germ-cell does not exist. Such organisms are reproduced by division, and if, therefore, any one of them becomes changed in the course of its life by some external influence, and thus receives an individual character, the method of reproduction ensures that the acquired peculiarity will be transmitted to its descendants" (pp. 277-278).

It is here that comes in his fundamental doctrine of the continuity of the germ-plasm. If not the germ-cells, at least the germ-plasm of either parent passes intact to the offspring. It is perpetual, or as he calls it, immortal. It gives to the new being its special character, but receives nothing from it. It remains in the offspring until it in turn becomes a parent, and again passes to the third generation without ever having ceased to live. Every living being on the globe to-day contains in its germ-plasm something that has never ceased to live since the original life-breath was breathed into organic nature. Through all the ancestral types of the phyletic chain it has persisted, passing from parent to offspring through the transforming series, so that in the loins of the highest types of man there is something which was still living in the lowest primordial worm and even in the bathybian ooze of those primeval waters which in the earliest Cambrian times succeeded the formation of the original crust of the globe.

Upon this series of brilliant speculations and startling assertions, including much that it has been impossible for me to bring forward, has been founded the school of Neo-Darwinism. In Germany they attracted comparatively little attention, in France none, but in England they have become almost a shibboleth in the mouths of a large class of leading biologists.

It unfortunately requires something more than mere truth to arouse enthusiasm in many minds, and however much it may be disclaimed, it cannot probably be justly denied that the peculiar position of prominence and honor which this theory gives to the doctrine of natural selection, conceived and elaborated by Englishmen, had much to do with its especial charm for English ears. It is not to be supposed that Weismann deliberately bid for applause from England, but he could clearly see the tendency of his doctrines to exalt natural selection. He does not allude to this in any of his earlier essays, nor until he had begun to observe the effect his writings were producing in England. In the preface to his fifth essay, dated Nov. 22, 1885, however, he says: "The transmission or non-transmission of acquired characters must be of the highest importance for a theory of heredity, and therefore for the true appreciation of the causes which lead to the transformation of species. Any one who believes, as I do, that acquired characters are not transmitted, will be compelled to assume that the process of natural selection has had a far larger share in the transformation of species than has been as yet accorded to it; for if such characters are not transmitted the modifying influence of external circumstances in many cases remains restricted to the individual, and cannot have any part in producing transformation" (pp. 252-253). And in the last essay of this series, originally delivered in September, 1888, he further remarks: "But if the transmission of acquired characters is truly impossible our theory of evolution must undergo material changes. We must completely abandon the Lamarckian principle, while the principle of Darwin and Wallace, viz., natural selection, will gain an immensely increased importance" (p. 423).

A CRITIQUE OF WEISMANN.

I have now, as I believe, fairly if not fully stated, chiefly in the language of its founder, the Neo-Darwinian theory, and before passing to consider what has been said on the other side, and the position of the Neo-Lamarckians in general, I would like to pause a moment in order to offer a few reflections of my own upon Weismann's teachings. I am emboldened to do this the more not only because I have not seen the exact point of view from which they especially strike me touched upon by others in the voluminous discussion which has grown out of them, but also because what I shall say will be based entirely upon his own statement of the facts, and therefore the objection that, not being an embryologist, I am not competent to weigh the considerations from that side (which I would freely admit), cannot properly be raised.

The question is whether, accepting the continuity of the germ-plasm, accepting the nature which he ascribes to the fertilized ovum with its multitudes of ancestral plasms out of which selections are made, accepting his explanation of the meaning of the first and second polar bodies, accepting his differentiation into reproductive and somatic cells, and all the other details which he brings forward, many of which are, of course, only hypotheses, there do not still remain grounds on which to base a theory of the transmission of certain kinds of acquired characters, and especially those of a strictly functional nature. In fact, the question seems to me rather to be whether his line of argument carried to its extreme logical conclusion would not preclude the possibility of any variation whatever even in the germ-plasms themselves. It is not sufficient to say that all variation is due to the varied character of multitudinous germ-plasms in the fertilized ovum, brought there from many often remote ancestors possessing very different characters. This is a *petitio principii*, since it assumes these differences in those ancestors, and the primary question must be answered; whence these ancestral differences? How does he account for any differences at all?

We have already seen that Weismann restricts his denial to multicellular organisms and admits as a necessary part of his theory, that unicellular organisms are easily affected by the nature of their surroundings and activities, and that the changes thus produced are directly transmitted. "If for instance," he says, "a protozoan, by constantly struggling against the mechanical influence of currents in water, were to gain a somewhat denser and more resistent protoplasm, or were to acquire the power of adhering more strongly than the other individuals of its species, the peculiarity in question would be directly continued on into its two descendants, for the latter are at first nothing more than the two halves of the former. It therefore follows that every modification which appears in the course of its life, every individual character, however it may have arisen, must necessarily be directly transmitted to the two offspring of a unicellular organism (p. 278). . . . We are thus driven to the conclusion that the ultimate origin of hereditary individual differences lies in the direct action of external influences upon the organism" (p. 279). But he even goes further and asserts that there is no other way by which the germ can be affected. "I have never doubted" he says, "about the transmission of changes which depend upon an alteration in the germ-plasm of the reproductive cells, for I have always asserted that these changes, and these alone must be transmitted (p. 410) . . . In what other way could the transformation of species be produced, if changes in the germ-plasm cannot be transmitted? And how could the germ-plasm be changed except by the operation of external influences, using the words in their widest sense?" (p. 411).

