PROCEEDINGS

OF THE

American Philosophical Society

HELD AT PHILADELPHIA

FOR

PROMOTING USEFUL KNOWLEDGE

VOLUME LXVII

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MINUTES OF THE MEETINGS

OF THE

AMERICAN PHILOSOPHICAL SOCIETY

DURING 1928

Stated Meeting, January 6, 1928

FRANCIS X. DERCUM, M.D., Ph.D., Sc.D., President, in the Chair

The decease was announced of the following members:

Israel C. White, A.M., Ph.D., LL.D., Sc.D., at Morgantown, W. Va., November 24, 1927, æt. 79.

Edward V. d'Invilliers, E.M., Sc.D., at Philadelphia, January 4, 1928, æt. 71.

Professor Edwin G. Conklin read a paper on "Some Recent Criticisms of Eugenics." The paper was discussed by President Dercum, Secretary Miller, Dr. Harshberger and Dr. Seifert, a guest.

Mr. Price moved the following resolution which was adopted by the Society:

"RESOLVED that the Society lend to the Pennsylvania Museum the four busts modeled by Houdon for display in the special exhibition of Houdon's works, which will form a part of the opening exhibition of the new Philadelphia Museum of Art."

Stated Meeting, February 3, 1928

FRANCIS X. DERCUM, M.D., Ph.D., Sc.D., President, in the Chair

The decease was announced of the following members:

Thomas Frederick Crane, A.M., Litt.D., at LeLand, Florida, December 9, 1927, æt. 83.

George Washington Goethals, Sc.D., LL.D., at New York City, January 21, 1928, æt. 70.

Talcott Williams, A.B., A.M., L.H.D., LL.D., at New York City, January 24, 1928, æt. 79.

Dr. W. F. G. Swann read a paper on "Theories of the Atom." The paper was discussed by President Dercum, Secretary Goodspeed, Dr. Seifert and two other guests.

A letter was received from J. Fair Hardin asking for a photostat copy of George Hunter's Journal on his trip up the Red and Quachita Rivers with William Dunbar, in 1804 to be published in the Louisiana Historical Quarterly.

The Library Committee presented the following resolution concerning the above request:

THE Library Committee is favorably inclined toward granting the request of J. Fair Hardin to have a photostat copy made of the manuscript Journal of Dr. George Hunter and the same to be published in the Louisiana Historical Quarterly; under the following conditions:

"THAT the Society shall be under no expense; that the Society shall be given full credit as possessing the original manuscript; that no commercial use be made of it; that a positive photostatic copy be sent to J. Fair Hardin the negative being retained by the Society at Mr. Hardin's expense; and that a full printed copy be presented to the Society either as a reprint, or as published in the Louisiana Historical Quarterly."

The resolution presented by the Library Committee was adopted.

The following proposed amendment to the Laws was announced, Chapter I, Section 1 of the Laws by striking out the sentence:

"NOT more than fifteen members residing within the United States shall be elected in any one year" and inserting in lieu thereof the following: "AT such election no more members residing within the United States shall be elected than will make the total number of such members four hundred."

Stated Meeting, March 2, 1928

FRANCIS X. DERCUM, M.D., Ph.D., Sc.D., President,
in the Chair

The decease was announced of the following members:

William Charles Lawson Eglin, at Philadelphia, February 8, 1928, æt. 58.

Hendrik Antoon Lorentz, at Harrlem, February 6, 1928, æt. 75.

Professor Ulric Dahlgren read a paper on "Equilibration and Hearing" which was illustrated by lantern slides. The paper was discussed by President Dercum and Professor Snyder.

Professor Monroe B. Snyder presented an informal communication on "A New X-Ray Spectroscopy."

The following proposed amendment to the Laws was announced:

> "IT is hereby moved that Section 1 of Chapter XII of the Laws of this Society be amended so as to annul the phrase: 'and enacted at the subsequent general meeting in the month of April,' and the same be replaced with the phrase, 'and enacted at a subsequent stated meeting of the Society.'"

General Stated Meeting, April 19, 20, 21, 1928

FRANCIS X. DERCUM, M.D., Ph.D., Sc.D., President,
in the Chair

Thursday Afternoon, April 19th
Opening Session, 2 o'clock

FRANCIS X. DERCUM, M.D., Ph.D., Sc.D., President,
in the Chair

The following papers were read:

"Tundra Vegetation of Central Alaska," by John W. Harshberger, Professor of Botany, University of Pennsylvania.

"Features of Cells that Live Long," by Daniel T. MacDougal, Carnegie Institution of Washington; Desert Laboratory, Tucson, Arizona.

"A Genogeographical Study of the Genus Bursa," by George H. Shull, Professor of Botany and Genetics, Princeton University.

"Trianaeopiper, a New Genus of Piperaceae," by William Trelease, Professor of Botany, University of Illinois.

"Cell Division and Differentiation," by Edwin G. Conklin, Professor of Biology, Princeton University. Discussed by Drs. Sajous and Stockard.

"Probable Rôle of Internal Secretions in Structure and Growth as Illustrated by Breeds of Dogs and Peculiar Types in Man," by Charles R. Stockard, Professor of Anatomy, Cornell University. Discussed by Dr. Conklin.

"Textual Criticism of the Greek Old Testament," by Max L. Margolis, Professor of Biblical Philology, Dropsie College for Hebrew and Cognate Learning. Discussed by Dr. Sajous.

"Cod Liver Oil and the Cod," by Alfred F. Hess, Clinical Professor of Pediatrics, University and Bellevue Hospital Medical College, New York City. Introduced by Dr. Dercum. Discussed by Dr. Sajous.

"Different Rates of Growth Among Animals," by Philip P. Calvert, Professor of Zoology, University of Pennsylvania.

"The Genetics and Cytology of a Haploid Sport from Oenothera Franciscana," by Bradley M. Davis, Professor of Botany, University of Michigan. (Read by title.)

Friday, April 20th
Executive Session, 9.30 o'clock

Francis X. Dercum, M.D., Ph.D., Sc.D., President, in the Chair

Max L. Margolis and Joseph Erlanger, recently elected members, subscribed the laws and were admitted into the Society.

The decease was announced of the following members:

Henry S. Pancoast, M.A., L.H.D., at Philadelphia, March 25, 1928, æt. 78.

William C. Sproul, B.S., LL.D., at Chester, Pa., March 27, 1928, æt. 57.

Theodore W. Richards, S.B., A.M., Ph.D., Sc.D., Chem.D., M.D., LL.D., at Cambridge, April 3, 1928, æt. 60.

The following amendment was presented and on vote was unanimously adopted: Chapter I, Section 1, of the Laws by striking out the sentence:

"NOT more than fifteen members residing within the United States shall be elected in any one year" and inserting in lieu thereof the following: "AT such election no more members residing within the United States shall be elected than will make the total number of such members four hundred."

The following amendment was also voted on but was defeated:

THAT Section 1 of Chapter XII of the Laws of this Society be amended so as to annul the phrase: "and enacted at the subsequent general meeting in the month of April," and the same be replaced with the phrase: "and enacted at a subsequent stated meeting of the Society."

Morning Session, 10 o'clock

HENRY FAIRFIELD OSBORN, Sc.D., Ph.D., LL.D., Vice-President, in the chair

The following papers were read:

"Omorhamphus, a New Flightless Bird from the Eocene of Wyoming," by William J. Sinclair, Associate Professor and Curator, Princeton University. Discussed by Drs. Scott and Shull.

"The Reports of the Princeton University Expeditions to Patagonia," by William B. Scott, Professor of Geology, Princeton University.

"The Astrapotheria of the Miocene of Patagonia," by William B. Scott, Professor of Geology, Princeton University. (Read by title.)

"Were the Ancestors of Man Primitive Brachiators?" by William K. Gregory, Professor of Paleontology, Columbia University. (Read by Francis Montague Ashley-Montague.) Discussed by Dr. Osborn.

"Racial Characters in Human Dentition," by Milo Hellman, New York City. Introduced by Dr. Osborn. Discussed by Drs. Kennelly, Osborn, Conklin and Shull.

"Present Status of the Problem of Human Ancestry," by Henry Fairfield Osborn, American Museum of Natural History. Discussed by Drs. Scott and Conklin.

"Flood Control," by Arthur E. Morgan, President of Antioch College. Introduced by Dr. Conklin. Discussed by Drs. Scott and D. Johnson.

"Storms Which Issue from the Inland-Ice of Greenland," by William H. Hobbs, Professor of Geology, University of Michigan.

"A Guide Book to the World's Weather and Climates," by Robert DeC. Ward, Professor of Climatology, Harvard University. (Read by title.)

"Brain Characters of the Men of the Stone Age," by Frederick Tilney, Professor of Neurology, College of Physicians and Surgeons, Columbia University. Introduced by Dr. Osborn. (Read by title.)

"The Anatomy of Pithecanthropus Erectus," by J. Howard McGregor, Professor of Zoology, Columbia University. Introduced by Dr. Osborn. (Read by title.)

Afternoon Session, 2 o'clock

CYRUS ADLER, M.A., Ph.D. in the Chair

A Symposium on Aviation held at the Ledger Club, Independence Square.

"Commercial Aspects of Aviation," by William P. MacCracken, Jr., Assistant Secretary of Commerce for Aeronautics. (Read by title.)

"The Application of Aerodynamics," by Edward P. Warner, Assistant Secretary of the Navy for Aeronautics.

"Lighter than Air Machines," by C. E. Rosendahl, Lieutenant Commander, U. S. Navy.

"Heavier than Air Machines," by C. H. Biddlecombe, New York City (formerly Major in the Royal Air Force).

"Meteorology for Aviation," by William R. Blair, Major, Signal Corps, U. S. Army.

Friday Evening, 8 o'clock

Richard P. Strong, Professor of Tropical Medicine, Harvard University

spoke on

Studies of Human and Animal Diseases Made During the Recent African Expedition

Saturday, April 21st

Executive Session, 9.30 o'clock

Stated Business, Election of Officers and Members

Francis X. Dercum, M.D., Ph.D., ScD., President, in the Chair

The Society proceeded to an election of Officers and Members.

The Tellers subsequently reported that the following officers and members had been duly elected:

President

Francis X. Dercum

Vice-Presidents

William W. Campbell
James H. Breasted
Elihu Thomson

Secretaries

Arthur W. Goodspeed
John A. Miller

Curator

Albert P. Brubaker

Treasurer

Eli K. Price

Councillors

(To serve for three years)

Henry H. Donaldson
Russell Duane
Herbert S. Jennings
Arthur E. Kennelly

Members

James W. Alexander
John Ashhurst
Charles Peter Berkey
Nathaniel Lord Britton
Anton Julius Carlson
Theodore Dru Alison Cockerell
Paul P. Cret
Max Farrand
William (Jacob) Holland
Eldridge R. Johnson
William Smith Mason
David Hunter Miller
Clifford Herschel Moore
Agnes Repplier
Edwin Wilbur Rice, Jr.
A. S. W. Rosenbach
Charles Edward St. John
St. George L. Sioussat
Hugh S. Taylor
Charles Hyde Warren

Morning Session, 10 o'clock

Albert P. Brubaker, A.M., M.D., LL.D.,
in the Chair

The following papers were read:

"Can Business Be Made a Science?" by Emory R. Johnson, Professor of Transportation and Commerce, University of Pennsylvania. Discussed by Dr. Kennelly and Mr. Fisher.

"Some Economic Implications in America's Changing World Status," by Ernest M. Patterson, University of Pennsylvania. Introduced by Dr. Johnson. Discussed by Dr. Kennelly and Mr. Fisher.

"An Enactment of Fundamental Constitutional Law in Old South Arabia," by James A. Montgomery, Professor of Hebrew and Aramaic, University of Pennsylvania. Discussed by Drs. Shapley and E. Johnson.

"Functions of the Internal Secretions or Endocrine Organs that Scientific Progress has Sanctioned," by Charles E. de M. Sajous, Professor of Endocrinology, University of Pennsylvania Graduate School of Medicine. Discussed by Dr. Keen.

"Research in Education," by Frank Pierrepont Graves, President of the University of the State of New York.

"An Early Colonel House: Unofficial Missions to England in 1842 and 1843 of General Duff Green," by St. George L. Sioussat, Professor of American History, University of Pennsylvania. Introduced by Dr. Lingelbach.

"Fact," by Henry Osborn Taylor, New York City.

"Noah, a Suggestion," by Robert P. Field, Philadelphia.

Afternoon Session, 2 o'clock

Francis X. Dercum, M.D., Ph.D., Sc.D., President, in the Chair.

The following papers were read:

"Metabolism in the Tropics: A Study on some Browns and Blacks in Jamaica," by Francis G. Benedict, Director of Nutrition Laboratory, Carnegie Institution of Washington, and Morris Steggerda.

"Racial Chromosomal Differences in Datura and their Bearing on Differentiation of Species," by Albert F. Blakeslee, Assistant Director in Plant Genetics, Carnegie Station for Experimental Evolution, Cold Spring Harbor.

"Influence of Groups Containing Sulphur on the Color of Azo Dyes," by E. Emmet Reid, Professor of Chemistry, Johns Hopkins University. Introduced by Dr. Smith.

"A Method for Determining the Constants of an Electromagnetic Oscillograph," by Arthur E. Kennelly, Professor of Electrical Engineering, Harvard University.

"Discussion of the Kinetic Theory of Gravitation IV: Correlation of Continual Generation of Heat in some Substances, and Impairment of their Gravitational Acceleration," by Charles F. Brush, President of the Cleveland Chamber of Commerce.

"A Search for the Galactic Center," by Harlow Shapley, Director of the Harvard Observatory. Discussed by Secretary Miller, Drs. Russell and Kennelly.

"The Distances of the Stars," by Samuel A. Mitchell, Professor of Astronomy and Director of the Leander McCormick Observatory, University of Virginia. Discussed by Secretary Miller, Drs. Shapley and Russell.

Saturday Evening

Annual Dinner held in the Bellevue Stratford Hotel.
The responses to the four Toasts
"Franklin," by William E. Lingelbach
"Our Sister Societies," by Arthur E. Kennelly
"Our Universities," by Frank P. Graves
"The American Philosophical Society," by Harvey W. Wiley

Stated Meeting, November 2, 1928

Francis X. Dercum, M.D., Ph.D., Sc.D., President, in the Chair

The decease was announced of the following members:

Hideyo Noguchi, M.D., Ph.D., M.S., Sc.D., at Accra, W. Africa, May 2, 1928, æt. 52.

Edgar F. Smith, Ph.D., Chem.D., Sc.D., L.H.D., LL.D., Litt.D., at Philadelphia, May 3, 1928, æt. 74.

Maurice Bloomfield, A.M., Ph.D., LL.D., L.H.D., at San Francisco, June 13, 1928, æt. 73.

Wilhelm L. Johannsen, in Copenhagen, November 11, 1927.

Dr. William K. Gregory read a paper on "The Upright Posture of Man; its Origin and Evolution," which was illustrated by lantern slides. The paper was discussed by Drs. Schaeffer and McClung.

Professor Monroe B. Snyder presented an informal communication on the "Interpretation of the positive Ray Analysis."

Mr. Price presented the following report from the Committees on Site and Building Fund:

"The Committees on Site and Building Fund have held a number of meetings, in order to determine the best method of securing funds for the erection of the Society's New Hall on the Parkway and for the increase of its endowment fund to an amount sufficient to ensure an adequate development of the purpose of the Society for the future, and, in order to obtain the necessary technical advice in the conduct of such a campaign, they engaged the services, as advisor, of the John Price Jones Corporation, an organization, which has aided in conducting to a successful conclusion a large number of similar campaigns in Philadelphia and its vicinity within the past five years. The Committees have also requested the President to appoint the necessary committees recommended by the John Price Jones Corporation to conduct the Campaign."

They therefore offer the following for adoption:

"RESOLVED that the American Philosophical Society approves and ratifies the action of its Committees on Site and on Building Fund in engaging the John Price Jones Corporation as advisor in conducting a campaign for the increase of the building fund and the endowment fund and in requesting the President to appoint the several committees required for the conduct of the campaign."

The above resolution was unanimously adopted.

Stated Meeting, December 7, 1928

FRANCIS X. DERCUM, M.D., Ph.D., Sc.D., President, in the Chair

Agnes Repplier and St. George L. Sioussat, recently elected members subscribed the laws and were admitted into the Society.

A letter was received from Dr. Adler presenting the following resolution:

"RESOLVED that the President be authorized to appoint a Committee to draft an address to Mr. Herbert Hoover, a member of the Society, upon the occasion of his election to the Presidency of the United States."

Names of committee: Cyrus Adler, Eli Kirk Price, and Arthur W. Goodspeed.

The above resolution was unanimously adopted.

The decease was announced of the following members:

Thomas Chrowder Chamberlin, Ph.D., Sc.D., LL.D., at Chicago, November 16, 1928, æt. 85.

Burnet Landreth, at Bristol, Pa. December 2, 1928, æt. 86.

Professor Walton Brooks McDaniel, Professor of Latin, University of Pennsylvania, read a paper on "When Terpsichore was Young," which was illustrated by lantern slides.

Professor Monroe B. Snyder presented an informal communication on "The Interpretation of the Transmutations of Atoms in Radioactivity."

The following paper by Dr. William Trelease was read by title:

"Two additional species of Trianaeopiper."

The minutes of the Stated Meeting of the Council held on November 16, 1928, were read and approved.

The Annual Report of the Girard Trust Company, Trustees of the Building Fund was presented and on motion referred to the Committee on Audit.

ADDENDUM

Please insert the following in your copy of the PROCEEDINGS, Vol. LXVI, 1927, Minutes, page xiii, before the last paragraph.

Dr. Daniel T. MacDougal, Director of Plant Physiology, Carnegie Institution of Washington, read a paper on "Substances Regulating the Passage of Materials into and out of Plant Cells; the Lipoids." The paper was discussed by Drs. Sajous, Donaldson, Brubaker and Harshberger.

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Vol. 67 1928 No. 1

SOME NEGLECTED BOTANICAL RESULTS OF THE LEWIS AND CLARK EXPEDITION

By RODNEY H. TRUE

(*Read at the General Meeting of the Society on April 24, 1925*)

The records of the Lewis and Clark Expedition show that while much botanical material was lost en route, a considerable number of herbarium specimens was brought back. This formed the basis of most of the work done by Pursh on Lewis's plants, published in his Flora Americae Septentrionalis issued in 1814. The records also refer to seeds, cuttings and other living material brought in by Lewis and Clark. Reference to this part of the collections is sparing and lacking in detail. In his report to Jefferson dated at St. Louis, Mo., September 23, 1806, announcing the return of the expedition, Lewis [1] writes: "I have also preserved a pretty extensive collection of pla(n)ts in Horteo."

In a letter written by Jefferson [2] to his French correspondent, de la Cepède, he says: "In the meantime, the plants of which he (Lewis) brought seeds, have been very successfully

[1] Lewis, Meriwether, to Thomas Jefferson, in Thwaites, "Original Journals of the Lewis and Clark Expedition," Vol. 7, 327.

[2] Letter Jefferson to de la Cepède, July 14, 1808. "Memorial Edition of the Writings of Thomas Jefferson," 12: 85.

(22)

raised in the botanical garden of Mr. Hamilton of the Woodlands and by Mr. McMahon, a gardener of Philadelphia." . . .

In writing to another of his European friends on December 8, 1813, Madame de Tessé, Jefferson [1] again mentions these living plants. "All Lewis's plants are growing in the garden of Mr. McMahon, a gardener of Philadelphia." . . .

In the introduction to his Flora Americae Septentrionalis, Pursh [2] states that "A small but highly interesting collection of dried plants was put into my hands by those gentlemen (Lewis and Clark), in order to describe and figure those I thought new." . . . "Several of them I have had an opportunity of examining in their living state, some being cultivated from seeds procured by Mr. Lewis." . . .

The history and contents of the collection of herbarium specimens are a matter of botanical knowledge, but the story of the living plants held in St. Louis on the return of the expedition "in Horteo" by Lewis and those grown in Philadelphia by McMahon and William Hamilton from the seeds forwarded to President Jefferson while Lewis himself tarried in the West seems not to have been clearly traced. It is my hope to throw some light on this part of the botanical result growing out of this famous expedition.

While the work of exploration itself seems to have been carried out with an almost unexpected degree of success, the fate of the records and of the materials brought back seems to have been one of greatest difficulty. The death or bankruptcy of those most essential to success in harvesting the results of the field work more than once delayed and confused those remaining. The records passed from hand to hand and only after a century had elapsed did they receive more than a partial and inadequate publication.

The monumental work of Thwaites in eight volumes was believed to place on record the whole story of the Expedition.