Now if, as he insists, external influences cannot possibly affect the germs of metazoans, and if, as he here maintains, it is external influence alone that can influence any germs, it must follow that the only variation that could have taken place in the germ-plasms of the highest animals are those which occurred in the protozoan stage of their development.

This is clearly a *reductio ad absurdum*, derived entirely from his own statements, some of them among his latest utterances. The difficulty is to see why he should adhere so tenaciously to the idea that the germ-cells cannot be influenced by functional changes in the organism containing them. The mere fact that they are lodged within the body of an animal does not affect the question unless it can be shown that they are so lodged that no change is possible in the nature of their immediate surroundings. To assume this is gratuitous and contrary to what would be naturally supposed. In reading certain passages in his own book one is strongly tempted to doubt whether he believes it himself. For example, he says in one place speaking of hereditary variations: "I believe however that they can be referred to the various external influences to which the germ is exposed before the commencement of embryonic development. Hence we may fairly attribute to the adult organism influences which determine the phyletic development of its descendants. For the germ-cells are contained in the organism, and the external influences which affect them are intimately connected with the state of the organism in which they lie hid. . . It is even possible that the effects of these influences may be more specialized; that is to say, that they may act only upon certain parts of the germ-cells" (pp. 103–104). But he seems to see a great difference between this and the transmission of characters acquired in certain special organs to the same organs of the offspring. This would probably be clear only to an embryologist. One of the most suggestive thoughts in his whole philosophy is that of the total dissimilarity between the germ and the developed organism which is to result from it. He maintains with every semblance of truth that there can be nothing in common between them except the fact that the molecular structure of the germ is such that if allowed to develop it will produce a being similar to the one from which it sprung. This principle seems to be peculiarly applicable to the subtle influences which effect heredity, and without appealing to anything occult or abandoning the strictly casual and mechanical theory of heredity, it may be submitted whether we know enough about it as yet to assert that influences affecting the parental organism, even any of its organs, may not react specifically and in kind upon the germ and set up molecular tendencies in the same direction. This may be said quite independently of any attempt to explain precisely how it can do so, as the theory of pangenesis claims to do.

If the germ-plasms vary within the body of either parent before they are brought together that variation must be due to influences acting upon them in the animal body. All this Weismann admits, but he denies that the changes which he admits to take place in the individual as the result of changes in the environment and subsequent changes in the habits and activities of the creature can be regarded as among the causes which produce changes in the germ-plasm. Is this logical or even reasonable? If not due to such changes to what causes are they due? Without pretending to explain how such a thing could happen, I claim that the indications are that it does happen. To say without proof that it cannot happen adds nothing to the argument. We have an antecedent and we have a consequent. Both are facts. There is no possibility in the present state of our knowledge of either proving or disproving the casual connection between these facts. Variation takes place in the direction of adaption to changed conditions and activities. So far the inference is confirmed by a third fact. If the inference had not been challenged in the interest of another principle this would be regarded as proof. I do not agree with Weismann that the burden of proof rests on those who draw this natural inference. It rests on him and the Neo-Darwinians to show that the assumed cause is not a cause. This they have thus far failed to do.

You will understand that I am speaking of variations which take place in the germ-cells and sperm-cells of parental organisms before they blend in the fertilized ovum. Most of Weismann's argument is directed to show that the fertilized ovum itself cannot be affected by any transforming influence acting upon the mother during the growth of the embryo. This may be true but it is unimportant. The time required to develop the embryo is too short for the environment to produce any material change however strongly the tendency might be at the time in the direction of such change. It is chiefly the uncombined sexual elements which are admitted by all to be undergoing specific transformation. The Neo-Darwinians deny that this is due to admittedly parallel transformations going on in the individual, the result of external and internal influences upon the developed body; the Neo-Lamarckians consider the latter as in great part the cause of the former, while admitting that other variations are taking place due to unknown causes and that these are seized upon by natural selection to the advantage of the species.

The difficulty, on Weismann's theory, of accounting for any variation at all above the protozoans still confronts us. If external influences can only act on unicellular organisms in such a way as to be transmitted, it must follow that so soon as the multicellular stage is reached a rigid fixity must result. One of these lower metazoans may undergo important modifications during its lifetime, but its offspring are always set back to precisely the same place where the parent was when it set out. All these functionally produced changes are, according to him, utterly lost because they cannot react upon the germ-plasm. Where is the room for the action of natural selection? He has not dwelt upon this point, but he would probably say, though contrary to statements above quoted, that the germ-plasms are constantly undergoing spontaneous variation and that natural selection works on these. We would then be brought back to where we were a moment ago, with the question still before us, how spontaneous variations differ from functional ones (for he would not maintain that they were wholly uncaused effects), and why it is not logical and rational to assume that functional changes are impressed upon the germ-cells in ways which, though unknown to us, are no more unknown than is the cause of spontaneous variations. This seems to be far more reasonable than the far-fetched, and, as it seems to me, childish view recently expressed by Prof. E. Ray Lankester, that the environment does indeed influence the germ-cells but only by kaleidoscopically shaking up their contents, thus causing what are called "sports" in the progeny, and that natural selection seizes upon these, thereby securing advantageous transformations.