There is reason, however, to believe that the tale has not yet been completely told. During the confusion following

[1] "Jefferson to Madame de Tessé, Monticello," December 8, 1813.
[2] "Memorial Edition of the Writings of Thomas Jefferson," 14: 28.

the death of Dr. Benjamin S. Barton, who acted as a sort of "central" for the collection and deposit of the various sorts of proceeds and records, responsibility seems to have become diffused and only the active and anxious seeking of Jefferson through both direct and indirect channels seems to have led to the salvaging of the result. Confusion came about the more easily because a wide latitude was left to Captain Lewis and Captain Clark in directing the disposition of their collections and records, in order that they might have the first advantage in the publishing of their results. Lewis neglected to make adequate arrangements and Captain Clark found chaos after his colleague's tragic death.

The records of cuttings, seeds and other living material begins with a letter from Clark to Jefferson, then President, sent from St. Louis not long before the winter camp on the River Dubois was broken up preparatory to the long journey up the Missouri.

Since the circumstances of the introduction of the Osage orange into the East have been a matter of some doubt, I shall let Clark's letter introduce this discussion.

St. Louis, March 26, 1804.

Dear Sir:

I send you herewith enclosed, some slips of the *Osage Plum*, and *Apple*. I fear the season is too far advanced for their success. had I earlyer learnt that these fruits were in the neighborhood, they would have been forwarded at a more proper time. I would thank you to send a part of them to Messrs. John Mason and William Hamilton, should they not succeed, Mr. Charles Gratiot, a gentleman of this place, has promised me that he would with pleasure attend to the orders of yourself or any of my acquaintancies, who may think proper to write him on the subject. Mr. Gratiot can obtain the young plants at the proper season, and send them very readily to Mr. Trist if requested to do so. I obtained the cuttings now sent you, from the garden of Mr. Pierre Choteau, who resided the greater portion of his time for many years with the Osage nation. it is from this gentleman, that I obtained the information I possess with respect to these fruits. . . . (The Osage plum is described)

The *Osage Apple* is a native of the interior of the continent of

North America, and is perhaps a nondiscript production. the information I have obtained with respect to it is not so minute as I could wish nor such as will enable me to describe it in a satisfactory manner. Mr. Peter Coteau, who first introduced this tree in the neighborhood of St. Louis, about five years since, informed me, that he obtained the young plant at the great Osage vilage from an Indian of that nation, who said he procured them about three hundred miles west of that place. the general contour of this tree is very much that of the *black haw*, common to most parts of the United States, with these differences however, that the bark is of a lighter colour, less branced (sic) and arrives to a larger size, sometimes rising to the hight of thirty feet. its smaller branches are armed with many single, long and sharp, pinated thorns. the particular form of the leaf or flower I have been unable to learn. so much do the savages esteem the wood of this tree, for the purpose of making their arrows that they travel many hundred miles in quest of it. The particulars with respect to the fruit, is taken principally from the Indian discription: My informant never having seen but one specimen of it, which was not full ripe, and much shriveled and mutilated before he saw it. the Indians gave an extravagant account of the exquisite odour of this fruit when it has obtained maturity, which takes place at the latter end of summer, or the begining of Autumn. they state, that at this season they can always tell by the scent of the fruit when they arrive in the neighborhood of the tree, and usually take advantage of this season to obtain the wood; as it appears not to be a very abundant growth, even in the country where it is to be found an opinion prevails among the Osages, that the fruit is poisonous, tho' they acknowledge that they have never tasted it. They say that many anamals feed on it, and among others, a large species of *Hare*, which abounds in that country. This fruit is the size of the largest orange, of a globular form, and a fine orange colour. the pulp is contained in a number of conical pustules, covered with a smooth membranous rind, having their smaller extremities attached to the matrix, from which they project in every direction, in such a manner, as to form a compact figure. the form and consistency of the *matrix* and *germ*, I have not been able to learn. the trees which are in the possession of Mr. Choteau have as yet produced neither flower nor fruit . . . (Note on the large Hare follows).

I have the honour to be with sincere esteem

Your Obt. Servt.

MERIWETHER LEWIS

Capt. 1st. U. S. Reg't. Infty.

A later reference to this plant occurs in Lewis' field notes on meteorology written while in camp on the Dubois River in 1804.

"Apr. 10th no appearance of the buds of the *Osage Apple*."

Since the Expedition broke camp for the journey up the Missouri on May 14, he might have seen the tree in flower, if it had reached sufficient maturity. The herbarium material brought back by Lewis could certainly not have been collected at this time. As seen in the herbarium of the Philadelphia Academy of Natural Sciences, the specimen seems to consist of a sterile branch from a young tree showing vigorous growth of wood and leaves of unusually large size. The mature condition of the leaves, the well-ripened wood and the fully developed winter buds suggest that the specimen was taken late in the season. No sign of flowers or fruit appears in the collection. The conclusion seems to follow that this specimen, undated, was probably collected at St. Louis on the return of the Expedition in late September or October, 1806.

In the Preface to his "Flora Americae Septentrionalis," Pursh [1] mentions the material seen by him in his study of the Lewis plants and presumably now preserved at least in part in the collection of the Academy above referred to. "Above the village of the Osage Indians a few trees have been planted, from which one has been introduced into one of the gardens at St. Louis on the Mississippi. Perfect seeds from the last-mentioned tree were given by Mr. Lewis to Mr. McMahon, nursery and seedsman, at Philadelphia, who raised several fine plants from them, and in whose possession they were when I left America." If Pursh be correct in these statements, but one conclusion can be drawn. The tree, too young to bear fruit when seen in 1804 by Clark when outward bound, matured sufficiently during the three vegetative seasons that elapsed before the return in late September of 1806 to produce the seed that Clark sent to Jefferson.

It will be our next task to trace these and accompanying

[1] Pursh, Frederick, "Flora Americae Septentrionalis," etc., second edition, Vol. 1: XII.

seeds after they reached him. Upon his arrival at St. Louis, Lewis promptly informed Jefferson that he had collected seeds among other material and that he expected to bring them with him to Washington shortly. This was sufficient notice for the naturalist President, who foresaw the importance of promptly placing these seeds in the hands of those who could plant them at the proper season. Hence he immediately began a correspondence with those having experience and facilities calculated to secure the best results. I believe this correspondence to be unpublished in the main and quote the most important letters as copied from the manuscript originals in the Division of Manuscripts of the Library of Congress at Washington. Jefferson selected two of his botanical acquaintances in Philadelphia to be the repositories of these seeds; Bernard McMahon,[1] a gardener and seedsman, and William Hamilton, the wealthy plant grower and collector of the "Woodlands."

Jefferson approached McMahon in a letter dated at Washington, January 6, 1807.[2] "Captain Lewis has brought a considerable number of seeds of plants peculiar to the countries he has visited. I have recommended him to confide principal shares of them to Mr. Hamilton of the Woodlands and yourself as persons most likely to take care of them, which he will accordingly do. He will carry them on to Philadelphia himself."

McMahon replied on February 25, 1807.[3] "I am extremely obliged to you for your kindness in speaking to Captain Lewis about the seeds; I anxiously wish for his arrival in this city, fearing to lose the advantage of early sowing of some articles that might require it."

The longed-for Lewis, accompanied by his colleague, Captain William Clark, arrived in Washington near the middle of February[4] to accept the appreciative action of

[1] For a short biographical account of McMahon, see Bailey's "Standard Cyclopedia of Horticulture," 1915, Vol. 3: 1586.

[2] Jefferson papers, Ser. II, V. 59, No. 67.

[3] Jefferson papers, Ser. II, V. 59, No. 57.

[4] Jefferson, "Statement on life of Lewis introductory to the Biddle edition of the journals of the Lewis and Clark Expedition." (Seen also in Ms. at Library of Congress.) Edition reprinted by Barnes & Co., New York, 1904, Vol. 1: X (iii).

Congress then in session. With them came the anxiously-awaited seeds, which about a month later were started toward Philadelphia. Those forwarded were the major part of a lot selected by Lewis for Jefferson's own planting.

Under date of March 20, 1807, Jefferson writes to McMahon:[1]

"I am in hopes I am more fortunate in the seeds I now send you than (in) the effete roots before sent. The enclosed seeds are given me by Captain Lewis for my own garden, but as I am not in a situation to do them justice and am more anxious they should be saved in any way than merely to see them in my own possession, I forward them to you who can give them their best chance. it will give you too an opportunity of committing them earlier to the ground than those you will recieve from Captain Lewis for yourself as it may yet be some time before he is with you. perhaps you may as well say nothing of your recieving this list it might lessen the portion he will be disposed to give you; and believing myself they will be best in your hands, I wish to increase the portion deposited with you."

Hardly was this letter dispatched than a change of plan whereby Captain Lewis decided not to visit Philadelphia for a considerable period, perhaps because of his appointment as Governor of the newly acquired Louisiana territory, led to the forwarding of the seeds in advance of his coming. Jefferson now sends McMahon's portion with a letter dated March 22, 1807.[2]

"Governor Lewis's journey to Philadelphia being delayed longer than was expected, and the season advancing, we have thought it best to forward to you by post the packet of seeds destined for you. they are the fruits of his journey across the continent and will I trust add some useful or agreeable varieties to what we now possess. I send a similar packet to Mr. Hamilton of the Woodlands. in making him and yourself the depositories of these public treasures, I am sure we take the best measures possible to insure them from being lost. I sent you a small packet a few days ago which he had destined for myself, but I am in too indifferent a situation to take the care of them which they merit."

[1] Jefferson papers, Ser. II, V. 59, No. 68.
[2] Jefferson papers, Ser. II, V. 59, No. 69.

On the same day, Jefferson dispatches another package of Lewis's seeds to William Hamilton of the Woodlands. Since this letter speaks somewhat more broadly of the results of the Expedition, I copy it in full from the original in the Library of Congress.[1]

"it is with great pleasure that, at the request of Governor Lewis, I send you the seeds now enclosed, being a part of the Botanical fruits of his journey across the continent. I cannot but hope that some of them will be found to add useful or agreeable varieties to what we now possess. these with the descriptions of plants, which, not being in seed at the time, he could not bring, will add considerably to our Botanical possessions. he will equally add to the Natural history of our country. on the whole, the result confirms me in my first opinion that he was the fittest person in the world for such an expedition. he will be with you shortly at Philadelphia, where I have no doubt you will be so kind as to shew him those civilities which you so readily bestowed on worth. I send a similar packet to Mr. McMahon to take the chance of a double treatment in confiding these public deposits to your and his hands, I am sure I make the best possible disposition of them."

McMahon promptly inspects the seeds and roots sent him and replies to Jefferson on March 27, 1807.[2]

"I duly received the roots and seeds you were so good as to send me, for which I return you and Governor Lewis my hearty thanks. I have no doubt but I will be able to give a good account of the produce, for I never saw seeds in a better state of preservation, and their having reached me in good time will be a considerable advantage. I have already sowed several kinds, will treat the whole with very particular care and have no doubt but I will be able to send you in due time, *plants of every kind* committed to my care. . . .

"The dwarf Cedar of the plains of Missouri, I take, from the seed, to be a species of Juniperus; the Shallan of the Clatsops, a Vaccinium; and the flowering pea of the plains of Arkansas, a Lupinus. I shall from time to time report to you or to Governor Lewis the progress of this precious collection, and of any other articles with which I may be favoured."

On April 2, 1807[3] he reports progress.

[1] Jefferson papers, Ser. II, V. 42, No. 26.
[2] Jefferson papers, Ser. II, V. 59, No. 58.
[3] Jefferson papers, Ser. II, V. 59, No. 59.

"I have fine crops already up of Aricara Tobacco, and perennial flax, and expect numbers of the others up in a few days."

On April 10, 1807 [1] he again addressed President Jefferson.

"I have several sorts growing of the seeds you were pleased to send me, among which are four varieties of Currants, and I am confident that I shall have plants from every kind received."

William Hamilton seems to have taken his share of the seeds less seriously than did McMahon. At least I have seen no letters from him touching on this subject prior to one of February 5, 1808,[2] in which, writing from the Woodlands, Philadelphia, to Jefferson he closes a long letter concerning things that he wishes the President to procure for him, with a paragraph on the Lewis material. He writes: "Mr. Lewis's seeds have not yet vegetated freely, more however may come up this coming spring. I have nevertheless obtained plants of the Yellow Wood, or *Osage Apple*, seven or eight sorts of *gooseberries* and *currants*. One of his kinds of Aricara tobacco, have flowered so well as to afford me an elegant drawing from it." He complains that "my gardener is an indifferent one." (At that time Lyon, R. H. T.)

McMahon continues to report but unfortunately for our purpose does so in rather general terms. On June 28, 1808, he writes:

"I am happy to inform you that I have fine plants of all varieties of Currants (7) and Gooseberries (2) brought by Governor Lewis, and of about 20 other *new species* of plants, as well as five or six new *genera;* this will add to natural history and the plants are forthcoming. . . . I would be very happy to know when Governor Lewis may be expected here."

Thus early the richness of the collections made by Lewis began to appear, at least to the obviously enthusiastic McMahon.

In his next letter to McMahon, written from Washington on July 6, 1808,[3] Jefferson mentions specifically several kinds

[1] Jefferson papers, Ser. II, V. 59, No. 60.
[2] Jefferson papers, Ser. II, V. 42, No. 24.
[3] Jefferson papers, Ser. II, V. 59, No. 70.

of seeds reserved for his own growing at Monticello. It will be noted that they were those that offered good prospects of being either useful or ornamental. He begins to anticipate the pleasure of gardening that he hopes will be his after his retirement to private life early in the coming spring.

"I shall be at home early in March for my permanent residence, and shall very much devote myself to my garden. I reserved very few of Governor Lewis's articles, and have growing only his salsafia, Mandane corn and a pea remarkable for its beautiful blossom and leaf. his forward bean is growing in my neighborhood."

Jefferson and McMahon continue their correspondence on gardening and botany by an occasional interchange of letters, but no mention of the Lewis plants appears until McMahon, on January 17, 1809 [1] finds himself in a quandary. He turns to Jefferson for help. The always-longed-for but never-present Lewis is again in demand.

"I am very anxious to learn when Governor Lewis may be expected here, as I have detained a man in my house upwards of twelve months drawing and describing his plants, which he left with me for that purpose: this was accomplished in May last, as far as it could be done in the absence of Governor Lewis and he told me on his leaving this city he expected to be here again in that month. This man, who is completely adequate to the task, is becoming very uneasy and I wish him not to leave the neighborhood till the arrival of Mr. Lewis, by whose particular instructions only, he can finish the drawings of some very important but imperfect specimens."

This letter introduces Frederick Pursh, later the author of a two-volume work on the flora of North America published in England in 1814.[2]

The troubles of McMahon and Jefferson begin to multiply. The death of Captain Lewis on October 18, 1809, when so much seems to have been awaiting his appearance and decision, complicated the affairs of McMahon in more ways than one.

[1] Jefferson papers, Ser. II, V. 59, No. 63.

[2] Pursh, Frederick, "Flora Americae Septentrionalis; or a systematic arrangement and description of the plants of North America," etc. In two volumes, with twenty-four engravings. London, 1814. Second edition, London, 1816.

Writing to Jefferson from Philadelphia on December 24, 1809,[1] he explains very clearly the quandary in which that event had left him.

"I am extremely sorry for the death of that worthy and valuable man, Governor Lewis, and the more so for the manner of it. I have, I believe, all his collection of dried specimens of plants procured during his journey to the pacific ocean, and several kinds of *new* living plants, which I raised from the seeds of his collecting which you and himself were pleased to give me. In consequence of a hint, to that effect, given me by Governor Lewis on his leaving this city, I never yet parted with one of the plants raised from his seeds, nor with a single seed the produce of either of them, for fear they should make their way into the hands of any Botanist, either in America or Europe who might rob Mr. Lewis of the right he had to first describe and name his own discoveries in his intended publication, and indeed I had strong reasons to believe this opportunity was coveted by (blank) which made me still more careful of the plants.

"On Governor Lewis's departure from here, for the seat of his government, he requested me to employ Mr. Frederick Pursh, on his return from a collecting excursion he was then about to undertake for Dr. Barton, to describe and make drawings of such of his collection as would appear to be new plants and that himself would return to Philadelphia in the month of May following. About the first of the ensuing November Mr. Pursh returned, took up his abode with me, began the work, progressed as far as he could without further explanation, in some cases, from Mr. Lewis and was detained by me, on expectation of Mr. Lewis's arrival at my expense, without the least expectation of any future remuneration, from that time till April last; when not having received any reply to several letters I had written from time to time, to Governor Lewis on the subject, nor being able to obtain any information when he might be expected here; I thought it a folly to keep Pursh longer idle and recommended him as Gardener to Doctor Hosack of New York, with whom he has since lived.

"The original specimens are all in my hands, but Mr. Pursh, had taken his drawings and descriptions with him, and will, no doubt, on the delivery of them expect a reasonable compensation for his trouble.

"As it appears to me probable that you will interest yourself in having the discoveries of Mr. Lewis published, I think it a duty encumbent on me to give you (illegible) preceding information and

[1] Jefferson papers, Ser. II, V. 62, No. 111.

to ask your advice as to the propriety of still keeping the living plants I have, from getting into other hands who would gladly describe and publish them without doing due honor to the memory and merit of the worthy discoverer."

This letter makes one thing clear, that the plants grown from Lewis's seeds were kept strictly "incommunicado" by him as late as the end of the year 1810. We have no letters from William Hamilton of the Woodlands beyond the first notice of his success in germinating the plants. It seems almost impossible to suppose that McMahon kept the plants in his garden from the eye of his botanical boarder and coadjutor, Pursh, during the months devoted by the latter to Lewis's herbarium material, but this conclusion seems to follow from the fact that in his Flora Pursh indicates that he saw only as dry herbarium material most of the very species that McMahon was then growing from seeds and cuttings. He saw several of these later in London, where they were being cultivated from seeds and roots collected by Nuttall and others.

The fate of the seeds retained by Jefferson for growth at Monticello is indicated in a letter written on October 6, 1810,[1] from Monticello to Dr. Benjamin S. Barton in Philadelphia. He sent to Professor Barton "some Ricara snap beans and Columbian salsafia brought from the western side of the continent by Captain Lewis." He says he now has better beans and will give these up. He is still undecided about the "salsafia," that he has not tried adequately yet.

This reveals Jefferson on his practical side. The beans and "salsafia" possessed interest for him primarily, as they were more or less satisfactory garden vegetables. Their far-away source or their scientific interest did not justify him in retaining them. This is puzzling in view of his great attachment to certain fossil bones and other objects that possessed no other than a scientific interest.

After the death of Lewis, the responsibility for the results of the botanical work seems to have gone over to the surviving

[1] Jefferson papers, Ser. II, V. 10, No. 81.

leader, Clark, who was also the executor of Lewis's will. We next hear of him in a letter from Jefferson to McMahon written on January 13, 1810[1] from Monticello.

"Before you receive this, you will probably have seen General Clarke, the companion of Governor Lewis in his journey and now the executor of his will. the papers relating to the expedition had safely arrived at Washington, had been delivered to General Clark, & were to be carried on by him to Philadelphia, and measures to be taken for immediate publication, the prospect of this being now more at hand, I think it justice due to the merits of Governor Lewis to keep up the publication of his plants till his work is out, that he may reap the well deserved fame of their first discovery. with respect to Mr. Pursh I have no doubt General Clark will do by him whatever is honorable & whatever may be useful to the work."

Thus the secrecy that McMahon had so long maintained as a protection to the botanical rights of the dead leader was to be maintained till that day when the scientific results of the Expedition were to be made known to the world.

Jefferson little guessed that these results were to wait an even century before seeing the light, and that then the historian who so ably marshalled the publication of the results was to overlook the plants grown by McMahon and by him so zealously protected against premature introduction. Jefferson was to be impressed later with the difficulties of the situation, as he coped with death and bankruptcy in his endeavor to bring the results of the Expedition to the light.

In the meantime the existence of living material brought back by Captain Lewis had become a matter of knowledge among botanists and a keen curiosity was expressed by Pastor Henry Muhlenberg at Lancaster. He writes to Dr. William Baldwin at Wilmington, Delaware on May 22, 1811.[2]

"Pray have you specimens of any of Lewis's plants? I have tried every method to get a sight of them, but in vain. My friends in Philadelphia have denied me the pleasure of seeing them in flower. I wish I could add them to my catalogue, without any description, leaving that to the compilers of Lewis's work. I am afraid the descriptions will be made in England, and Lewis's work

[1] Jefferson papers, Ser. II, V. 62, No. 121.
[2] Darlington, William, "Reliquiae Baldwinianae," 1843: 34.

will come too late. Perhaps you can get the specimens from Mr. McMahon or Dr. Barton." Baldwin in his reply states that he too has "no specimens of Lewis's plants." (Darlington, p. 35.)

This anxiety expressed on all sides that justice might be done the merits of Lewis casts an interesting flash of light on the jealousies prevailing among the botanists of Philadelphia at that time. Pastor Muhlenberg's prophesy was fulfilled, since descriptions of Lewis's plants were published in London by Pursh in 1814. If he had not done so, the effects of a century of neglect would have robbed the material seen by Pursh of the greater part of its value and would have denied to the memory of Lewis most of the credit now awarded him by botanists.

Two years elapse before we hear anything more from either Jefferson or McMahon about the Lewis plants. In the meantime Jefferson gardens ardently at home and McMahon's seed business grows. During this interval, too, McMahon seems to have felt the strictness of his guardianship in some way relaxed, since on February 28, 1812,[1] he sends to Jefferson plants grown from Lewis's introductions.

"No. 1. Ribes odoratissimum (mihi). This is one of Captain Lewis's and an important shrub, the fruit very large, of a dark purple colour, the flowers yellow, showey and *extremely fragrant.*

No. 2. Symphoricarpos leucocarpa (mihi). This is a beautiful shrub brought by C. Lewis from the River Columbia, the flower is small but neat, the berries hang in large clusters and of a snow white colour and continue on the shrubs, retaining their beauty, all the winter, especially if kept in a greenhouse. The shrub is perfectly hardy: I have given it the trivial english name of Snowberry-bush.

No. 3. The Yellow Currant of the River Jefferson; this is specifically different from the other, but I have not given it a specific botanical name."

Jefferson takes the season in which to try out the new plants before writing to McMahon. This he did on October 11, 1812,[1] writing from Monticello.

[1] Jefferson papers, Ser. II, V. 62, No. 113.
[2] Jefferson papers, Ser. II, V. 62, No. 125.

"The articles received in the spring . . . have been remarkably successful. one only of the cuttings of the snowberry failed. the rest are now very flourishing and shew some of the most beautiful berries I have ever seen. the sweet scented currant, the yellow currant, the red gooseberries and Hudson strawberries are all flourishing."

Jefferson and McMahon continue to correspond on many garden matters, such as crown imperials, dahlias and gladioli and on matters botanical, such as the hickory from Virginia growing the huge nut, and other things. McMahon is happy in September 23, 1812 to be able to report that his nursery and seed business is making better progress than he had hoped. (Jefferson papers, Ser. II, V. 62, No. 115.)

During his stay in France, Jefferson had made a friend of M. Thouin, the head of the Royal Gardens of Paris, and was the recipient for some years of annual packages of seeds collected by M. Thouin. These seeds usually found their way to McMahon. In time Jefferson feels himself able to repay the compliment by sending Clark's novelties to Paris. He broaches the subject to McMahon in a letter from Monticello dated May 30, 1813.[1]

"If you could make up a collection of the seeds of the plants brought to us by Governor Lewis from beyond the Mississippi, it would be a just and grateful return which M. Thouin merits at our hands. He expresses to me a great desire for the plants of the region beyond the Mississippi."

He follows with characteristically complete directions for sending them via Monticello to M. Thouin.

The correspondence between Jefferson and McMahon closed, as far as I have been able to learn, when Jefferson on July 23, 1815 writes to McMahon sending the usual shipment of seeds from M. Thouin. This part of the record was closed by the death of McMahon.

Jefferson clearly gave up the idea of withholding knowledge of the Lewis plants until the publication of the records of the Expedition could take place, at some date prior to the request

[1] Jefferson papers, Ser. II, V. 62, No. 128.

that McMahon send a collection of seeds to Paris in 1813. He himself feels free to send the snowberry to his old friend, Mme. de Tessé, an aunt of General Lafayette.

On December 8, 1813,[1] he writes her from Monticello:

"Lewis's journey across our continent to the Pacific has added a number of new plants to our former stock. Some of them are curious, some ornamental, some useful, and some may by culture be made acceptable to our tables. I have growing, which I destine for you, a very handsome little shrub of the size of a currant bush. Its beauty consists in a great produce of berries of the size of currants, and literally as white as snow, which remain on the bush through the winter, after its leaves have fallen, and make it an object as singular as it is beautiful. We call it the snow-berry bush, no botanical name being yet given to it, but I do not know why we might not call it Chionicoccus, or Kallicococcus. All Lewis's plants are growing in the garden of Mr. McMahon, a gardener of Philadelphia, to whom I consigned them and from whom I shall have great pleasure when peace is restored, in ordering for you any of these or of our other indigenous plants."

The beauty of the snowberry (*Symphoricarpos albus* Blake var. *laevigatus* Blake) seems to have greatly impressed Jefferson, who later yielded to generous impulse and in one of the curiously stiff notes that people in those days sometimes wrote even to their friends, sends a bush to his old neighbor, General John Cocke on March 27, 1817.[2]

"Th. Jefferson . . . adds . . . some cuttings of the snowberry bush, brought from the Pacific, by Captain Lewis. Its beauty is in the snow white bunches of berries which it retains after the leaf has fallen; it is of the size of a goose-berry bush. He does not know certainly that it will grow from a cutting, but believes it will, and is sorry he has not a bush to spare with roots. . . ."

It is a matter of common knowledge that several of Lewis's plants eventually found their way into general cultivation and have long since become widely distributed. Of the species recorded by Pursh and others as present in the

[1] Thwaites, R. G., "Original Journals of the Lewis and Clark Expedition, 1804–1806," Vol. 7, 393.

[2] Cabell Manuscripts in Virginia State Library, Richmond, Va.

herbarium material brought back by Lewis and represented in his seed collection the following are to-day listed among the common ornamental or useful plants:

(1) The Osage orange (*Toxylon pomiferum* Raf.) widely used some decades ago as a hedge plant, and still often planted because of the beauty and vigor of its growth as well as for its curious fruit.
(2) The Snowberry bush (*Symphoricarpos albus* Blake var. *laevigatus* Blake) so frequently mentioned above.
(3) The Golden, yellow-flowering or Buffalo currant, also called the Missouri currant in many writings (*Ribes odoratum* Mendl.—*R. longiflorum* Nutt.)
(4) The winter currant (*Ribes sanguinium* Pursh).

Several other plants represented in the herbarium collection but not known to have been represented in the list of seeds are now of general value.

(1) Oregon Grape (*Mahonia Aquifolium* Nutt. *Berberis Aquifolium* Pursh) is largely cultivated for the stiff evergreen foliage and for the inflorescence.
(2) *Clarkia pulchella* Pursh, a hardy annual plant. Named for Captain Clark, now cultivated in dwarf and some double horticultural forms.
(3) The bitter root (*Lewisia rediviva* Pursh) named for Captain Lewis, with edible root and fleshy leaves grown among succulent ornamentals.

The list might be lengthened, since the complete list of plants grown from seed was perhaps never made a matter of record. Probably, however, McMahon was not very wrong in estimating that he had under cultivation as many as twenty-five species. I think it quite likely that the list was much larger than that of which we have specific mention.

The question is immediately presented as to how these plants came to be so widely disseminated after the effort to hold them for Captain Lewis's direction was terminated following his death.

We know that McMahon was in business as a seedsman and nurseryman before the time of the Lewis and Clark Expedition and that his business was flourishing at the time of his death in 1816. His wife continued his work after his demise, issuing from time to time the well-known seed catalogues. I have examined the catalogue of 1815 and find little mention of the plants in question. He lists [1] for sale (1) The early Arikara Kidney bean, "a form of *Phaseolus vulgaris* cultivated by these Indians (p. 6); the perennial flax, that may have been the old world form known as *Linum perenne* L. or its American form named *Linum Lewisii* Pursh. The western flax is now listed as an ornamental under the name of "prairie flax." In the same catalogue the dwarf maize known as Mandan corn was also offered for sale as a seed used in Rural Economy (p. 30). Mention is made of Symphoricarpos vulgaris, a name now applied to the western Coral berry, a near neighbor of the snowberry of Lewis's list. It is probable that the plant here concerned is that now known by the latter name and was the plant so admired by Jefferson. One looks in vain for the Osage orange and other plants that McMahon undoubtedly grew.

An inspection of the plants offered for sale by Robert Carr, from Bartram's Garden in 1828 reveals two of Lewis's plants: The Osage orange (*Maclura aurantiaca*) valued for its "beautiful foliage and curious fruit" (p. 23); and Lewis's Missouri ornamental Currant" (p. 44).[2]

That these Lewis plants continued to fill their places in American horticulture may be seen from works of later date.

Kendrick in his Orchard list, published in Boston in 1833, furnishes evidence that these plants were attracting attention at more distant places. He lists "Jefferson's Missouri fragrant currant" (p. 293), and "Snowberry, a very hardy shrub from the Rocky Mountains" (p. 396).

[1] McMahon, Bernard, A catalogue of Garden, Herb, Flower, Tree, Shrub, and Grass seeds; Gardening, Agricultural and Botanical Books, Garden Tools, etc. Sold By Bernard McMahon, No. 30 South Second Street, Philadelphia. Who has likewise for sale, Plants of above fifty varieties of *Most Superior* English, Irish and Scotch Gooseberries. 1815 (dated in pen), Philadelphia, Printed by William Duane.

[2] Carr, Robert, Periodical Catalogue of Fruit and ornamental Trees and shrubs, Green House Plants etc. Cultivated and for sale at Bartram's Garden Kingsessing, Near Gray's Ferry—Four miles from Philadelphia. Robert Carr, Proprietor. Philadelphia 1828.

It is beyond my purpose to attempt to follow the more recent history of these plants. It has been my object to assemble data concerning an important part of the botanical results of the Lewis and Clark Expedition and to add another chapter to the gradually accumulating record of that great undertaking.

Botanical Laboratory,
University of Pennsylvania.

FOSSIL AND RECENT CORALS AND CORAL REEFS OF WESTERN MEXICO. THREE NEW SPECIES

By R. H. PALMER

It is a matter of more or less common knowledge that along the western coast of the Americas there is an entire absence of extensive coral reefs and a paucity of coral fauna, both of which are so characteristic of the tropical and subtropical waters of the West Indies and adjacent islands.

This has been explained by the relatively warmer water of the east coast as compared with that of the west coast, the difference of temperature being attributed to the long exposure of the equatorial current to the tropical sun as it passes westward across the Atlantic. Although the assumed relative temperatures of the waters of the two coasts may be true as a generalization it is at variance with observations made along the Oaxaca coast of southern Mexico, where the water has a winter temperature of 28° C. and a summer temperature of 29° C. In Tortugas Bay on the Florida coast, where corals abound, Vaughan reports [1] 24.4° C. as the average January temperature over a period of eleven years and a July temperature of 28.7° C. during the same length of time. The same author states that vigorous reefs will endure a temperature of 18.15° C. or nearly 10° lower than the waters along the Oaxaca coast. The other factors involved in coral control are left for later discussion.

While reef-forming genera are present on the west coast they never assume the importance of their West Indian relatives. That this is a constant feature is indicated by a few glimpses into the geologic past that are afforded by the rock record.

The first known occurrence of corals in any abundance is in the Cenomanian rocks that are exposed in the States of

[1] Vaughan, T. W., Carnegie Inst. Wash. Pub. 213, pp. 319–329, 1918.

Puebla, Jalisco and Colima. Though the latter two localities are on the present Pacific slope, the fauna is entirely Atlantic in its relationship. In Puebla, these Cretaceous corals belong for the most part to reef-forming genera. So abundant are the remains that during life they must have built reefs of large extent.

Throughout the remaining Cretaceous no coral reefs and but few corals are known. The total absence of Pacific Cretaceous fauna in southern Mexico precludes any statement as to the Cretaceous corals of the Pacific Ocean.

Atlantic Eocene is known in southern Mexico but no corals have been reported from it. It is probable, however, that they occur, as Eocene coral reefs of some importance are known in the West Indies. On the Pacific side, the Eocene is not known.

During the Oligocene reef-building corals reached a degree of abundance along the shores of eastern Mexico never before nor since equalled. In the Tampico Embayment there are, in the San Rafael formation, extensive beds composed almost exclusively of corals. These beds are so characteristic and of such wide extent that they are valuable for correlation purposes. On the Pacific shore the Oligocene is also absent.

On the Isthmus of Tehuantepec there are rather extensive exposures of Upper Miocene and Pliocene with Caribbean fauna. In these a few corals of the solitary type occur but no reef-builders are known. No Miocene or Pliocene is known on the west coast of southern Mexico.

Curiously enough, near the head of the Gulf of Lower California there is a Miocene or Pliocene reef. The corals of this reef all belong to living genera of the Caribbean Sea.

Pleistocene corals occur in lately uplifted beds in the vicinity of Tampico. No reefs, however, have been reported.

Along the Oaxaca coast the Pleistocene occurs as a low cliff of loosely consolidated beach material. It extends along the coast in patches for some twenty-eight miles. In some places it is on the beach and in others it is inland about a mile and a half. Its elevation varies from low tide level to fifty feet or more. No reefs have been found in this exposure although there are abundant coral remains which include such reef-forming genera as Porites and Pocillopora.

Recent corals occur both along the Atlantic and Pacific coasts of Mexico. With the exception of a few genera that are nearly world wide in distribution, the coral faunas of the two coasts are quite distinct, indicating, I might add in passing, a somewhat lengthy separation. Curiously enough, corals are much more common along the western coast than along the *shores* of the Gulf of Mexico. It is possible that the explanation of this phenomenon is the same as that of the general paucity of corals along the west coast as compared with the *islands* of the Gulf of Mexico and Florida waters in general. It appears that this explanation is based on ecological factors that are under coast control. Corals are sessile animals and hence require stability to the base to which they are attached. This condition is best found along rocky shores where the beach sand is limited in amount or does not exist.

Sandy beaches and sandy bottoms are particularly unfavorable to either crawling or burrowing or sessile forms. Long stretches of beach sand with only an occasional beach worn shell bear ample witness of this fact. Here the soft shifting sands afford not only no foothold but subject any organism that does not swim to periodic burial which if sufficiently prolonged, is fatal and, what is equally destructive to a coral that cannot close itself up, to actual choking by the sand grains entering into the vital parts.

Practically the entire eastern coast of Mexico is a low lying plain that is but a few feet above sea-level. Long sandy beaches and bars extend from Vera Cruz almost continuously to the Rio Grande. Rocky coasts are absent. It is true that there are several areas of elevation but only the soft Tertiary rocks are exposed and they are nearly as hostile to shore life as the sandy beaches.

On the west coast, however, a different geological history has been followed by different shore conditions, hence different ecological conditions obtain.

Elevated areas alternate with depressed areas. The former are long, low, flat stretches faced seaward by an equally long, monotonous, sandy beach in which a scanty molluscan life is inferred from the stray shell fragments.

Depression of the alternating areas has allowed the sea to encroach upon the land to the base of the low mountain ranges. Here rocky coasts and headlands with their minor sculpturing and drowned valleys take the place of sandy beaches and here shore life abounds. It is in these areas of depression that the corals are found.

Their local occurrence is worthy of mention. In fracture planes the waves have excavated small caverns from a few feet to several yards deep. Many square yards of these caves are lined with incrusting corals such as *Astrangia* and *Porites*.

None of these colonies except the reef-formers are ever subject to direct sunlight for more than a short time during the day. They are some feet above low tide and out of reach of the local shifts of the sand. An occasional patch of dead corals, however, bears evidence that their calculation failed to consider all the vagaries of this destructive agent.

In small baylets or in places protected from drifting sand occur small patches of a reef-forming coral, *Pocillopora*. In two localities some fifty miles apart, *P. capitata* var. *robusta* Ver., forms extensive colonies. One occurs near Puerto Angel where it forms a patch about twenty yards in diameter (Pl. II, Fig. 1). Individual colonies form hemispherical heads four feet or more in diameter. The colony is exposed at low tide only and as the fleshy parts are a dull earth brown it is never conspicuous. The branches in different parts of the head show distinct variations. Those on the part of the head not exposed to the force of the waves are round and have numerous branchlets, while those on the exposed side are flat or spatulate and but little branched.

Near Escondido Bay, in a small inlet called Puerto Angelito, this species is so abundant that the aggregation may properly be called a reef. This inlet is some 500 feet wide and is so completely filled by coral that even small boats cannot enter. Coral fragments and coral sand make up a large percent of the beach material.

The distribution of corals on the west and on the east coasts of Mexico and the flourishing condition of the two colonies mentioned indicate that temperature is not the exclusively controlling factor and that the presence or absence of rocky coasts is an equally important factor of control.

But a word on the relationship of the Oaxaca corals. The genera are those common along the west coast of tropical America. The reef-forming coral, *Pocillopora capitata*, is closely related to Samoan and also to Hawaiian species. So close is this relationship that the species are separated on very small and unimportant details.

It is possible that the northward distribution of the tropical species may be accounted for by near shore currents that transported the larvae. Evidence of these currents is afforded by the fact that some weeks after the terrific volcanic explosion of 1902 in Guatemala large amounts of pumice are reported to have been washed ashore along the Oaxaca coast. That this shore current, flowing northward and then westward along the southern Mexican coast, is constant, is indicated by the fact that pumice fragments are still being deposited along the strandline by the waves.

DESCRIPTION OF SPECIES

Pocillopora capitata Verrill var. *robusta* Verrill

Plate II, Fig. 1

Pocillopora capitata var. *robusta* Verrill, Conn. Acad. Sci. Trans., Vol. 1, p. 521, 1866–71.

Two notable occurrences of this reef building coral have already been described.

Porites panamaensis Verrill

This species is a thin encrusting form and is not uncommon on rocky cliffs where it is protected from the rays of the sun. The fleshy parts are carmine in color.

It also occurs in the Pleistocene.

PLATE I

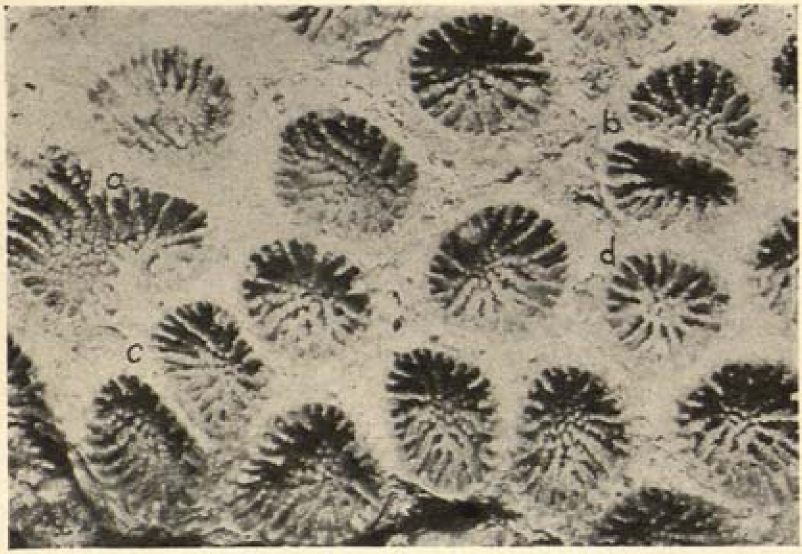


FIG. 1.—*Astrangia browni* Palmer n. sp. Holotype × 6. Palmer Collection. Recent. *a*, Fission, early stage. *b*, Fission, later stage. *c*, Fission, completed. *d*, Union of short septa with alternate long septa.

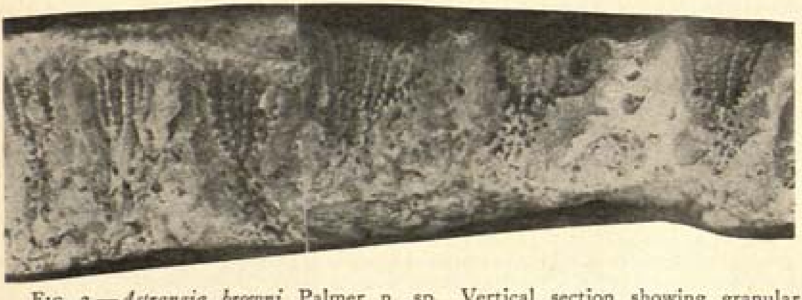


FIG. 2.—*Astrangia browni* Palmer n. sp. Vertical section showing granular dentate septa and perforations of septa which produce the porous columella. × 6.

Oculina species

In the Pleistocene deposits this genus is rather common. However, the preservation is too poor to warrant specific determination.

Astrangia browni n. sp.

Plate I, Figs. 1 and 2

Compound. Incrusting with but one layer of corallites, .12″ to .16″ thick. Basal disc very thin. Diameter of corallites .10″ to .12″; calice .10″ deep; walls of adjacent corallites touching producing polygonal corallites or separated as much as .04″. The walls of the corallites extend but little above the coenenchyma.

Septa: 28–30. They project slightly above the walls and often can be traced on the exterior and across the intermural area as low ridges or rows of papillae called *costae*.

The septa are in two series. The alternate ones are long and are fused into a porous columella. At the top of the corallite the septa are very narrow (.02″). The inner edge is vertical to bottom of calice where it widens and extends to the center. The inner edge is dentate with from six to eight teeth that show a tendency to turn upward (Pl. I, Fig. 2).

Both sides of septa are granulate. The granulations at or near the free ends of the shorter septa sometimes join those of the alternate adjacent longer septa forming *synaptaculae* (Pl. I, Fig. 1, *d*).