NEO-DARWINISM.*

We will next briefly pass in review the extraordinary discussion which has followed chiefly from the publication of Weismann's essays. As already remarked, they produced very little influence upon the German mind, and most German investigators who noticed them at all, either saw little in them, or else attacked them with greater or less violence. It is almost exclusively in England that they have found favor, and here a veritable school of biologists has sprung into existence prepared to defend even the most extreme of Weismann's theories. It is due to the German investigator to say that, with the exception of the slight tendency above pointed out to dogmatize on the subject of the non-transmissibility of acquired characters, his essays are dignified and courteous and often evince an almost Darwinian modesty with regard to his own theories. Far different was the case with most of his English disciples. What he states as probable they assert as forever settled, and his working hypotheses become for them the fundamental truths of science. His papers were translated and reviewed, usually in an aggressive manner before any one had ventured to criticise them. Being usually beyond the reach of any but the embryological specialist all except ardent disciples reserved their judgment and declined to enter the field. At first there was an attempt to make it appear that Weismann's views reflected only those of Darwin himself and that all outside of them consisted in deviations and wanderings from his doctrines. It was sought to stamp them with the name of

* The expression *Neo-Darwinian* was first used, so far as I am aware, by Dr. G. J. Romanes in a letter to *Nature* for Aug. 30, 1888 (Vol. XXVIII, p. 413), and occurs frequently in subsequent discussions. The substantive form *Neo-Darwinism* was a natural outgrowth from it.

"pure Darwinism,"* and the reader was frequently informed what Darwin really intended to say in certain passages which could not otherwise be made to harmonize with the new doctrine, and even in some still more refractory passages we are told what we would have said "if it had occurred to him." †

In default of any real opponent the Duke of Argyll, with his strong theological bias, his medieval spirit of logomachy, and his total lack of scientific ideas, was called out and set up as a sort of man of straw to be repeatedly demolished. But like the shadows in the valley of Walhalla, he emerged each time unscathed and renewed the deathless struggle. His presence in the arena had the further advantage for the new school of affording them an opportunity to point to him as a sample of the opponents of Weismann.

Against all this a few protests were raised from time to time and after the appearance of the English edition of the essays a few able and critical analyses were made. But the general character of the discussion as it has gone on in the columns of *Nature* and in the British magazines is such as I have described. The only other prominent or frequent contributor in answer to the disciples of Weismann is Dr. G. J. Romanes, and he has been more especially concerned with defending his priority to the idea which he has elaborated under the name of Physiological Selection, and to the discussion of certain phases of the law of *panmixia* which he claims to have discovered. It would, however, be unjust to deny that the discussion has been of value to science, since, had it done no more than to attract wide attention to so momentous a question it could not have been without its uses.

* *Nature*, Vol. XXXVIII, Aug. 16, 1888, p. 364; Aug. 23, 1888, p. 388; Vol. XL, pp. 567, 619.

† See *Nature*, Vol. XLI, March 27, 1890, pp. 487, 488.

NEO-LAMARCKISM.*

Let us inquire what has really been done from first to last toward the demonstration, or scientific establishment of the law of transmission of functionally acquired characters and the preservation through heredity of the modifications produced by changes in the environment. It will not be necessary to go back to Lamarck as his presentation of the subject has been sufficiently dwelt upon. But I cannot agree with some recent writers that Lamarck was defending a totally different principle from that which is being defended to-day. It is true that Neo-Lamarckians recognize natural selection as an equally, and in some respects far more potent law, although, as has been justly insisted upon, it does not explain the cause of the variations of which it makes use. The Lamarckian principle does this, so far as it goes, and affords a true mechanical, that is, scientific explanation of the origin of species.

After Darwin himself, whose methods were always those of the true naturalist, unquestionably the most successful defender of this view is Mr. Herbert Spencer, whose methods are always those of the true philosopher. A man of such originality would be incapable of approaching the subject from the same standpoint as any of his predecessors, and we find him evolving this law from his great general scheme of mechanical cosmology, in which it appears as one of the equilibrating forces of the organic world. It is his law of "direct equilibration," natural selection forming a second law of "indirect equilibration."

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* Prof. A. S. Packard is believed to be the first to use the term *Neo-Lamarckian*. This he did in the introduction to the Standard Natural History (Vol. I, Boston, p. iii) in 1885, and on page iv he adopts the substantive form *Neo-Lamarckianism*. As the word *Lamarckism* had already been long in use the shorter form *Neo-Lamarckism* should be preferred.