Near the base where the longer septa extend to the center their inner ⅓ is so extensively perforated that this part is reduced to *trabeculae* (Pl. I, Fig. 2). The fusion of these trabeculae or extensions of the septa produce a very porous or spurious columella.

The corralites reproduce by fission (Pl. I, Fig. 1, *a*, *b* and *c*).

The intermural space or *coenenchyma* is usually extensively perforated by burrowing organisms.

The fleshy parts are a deep sea green.

Locality: Four miles west of Puerto Angel, Oaxaca, Mexico.

The species is named in honor of Mr. T. D. Brown, through whose many courtesies collections along the Oaxaca coast were made possible.

Remarks: This coral locally occurs in abundance incrusting many square yards of rock in and around the mouths of small wave-cut caves.

None have been seen except considerably above low tide level nor are they known except where exposed to the force of the waves. The shifting of the sands along the beach precludes the use of any part of the strand line that is subject to even occasional burial. It is entirely possible that this species may occur in deeper water where the sand is not transported.

FIG. 1.—Reef of *Pocillopora capitata* Ver. var. *robusta* Ver. (foreground) near Puerto Angel, Oaxaca.

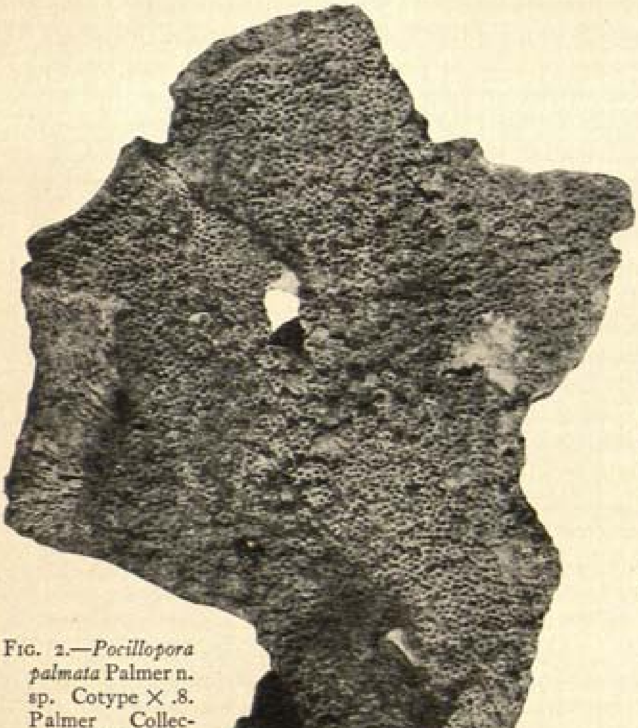


FIG. 2.—*Pocillopora palmata* Palmer n. sp. Cotype × .8. Palmer Collection. Pleistocene. Note flat growth habit and abundance of small verrucae.

FIG. 3.—*Pocillopora palmata* Palmer n. sp. Cotype × .8. Palmer Collection. Recent.

The new species is similar to *Astrangia conferta* Verrill (Conn. Acad. Sci. Trans. Vol. 1, p. 530). That species, however, is .20″ in diameter and has no more than twenty-four septa and reproduces by budding.

Astrangia oaxacensis n. sp.

Plate III, Fig. 2

Compound, encrusting. Corallites cylindrical .12″ in diameter and .16″ to .25″ high; separated by irregular intervals which average about their diameter. Calice .10″ to .12″ deep.

Outer walls very thin and often faintly costate. Basal disc often shows tendency to expand. Coenenchyma thin and irregular.

Reproduction by budding and eggs.

Septa: 33 to 37, extensively granulated; usually free but often adjacent ones are joined by union of opposite granulations, usually about flush with the wall or slightly projecting. The inner edge is deeply serrate. They are perforated throughout except near the periphery or attached edges in contrast to *A. browni* in which the perforations are confined to the distal ends of the septa (Pl. I, Fig. 2). In other respects the septa of the two species are identical. The extensive perforations of the septa reduce them to trabeculae towards the center or in the region of the columella.

The surfaces of the septa are well covered with oval granulations or synaptaculae. Most of these are in irregular rows and are so closely spaced that they form many irregular ridges which have a general upward and outward direction.

The union of the trabeculae and of the synaptaculae of adjacent septa produce the very spongy spurious columella.

The perforations are arranged in an irregular rhombic plan so that the general result is that the columella has the appearance of a number of rods that rise from the base diagonally towards the center. These rods that form the columella are knobbed.

Some of the adult corallites and many of the immature ones have a collar-like band which covers the upper edge of the septa (Pl. III, Fig. 2, *a*). In some specimens thickenings of similar parts of the septa lower in the calice form a ring. This is sporadic in appearance and may represent a pathologic condition.

Locality: Five miles west of Puerto Angel, Oaxaca, Mexico.

Remarks: This species occurs in crevices of rocks that are exposed to the full force of the waves. It has not been observed below medium high tide.

This species is similar to *A. pederseni* Verrill (Conn. Acad. Sci. Trans., Vol. 1, p. 529) but differs from that form in having a deeper calice and a shorter corallite.

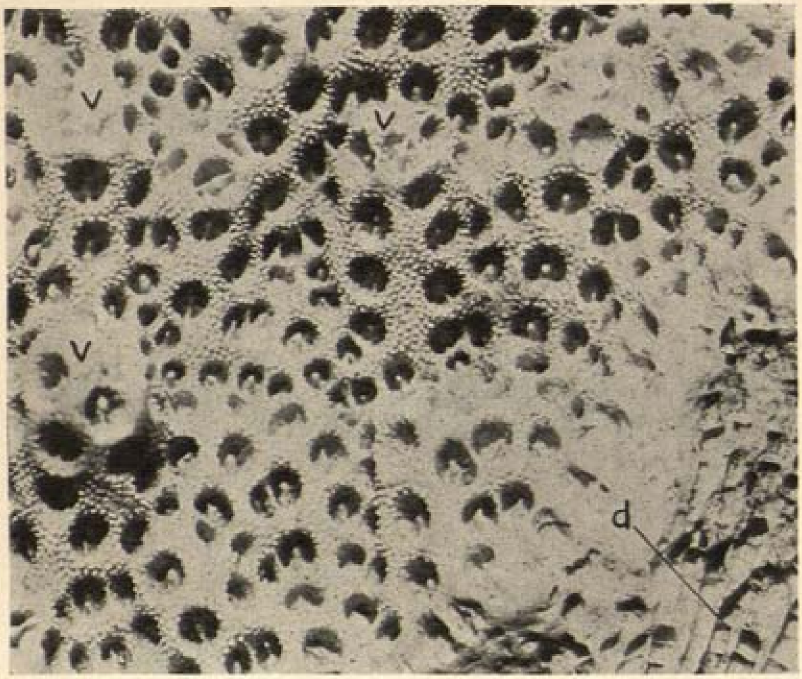


FIG. 1.—*Pocillopora palmata* Palmer n. sp. Detail of Cotype, Pl. II, Fig. 2. × 6. *d*, Dissepiments. *v*, Verrucae. Note papillate surface and development of the two vertical septa.

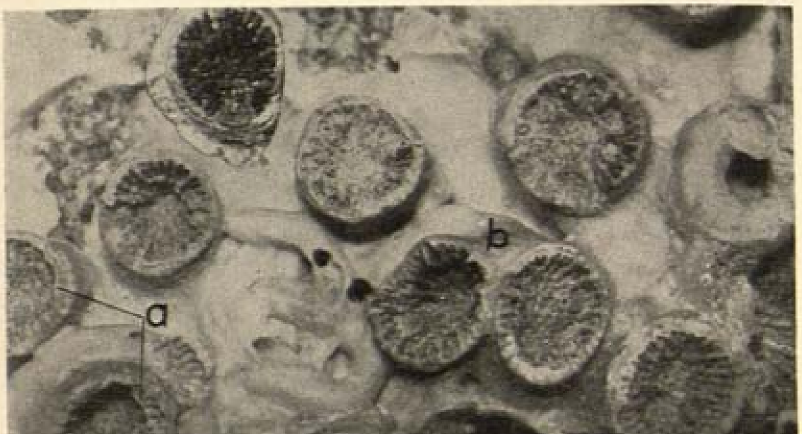


FIG. 2.—*Astrangia oaxacensis* Palmer n. sp. Holotype × 5⅓. Palmer Collection. Recent. *a*, Inner collar. This is usually present in younger individuals but is less common in adult ones. *b*, Two individuals produced by fission.

Pocillopora palmata n. sp.

Plate II, Figs. 2 and 3

Plate III, Fig. 1

This coral differs from all other described species by its decided habit of developing in one plane thus producing a flat palmate colony and not the hemispherical heads of rounded branches commonly seen in other Pocillopora. The larger specimens figured is 6″ by 5″ and ½″ to ¾″ thick.

The *verrucae* are low and ascending and form low papillae in place of the small branchlets of other Pocillopora. They contain three to seven corallites.

There are six septa. In practically all the corallites of the Recent specimen and in a majority of the Pleistocene specimens the two opposite vertical septa are abnormally developed. These join and form a conspicuous partition. (Plate III, Fig. 1).

Locality: Living in Puerto Angel harbor and Pleistocene near Escondido Bay, Oaxaca, Mexico.

Remarks: No living colonies are known. However, it probably lives in shallow water since fragments found along the strand-line are not beach worn.

This species is rare in the Recent but abundant in the Pleistocene.

SUBSTANCES REGULATING THE PASSAGE OF MATERIAL INTO AND OUT OF PLANT CELLS: THE LIPOIDS

By D. T. MACDOUGAL

Carnegie Institution of Washington

(*Read December 2, 1927*)

THE earliest contributions as to importance of these oil and ether-soluble substances in the activities of plant cells may be taken to have been those of E. Overton in 1890–1895 in which he framed the so-called mosaic theory of the boundary layer of protoplasm, in wh'ch oil-soluble phosphatides (phospholipins) were taken to alternate with proteins in an arrangement allowing the passage of both water and oil-soluble substances. The structure proposed is one now known to be impossible in the behavior of colloids, and during the following twenty years interest swung away to be concentrated on the proteins, in attempts at solution of the problems of permeability.[1] The present occasion does not warrant a balanced or extended historical résumé. My purpose is to outline recent advances, and to note the phases of activity of the lipoids which may and must be taken into account in any consideration of the entrance of nutritive materials into plants and in the transference of metabolic products between cells.

Notable contributions on the subject were made in the first decade of the century. The findings of Pascucci as to the presence of lecithin, cholesterol and cerebrosides with proteins in the blood, the conclusions of Palladin and his associates as to the part played by lipins in respiration of plants are to be noted.[2] It was also within this period (the first decade of the 20th century) that physiologists made observations as to the

[1] Overton, C., "Osmotische Eigenschaften Vierteljahrschrift. d. naturf. Gesell. Zurich," 40, 1, 1895. Also "Allgemeine Osmotische Eigenschaften d. Zelle," 44, 110, 1899.

[2] Palladin, W., und E. Stanewitsch, "Die Abhängigkeit der Pflanzenaturung von den Lipoiden," *Biochem. Ztschrift*, 26, 351, 1910.

wide prevalence of the phospholipins in plants, so that the term "Lipoplasm" was applied to the living matter of growing points and generative layers.

In 1903, a physician, Dr. Sajous,[1] on the basis of much evidence, advanced the view that a zymogen termed by him "adrenoxin" was secreted by the medulla of the adrenals, which zymogen, on reaching the lungs, took up the oxygen of the air and then became, in the red corpuscles "adrenoxidase." As a component of the corpuscular hemoglobin adrenoxidase became the catalytic oxidizing enzyme of all tissue cells. On reaching the latter, this oxidase by reacting with the phosphorus of the cellular nucleins liberated the heat energy (now erroneously, he holds, attributed to tissue oxidation) to which all tissue enzymes owe their activity in the vital process. In 1923, Sajous [2] pointed out that the cellular nucleins referred to above, owed their thermogenic activity to phospholipins, lecithin mainly, with cholesterol as inhibiting agent. Both these bodies, for which the adrenal cortex acts as storehouse, are also, according to Sajous, transported by the red corpuscles, which acquire them while traversing the adrenals, to the tissue cells to sustain and regulate therein heat production.

Now, while the earlier hypothesis of E. Overton as to permeability depending on a mosaic surface of lipoid and protein, had been found inadequate, yet it has long since been evident that no explanation of the passage of materials into and out of cells was possible without the intervention of substances of these group in the boundary layers.

Briefly stated, cells are always permeable to oil-soluble substances, and not so readily and only under certain conditions to sugars, amino-acids and salts.

Nothing is to be gained here by a chemical discussion of the structure of the lipoids except to say that they are compounds of fatty acids with nitrogen-containing groups;

[1] Sajous, C. E. de M., "Internal Secretions and Principles of Medicine," 1903, Vol. 1, and 1907, Vol. 2, and subsequent ten editions.

[2] Sajous, C. E. de M., "The Relation of the Endocrine Organs to High Blood-Pressure: A Newer Interpretation." *American Medicine*, 1923, Vol. XVIII, pp. 393–398. See also "Thermogenesis as the Fundamental Function of the Adrenal Medulla and Cortex." *Endocrinology*, 1925, Vol. IX, pp. 441–455.

phospholipins, represented by lecithin include phosphorus, and galactolipins represented by cerebrosides include carbohydrates instead of phosphorus. Cholesterol, which may be taken to represent a third group, is a monatomic alcohol of a substance related to the terpenes.[1] It is widely prevalent, and is intimately bound up with lecithin in living matter. As will be shown in another section of this paper, it is to the properties of suspensions or solutions of phospholipins and cholesterol and to their combinations with proteins that we must look for some of the most important activities of the cell.

The phospholipins may be associated with anthocyans, and with such carbohydrates as glucose or pectin. When obtained from plants by extraction with water or oil solvents such a varied complexion is shown that it is impossible to say much as to the nature of the combination of the lipoid nucleus with the associated substances; it may be stöchiometric, or by absorption or what some writers characterize as mechanical entanglement.

The lipoids may form salts with either acids or bases, but so far as cytological technique is concerned cell-structures including the lecithins are stainable with basic dyes only, such as methylene, a result suggesting the deposition of the dye as surface layers on lipoid aggregates. Special reactions to ammonia and to various electrolytes occur.

The lipoids have been characterized as autooxidizable, but it is highly probable that such action is due to the presence of oxidases, as proposed by Dr. Sajous in connection with his studies of the suprarenals and of the blood.[2]

For the purposes of the present discussion the properties displayed by the properties of lipoidal extracts of carrots, as studied by Grafe au Magistris in 1926, are of especial interest.[3]

[1] MacLean, H. and I. D., "Lecithin and Applied Substances," 2d edition, 1927. Longmans, Green & Co.

[2] Sajous, C. de M., "Thermogenesis as the Fundamental Function of the Adrenal Medulla and Cortex," *Endocrinology*, 9, 441–445, 1925 and personal communications 1927.

[3] Grafe, V. and H. Magistris, "Ueber die Phosphatide aus Daucus Carota," *Planta*, 2 Band, 3/4 (Schluss) Heft, 429–437, 1926.

The water-soluble lipoids in such extracts are taken to be diamino-phosphatides, from which some carbohydrates of a pectic nature may be split off.

When the cold extract or dialysate is shaken with ether, a part of the lipoidal material goes into solution in this solvent. Evaporation of the ether leaves a residue which is insoluble in water but which swells in water. Conversion from the water-soluble to the insoluble condition may supposedly be caused by changes in temperature, or by the action of various ions. Alkaline earths and mineral acids exercise a precipitating action, but weak alkalies, a dissolving action which changes to precipitating action in strong concentrations.

Water and water-soluble lipoids may pass layers of these substances carrying various organic and inorganic groups. When concentrated sugar solutions or electrolytes come into contact with these substances in bounding layers, the water hydratable phosphatides are precipitated and constitute a membrane impermeable for the precipitating agent. It is on account of these facts that plasmolyzing experiments are so indeterminate.

The characters of the extracted substances acquire their chief value when taken in connection with the findings of Hansteen-Cranner as to the localization and condition of lipoids, especially of the lecithin type in plasmatic masses and in enclosing cell walls.[1]

The picture of the arrangement of the lipoids in the plant cell presented by Hansteen-Cranner was one which suggested to me the incorporation of such substances in the jellies used as lining layers of the artificial cell which I first described before this Society in April, 1922. Such a cell was, in brief, a capsule, first made of porcelain clay; later, models of wood

[1] Hansteen-Cranner, B., "Beiträge zur Chemie und Physiologie der Zellwand und der plasmatischen Grenzschichten," *Ber. d. deut. Bot. Ges.*, 37; 380–391, 1919. "Zur Biochemie und Physiolgie der Grenzschichten lebender Pflanzenzellen." *Meld. f. Norges Landbrukschiskole*, Hft. 2, 1922. "Weiter Beiträge zur Chemie und Physiologie der Pflanzlichen Zellphosphatides," herausg. V. Grafe u. Magistris, *Meld. f. Norge Landbrukshiskole*, 1925. "Untersuchungen ueber die Frage," ob in der Zellwand losliche Phosphatide vorkommen. *Planta*, 2 Band, $\frac{3}{4}$ (Schluss) Heft, 1926, 438–445. Prepared for publication by Magistris.

or paper impregnated and lined with proteins, mucilages, and soaps were used.[1]

In some earlier experiments with colloidal mixtures my attention was concentrated in finding an assemblage of material occurring in the plant cell which would be capable of the degree of hydration and sensitiveness to the concentration of the hydrogen ion that characterizes living matter. It was concluded from the results obtained that proteins, mucilages or pentosans and the soaps were the main components. Mixtures of these substances in the form of thin plates were seen to imbibe so much water as to increase their volume thirty or forty times, in which hydrated condition sensitivity to the presence of electrolytes was much like that of protoplasm.[2]

Now, when these tests were carried farther to analyses of the penetrability or permeability of colloidal masses such as those which make up the plasmatic mass, it was concluded, as to the nature of the bounding membrane of protoplasm which regulates or controls the substances which pass in and out of its mass, "that it is a polarizable protein layer, or pentosan anhydride, or that it is an albumen-lipoid mosaic, are all theses which fall to the ground when tested by the laws of colloidal action, or by the phenomena of cell-behavior."

Interesting effects, including some in negative osmose, were obtained with first types of cells constructed. The next stage of the experiments included the incorporation of lecithin in the plasmatic layers of the artificial cell. It was found that this substance, when arranged in a coating or layer external to the mucilages and proteins, gave rise to increased osmotic action and to differential effects in the passage of ions. The influence of acidity and alkalinity, concentration of salts, etc. gave effects in striking parallelism with the behavior of living cell-masses, the expansion and deflation of which were measured by the auxograph in similar solutions. In the

[1] MacDougal, D. T., "The Probable Action of Lipoids in Growth," *Proc. Amer. Phil. Soc.*, 61, 33–52, 1922. "The Distentive Agencies in the Growth of the Cell," *Proc. Exper. Biol. and Med.*, 19, 103–110, 1921.

[2] MacDougal, D. T., "Water Deficit and the Action of Vitamines, Amino-compounds and Salts on Hydration," *Amer. Jour. Bot.*, 8, 296–302, 1921.

same year as my own experiments Boas brought out his contribution on the action of lipoids in exchanges between the plant cell and the medium, in which these substances are assigned a major rôle in permeability. Boas' tests consisted largely of measurements of changes in volume of masses of living cells when placed in solutions of salts, in lipoid solvents, and with varying H–OH concentration. His results, confirmed by later experiments, are in entire agreement with my auxographic measurements of the changes in volume of large sections of living cell-masses, and support the thesis as to the dominating action of the lipoids in cell-exchanges.[1]

My experiments up to this point were with arrangements of biocolloids which included only phospholipins. Certain phases of permeability, particularly, with reference to H–OH concentration, were not duplicated in the artificial cells thus constructed. The inclusion of cholesterol was known to be highly desirable. Furthermore, it did not seem that the experiments in which mixtures of lecithin and cholesterol were used to impregnate sheets of silk and other materials, or in other plates or layers, were adequate. These substances in the living cell are in the condition of joint suspensions, in solution, and in intimate association with highly hydrated protein and pentosan jellies. The more nearly these conditions could be reproduced, the more exactly the action of the cell might be analyzed.

Cholesterol is oil-and-ether-soluble but not water-soluble. At the time of the earlier experiments no demonstration had been made as to the manner in which this substance might be carried in a liquid condition with lecithin. No plausible explanation of the manner in which it occurs in the plant cell which is to be taken as the principal scene of origin of such substances had been formulated. Physiological chemists assumed that solution of lipoid in blood or animal tissues was in bile salts or taurocholic acid, but repeated trials failed to secure solutions with these substances.

[1] Boas, Fr., "Untersuchungen ueber die Mitwirkung der Lipoide beim Stoff-Austausch der Pflanzlichen Zelle," *Biochem. Ztschrift*, 117: 166–214, 1921.

Finally a satisfactory method of making solutions and suspensions of lecithin and cholesterol was perfected by Doctor Moravek, which made it possible to construct colloidal cells which would thus include the main materials participating in the regulatory mechanism controlling permeability in the living cell.[1]

The procedure for including both lecithin and cholesterol in a colloidal cell, the layers of which also contained pectin, agar, and gelatine, was as follows: Equal amounts of the two substances, to make 0.1 per cent. of the colloidal mass in which they are to be incorporated, are ground together in a mortar. A few drops of ether are added to dissolve both and allow the formation of a homogeneous viscous solution in this solvent. The liquid is now placed on a water bath and the ether expelled; while still hot, a drop or a few drops of 5 per cent. gelatine solution is added and the mixture is stirred thoroughly; finally the entire amount of liquid gelatine is added which is to be used in the preparation. The result is taken to be an emulsion of cholesterol-lecithin in the gelatine solution in which lecithin becomes a protective colloid of the cholesterol and as a solution with cholesterol, and also with gelatine, and as it might also with pectin and agar. The behavior of cells including a layer of this material through all exchanges between the contents of the cell and with the medium as given below are to be taken as including several conditions not presented by simple suspensions of these two lipoids. Some of the reactions of such suspension have been recently studied by Rona and Deutsch.[2]

Suspensions of cholesterol in water were secured by evaporating 40 cc. of a 1 per cent. solution in acetone in 200 cc. conductivity water in a pressure flask at 60 to 70 degrees C. by the air of an air-pump. A suspension of cholesterol was made by evaporating the ether from 40 cc. of a 10 per cent.

[1] MacDougal, D. T. and V. Moravek, "The Activities of a Constructed Colloidal Cell," *Protoplasm*, 2, Hft. 2, 161–188, 1927.

[2] Rona, P. and W. Deutsch, "Untersuchungen ueber Cholesterin und Lecithin-suspensionen." (Chem. Abt., pathol. Inst. Univ. Berlin) *Biochem. Zeitschr.*, Bd. *171*, H ½, S 89–118, 1926.

solution in 200 cc. water so that the final concentration was calculated as being about 0.4 per cent.

The cholesterol suspension was found to precipitate with HCl between P_H 2.4 and 3.2 irrespective of the character of the buffer solution at 1/100 M. Coagulation occurring in neutral salts increased from 1/128 to 1/16 and 1/32 M. Not much difference could be found in the action of the chlorides of K, Nh_4, Li, Cs, and Ro. Precipitation by chlorides of barium and calcium resulted from concentrations as low as 1/2048 M to 1/4096 M. With the trivalent bases coagulation was shown in solutions as dilute as 1/150,000 M. The effects of H ions and other kations were additive.

A much greater concentration of H ions was necessary to coagulate lecithin suspensions than cholesterol, this occurring at P_H 1.73 to 1.75. Coagulation in NaCl was greatest between 1/4 and 1/2 M. Other salts were comparatively inactive. Coagulation by NaCl was retarded in the presence of $CaCl_2$, and this antagonism or interference decreased with the concentration of the calcium solution from 1/4 to 1/32 M.

The protective action of lecithin upon cholesterol coagulation decreased with its concentration but was found to end at about 0.0025 per cent. lecithin, and such coagulation actually proceeds more slowly than in a lecithin suspension alone. No effects were secured by adding proteins, and coagulations by human serums were very irregular. The suspensions studied by Rona and Deutsch are in fact similar to those which might be secured by metallic salts.

While lecithin and cholesterol are taken by Dr. Sajous not to be combined with each other, it is highly probable that they are associated with proteins in living material. It is assumed that in the solutions by MacDougal and Moravek that lecithin forms some true solution in which may be included proteins and mucilages, and in which are swollen globules of lecithin. Cholesterol is probably in both solution and suspension, toward which a protein may act as a protective, a rôle played by gelatine in our preparations.

With these features of the association of lecithin and cholesterol in mind we may now with profit take up the effects of the mixture in artificial cells in which lecithin alone of the lipoids had been previously used. It may be well to recall that in living vacuolated cells, the lipoids are taken to accumulate in association with proteins and carbohydrates in a peripheral layer of the plasmatic mass and to extend in irregular strands into the plasma. A slightly different complex is found in the nucleus. In addition, the view that the independent middle lamella, originally laid down between two separating cells, is a lipoidal layer, is now gaining additional support. This layer remains essentially lipoidal, although many inclusions may be added during the life of the cells.

Some of the features of the action of colloidal cells constructed to simulate the arrangement of the lipoids in the plasma were described at the annual meeting of this Society in April, 1922. Some further interpretations are now offered.

The results of the activity of the artificial cell are measured by its osmotic action, and by its permeability to various ions. Permeability of a layer to ions increases with the degree of hydration of its colloids, and the more highly hydrated a layer may be, the less endosmotic action it may show. The presence of a phospholipin such as lecithin alone in a cell lessens permeability and allows the attainment of a high degree of turgidity or osmotic distention of the cell. It is notable that in artificial cells in which lecithin was incorporated without cholesterol, the speedy entrance and accumulation of K was very marked. This specialization, so notable in the plant cell, was not displayed by the series of artificial cells constructed with the use of cholesterol in addition, a feature demanding further investigation. This phase of activity may depend upon the relative concentration of the two substances.

Next it was found that artificial cells including both lipoids showed an action concurrent with that of living cells by greater osmose in balanced solutions of Na and Ca; the effect diminished with the proportion of lipoids in the cell and with the substitution of Mg for Ca. In general it was found that ions penetrate a lipoidal cell most rapidly from solutions at P_H 4.5 in contrast with the maximum of a lipoid-free cell at P_H 5.4 of the solution—a fact significantly in agreement with the behavior of lecithin-cholesterol solutions or suspensions. It is, however, in the stabilization of H ion concentration of cell contents that the most striking effects of the presence of the lecithin-cholesterol suspension is found. Without the lipins the change in the acidity of the contents of an artificial cell may change as much as 0.45 P_H. When the suspension of the two substances is included, cells with contents of sugar solutions immersed in a series of salt solutions ranging from acid at P_H 3.05, equivalent to 0.001/M HCl, to alkaline solutions at P_H 8.2 operate for 48 hours, an increase of acidity of the cell-contents amounting to P_H 0.25 is found in the cells in the more acid solution. In the remainder of the series extending past neutrality and into alkalinity the H–OH concentration of the cell-contents remains practically unchanged, although water was passing endosmotically into cells, K or Na were traversing the colloidal layers to be found in the cell-contents at the conclusion of the tests, while some sugar was escaping. Such a description would fit the consequences of immersion of sections of living cell-masses in acid or alkaline solutions. The regulatory action noted is one which hitherto has been ascribed solely to the "vital" activities of protoplasm. It is displayed in the artificial cell only when suspensions of lecithin-cholesterol are included in the plasmatic layers. It seems hardly necessary to point out that something of a similar action may occur in red corpuscles. The stabilizing action has been taken by us to rest upon the fact that the lecithin-cholesterol layer is almost completely impermeable to OH-ions and has a very low permeability to H-ions. Cholesterol is completely saturated with its own OH group and repels OH-ions from the solutions, while absorbing some H-ions; lecithin on the other hand absorbs OH-ions and makes difficult the transference of H-ions.

Coagulation or aggregation of the lipoidal material in a layer or membrane will increase permeability. That of the artificial cell was found to be greatest at P_H 2.9, when the two lipoids were included. According to Rona and Deutsch, lecithin suspensions in water coagulate at about P_H 2 and cholesterol at P_H 2.4 to 3.2. The coagulation of mixtures of the two would be at a resultant between these limits which would be in the region of action of the artificial cell, and also is in the region of H-ion concentration, in which I have found living cell-masses to show increased permeability and rapid decrease in volume. The possible presence of varying proportions of carbohydrates and of electrolytes must always be taken into account. Then, too, the external layer of a protoplast may be safely taken to be the seat of continuous oxidizing processes, which affect the transference of material profoundly, a possibility which has hitherto escaped attention entirely.

It is not easy to conceive that such layers should undergo the phase-reversals, suggested by Clowes as taking place in oil and water mixtures under the influence of the action of hydroxides, for example.[1] Some features of the theoretical arrangement of cell-colloids proposed by Free are of interest in connection with the known facts as to the occurrence of lipoids in cells.[2]

Free's theory was meant to concern the arrangement of the entire plasmatic mass. Its predication of a substance distributed in varying irregularity between swollen globules and a continuous liquid medium of the same material is an approximation of the relations of the two fractions of lecithin as described above. Passage through a layer of such structure is supposed to depend upon the viscosity of the continuous medium. The facts brought to light since these proposals were first made in 1918, include a number of phenomena

[1] Clöwes, G. H. A., "On the Action of Antagonistic Electrolytes on the Electrical Resistance and Permeability of Emulsion Membranes," *Proc. Soc. Exper. Biol.*, 15, 108, 1918.

[2] Free, E. E., "A Colloidal Hypothesis of Protoplasmic Permeability," *Plant World*, 21, 141, 1920.

which would not be covered. Among these would be precipitations or coagulations, modifications of protective layers of proteins or of lecithin, and the interferences of lipoids of the two main groups of lipoids present.

Our discussion has up to this point concerned the effects of lipoid-suspensions upon the passage of materials into and out of the cell. What variation in this action may result from the continuous action of oxidases on the phosphorus of the lecithin constituent, as proposed by Dr. Sajous, remains to be studied. Passage of material into and out of the cell is seen to take place through a layer in which the respiratory activity is very great and colloidal conditions may be profoundly affected by energy releases. These, in fact, may be an essential link in the chain. It seems reasonable to suggest that the changes in permeability consequent upon death may be closely associated with disturbances in respiration. It is clear that the joint action of lecithin and cholesterol control endosmose, movements of organic substances and of ions in living cells, and that this control vanishes with the death of the cell. The mistake must not be made, however, of attributing changes in permeability consequent upon death to any simple factor. The breaking up of loosely held combinations of lipoids and proteins accompany death of protoplasm as suggested by Lepeschkin [1] may be the principal condition of such a change. It is evident that an extension of studies of the action of lipoid solutions in the presence of other bio-colloids promises much to the physiologist whether he be concerned in oxidations in animal tissues, the derivation of platelets in the blood, or in the action of plant cells. It may be mentioned in this connection that the fact that lecithin solutions absorb much more carbon dioxide than a similar volume of distilled water is a matter of great importance in the intake and reduction of this gas by green leaves by the action of chlorophyll bodies. This capacity for holding and yielding carbon dioxide under diminished pressure may also be held to account partially for

[1] Lepeschkin, W. W., "Ueber die Zusammensetzung der lebenden Materie," *Biochem. Ztschrft.*, 171, 126–145, 1926.

the high proportions of carbon dioxide (from 2 to 26 per cent.) found in gases drawn from the interior of massive stems such as trunks of trees, by suction equivalent to 0.4 to 0.6 atmosphere.

The presence and many important facts of localization of lipoids in nuclei and in plasmatic masses are well known. The nature of the combinations of these substances with proteins in plasmatic masses can only be conjectured. The thermogenic activity of the lipoids have been examined by Sajous in animals and their participation in the respiratory processes of the plant has been studied by Palladin. The recent contributions of Hansteen-Cranner as to the occurrence of lipoids in cells have made possible a new conception of the part played by these substances in permeability. Results supporting the theory of lipoidal control of the passage of electrolytes and organic material into and out of the cell have been contributed by Boas and by myself, using living material. The study of lecithin-cholesterol suspensions by Rona and Deutsch and the use of such material in artificial cells have shown that when included in layers of protein pentosan jellies, the passage of ions is controlled in a manner much the same as in the living cell.

TRIANAEOPIPER, A NEW GENUS OF PIPERACEAE

By WILLIAM TRELEASE

(*Read April 19, 1928*)

THE position of the "spikes" or "aments" that constitute the inflorescence of Piperales differs both in fact and in fixity among and in the genera that constitute this order of plants.

In the family Saururaceae the raceme-like inflorescence is sympodially lateral and opposite a leaf in *Saururus;* but in *Anemopsis* the short inflorescence is terminal and resembles a bracted head.

In the family Piperaceae, the subfamily Peperomieae offers terminal, sympodially lateral, or axillary spikes. The very large and varied genus *Peperomia* affords examples of each of these positions—sometimes only one in a species, sometimes terminal and sympodial or terminal and axillary combined, but never, so far as I have seen both sympodial and axillary spikes in a single species.

The subfamily Pipereae usually lacks apical spikes and in all but a very insignificant fraction of its vastly numerous species the inflorescence is constantly sympodially lateral—opposite a leaf. Notwithstanding many efforts to group the species of this subfamily under several generic heads (*e.g.*, *Enckea*, *Carpunya*, *Schilleria*, *Steffensia*, *Artanthe*) the floral characters employed have proved so inconstant, fugacious, or difficult of application that the entire subfamily is regarded usually as consisting of a single genus, *Piper*. Apparently the only workable generic segregate of these Pipers with sympodial spikes is *Ottonia* which characteristically has pedicelled flowers while they are constantly sessile on the spike in the others.

The case is somewhat different with the few Pipereae that have an apical or axillary inflorescence. Under the synonymous names *Pothomorphe*[1] and *Heckeria*[2] a small group of very closely interallied suffruticose species has been segregated because their perfect flowers occur in umbellate spikes on an axillary peduncle. Though floral details, apart from a prevalent reduction of the stamens to 2, are not very different from those of innumerable species left in *Piper*, the segregation of Miquel's *Pothomorphe* recognizes that it is taxonomically of a different rank from the Enckeas, Steffensias, etc., making up the rank and file of what is left in *Piper*.

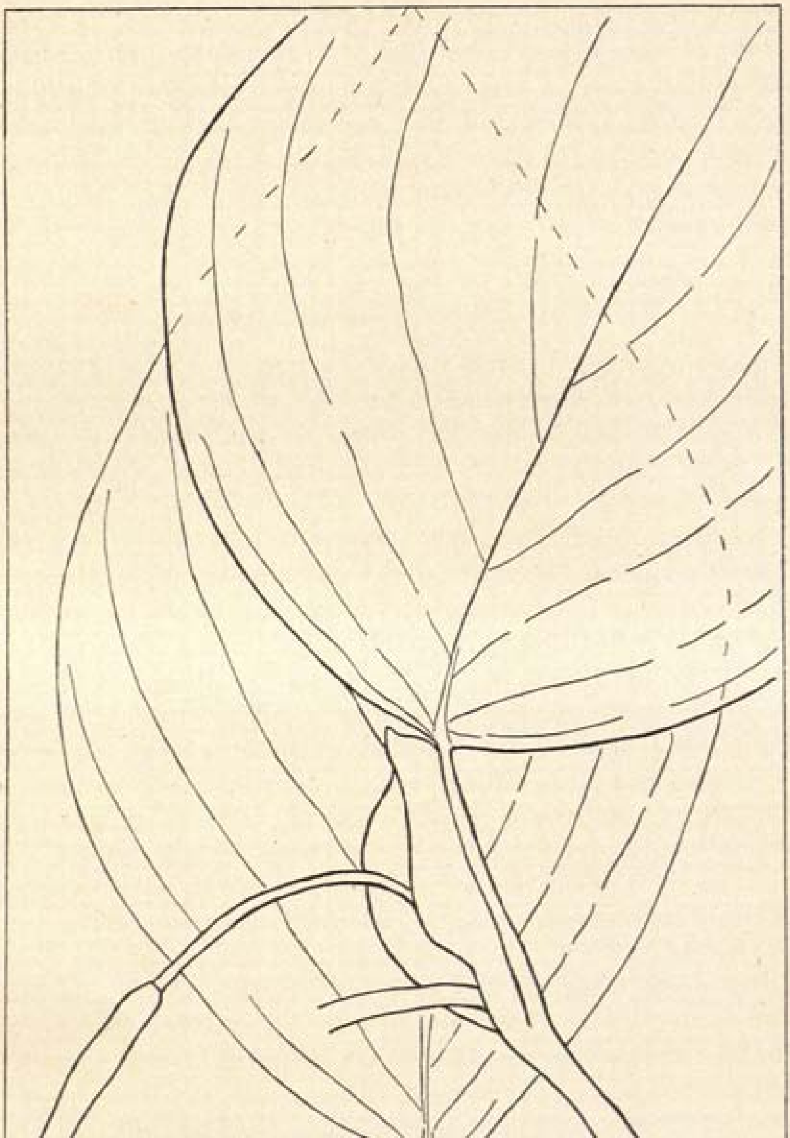
Comparably, a small group of Oriental species with imperfect flowers—in terminal spikes when staminate and clustered on reduced axillary branches when pistillate—may be kept apart from *Piper* conveniently under the generic name *Macropiper* which Miquel applied to it.

The same consideration pertains to the single scandent species of Haiti to which I have given the name *Manekia Urbani*,[3] in which the spikes, though on a common axillary peduncle, differ from those of *Pothomorphe* in secondary position while the aspect of the plant is totally different.

Quite as aberrant as these, from the general run of Pipers, is the very small group of scandent hardly woody species of tropical America centering about *Piper incurvum*, with single axillary spikes or less characteristically a terminal spike, to which I have given the name *Sarcorhachis*.[4]

The purpose of the present communication is to propose segregation under the generic name *Trianaeopiper* of the only other species with axillary spikes which remain in the genus *Piper* after removal of *Pothomorphe*, *Macropiper*, *Manekia* and *Sarcorhachis*. These are only four in number and appear to be confined to the Colombian mountains. They agree with *Sarcorhachis* in producing a single spike of perfect flowers in each fertile axil, but are of quite different aspect and spike structure. This genus and its component species may be characterized as follows:

[1] Miquel, Comment. Phytogr. 36 (1839).
[2] Kunth, Linnaea. 13: 565 (1839).
[3] Fedde, Repert. 23: 313 (1927).
[4] Contr. U. S. Nat. Herb. 26: 16 (1927).



TRIANAEOPIPER PEDUNCULATUM
(The type—natural size).

TRIANAEOPIPER n. gen.

Shrubby. Leaves alternate with broadly winged petioles, multiple-nerved from below the middle. Inflorescence 1 or rarely 2 compactly flowered pedunculate spikes in each axil. Flowers perfect, sessile; stamens 2 or 4, free. Berries depressed-globose with 3 stigmas.—Generic type *Piper pedunculatum* C. DC.

Leaves acute-based; stigmas sessile.
 Apex acute: nerves rather crisp-pubescent beneath. *T. pedunculatum.*
 Apex acuminate: nerves merely puberulent.... *T. timbiquinum.*
Leaves cordate: stigmas on a style.
 Petiole no longer than the sinus: leaves pubescent on both sides.................................... *T. cordilimbum.*
 Petiole elongated: nerves at most puberulent beneath. *T. Trianae.*

Trianaeopiper pedunculatum (C. DC.) n. comb.
 Piper pedunculatum C. DC., Prod. **16**[1]: 334.
 Colombia. Tuquerres to Barbacoas (Triana 756).
Trianaeopiper timbiquinum (C. DC.) n. comb.
 Piper timbiquinum C. DC., Bot. Jahrb. **40**: 252.
 Colombia. Cotaje, Rio Timbiqui (Lehmann 9010).
Trianaeopiper cordilimbum (C. DC.) n. comb
 Piper cordilimbum C. DC., Bot. Jahrb. **40**: 252.
 Colombia. Cotaje, Rio Timbiqui (Lehmann 9012).
Trianaeopiper Trianae (C. DC.) n. comb.
 Piper Trianae C. DC., Prod. **16**[1]: 334.
 Colombia. Pasto (Triana 2).

THE UNIVERSITY OF ILLINOIS,
February 20, 1928

OMORHAMPHUS, A NEW FLIGHTLESS BIRD FROM THE LOWER EOCENE OF WYOMING

By WILLIAM J. SINCLAIR

(*Read April 20, 1928*)

THE presence in the North American Eocene of gigantic flightless birds comparable in size to the New Zealand Moas has been known since 1874 when the first specimen was found in the so-called Wasatch of New Mexico. All have been referred to Cope's genus, *Diatryma*,[1] and, with the exception of the magnificent skeleton of *Diatryma steini* in the American Museum of Natural History, New York, are based either on phalanges or fragments of the tarso-metatarsus. To these must now be added a second genus, possibly related to *Diatryma*, but certainly sufficiently different to be regarded as distinct, for which the name *Omorhamphus* is proposed (ὠμὸς, cruel; ῥάμφος, beak of a bird). The type specimen No. 13106 Princeton University Palæontological Museum, consists of the tip of the huge curved beak which suggests the generic name; some additional skull and other fragments; the left leg complete with the exception of a few phalanges, the terminal portions of certain of the limb elements and most of the fibula; a right tarso-metatarsus with several phalanges and a number of broken vertebræ. It was found during the past summer by Mr. T. C. von Storch, a member of the Princeton 1927 Expedition. The horizon is the Lower Gray Bull formation, Lower Eocene, and the locality is about one and one half miles southeast of Dorsey Creek and, perhaps, a couple of miles or so south

[1] *Diatryma gigantea* Cope, "U. S. Geographical Surveys West of the One Hundredth Meridian," Vol. IV., Part II., pp. 69–71, Pl. XXXII., Figs. 23–25, 1876. *Diatryma* (*Barornis*) *regens* Marsh, *Am. Jour. of Sci.* (3), Vol. XLVIII., p. 344, figures, 1894. *Diatryma ajax* Shufeldt, *Bull. Am. Mus. Nat. Hist.*, Vol. XXXII., Article XVI., pp. 283–306, Pls. LI.–LIV., Figs. 1–16, 1913. *Diatryma steini* Matthew and Granger, *Bull. Am. Mus. Nat. Hist.*, Vol. XXXVII., Article XI., pp. 307–326, Pls. XX–XXXIII., 1917.

of the old Otto-Basin road, Bighorn Basin, Bighorn County, Wyoming.