Through the operation of these two principles the phenomena of adaptation are explained. Adaptation is placed by him in what seems to be a new light, as the tendency of the organism to respond through modification of form and structure to an ever-changing environment. The introduction of this form of words by Mr. Spencer has been of the utmost value to science in affording it a clear and precise terminology for the most important of all phenomena. Lamarck floundered about in straining after such a terminology. As I have shown he generally used the word *circumstances* for Spencer's *environment*, but in many cases he employed the word medium (*milieu*) and he occasionally approached the Spencerian expression so nearly as to speak of the environing medium (*milieu environnant*).* His idea was undoubtedly the same, but he lacked both the literary training and the philosophic power to present it in its best light.

Mr. Spencer showed that the general proposition that the organism must be permanently, constantly, and profoundly influenced by the environment is one that cannot be logically escaped. It is not a mere *a priori* deduction, but rests upon all the facts and phenomena of the organic world which he marshaled in a most masterly manner in its support. But the Neo-Darwinians who deny this because it conflicts with their new hypothesis, never cease to demand facts. Haeckel's reply to this was eminently just, that this new hypothesis is itself wholly unsupported by facts, in the sense in which they use the term. It is an inference from the study of embryology, and an opposite inference is as legitimate as the one they draw. The truth is that the real phenomena of heredity are too recondite for direct observation. We are dealing with the

* Philosophie Zoologique, Vol. II, pp. 5, 304.

ultimate units of organic being and are compelled to judge of their actions by the general results. But Mr. Spencer went further than any one had done before him and brought together an immense array of the most convincing facts upon his side of the question. Although he wrote before the new hypothesis had been proposed he seems to have fairly anticipated it, and one is surprised to find the objections of the Neo-Darwinians clearly stated and squarely met. It would be needless to repeat his arguments here, even if there were time, but I may call attention especially to that which relates to the origin of those correlated structures which are necessary to render effective the modifications which natural selection or sexual selection has produced. He shows that unless these are due to inherited functional variations a series of violent assumptions must be made which put one's credulity to the severest test—not a pre-established harmony, but a multitude of pre-established harmonies, all of which must co-operate with unerring exactness. Under the hypothesis of the hereditary preservation of the functionally produced modifications necessary to secure these correlations the explanation is perfectly simple and rational. This argument, so far as I know, has never been answered, nor has any attempt been made to answer it.

Early in the discussion of Weismann's theory and three years before the appearance of the English edition of his essays, Mr. Spencer seems to have foreseen their probable effect in England, and he turned aside from his systematic labors to reargue this question in the light of fresh facts and evidence. This he did in two articles in the Nineteenth Century for April and May 1886, which are characterized by an unfailing vigor of treatment and all the philosophic power which he is wont to display in the discussion of biological questions. I would especially commend the second of these articles as an altogether fresh presentation of the case, replete with facts from the lowest forms of organized life. Many of these taken from the vegetable kingdom come home to me with great force, and it seems difficult to see how another interpretation can be put upon them.

Prof. Karl Semper published in 1881 as one of the International Scientific series his Natural Conditions of Existence as they effect Animal Life, in which he supports the same class of views by many observations from his own profound studies. Prof. Sidney H. Vines in his Lectures on the Physiology of Plants (1886) offered some direct and telling strictures upon Weismann's teachings (Chap. XXIII), and after the English edition of the essays appeared he repeated these and answered categorically a large number of points in a communication to *Nature*.* Professor Weismann replied to this review, defending himself satisfactorily at some points, but was compelled to recede from several of his most important positions.

Mr. Patrick Geddes advanced in the Encyclopedia Britannica (Art. Variation and Selection) a somewhat novel theory of variation in plants, substantially in the same line, but probably with some vulnerable points, and Professor Henslow's recent work on the Origin of floral structures, seeks to show that "the responsive actions of the protoplasm in consequence of the irritations set up by weights, pressures, thrusts, tensions, etc., of insect visitors," have played the principal rôle in determining the forms of irregular flowers. In much of all this there is a tendency to extremism, and harm is often done by neglecting to recognize the action of natural selection where it is clearly present, but there always remains a residuum of facts which cannot be explained by that hypothesis.

* Vol. XL, Oct. 24, 1889, p. 621.

Among those Germans who have so ably and systematically opposed the views of Weismann should doubtless first be mentioned Dr. G. H. Theodor Eimer. The work * in which he has most effectively undertaken this has been translated into English by Mr. J. T. Cunningham,† who is one of those who early took part in the discussion. The title of this work as well as the heads of some of the chapters (such as: "the influence of adaptation in the formation of species," "mental faculties as acquired and inherited characters," "evolution of the living world as the result of function," etc.) shows how directly Eimer antagonizes Weismann, and one of the leading merits of the book is the great number of new illustrations that it contains in support of his position.

Perhaps I should not pass over, in this imperfect survey, the able and very temperate paper of Mr. J. Arthur Thompson,‡ who, of all the writers here noted, comes the nearest to having anticipated the point of view of my own criticisms. The bibliography of the general subject which this writer gives at the end of his paper will enable any one who desires to pursue it further to supplement this brief enumeration to any extent, and also to take a retrospective view into its history and progress.