In recognition of Mr. von Storch's enthusiastic participation in the program of the expedition, the new form is named after him: *Omorhamphus storchii.* There is no reason to believe that more than a single individual is represented by the parts preserved.

The Beak and Other Skull Fragments

As in *Diatryma*, the beak of *Omorhamphus storchii* (Pl. I., Figs. 1, 2) is extremely powerful, but evidently much shorter and certainly not as high. It had thin elevated lower margins, now broken off, and curved downward at the tip, unlike *Diatryma*, from which it also differs in the anterior position of the nares (*a*, Pl. I., Fig. 1). No furrow extends in front of them, on the surface of the beak, but this area is broadly depressed. The anterior margin of the nareal opening is sharply defined and thin-edged. It is quite unbroken except where the main fracture severing the beak-tip from the rest of the skull crosses its course. The beak of *Diatryma* lacks teeth and all trace of grooves or pits along the lower margin, whereas, in *Omorhamphus*, large circular to oblong pits, presumably vascular, occur in linear series just within the attenuated lateral margin (Pl. I., Fig. 2, *p*). There are four of these pits preserved on the left side of the lower surface and three on the right, the first very long. Most of them resemble alveoli for teeth, but the second in the left hand series is small and out of line with the rest, while the first on the right is a long deep depression resembling a cavity occupied by a blood-sinus. So far as I know, *Gastornis parisiensis* [1] is the only bird yet described which has similar perforations on the beak-margin. *Omorhamphus* differs from it however in other details and shows no trace of having possessed bony marginal denticulations on the beak. Externally, the beak is intimately pitted with minute vascular

[1] Lemoine, 1878. Recherches sur les Oiseaux Fossiles des Terrains Tertiares Inferieurs des Environs de Reims, Pt. I., pp. 1–69, Pl. I.–V.; 1881, *ibid.*, Pt. II., pp. 75–170, Pls. VI.–XI.

perforations among which larger foramina, also vascular, occur at random. The fragment preserved is uncrushed, with strong dorsal convexity and semicircular superior cross-section. It measures 61.5 mm. from the tip to the upper margin of the nareal border as preserved, has a maximum depth of 48.5 mm., a thickness of 20 mm. at the broken end above the anterior nares and a maximum transverse width of 24.5 mm. on the lower surface.

Two additional skull fragments are shown, about two thirds natural size, in Pl. I., Figs. 7 and 9. The first of these is interpreted as part of the left orbital margin. The other is thought to belong to the opposite side of the skull at the line of junction of the parietal (above the suture *a–b*, Pl. I., Fig. 9) with the squamosal below, involving also part of the supraoccipital, *c*. Their measurements are given in the plate explanation.

The Femur

Except for a depressed area on the lower part of its front surface, the left femur (Pl. II., Figs. 1, 2, 7) is uncrushed, but, unfortunately, lacks the entire summit of the trochanter and the articular surface of the head as well as the condyles, distally. Viewed proximally (Pl. II., Fig. 2 and Text Fig. 1)

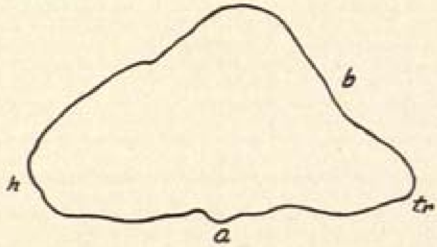


Fig. 1.—*Omorhamphus storchii.* Outline of the proximal end of the femur, 2/3 natural size. *a*, pretrochanteric surface; *b*, ectotrochanteric surface; *tr*, trochanter; *h*, head.

the upper end of the femur is seen to be strikingly triangular in cross-section, with no backward projection of the trochanter so far as preserved, but, anteriorly and outwardly, it projects considerably, the pretrochanteric surface (*a*, Text Fig. 1) meeting the ectotrochanteric surface (*b*, same figure) at an acute angle and forming a prominent crest (*tr*, Pl. II., Fig. 2 and Text Fig. 1). A circular, roughened and depressed area (Pl. II., Fig. 2, *x*) lies opposite the point of subsidence of this crest (*tr*) on the anterior inner surface of the femoral shaft. The latter, in our specimen, was considerably damaged by weathering, but contacts were obtained between all the fragments from which it has been rebuilt, so that no error is involved in its length and probably but little, if any, in its width. On the rear face (Pl. II., Fig. 1) which is the most complete, there is no trace of intermuscular ridges. The lower extremity of the femur has had most of its anterior surface depressed, but is not otherwise distorted, and, as already indicated, the condyles are lacking. The configuration of the parts remaining can better be appreciated from the figure (Pl. II., Fig. 7) than described in words. The posterior surface of the shaft drops gradually into the popliteal fossa (Pl. II., Figs. 1, 7, *p*), a depression bounded laterally by broadly rounded ridges of subequal size rising rather rapidly from the shaft. No trace of pneumatic orifices appears on the floor of the fossa, where the only perforation is a small vascular foramen centrally placed at the entrance to the fossa and scarcely visible in the photograph. As preserved, the femur has a length of 180 mm., a width from head to trochanteric crest of 77 mm. and a transverse width distally of 71 mm. In general it resembles the femur of *Diatryma*.

The Tibio-Tarsus

As the type of *Omorhamphus* pertains to a young bird perhaps from one half to two thirds grown, the proximal row of tarsalia had not fused with the tibia, which was recovered in fragments. Much of the shaft was badly weathered and contact is lacking between the proximal and distal moieties. As reconstructed, its length and the orientation of the terminal portions have been approximated to those of the tibia of a young Moa in the Princeton collection, in which the femur and tarso-metatarsus approach the size of these elements in *Omorhamphus*.

Unfortunately, the articular surfaces and processes of the upper extremity are entirely lacking, the weathered surface remaining having the outline indicated in Text Fig. 2. The

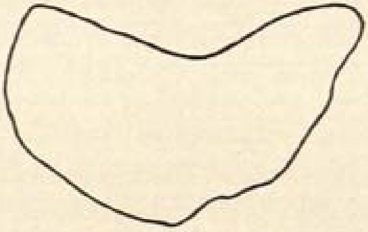


Fig. 2.—*Omorhamphus storchii*. Outline of the proximal end of the tibio-tarsus, 2/3 natural size. *pc*, procnemial crest.

anterior surface of the shaft has been so severely damaged that little of the procnemial crest (*pc*, Text Fig. 2) remains, and this apparently did not continue very far down the shaft, as there is no trace of it on the distal section preserved. As in *Diatryma*, the shaft is straight, but distally, in the vicinity of the inner condyle, just below the extensor bridge, it curves inward much more sharply. The bridge itself appears to have been incomplete, perhaps a juvenile character. Its outer abutment is extensive and the center of the bridge

Fig. 3.—*Omorhamphus storchii*. Distal end of tibio-tarsus showing the incomplete extensor bridge, 2/3 natural size.

well ossified. At about two thirds of its span across the extensor groove it is broken off and no trace of its inner abutment can be found on the parts preserved. Beyond it, the extensor groove continues as a deep depression sharply notching the intercondylar area (Pl. II., Figs. 3 and 6, and Text Fig. 3). The outer wall of the groove throughout a distance of 18 mm. above the bridge is steep and this border continues to be steep beyond the bridge, distally. On the other side, the approach to the groove is gradual proximally, but rapidly steepens distally.

As reconstructed, the tibio-tarsus measures 297 mm. in length, has a maximum proximal expansion of 72 mm. and a distal width of 73 mm.

The Fibula

A fragment supposed to be the proximal end of the fibula is shown, about two thirds natural size, in Pl. I., Fig. 8. Presumably it pertains to the left side, as does the tibio-tarsus and the convex surface shown in the photograph is probably the internal one. It measures 33.5 mm. in maximum transverse diameter.

The Tarso-Metatarsus

Fortunately, both tarso-metatarsi are preserved and, although neither is complete proximally, the same parts are not missing in each, so that they supplement each other admirably (Pl. II., Figs. 4, 5, 8). As a consequence of youth, the distal tarsalia, which are lacking, had not yet fused with either metatarsus, which, accordingly, presents two deep notches, one on either side of the proximal end of the third digit instead of the pair of interosseous canals which suggested Cope's generic name *Diatryma*. As this part of the metatarsus changes materially with age, on fusion with the distal tarsals to form the tarso-metatarsus, it is difficult to decide how it would have appeared in the fully adult bird. Below the interosseous perforations, on its anterior aspect, the shaft (Pl. II., Fig. 4) is broadly concave, flattening out at about midlength and becoming broadly convex distally as the trochleæ are approached. A broad shallow depression in this convex area leads into the outer intertrochlear interspace, but no foramen for the anterior tibial artery is present, nor does its absence appear to be due to a juvenile lack of ossification, although both tarso-metatarsi show a deep, narrow crack-like cleft on the inner side of the groove between the proximal ends of the median and outer trochlear processes, the presence of which is probably ascribable to this cause. In this region, in Cope's type of *Diatryma gigantea*, Shufeldt [1] reports "a smooth, hemicylindrical, anteroposterior groove, which is, beyond all question, the distal half of the *interior surface* of the *outer side* of the foramen above described." This is figured on his Pl. IX., Fig. 68, *atf*. No reference is made to this foramen by Matthew and Granger in their description of *Diatryma steini*, nor does any trace of it appear in their specimen. In *Gastornis parisiensis* a large perforation is indicated in Lemoine's figures. In the left tarso-metatarsus of *Omorhamphus* a nutrient foramen perforates the shaft just below the transverse fracture seen in Fig. 4, Pl. II., near the beginning of the intertrochlear groove, but this does not appear on the opposite element.

On the posterior surface of the shaft (Pl. II., Fig. 5) the broad median convexity produced by the posterior position of the proximal end of the shaft of the third metatarsal is most prominent in the vicinity of the interosseous canals, but rapidly flattens, so that half way down, the shaft is broadly convex, passing gradually to broadly concave as the trochleæ are approached. On both elements, a smooth shallow depression (*I*, Pl. II., Fig. 5) occurs on the posterior surface of the shaft, its center lying about 50 mm. above the point of maximum downward extension of the inner trochlea. This undoubtedly marks the position of attachment of a hallux, of which the element figured on Pl. I., Fig. 3 is interpreted as the vestigial first metatarsal.

[1] Shufeldt, 1915. Fossil Birds in the Marsh Collection of Yale University, *Trans. Connecticut Acad. of Arts and Sciences*, Vol. 19, p. 37, *atf*, Fig. 68, Pl. IX.

The median is the largest of the three trochleæ, the external next in size and the internal the smallest. Their maximum widths are respectively 27 mm., 17.5 mm., and 16 mm., but their widths vary from point to point. As preserved, the central element of the right tarso-metatarsus (Pl. II., Fig. 5) has a length of 160 mm. and the tarso-metatarsal shaft is 32.5 mm. wide at its narrowest point and 18.5 mm. from front to rear at the same place. The median trochlea has greatest prominence anteriorly and inferiorly (Pl. II., Fig. 8) while the outer is a little shorter than the inner although considerably larger and projects farther forward. In *Diatryma* the outer is the larger and also the longer. The median trochlea projects considerably anteriorly beyond the plane of the shaft. Posteriorly the two lateral trochlear processes are produced to about the same level which exceeds that of the median trochlea by about 7.5 mm. All three extend backward well beyond the plane of the posterior surface of the shaft. The median trochlea (Pl. II., Fig. 8) is widest in front, diminishing in transverse diameter posteriorly. It is divided in front by a broad shallow groove, almost invisible distally, but reappearing posteriorly, into two condyles of which the inner is the larger and has the maximum extension both anteriorly and proximally while the outer projects farther backward and rises higher proximally and posteriorly than does its fellow. Both lateral surfaces of the trochlear process are concaved to about an equal degree.

The outer trochlea maintains about the same width throughout most of its expanse, and has its proximal anterior and distal posterior corners elongated, the latter everted outwardly. Unlike *Diatryma*, it is ungrooved and broadly convex throughout instead of exhibiting a broad anteroposterior groove and an elongated inner condyle, features well shown in Shufeldt's plate [1] of *Diatryma gigantea* and exhibited also in Matthew and Granger's illustration of *D. steini*.[2] Furthermore, the outer trochlear process is swung farther outward

[1] *Loc. cit.*, Pl. IX., Fig. 68.
[2] *Loc. cit.*, Pl. XXXII., Fig. 3.

from the median element than in *Diatryma* so that the space between them is proportionately wider.

The small ento-trochlea has its maximum expansion of ungrooved articular surface in the rear, is constricted medially and expanded again in front. Its posterior inner corner is prominent and both the outer and inner surfaces of the trochlear process concaved rather more so than in the case of the ectotrochlear process, where the inner surface is fairly flat and the outer slightly concave.

The Phalanges

A fairly complete set of phalanges can be assembled for one foot (Pl. I., Fig. 10), although it is by no means certain that they all constitute a natural series, nor do we know whether *Omorhamphus* conformed to the normal avian phalangeal formula, although I assume that it did, and interpret the element shown in Fig. 3, Pl. I., as the vestigial metatarsal of the hallux of the right foot. The element shown in Fig. 4 of the same plate may be one of the phalanges of the hallux, but this is uncertain.

The large phalanx assembled as the proximal member beneath digit III (Pl. I., Fig. 10) resembles the same element of one of the lateral digits of *Diatryma* much more strongly than it does the proximal phalanx of the central toe in the foot of that genus. Instead of being bilaterally symmetrical, as is the proximal phalanx of digit III in *Diatryma*, the same element in *Omorhamphus* is wider anteroposteriorly at the inner side of the proximal end than it is on the outer side and its inner distal trochlea is much larger than the outer one. It has a length of 56 mm., a proximal width of 29 mm. and is too large to fit beneath either digit II. or IV. A second element of similar size and evidently the same phalanx of the right foot is shown from the rear, about two thirds natural size in Pl. I., Fig. 5. Neither fits its metatarsal trochlea very accurately but agrees with it in having its greatest anteroposterior depth beneath the larger inner condyle on the trochlea of digit III. Two smaller elements,

DESCRIPTION OF THE PLATES

PLATE I

Omorhamphus Storchii

Fig. 1.—Beak tip from the left side, about 2/3 natural size. *a*, anterior nareal opening. Maximum length of fragment from tip to upper margin of nareal border 61.5 mm.

Fig. 2.—Lower surface of the beak, about 2/3 natural size, showing the pits (*p*) just inside the broken margin. Maximum length, right side, 54 mm.

Fig. 3.—Vestigial metatarsal of the right hallux, posterior surface, about 2/3 natural size. Total length 28 mm.

Fig. 4.—Problematic element, phalanx (?) of (?) first digit, about 2/3 natural size. Length 30.5 mm.

Fig. 5.—Proximal phalanx of digit III., right foot, from the rear, about 2/3 natural size. Length 56.5 mm.

Fig. 6.—One of the better preserved vertebræ, about 2/3 natural size. Length, middle of centrum to highest point on arch 61 mm.

Fig. 7.—Skull fragment interpreted as part of the left orbital margin, about 2/3 natural size. Length of fragment 41 mm.

Fig. 8.—Left fibula, proximal end, internal side, about 2/3 natural size. Transverse diameter 33.5.

Fig. 9.—Skull fragment interpreted as parts of parietal, squamosal and supraoccipital of the right side, about 2/3 natural size. Maximum antero-posterior diameter 65. *ab*, suture between parietal (?) above and squamosal (?) below. *c*, supra-occipital (?).

Fig. 10.—Left foot from in front, about 1/3 natural size. Terminal phalanx of digit II. turned sideways to show point and curvature of claw. Terminal phalanx of digit IV. seen directly from in front, tip of claw broken off. Maximum length of tarso-metatarsus as preserved 170.5.

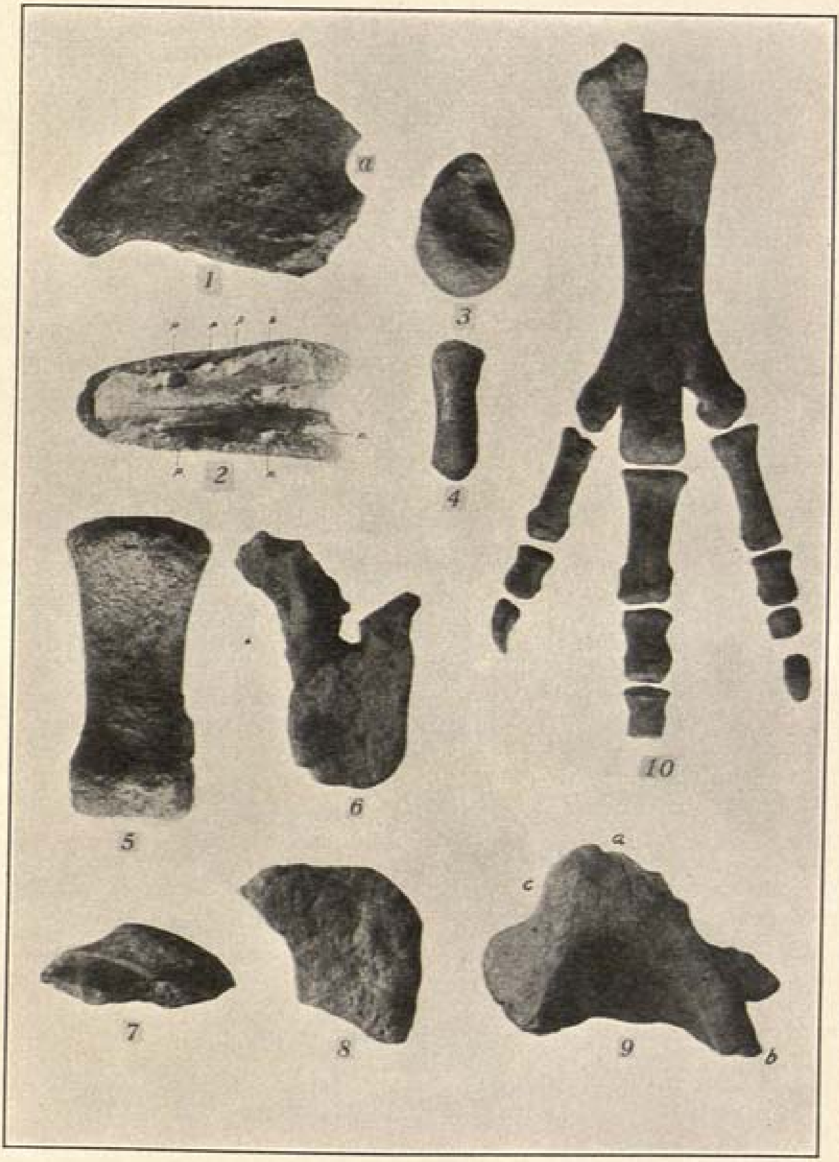


PLATE I

PLATE II

Omorhamphus Storchii

Fig. 1.—Left femur from behind, with 5 cm. scale to the same degree of reduction. *h*, head; *i*, entocondylar expansion; *p*, popliteal fossa.

Fig. 2.—Left femur from in front, with 5 cm. scale to the same degree of reduction. *h*, head; *tr*, trochanter; *i*, entocondylar expansion; *e*, ectocondylar expansion; *X*, circular rugose depression on shaft.

Fig. 3.—Left tibio-tarsus from in front, 1/4 natural size.

Fig. 4.—Left tarso-metatarsus from in front, with 5 cm. scale to the same degree of reduction.

Fig. 5.—Right tarso-metatarsus from behind, with 5 cm. scale to the same degree of reduction. *I*, surface for attachment of the hallux.

Fig. 6.—Left tibia-tarsus, distal end, a little less than 1/2 natural size. Maximum transverse diameter 73 mm.

Fig. 7.—Left femur, distal end, a little less than 1/2 natural size. Maximum transverse diameter 71 mm. *i*, entocondylar expansion; *p*, popliteal fossa; *e*, ectocondylar expansion.

Fig. 8.—Left tarso-metatarsus, end contour of distal extremity. Maximum transverse diameter 70 mm.