It would be easy to select from these and other works any required number of illustrations of the transmission of acquired characters, but there would not probably be one that Weismann would not find means of explaining away. He has

* Die Entstehung der Arten auf Grund von Vererbung erworbener Eigenschaften nach den Gesetzen organischer Wachsens, Jena, 1888.

† Organic Evolution as the Result of the Inheritance of Acquired Characters, London, 1890.

‡ The History and Theory of Heredity. Proc. Roy. Soc. Edinburgh, Vol. XVI, 1888–'89, pp. 91–116. (Read Jan. 21, 1889).

taken up a number of such in his essays, stated them with sufficient fairness, and then proceeded to show that they are also capable of another interpretation. In some cases this is doubtless true, but in most cases his explanations seem strained and unnatural. In many they amount to an admission that the quality transmitted was functionally acquired and that the changed environment has actually influenced the germ. But he always insists that this does not constitute an acquired character. I do not see why it does not. For example, he says: "It is difficult to say whether the changed climate may not have first changed the germ, and if this were the case the accumulation of effects through the action of heredity would present no difficulty" (p. 98). I cannot see why this is not conceding the whole issue. Of course all modifications must first affect the germ, otherwise there could be no hereditary transmission. The only question is: Can the climate or the environment impress changes upon the germ? If yes, the Neo-Lamarckian asks no more. All that he contends for is conceded.

The quotation just made is from one of his earlier essays and he has objected to its being urged against him on the ground that it does not represent his latest conclusions. But what has he to say to the following from his eighth and last essay originally delivered in September, 1888?

"It is therefore possible to imagine that the modifying effects of external influences upon the germ-plasm may be gradual and may increase in the course of generations, so that visible changes in the body (*soma*) are not produced until the effects have reached a certain intensity" (p. 433).

It matters nothing to the Neo-Lamarckian whether the effects of external influences become visible in the first or the hundredth generation. The whole question is: Are they the cause of the modifications that actually take place? Weismann's English followers deny this and say that such modifications are due to the selection of accidental variations in the germ, and so in all cases. If the term "acquired" is to be any further refined away, then discussion is useless, for it is not a mere dispute about a word that interests us, but the fundamental question whether external conditions do or do not permanently and progressively influence the development of organic beings.

THE AMERICAN "SCHOOL."

Probably the strongest arguments that have been brought forward upon the affirmative side of this question are those derived from paleontology, and singularly enough, hitherto, so far as I am aware, this view of the question has been presented, with the single exception of Kowalevsky, entirely by Americans. This work was not done under the stimulus of Weismann's writings, because most of it was already accomplished before his essays appeared.

As far back as 1866 Prof. Alpheus Hyatt read a paper "On the Parallelism between the different stages of life in the Individual and those of the entire Group of the Molluscan order Tetrabranchiata," * in which were foreshadowed the views more definitely expressed in 1880 in his papers "Upon the Effects of Gravity on the forms of shells and animals," † and "The Genesis of the Tertiary Species of Planorbis at Steinheim." ‡ In these papers Professor Hyatt showed the moulding influence of what in this case happened to be an

* Mem. Bost. Soc. Nat. Hist., Vol. I, p. 193. (Read Feb. 21, 1866).

† Proc. A. A. A. S., 1880, p. 527.

‡ Mem. Bost. Soc. Nat. Hist., Fiftieth Anniversary, 1880. Second Memoir.

environment growing gradually less and less favorable, but not the less adapted to display in a very clear light some of the most important laws of transformation.

In 1877 Mr. John A. Ryder read a paper "On the laws of digital reduction,"* showing the obvious adaptations to the changing environment which had taken place in vertebrates in this respect, and a year later he pursued the same line of argument for modifications of the teeth.†

Mr. Ryder has for many years past been engaged in embryological researches, but there is no evidence that they have led him to abandon the views expressed in these earlier papers in favor of those of Weismann. On the contrary, several comparatively recent papers of his ‡ consist in great part of direct attacks upon Weismann's teachings and criticisms of his embryological theories.

Professor Cope commenced publishing on this subject at about the same time and has continued to study the vertebrate fauna of America without interruption to the present time. It appeared to him from the first that paleontology affords proof of the causes of variation, as revealed in the wonderfully complete transition series that are found in the teeth, toes, and various parts of the skeleton of extinct animals, adapting them to a changing environment and higher structural perfection. The study of living animals cannot, in the nature of things afford any such series of forms, and the evi-

* American Naturalist, Vol. XI, October, 1877, pp. 603–607.

† On the mechanical genesis of tooth forms, by John A. Ryder. Proc. Nat. Sci. Phil., Vol. XXX, 1878, p. 45; Vol. XXXI, 1879, p. 47.

‡ The Origin of Sex through Cumulative Integration and the Relation of Sexuality to the Genesis of Species. Proc. Am. Phil. Soc., Vol. XXVIII, May 29, 1890, pp. 109–159.

A Physiological Hypothesis of Heredity and Variation. Am. Nat., Vol. XXIV, January, 1890, pp. 85–92.

dence from paleontology is particularly striking in this respect. A volume of Professor Cope's memoirs was published in 1887 under the title of "The Origin of the Fittest," by which title he aimed to express the idea of the cause or origin of modifications that have taken place, as distinguished from Darwin's explanation of the laws of transformation based on the assumption of such modifications taken as simple facts of observation. Both methods are scientific, but the former carries us one step nearer to the true origin of things.

More recently Prof. H. F. Osborn of Princeton College has taken up this line of argument and presented it in several memoirs in which he has attempted a direct answer to Weismann's charge that no facts have been furnished in support of the transmission of acquired characters.*

In the latest of these papers, that read before the Society of Naturalists in Boston December 31, 1890, not yet published, but of which an advance copy was kindly sent me by him, he has stated the whole problem with a judicial fairness which all must admire, and with a keenness of analysis which places him in the front rank of modern biological thinkers.

Perhaps the most important contribution which he has made to the subject is that in which he shows that "the main trend of variation is determined not by the transmission of the full adaptive modifications themselves, as Lamarck supposed, but of the disposition to adaptive atrophy or hypertrophy at certain points."

This principle goes farther than any other that has been brought forward to differentiate Neo-Lamarckism from Lam-

* Proceedings of the American Association for the Advancement of Science, Vol. XXXVIII, 1889 (Toronto), pp. 273–276.

British Association Report, 1889 (Newcastle-upon-Tyne), p. 621; Nature, Vol. XLI, Jan. 9, 1890, p. 227.

American Naturalist, Vol. XXIII, July, 1889, pp. 561–566.

arckism proper, while at the same time it is an effective answer to a large part of the argument directed against the transmission of functionally acquired characters.

Professor Osborn has probably made the most of the argument from paleontology, and it must be left to the candid judgment of scientific men to say whether the case is made out. It is of course always possible to say that the initial variations which inaugurated each new adaptation were merely accidental and were seized upon by natural selection, and it is to a large extent a question of faith in the universal efficacy of that theory; or rather a question in candid minds of the relative reasonableness of that view and of the view which ascribes a considerable part of this initial variation to functionally produced modifications transmitted by heredity.

It would be unjust to this Society to omit in an enumeration, however imperfect, of the American defenders of the transmissibility of acquired modifications, your former president Prof. W. H. Dall, whose protracted studies in invertebrate paleontology, conchology, and especially the molluscan life of the deep sea have led him to a full accord with other American workers as regards questions of this class. In his presidential addresses, not to speak of earlier papers, he has emphasized the molding influence of the environment upon the plastic organisms with which he is most familiar, and during the past year he has contributed to the Society one paper* dealing directly with the Neo-Darwinian claims, in which the case is as clearly presented as it has been by any other writer, and in many respects in an entirely new light.

For myself, I cannot claim to have made any direct contribution to this specific subject. I have been deeply interested

*On Dynamic Influences in Evolution, by W. H. Dall. Proc. Biol. Soc. Wash., Vol. VI, pp. 1–10.

in the development of plant life and have from time to time within the past fifteen years presented this theme from every point of view that I have been able to see it. I recognize the law of natural selection as probably the most potent of all organic laws, but I have never doubted that a great part of the variations upon which its action depends are due to reactions of the organism upon the environment, and after reading Weismann's essays and every scrap of discussion that I have been able to find arising from them, I am still so dull as to remain unconvinced that such modifications are incapable of hereditary transmission. To say that the environment may and must influence the germ, but that it can only influence it in a hap-hazard way analogous to that in which a jar affects the figures of a kaleidoscope, is to my mind a begging of the question, and I prefer to assume that there is a causal connection between the nature of the influence on the germ and the alterations that result, especially as the latter are admitted to be in harmony with the former.

If I have succeeded in showing in one of my papers before this Society* that considerable variation is constantly taking place irrespective of any advantage to the species, this much at least has been withdrawn from the domain of natural selection, and if these changes are not produced by that law there seems no escape from the conclusion that they are caused by some unknown external influences.

In the foregoing review of the work that has been done toward the scientific demonstration of the transmissibility of functionally acquired characters I do not pretend to have given the arguments themselves. I have only pointed out the fact that they have been presented, by whom, from what

* Fortuitous Variation as illustrated by the genus Eupatorium. Abstract in Nature (London) Vol. XLI, July 25, 1889, p. 310.

branch of science, and under what circumstances, and I must leave it to each of you, if sufficiently interested, to study them for yourselves from the original sources.

APPLICATION TO THE HUMAN RACE.

The wide-spread agitation of a problem of this nature, however technical or recondite it may be, lying as it does on the very ocean bed of science, cannot help sooner or later making itself felt at the surface and producing its normal influence upon the great practical questions of the moral and social world. And the nature of this influence, fortunately for us, is some indication of the truth or falsity of the views defended. Just as the mathematician knows, when his calculations lead him to just and rational results that his assumption was a true one, and when they lead to a series of negations and absurdities, that it was a false one, so we may expect that if the assumption of the non-transmissibility of acquired characters is a sound one the practical conclusions that flow from it bearing upon the affairs of life will harmonize with the best thought on the development of the human race ; and conversely, if its application to practical life conflicts with such best thought and with the facts of history and of social progress we are justified in the inference that it is an unwarranted assumption. What do we find ?