PLATE II

regarded as members of a pair, and of a size to correspond with the trochlear expanse on digits II. and IV. have been assembled as their proximal phalanges respectively (Pl. I., Fig. 10). This illustration renders unnecessary a description of the remaining phalanges with the exception of the unguals, which are counterparts of those of *Diatryma steini* except on a smaller scale. That assembled with the second digit of the left foot is entire and has been turned sideways to show its curvature. It is, however, by no means certain that it belongs there, as its articular surface convex in all directions, fails to register with the trochleæ of any of the phalanges preserved. The ungual assembled on digit IV. is seen from in front and has lost its tip. Both are quite short, pointed and moderately curved, semicircular in cross-section with a marginal groove on either side of their upper surfaces converging toward the tip and leaving a strongly convex central area between.

Vertebræ

The most complete of several vertebræ is shown about two thirds natural size in Pl. I., Fig. 6. None of these is well enough preserved to describe, but it is evident from their differences in size and shape that several parts of the column are represented.

Relationships

Comparison with any of the more modern types of flightless birds seems futile in view of the great time gap separating them from the Lower Eocene. The almost total absence of skull and the lack of other diagnostic parts of the skeleton render impossible the certain determination of the systematic position of *Omorhamphus*. It was, certainly, a large ground-dwelling bird with feet in many respects like those of *Diatryma*, but smaller and with a proportionately much shorter tarso-metatarsus, for even allowing for growth, I doubt if these elements ever reached the proportions of those of any known *Diatryma*. They are comparable in size to those of some of the smaller Moas. The anterior position of the nares indicates a beak of different proportions from *Diatryma* but it has the same thick and strong character indicated in the plaster reconstructions of *Diatryma steini* prepared by the American Museum (the beak of the original is crushed flat). On the basis of these points in common *Omorhamphus* may, perhaps, be referred tentatively to the order Diatrymæ, family Diatrimidæ as defined by Matthew and Granger.

Doctor Alexander Wetmore has kindly looked over a copy of my manuscript and has offered several suggestions which have been incorporated in the above text.

Department of Geology,
Princeton University,
Princeton, N. J.

The William Berryman Scott Research Fund.

A PROPOSED GUIDE-BOOK TO THE WORLD'S WEATHER AND CLIMATES

By ROBERT DeC. WARD

Harvard University

(Written for the Annual General Meeting, April, 1928, of the American Philosophical Society)

In a paper read before the American Philosophical Society April 24, 1925, attention was directed to the importance of simple, non-instrumental observations of weather and climate which any traveller who is interested in such "field-work," and who is something more than a mere "globe-trotter," is able to make.[1] It was there suggested that such "snap-shot" observations, which may well be termed "car window" or "automobile" climatology, add greatly to the interest of any journey. Furthermore, they are distinctly instructive, and often furnish a proper setting for much of the historical, economic, ethnographical and even literary background of the regions visited.

It is, of course, obvious that a student, and still more a teacher, of meteorology and climatology will profit most by such field-work. In fact, he should undertake it whenever and wherever possible. However well a teacher may know his text-books, he can never really appreciate other climatic conditions than his own unless he has had the opportunity of observing these conditions for himself. Only by this method can he have real confidence in what he is trying to impart to his classes. A geologist travels, often over great distances, in order to visit some typical rock formation, or a well-known volcano, or a famous mine. A botanist must go far afield from his laboratory to see some special plant form in its ideal development, or to study peculiar environmental

[1] R. DeC. Ward: "The Importance of Field-Work in the Study of Climates," *Proc. Amer. Philosoph. Soc.*, Vol. LXIV, No. 1, 1925, pp. 64–77.

effects. An astronomer must travel to other latitudes than his own if he is to see planets and stars not visible at home. Similarly the student of the earth's atmosphere must travel if he is to have first-hand knowledge of his subject. He will take advantage of every opportunity to travel: to see and to have personal experience with as many varied weather types and climates as he can. He will plan his journeys so that they will take him to localities where there is something of special and peculiar interest in the way of some particular type of meteorological phenomenon or some outstanding climatic feature.

A teacher of climatology, thoroughly in love with his science, will naturally have an almost irresistible desire, however impossible that desire may be of fulfilment, to spend a January at the Siberian "cold pole," where the lowest mid-winter temperatures (January mean, − 60° F.) and the lowest single temperature reading ever recorded under standard conditions on the earth's surface (below − 90° F.) have been observed. He probably just as earnestly desires to spend a July or August in Death Valley, California, in the "Heat Island" and the "Sunshine Center" of North America, because of the typical development of a desert climate there, and because that is the place of the maximum shade temperature ever recorded in the United States (134° F.). Cherrapunji, in the Khasia Hills of India, north of the Bay of Bengal, is a station to be visited in the height of the monsoon rains, when in a few months by far the greater portion of a mean annual precipitation of over 400 inches falls. This station long enjoyed the distinction of having the heaviest rainfall in the world: hence its attraction for the climatologist. Recently, however, the rain gauge on Mt. Waialeale (5,080 ft.) on the island of Kauai in the Hawaiian Islands, has indicated that the annual rainfall there is even greater than that at Cherrapunji. A visit to that mountain, with its steady trade wind rainfall, should therefore be made. India offers numberless attractions for a meteorologically-inclined visitor. The Cherrapunji rainfall is but one of them. Personal experience with the awe-inspiring "burst of the monsoon" at the beginning of the summer rains; the three-season climate of the northern provinces, with the characteristic cold-weather rains; the somewhat puzzling desert in the far northwest; the hot and dry season preceding the summer rains; the significance of the rainy season in the economic life of the people—these and many other features in the climates and weather of India combine to draw the climatologist thither. Africa suggests many interesting phenomena, personal observation of which would well repay the time and expense involved in a trip to that continent. A visit to Cape Town at the proper time for seeing the famous "Table Cloth" cloud on Table Mountain, first described and figured by Sir J. Herschel; a voyage to the equatorial west coast in order to observe the harmattan wind of the Gold Coast; and the violent so-called "tornadoes," moving westward offshore; a voyage across the Red Sea in summer, in order to feel the intense heat of that region—these are more or less haphazard suggestions for a meteorologist's African cruise. A voyage in the stormy prevailing westerly winds of the Southern Hemisphere, as far south as is possible, is as essential in giving a vivid impression of those remarkable winds and of their relation to navigation as is a voyage, preferably on a sailing ship, in emphasizing the importance of the steady trades in controlling sailing routes in the trade wind latitudes. It is no wonder that the popular notion about the word "trade" as applied to these winds is that it relates to commerce. In reality, it refers to their steadiness, to "blow trade" meaning to blow steadily (tread). Travel is well worth while in order to observe the ocean fog on the coast of California; the thick "pea soup" winter fogs of London; the Helm Cloud and the Helm Bar over the Cross Fell range in northwestern England; the Foehn wind of the northern slopes of the Swiss Alps in winter; the almost unimaginable uniformity of temperature in the equatorial islands of the Pacific Ocean; the phenomenally deep snows on the western slopes of the Sierra Nevada and the Cascade Mountains;

the peculiar combination of topographic and climatic conditions which brings glaciers down to the sea in Alaska, Norway, southern Chile and western New Zealand. Such a list may be extended almost indefinitely.

The interest in, and the advantages to be derived from such field-work are by no means the sole privilege of professional meteorologists or climatologists. Those who have even an elementary knowledge of atmospheric conditions may derive great benefit from similar observations, although they are not trained observers and may not be especially interested in the causes of the phenomena which they see. In more than thirty years of climatological teaching the writer has come in contact with hundreds of students, many of whom planned, after leaving College, to take extended journeys to "see the world," or to enter business in distant places. These young men, all of whom already had a certain interest in the study of weather and climate, rather naturally wished to know about any especially noteworthy meteorological phenomena, and also about the climates of the places which they intended to visit. In this way, through the years, the material for a sort of meteorological or climatological guide-book, as yet still unwritten, has gradually been collected. The desire of these students for this information before they started on their travels, and their later reports as to their greatly increased interest in their journeys because of it, have suggested the possibility of reaching a much larger group of persons, travellers of education and intelligence with no more than the most superficial knowledge of atmospheric conditions and laws. If the attention of such persons were directed to some of the more noteworthy of the phenomena which they might see on their journeys, their interest would not only almost certainly be greatly increased, but there would be a distinct educational gain in the general knowledge acquired concerning the countries visited.

The idea of preparing a guide-book to the world's weather and climates has thus gradually been taking shape. Such a project seems in no way foolish, impracticable, or futile.

It is highly educational in its purpose. A complete guide-book such as that here contemplated should include three aspects of the general subject with which it deals. It should give descriptions of characteristic weather types, as, *e.g.*, a typical day in the heart of the trade wind belt at sea; a winter spell of bright, sunny weather in the Alps; a cold wave in the eastern United States; a summer rainy spell on the Highlands of Scotland, and so on. It should next give simple but scientifically accurate descriptions of special local meteorological phenomena, such as the winter monsoon on the west coast of Japan; the "cloud drip" on the island of Ascension; the "brickfielder" or the "southerly burster" of Australia; the typhoons of the Eastern Seas. Thirdly, it should give vivid descriptive accounts of the climates to be met with in different parts of the world and their economic and general human relations, as, *e.g.*, the damp marine coast climate of Alaska, with its dense vegetation, its glaciers and its unsuitability for general agriculture; the desert climate of the "dead heart" of Australia, a great barren waste separating south and north, without hope of any general reclamation or development; the modified tropical climate on the plateau of East Africa, with its possibilities for future white settlement; the continental climate of Central Europe, neither as extreme as that of the northern interior of North America on the one hand nor as even-tempered and mild as that of the British Isles on the other. Obviously, so inclusive an outline, if completely filled in, would involve the preparation of a large volume, far too detailed to be of value to the ordinary traveller for whose use it is intended, however instructive and valuable it might be to the professional climatologist. The task of the writer would be to reduce his selection of weather types and of climates to a minimum, retaining those only which are the most representative, the most striking, and the most important. These would be sufficient for all ordinary purposes in the case of non-scientific travellers. Bibliographic references could be given for the use of those who might desire to secure additional information on any special topic.

It would seem advisable in such a book as that here proposed to classify the material geographically, by continents or by countries, or it could also be perfectly well arranged to follow the order of places visited on a "world cruise," or on a shorter cruise, as to the Mediterranean, or to Norway, or to the West Indies. A distinction could easily be made between the more and the less significant phenomena to be observed, as is done in many general guide books by "starring" or "double starring." If there is a special season or time of the year at which a certain condition or phenomenon is observable, or is best developed, that fact should be noted. The object of the volume is to provide a convenient descriptive account of the most interesting atmospheric conditions and phenomena, similar in a general way to the geological guides already familiar to many persons in the United States.

These are days of much travel. Many thousands of tourists are constantly moving about the world, over the old beaten tracks and to new and as yet relatively unfrequented places. A recent number of a popular monthly magazine contained advertisements of innumerable personally conducted and independent trips to Europe; two or three "round the world" cruises; four Mediterranean cruises; tours to the West Indies; to South America; to the Orient; to the Hawaiian Islands; to Alaska; to Australia and New Zealand; motor trips in North Africa, and so on. The longer tours and cruises are obviously planned so that most or all of the climatically less attractive places are omitted. In addition, there were attractive notices of tours and trips, including "land cruises," all over the United States. Few travellers nowadays start off on any considerable journey without preparing themselves beforehand, by reading about the places which they are to visit. The elaborate illustrated guide-books prepared for most of the extended cruises contain a considerable amount of information, and also give excellent and carefully selected lists of books to be read before or during the trip. These books cover a wide range of subjects: historical, literary, artistic, archæological. Further it is the custom, on personally conducted cruises, to provide lectures for the passengers in preparation for visits to the different countries or cities. So far, however, no serious attempt, if indeed any attempt at all, has been made to furnish any accurate information, beyond general directions as to the kind of clothing to be worn, concerning the weather and climate of the places visited, nor has attention been called to outstanding meteorological phenomena to be looked for. It would seem scientifically as well as educationally desirable to provide this information by means of such a guide-book as the writer has in mind. Almost any tourist would surely find added interest in such information. His eyes would be opened to many conditions which now escape him. Would not a round-the-world traveller find more enjoyment on his trip if he knew something about the causes and characteristics of the prevailing westerlies, and the trades, and the doldrums? Would he not find it worthwhile to be on the look-out for a norther, or the monsoon, or even a tropical cyclone? Would he not gain added information concerning the life, and habits, and occupations of the people in strange lands if he knew in advance something about the climates of those countries, and the effects of those climates upon man and his work?

To attempt to give even an outline of such a guide-book for any considerable part of the earth's surface would extend the present paper far beyond any reasonable limits. A few brief suggestions may, however, be made concerning three regions which are nowadays visited by increasing numbers of tourists: the West Indies, South America and the Mediterranean. In a complete guide-book these suggestions would naturally be greatly extended and much more fully discussed.

Islands in the trade winds have long been known as possessing peculiar attractions of weather and climate. In fact, they are among the most favored parts of the Tropics as a resort for travellers, and also as places of permanent residence for those from colder climates. The West Indies, near at hand and easily reached by several regular steamship lines, as well as covered by numerous winter cruises, have naturally, and deservedly, become increasingly popular among American tourists. With their numerous islands—mountainous, volcanic and limestone; their variety of topography and of scenery; their tropical fruits and crops, it is inevitable that the West Indies should attract hundreds of persons who want to escape, for two or three weeks or more, from the rigors of the northern winters, and bask for a while in the bright sunshine of the Tropics. Blown over throughout the year by the steady and stimulating trades; with the average temperature of the coldest month (near sea-level) between 75° and less than 80° at most places; with the lowest single thermometer readings falling between 50° and 60° in the northern islands and 60°–65° elsewhere, and with prevailingly bright, sunny days, the popularity of these islands as a tourist winter resort is easily understood. The seductive advertising of the steamship companies and travel agencies, setting forth in glowing terms the climatic advantages of the West Indies trip in winter, is hardly, if at all, overdone. Here is found a typically tropical climate, but one modified by oceanic controls. The remarkable steadiness of the trades, both in direction and in velocity—so steady that the Windward and Leeward Islands are appropriate names—and the uniformity in the weather day after day, surprises those who experience such a climate for the first time. They soon realize why those who live in the Tropics do not make "the weather" a persistent topic of conversation as do people who live in latitudes where weather changes are frequent and often violent. It is soon seen why the Trades may be called the "yachting winds" of the world, and why it is that great nations of hardy deep-sea seamen were not developed in latitudes where sailing is for the most part easy, and violent storms are rare. Sturdy adventurous pioneers there have been among the Mediterranean peoples, but it took the stormy seas of the higher latitudes to breed the traditional seamen. "Perfect" climates do not exist. Perhaps trade wind island climates come as near climatic perfection as it is possible to come. The attractions of blue sea and white caps, and of blue sky and white cumulus clouds, have often been described, but cannot fail to charm any but the most unimpressionable and hardened traveller. "Outs" there are, as is everywhere the case. Some tourists will find the temperatures too high, the sun uncomfortably hot, and the glare very trying. Others will be surprised to find that rain falls at times. Others, again, will doubtless complain that the repetition of the same weather types becomes monotonous, or that a "norther" in the westernmost islands of the Greater Antilles is disagreeable and chilly.

The West Indies are by no means rainless. The seasons are "dry" and "wet," not hot and cold. The dry season is the winter; the wet, the summer. As is typical of trade wind islands, the rainfall is heavier on the windward mountain slopes, while the leeward slopes and the interiors are much drier. The contrasts are very marked, even within short distances, and can easily be noted in the character of the vegetation and in the types of agriculture. Wet tropical forests may be seen to clothe rainy windward slopes while grasslands are found in the drier interiors. While San Juan, P. R., has slightly over 60 inches of rain a year, Ponce, to leeward, has about 35 inches. Port Antonio, Jamaica, has over 130 inches; Kingston has nearly 100 inches less. The late winter and early spring are on the whole the driest season, and therefore winter visits to the West Indies are the most desirable. On the windward slopes of the mountains, however, rain falls in winter as well as in summer.

The summers in the West Indies are by no means unendurably hot, the seasonal variation in temperature being very slight. The mean temperature of the warmest month at places near sea-level averages generally around 81° or 82°; the maxima reach 95° to 100°, or locally and occasionally a little higher. Relief from the heat of the lower levels may be found on the slopes and uplands of the mountainous islands.

Hurricanes are limited to the late summer and autumn, but they are relatively infrequent and the southern islands are free from them. Occasionally these disturbances may occur in early or in mid-summer. Apart from such tropical storms, gales are rare, especially in the south. Travellers will not be inconvenienced by ocean fogs, and those who dread thunderstorms will have almost or complete immunity from them in the winter months.

Many West Indies cruises include the Isthmus of Panama, and some now take in Vera Cruz, with an excursion to Mexico City. On the Isthmus, travellers will experience a climate which is, to most persons, far more like what they expect in the Tropics than is the climate of the West Indies. The Isthmus has "equatorial" conditions under the control of the migrating belt of clouds and rains, as well as trades for part of the year. The rainfall is heavy, and it is easy to appreciate the difficulties that were involved in the control of the rainy season floods in connection with the operation of the Canal. In Mexico, the contrast between the hot, damp, forested lowlands of the *tierra caliente*, with their tropical products, and the drier interior plateaus of the *tierra templada* or *tierra fria*, with much less rainfall and greater extremes of temperature, is well worth seeing. The replacement of sugar, and cocoa, and rice, and cotton, by coffee and corn and then by wheat and other "temperate" zone products, is a lesson in the climatic effects of altitude easily learned on a trip from the coast to the Mexican plateau.

South America offers much of interest to the meteorologically-inclined traveller. No better latitudinal cross-section of the atmosphere can be obtained than that which a voyage around that continent affords. A steamer trip from New York to Rio de Janeiro, for example, passes through two or three days of the variable winds and weather of the "prevailing westerlies." Then follow, in succession, the "horse latitudes"; the northeast trades; about a day crossing the hot, steamy doldrums; another few days in the southeast trades; then again the "horse latitudes." Rio de Janeiro, lying close upon the Tropic of Capricorn, is under the control of the tropical high pressure belt of the South Atlantic during much of the year, and has very equable climatic conditions. The mean temperatures of the warmest and coldest months differ only 10°. Rain falls in every month. In its midsummer the hot and muggy weather of Rio is not unlike that of our Gulf States in July. In winter its mean temperature is about that of Miami, Florida. Travellers who sojourn in Rio in winter (June-August) will usually find it somewhat too warm for exercise in the middle of the day, but pleasantly cool after sunset, so that a light overcoat is not uncomfortable in the evening. Not far from the capital, across the famous Bay, there is a charming little spot, Petropolis, lying up among the Organ Mountains about 2,700 feet above sea level. In the old days of yellow fever Petropolis owed its popularity chiefly to the fact that it was above the zone of yellow fever and malaria, and became the permanent residence of foreign ministers and consuls, as well as of many of the wealthier business men of Rio. Its cool evenings and nights, and its location above the low-lying fogs common over the city and harbor at night, give it distinct climatic advantages. The sail across the Bay on the late afternoon boat from Rio, and the hour's ride up the cog-wheel railroad to Petropolis, make a delightful ending to a day in the hot and noisy streets of the city. Theresopolis is another attractive resort in the mountains not far from Petropolis. It is not generally known that the undesirability of Rio de Janeiro, owing then to its yellow fever epidemics, led to a plan for moving the seat of government to a more central and more elevated location in the interior state of Goyaz. Indeed, in the Federal Constitution adopted when the Republic replaced the Empire, there is an article which says that the capital shall be moved to Goyaz. This change has never been, and doubtless never will be made.

No traveller should miss a trip to the plateau, in order to experience the difference in climate which results from an ascent of 2,000–3,000 feet above sea level. Cooler and less humid than the sea-coast, the plateau offers more desirable and healthier conditions for Europeans, and it is natural that they have so largely settled there. The Southern Hemisphere winter is the best season for travellers. The plateau then has prevailingly fine weather. The early mornings and evenings are cool, crisp and exhilarating. The noon and early afternoon hours are warm. Indeed, in the strong sunshine it may even be uncomfortably hot. Winter is the "dry" season, although rain is not wholly absent, especially towards the south, and comes in thundershowers or in general storms. The city of São Paulo (2,430 feet) is easily reached from Rio by a day or night railroad journey. A still more instructive trip, if sharp climatic contrasts are desired, is to take the afternoon train from Santos over the famous São Paulo Railway. No one, not even the least observing traveller, can fail to be impressed by the change from the hot, steamy, depressing atmosphere of the world's most famous coffee port and the refreshing cool upper slopes and crests of the coast mountains, across which the railroad winds. The heavy rainfall on the seaward slopes of these mountains has necessitated some most remarkable masonry construction in order to protect the railway from damage by the rains. The whole face of the slope is almost literally covered with masonry, while numberless channels, all solidly built of stone, are provided to carry off the water safely.