The highly artificial character of what we call civilization is a fact which I have for many years sought to enforce by a variety of illustrations. That nothing like it could ever result from the natural flow of the forces that have combined to produce it is too obvious to require explanation, and that human advancement in general is exclusively the result of the exercise of man's intellectual power in the artificial direction of the raw forces of nature into channels of human advantage, is a proposition which only needs to be understood to be universally admitted. The tendency of the scientific mind to apply to social phenomena the canons that prevail in the non-intelligent world, is at least as ancient as the French physiocrats, Adam Smith, Ricardo, and Malthus, and it has been strengthened since Darwin by the writings of some of the ablest social philosophers. It rests on the seductive idea that what nature does must be well done, and that nature's methods must be the best methods for man to adopt. I have hitherto designated this kind of philosophy as a sort of nature-worship, and shown that the entire fabric of reasoning crumbles away at the first touch of critical analysis. But it is a fascinating habit of thought and difficult to dislodge from a certain type of mind.

Now on examining the practical applications which the Neo-Darwinians make of their underlying conception, I find them to be strikingly in line with those last described. If nothing that the individual gains by the most heroic or the most assiduous effort can by any possibility be handed on to posterity, the incentive to effort is in great part removed. If all the labor bestowed upon the youth of the race to secure a perfect physical and intellectual development dies with the individual to whom it is imparted why this labor? If, as Mr. Galton puts it, nurture is nothing and nature is everything, why not abandon nurture and leave the race wholly to nature? In fact the whole burden of the Neo-Darwinian song is: Cease to educate, it is mere temporizing with the deeper and unchangeable forces of nature. And we are thrown back upon the theories of Rousseau, who would abandon the race entirely to the feral influences of nature.

The great men who talk this way, trained in the methods of the university, their minds stored with the fundamental, comprehensive, and organized materials for thinking and working which modern methods of education could alone have given them, use these materials, and take advantage of this training to spin out a subtle thread of reasoning which results in condemning the only means by which they were enabled to comprehend questions of this nature. Professor Weismann could never have prosecuted those prolonged investigations which have given him such a grasp of the intricate problem of heredity had he not been trained in the rigid methods of the German universities. Nay, those rigid methods themselves have been the product of a series of generations of such training, transmitted in small increments and diffused in increasing effectiveness to the whole German people. It has not been brought about by natural selection which only selects such ancestral germ-plasms as increase the certainty of reproduction. Such habits of mind could have no such tendency. They secure no advantage in the struggle for existence. And the fact that out of the barbaric German hordes of the Middle Ages there has been developed the great modern race of German specialists is one of the most convincing proofs of the transmission of acquired characters, as well as of the far-reaching value to the future development of the race of such an educational system as that which Germany has had for the last two or three centuries.

It was said of Mr. Darwin that he was himself a good illustration of the law of atavism which he formulated since his habit of mind lay latent in his father and came to him from his grandfather Erasmus. Similarly it might be said that Professor Weismann is as good an example as need be asked of the transmission of acquired characters and of the hereditary embodiment of that wide-spread German characteristic which has been the increasing product of the German educational system and of German institutions.

Mr. Herbert Spencer has followed out this same line of reasoning as applied to the great development of the musical faculty in Germany, and shown that the Haydns, Mozarts, Beethovens, the Liszts, Rubensteins, and Wagners, have formed, as it were, the several peaks of a great hereditary musical uplift in the German nation. The same is true of Italy, not only in music, but especially in sculpture, and we have there, so to speak, a race of sculptors. Those who, without any patriotic bias, compared the Italian and American pieces at the Centennial Exhibition could not help being impressed with this. There could be seen the most exquisite pieces of statuary, in which not only features of rare perfection and beauty, but every form of drapery were represented in marble with a trueness to life that almost deceived the looker on. And to such pieces were attached, not one or two celebrated names, but a great number of names of artists unknown to the public outside of those who make sculpture a special study. Contrasting these perfect productions with the lifeless ones that represented the highest reaches of American sculpture, even those produced by Americans who had spent many years at Rome and worked in an atmosphere of Italian sculpture, I was impressed with the little that a single generation can accomplish in such things, and with the fact that in Italy we have a race of born sculptors who inherit their deftness from ancestors as remote as Michel Angelo.

Weismann has not ignored the arguments from this side, but his attempts to meet them are among the weakest of all his reasonings. Here are some samples of them: "The children," he says, "of accomplished pianists do not inherit the art of playing the piano; they have to learn it in the same laborious manner as that by which their parents acquired it; they do not inherit anything except that which their parents also possessed when children, viz., manual dexterity and a good ear" (p. 269). "The pianist . . . may by practice develop the muscles of his fingers so as to ensure the highest dexterity and power; but such an effort would be entirely transient, for it depends upon a modification of local nutrition which would be unable to cause any change in the molecular structure of the germ-cells, and could not therefore produce any effect upon the offspring" (p. 278). If this were true nothing is more certain than that the talent for piano execution could be no higher in the ten thousandth generation than that attained during the first, and that the curve representing the progress of music, sculpture, the talent for special scientific research, or any other form of genius, would be an irregular line with absolute average horizontality instead of what we know it to be in every case, an irregular, but progressively ascending curve marking a great forward movement.

It is universally conceded that the evidence for the transmission of acquired mental qualities is much stronger than for those of any other class, chiefly because they are entirely withdrawn from the action of natural selection, not tending in the least to the survival of the fittest. It has therefore been necessary for Weismann to deny their transmission at all. This is so palpably contrary to the facts of human history that few have been willing to follow him to this length. It is well known that Mr. Wallace has always excepted the human race from the action of natural selection, but in so doing he has seen fit to abandon the scientific method entirely, and in his last work he makes a complete break in the continuity of development with the advent of the higher psychic faculties, calling in an independent spiritual attribute to account for this class of phenomena. Prof. E. Ray Lankester, the foremost of Neo-Darwinians, in reviewing this work of Mr.

Wallace* makes the following remarks on this point: "Mr. Wallace's contention that the mathematical and artistic faculties of man have not been developed under the law of natural selection must in large part be conceded . . . . their sudden and rapid development to a very much higher level in civilized communities cannot be traced to the struggle between man and man. It does not however follow that, because natural selection will not account for these extraordinary developments of the human brain, therefore we must have recourse to the assumption of supernatural agencies. Mr. Wallace seems so much convinced of the capability of the principle of natural selection, that when it breaks down as an explanation he loses faith in all natural cause, and has recourse to a metaphysical assumption." But Prof. Ray Lankester, estopped by his consistent defense of Weismann's views, is obliged to ignore the obvious explanation that the intense exercise of these faculties, impressing itself profoundly upon the plastic brain substance and reacting upon the germs of posterity has been transmitted to descendants through centuries of developing civilization, and he has recourse to his doctrine of "sports" and to Gulick's law of "divergent evolution" which is nearly the same as what I have called "fortuitous variation."

But we need not confine ourselves exclusively to the mental qualities. A favorite illustration of the efficacy of selection is the progress which has been secured in the fleetness and other desired qualities in horses, and Mr. Wallace, in the Fortnightly Review for September 1890, has instituted a contrast between what would result in this direction from a system of intelligent breeding and one of mere feeding and exercise. His illustration is thoroughly unfair, even ridiculous, since he does not

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* Nature, Vol. XL, Oct. 10, 1889, pp. 569–570.

attempt to transmit the acquired superiority but allows it to be diluted and lost by promiscuous breeding with stock that has not been subjected to any training. As a matter of fact training enters largely into the development of superior breeds of horses, and great care is taken that educated strains be bred together. And breeders as a rule would ridicule the idea that all their training goes for nothing, and that it is only accidental variations that can be bred into the new race of horses.

But let us take another case in which natural selection is wholly excluded. It is well known that a steady and uniform progress has been going on for a century or more in all forms of gymnastic skill and feats of bodily suppleness by men constantly in training for the purpose, which is comparable to that which has taken place in the trotting power of horses. Every year new wonders are brought before the public and the feats of the previous year are exceeded by some fresh virtuoso. This is accomplished, I am told, by lifelong training of the children of acrobats and of their children, thus producing, as it were, a little race of acrobats. What care is taken to prevent the loss of much of this through marriage outside of the trained stock, I do not know, but certain it is that great progress in physical development has taken place and is taking place, and there is no doubt whatever that it is largely due to the transmission of the qualities directly acquired by training.

In fact, Mr. Galton's conclusions, notwithstanding his doubts about the transmission of acquired talents, are not only not opposed to that view but in great part confirmatory of it. He is led by a carefully conducted series of inquiries and investigations to believe that genius is in the main hereditary; that the exceptionally talented and highly endowed are descended from talented and highly endowed parents, etc. But this only throws the question back one generation farther, and it remains to be shown that such talent and endowment in their ancestors was not the result of education, personal effort, or some other form of acquirement and not of mere accident.

But the great debate on heridity seems destined to secure still other and more far-reaching advantages. Not only has it assured us that we may hand our good works down to posterity through the law of the transmissibility of acquired qualities, but it may and should teach us that the all-powerful law of selection is also an instrument in the hands of intelligence for the working out of human destiny. It is the right and the duty of an energetic and virile race of men to seize upon every great principle that can be made subservient to its true advancement, and undeterred by any false ideas of its sanctity or inviolability, fearlessly to apply it. Natural selection is the chief agent in the transformation of species and the evolution of life. Artificial selection has given to man the most that he possesses of value in the organic products of the earth. May not men and women be selected as well as sheep and horses? From the great stirp of humanity with all its multiplied ancestral plasms —some very poor, some mediocre, some merely indifferent, a goodly number ranging from middling to fair, only a comparative few very good, with an occasional crystal of the first water —from all this, why may we not learn to select on some broad and comprehensive plan with a view to a general building up and rounding out of the race of human beings? At least we should by a rigid selection stamp out of the future all the wholly unworthy elements. Public sentiment should be created in this direction, and when the day comes that society shall be as profoundly shocked at the crime of perpetuating the least taint of hereditary disease, insanity, or other serious defect as it now is at the comparatively harmless crime of insect, the way to practical and successful stirpiculture will have already been found.

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