The city of São Paulo itself, a busy modern metropolis with a large European population, is generally highly recommended as a place of residence. Its mean annual temperature is just under 65°; its warmest month has a mean temperature of between 71° and 72°; its coolest has between 57° and 58°. Thus, with only moderate heat in summer and no severe cold in winter, this is one of the many places in South America which are described as having a "perpetual spring." One enthusiastic writer has said, "São Paulo has all the charms of a tropical sky without any disagreeable heat."

The famous Brazilian coffee district may easily be reached in a day's railroad trip northward from São Paulo. Climate has been one of the most decisive factors in making a relatively small district of Brazil the most important center of the world's coffee crop. The conditions are singularly favorable. The rainfall is sufficient without being so excessive as to promote a rank growth of weeds. Most of it falls in the warmer months when the coffee is making its growth, and ripening. The dry season, in the cooler months, is of great economic importance in the harvesting and outdoor drying of the beans. The heat is not so intense as to necessitate protection for any but the very young trees. Frosts occur rarely, and very seldom do any considerable damage. The view of those miles and miles of coffee trees, reaching out of sight in the distance across the undulating plateau, is one which is worth a long journey to see.

Buenos Aires has much in common, meteorologically, with parts of the Mediterranean. The temperatures of its coolest and warmest months (50°; 73°–74°) differ by less than 25°. This is more than twice the seasonal range at Rio. In the United States we have approximately similar ranges in parts of the Gulf and southern Pacific coasts. Rome has colder winters (January mean about 44°) and slightly warmer summers (July mean over 76°) than Buenos Aires. Palermo has mid-winter temperatures very closely those of Buenos Aires, but also has warmer summers. Naples is slightly colder in winter and warmer in summer. These differences in mean monthly temperatures are, however, too small to be very distinctive. In mid-winter, Buenos Aires is about equivalent to New Orleans as regards its mean temperature. Two contrasted weather types are well known in Buenos Aires. One, the *norte*, corresponds to the warm spells with muggy southerly winds in winter in the eastern United States. The *norte* is a warm, damp northerly wind, the indraft from more northern latitudes into a general storm passing eastward over Argentina. It is commonly accompanied by warm rain, and most people find it depressing and disagreeable. The contrasted type is associated with a southwesterly wind. This, called the *pampero*, blowing across the great expanse of the pampas, corresponds roughly with the northwest wind in the eastern United States. It is cool and bracing, and has a stimulating effect. The reported exclamation of the earliest arrivals in that region, "que buenos aires son estos," must have referred to the conditions that prevail with a southerly wind. The name Buenos Aires perpetuates, as is not infrequently the case with geographical names, a climatic feature of the region in which the place is situated.

The journey across the pampas from Buenos Aires westward, and then by the Trans-Andine Railroad to Valparaiso, gives the traveller an excellent east-west climatic cross-section. This trip takes one from the section of sufficient rainfall near the Atlantic Coast, through the broad fields of a rich farming country and of abundant pasturage for thousands of cattle, on to sections with progressively less and less rainfall as the distance from the sea increases. Along the eastern base of the Cordillera irrigation is necessary, in the "rain-shadow" on the lee side of that massive mountain barrier. A winter trip from Argentina to Chile should give the tourist an opportunity to see the deep snows on the mountains which, before the Uspallata tunnel was put through, used so often to delay passengers and mails carried on horses or mules across the Uspallata Pass. Then, on the western slopes, on the Chilean side, the increasing rainfall which comes from the Pacific is easily inferred from the vegetation and the general appearance of the country.

Nowadays, when the Trans-Andine Railroad marks the southernmost latitude reached by the usual traveller, the South American tourist no longer has the opportunity to see the remarkable series of climatic pictures which a voyage along the west coast, from the Strait of Magellan to Panama, affords. There is climatically no more instructive a voyage than this in the world. It begins in the districts of very heavy rainfall in southern Chile, where dense, dripping forests clothe the mountains; where the sparse population is collected in a few places on the immediate sea coast, and where lumbering is a natural occupation. Farther north, where the rainfall is less, lies the agricultural zone of Chile, with its abundance of Mediterranean crops and fruits, and with much in the scenery and products to recall parts of southern California. Valparaiso, which lies about at the northern limit of the belt over which sufficient rain falls, has between 20 and 25 inches a year. This comes almost wholly during the winter months, with generally cool, but not cold, weather and spells of clouds and rain. In summer, Valparaiso is dry; the hills become brown or grey, and irrigation is necessary over the agricultural sections of the great valley of Chile. It is in summer that the sea breeze, so graphically described by Lieutenant Matthew Fontaine Maury, U. S. N., many years ago, blows so strongly at Valparaiso that it raises much dust, and people are tempted to stay indoors during the hours of its greatest velocity.

The remarkable desert belt which begins a short distance north of Valparaiso, and extends to within 3° or 4° south of the equator, is one of the most interesting regions in the world, climatologically, geologically and historically. It was in this coastal strip that many of the scenes of the Spanish Conquest were laid. In the southern portion of the desert lie the rich nitrate fields of northern Chile, whose development has played so important a part in the financial history of the world, and has led to serious political disagreements between Chile and Peru. The nitrate is there because the desert is there.

Steaming north from Valparaiso the successive ports of call clearly indicate, in the increasingly barren aspect of the country, the rapidly diminishing rainfall. Trees become fewer and fewer. There is less and less green. Towns with a rainfall of 10 inches a year; then 5 inches; then less than 2 inches, and finally with less than half an inch, are passed in succession. The extreme desert is seen at the well-known nitrate ports themselves. The coast all along presents the same feature of brown and barren hills and yellow sand. The monotony is relieved only where irrigation makes possible the growth of some vegetation. The water for the coast

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settlements is in many cases brought in pipes from a distance of many miles inland, across a perfectly barren desert where nothing grows except where water is supplied from the pipes, either at regular supply stations or through leakage.

In the south, the transition from the rainy coast to the desert is gradual. In the north, it is astonishingly sudden. Just north of Payta, Peru, at Point Parina, there comes so sudden a change in climate that no traveller should fail to notice it. At Payta are seen the same bare hills and sandy stretches that have become monotonously familiar during the voyage up the coast from Caldera (Chile). A short run north from Payta carries the ship from southerly winds, fair weather, relatively low temperatures and almost complete aridity into the equatorial belt of muggy "hot house" air, with heavily overcast skies and frequent showers. Not less striking than the change in atmospheric conditions is the change in the aspect of the country. Instead of the barren desert the shores are heavily wooded. Everything betokens the change from the régime of the cold Humboldt Current, with its accompanying cool, non-rainy southerly winds, to the control of the warm equatorial ocean waters and of the equatorial rain and cloud belt. The remainder of the voyage to Panama is continued under generally similar conditions, and on the final crossing of the Isthmus, whether by train or through the Canal, the dense tropical vegetation is a sufficient indication of the heavy rainfalls, while the temperatures and the mugginess of the air give a true taste of typical equatorial climate.

Three side trips are suggested during the voyage up the west coast. At Arequipa, in southern Peru (lat. 16° 22 S., long. 71° 15 W., altitude 8,050 feet), about at the center of the desert belt, and somewhat less than 100 miles from the coast, the Southern Station of the Harvard College Observatory was for some thirty years maintained. The favorable meteorological conditions of prevailingly fair skies, little rainfall and clear dry air, led to the choice of this as the then best available site for an astronomical observatory in the southern hemisphere. Very recently, this observatory has been removed to an even more favorable location in South Africa.

A journey should, if possible, be made to Bolivia, including Lake Titicaca and La Paz. The Bolivian plateau, the largest of the Andean plateaus (12,000–13,000 feet above sea level), has a climate quite different from that at sea level. The daily ranges of temperature are large; the noon hours in the sun may be very hot; the nights are cold or even frosty. Bleak and inhospitable these high plateaus surely are, with high winds and at certain times of the year violent squalls and storms. The cultivated portions are the *punas;* the higher and bleaker parts are the *paramos.* Below the main plateau level (9,000–11,000 feet above sea level) cereals are cultivated.

Finally, a very striking lesson in the effect of elevation upon climate may be learned by anyone who takes a trip over the famous Oroya Railway, from sea level at Callao or Lima to Oroya (about 12,000 feet), passing on the way, at the Galera Tunnel, a height of over 15,500 feet above sea level. The early portion of the trip is through fields of sugar cane and cotton. At about a mile above sea level a zone of fruit trees is passed through. At about two miles there is a famous potato district. Still higher up nothing but grass is seen. At the highest point reached, snow lies on the mountain summits, and a snowstorm may be enjoyed in summer. In the Oroya Valley, farm produce is again met with. This whole succession of climates is passed through during one day's journey—an opportunity which no traveller who wishes to learn striking lessons in climate should fail to embrace.

Many things appeal to the traveller who visits the countries bordering on the Mediterranean. Here great civilizations arose, and decayed. Here are the lands of the olive and the vine; the famous health and pleasure resorts of the Riviera and of northern Africa; the temples and the art of Greece, and of Italy, and of Egypt. The charms of sunny climates, of blue waters, of Italian skies, of freedom from severe cold, doubtless constitute one of the strong appeals to winter visitors from more rigorous and less hospitable northern climes. Wherever the Mediterranean type of climate is found, as in our own southern California, it is unfailingly awarded enthusiastic praise.

Far enough from the equator to be exempt from continual heat, yet near enough to it to be spared extreme cold, the transitional belt of Mediterranean latitudes is among the most favored regions of the world. Here countless invalids have sought and found health, and many thousands of temporary sojourners have enjoyed outdoor life in the midst of beautiful natural scenery. With generally fair skies, even temperatures and moderate rainfalls, these latitudes are singularly fitted to serve as health resorts. The belt of so-called "Mediterranean climates" embraces the countries bordering on the Mediterranean in northern Europe and northern Africa, and follows that Sea to its eastern limits. Because of the great irregularity of topography and of outline, the Mediterranean climatic province embraces many varieties of local climates, yet the dominant characteristics are essentially the same everywhere.

The Mediterranean basin is singularly favored in winter. It lies to leeward of the Atlantic; for much of its extent its mountain barriers protect it against invasions of cold from the north; the waters of the Sea itself are warm. The coldest month generally has a temperature well above 32°; over much of the area it is somewhat over 50°. The lowest temperatures run only a few degrees below freezing (20°–30°) in the more favored northern districts, although the more exposed lands in the eastern part of the basin have greater cold. Hence the popular health resorts are in protected locations along the northern shores, as on the Riviera; in the southern Alpine valleys, and in southern Spain, or they are well to the southward, where the temperatures are naturally higher, as in Sicily and in northern Africa. The summers are warm, or even at times unpleasantly hot. July mean temperatures are somewhat over 70°; in a few localities over 80°. Autumn is, as a rule, warmer than spring. Readings of 90° to 95°, or more occur in hot spells in northern districts, and they run to still higher points farther south. Everywhere the heat of summer increases inland, away from the sea. Such conditions are much more extreme than those of the California coast, and more nearly resemble those of the interior Sacramento-San Joaquin valley. In winter, passing storms frequently cause variable and shifting winds. These temporarily import "unseasonably" low or high temperatures, from the north or the south, into the otherwise uniformly mild Mediterranean area. Most travellers who visit these countries for the first time in winter are unpleasantly surprised to find many uncomfortably chilly days, especially in the eastern portion of the district. Such spells are perfectly normal incursions of cold air from the well-chilled lands to the north, or are cool because the skies are overcast, and damp or rainy winds blow from the sea. In summer, the prevailing winds are northerly, and over the eastern portion of the belt often develop such high velocities that on the Aegean islands the orchards are protected by rows of cypress trees which serve as barriers. These northerly winds, beginning in spring, continue during the summer. As the warmer months are the dry season, these winds are often disagreeably dusty. These are none other than the famous Etesian winds of the Greeks. So strong are they at times in the Aegean Sea that navigation for small craft becomes dangerous. Greek history gives numerous examples of the effects of the Etesian winds. While they facilitated the transport of food supplies from Black Sea ports to Greece, they were a distinct handicap when the Athenian fleet attempted to sail north. On the other hand, history relates that Philip of Macedonia found them helpful on his voyages to the southward.

Winter rains, generally moderate, are typical of the coasts and islands. The larger land areas have a tendency to a spring and autumn maximum. This winter rainfall, while generally exceeding in its total amount that over most of northern Europe in the same season, characteristically falls in larger volume at a time, but on fewer days. The spells of stormy weather usually do not last long. They are interpolations in more extended intervals of fine, sunny weather. Most of the well-known places visited by tourists have annual rainfalls between 25 and 30 or 35 inches, amounts similar to those over a considerable section of the Central United States. In summer, when northerly winds are common and when general cyclonic storms are few, dry and much more continuous fine weather is the rule. In the south, the warmer months are almost, or even wholly, rainless. In the north, the rainfall is more evenly distributed through the year. Thus, while Alexandria has 8 "dry" months and Tripoli, 7, Malta has 4 to 5; Sicily, 4 to 4½; Greece, 4; Naples, 3; Rome, 2; Florence, no "dry" month. In Malta and Sicily only 2–3 per cent of the year's rainfall comes in summer. In southern Italy summer has a little over 10 per cent. But in the north, the valley of the Po has even more rain in summer than in winter. The higher elevations naturally have more rain in summer than the lowlands, and the rivers supplied from these local areas of heavier rainfall are of importance in supplying water for irrigation during the dry season.

There is real reason for the tradition regarding "sunny Italian skies." The Mediterranean belt is numbered among the least cloudy climates in the world. Winter has the least sunshine, but the storms of that season do not bring the long overcast spells so familiar in northern Europe. It has indeed been well said that the problem of securing the most rainfall with the greatest possible number of clear days has been solved on the southern slopes of the Alps. The amount of cloud decreases markedly to the south. Northern Italy averages 50–60 per cent of cloudiness in winter and 30–40 per cent in summer. Cairo has an annual mean of 20 per cent; with less than 10 per cent in June. Mediterranean air has the reputation of being very clear. This is true in winter. In summer, on the other hand, when the dry season is at its height, there is often so much dust in the air that it becomes hazy, and distant views become faint or are obscured.

Most travellers who visit the Mediterranean in winter for the first time will doubtless be surprised as well as disappointed to encounter occasional storms, with high winds and rough seas, and, especially in the eastern parts of the area, chilling cold and even snow. Such conditions seem abnormal where blue skies, balmy air, and smooth seas are proverbial. Yet, as already observed, winter storms and occasional low temperatures are "normal" in these latitudes, and while snow is very rare in southernmost Italy and Greece, central Italy averages about four days with snowfall; Rome, one to two; Athens, about six. These snows melt quickly and become less and less frequent the farther south one goes. Over eastern sections, which are more exposed to invasions of cold from the north, snow falls oftener, and is heavier. It so happens that while this paper is being written, European cable despatches in the daily papers have noted severe weather in portions of the Mediterranean.[1]

However surprising such conditions may appear to the ordinary traveller or reader, to the climatologist they are interesting examples of weather types which are not abnormal, although in their most extreme manifestations they may occur only at long intervals and may, at any time, "break the record," as to degree of cold or depth of snowfall, which happens to have been kept during the comparatively short period of accurate observation.

There are certain characteristics of the vegetation in the

[1] London, Feb. 27 (by Associated Press): "Several parts of southern Europe and Asia Minor were today in the grip of semi-Arctic weather. A severe storm swept Turkey, and showed no signs of abating. . . . Land and sea communications were disrupted. . . . Jerusalem was under a deep snow. The island of Malta was swept by a fierce and chilly gale, and mail boats were held up." Constantinople, Feb. 28 (A. P.): "Constantinople today was under snow for the seventh successive day, with the temperature at 27° (F.). The Golden Horn froze last night for the first time in 25 years. . . . Smyrna experienced a violent earthquake of short character, which did no damage, in the midst of a heavy blizzard yesterday morning." Madrid, Feb. 28 (A. P.): "All Spain is suffering from heavy wind and rain storms. . . . In Valencia, open air festivities which were under way have been suspended. . . . Trains arriving in Barcelona are behind schedule, and snow has been falling over the Province of Catalonia." Personal correspondence (Feb. 1928) mentions snow in Palma; "very cold weather" in Athens, with recent snowfall, the mountains being still covered with snow; a snow covering, with disagreeable cold in Constantinople (Feb. 25).

Mediterranean basin which, if not previously observed, may well attract a traveller's attention. Thick stems and bark, and small leaves, with smooth waxy surfaces, are protective devices against evaporation in the dry summers. The live oak, olive, mulberry and vine are typical and widespread. The raisins of Spain and the currants of Greece are good examples of the commercial use of a dry and sunny climate. Thorny evergreen scrub is common. Figs and dates come from the eastern end of the Mediterranean. On the higher and wetter slopes chestnut and walnut trees are at home. The summers are generally too dry for wild flowers. In the north, the cool winters check plant growth, while in the south it is warm enough for plants to remain active through the winter months.

So many different places are visited by tourists in the Mediterranean area that it is obviously impossible to attempt any detailed or comprehensive meteorological guide-book. Note is here made concerning a few items only.

A stop at Gibraltar and a visit to Seville and Granada, are usually part of a winter Mediterranean cruise. Most of Spain is semi-arid; its natural vegetation is adapted to a dry climate, and irrigation is necessary in order to secure crops. The "garden spots" are in the fertile irrigated districts of the southern provinces. This coast, especially between Gibraltar and Almeria, sheltered by mountains on the north, has a wonderfully mild, almost tropical, winter climate. Granada, "made famous forever by the natural beauty of the neighboring Vega," has the added glory of the snow-capped Sierra Nevada. From the melting snows on these mountains came most of the water used by the Moors in those wonderful irrigation works of theirs which made this whole district blossom as a rose. Granada's January has a temperature between 40° and 45°. Seville is somewhat warmer (about 50°). The lowest readings are about, or slightly below, freezing. Except on the mountains and on the central plateau, snow is rarely seen. Over sections of the southern coast it is reported as "not known." The rainfall, which is small, comes in winter. Spanish summers are too warm for comfort in the south and on the central plateau. Heat, dryness and dust combine to make travel uncomfortable there. Seville's mid-summer mean temperature is over 80°. Granada is only a few degrees behind. Cordoba is known as "the frying pan of Andalusia." A wind known as the *leveche* is an unpleasant characteristic of portions of the southeastern coast. This is similar to the *sirocco* of Italy. It is dry, hot, and dusty. Its direction is between S.E. and S.W. During a *leveche* it is pleasanter to remain indoors than to go out. A similar wind, the *leste*, occasionally blows from between N.E. and S.E. in winter, spring and autumn on the island of Madeira. This is also very dry, and carries a fine red dust from the African deserts. The *levanter*, an easterly wind at Gibraltar, is damp and cool. In general, easterly winds on the southern coast of Spain are associated with stormy weather.

In southern France we have that "azure coast where a vegetation of almost tropical character thrives on the sun-glistening slopes of the southern Alps"—a favored strip, sheltered from the cold north winds, and turned towards the sun and the blue waters of the Mediterranean. No wonder that the famous Riviera, extending eastward into Italy, is a favorite winter resort. The charms of its scenery; the wealth of its flowers; the abundance of its sunshine; the mildness of its winters; the many comforts provided in its hotels and villas; the variety of its amusements—all these combine to attract increasing thousands of travellers and of winter residents. Yet even this favored coast is not without its drawbacks. Anyone who has been on the Riviera during a drenching winter rain will wonder how that garden spot ever gained its reputation as a desirable pleasure resort. Travellers who visit Marseilles and the lower Rhone valley in winter will probably have an opportunity of experiencing a *mistral*, famous as one of the scourges of Provence. This violent cold northerly wind is so frequent that the trees are permanently bent towards the southeast; high walls or wind-breaks of cypresses protect houses and gardens, and people and even vehicles have been blown over.

Violent winds are so common in Malta, which is visited on many winter cruises, that the gardens there are also surrounded with high walls, and from a distance the island looks as if it were a mass of stone quarries, to use the description given by another.

Surrounded by warm waters, and protected by its northern mountain barriers, Italy enjoys many climatic advantages. South of the Apennines the temperature seldom falls to the freezing point, and it never goes far below 32°. Winter storms, with chilly winds and clouds and rain cross it, especially the northern and central portions, and snow occasionally falls on the lowlands even as far south as Sicily. Yet on the whole the winters are mild and the sunshine is abundant. Travellers should expect spells of cool weather and rains in winter. January in Rome averages about 45°. Naples is slightly warmer and its latitude ensures it somewhat less stormy winters than are found farther north. The climate there, as one writer has expressed it, is "typical of the south at its best." In the north, the Po lowland has winters not unlike those of Central Europe. In the east, as at Venice, winds from the Adriatic have freer access and temper the winters somewhat. Sicily in January is not unlike May in England. It is milder than the adjacent mainland, and has more settled weather than do the northern latitudes. The fertility of the lowlands made the island one of the granaries of the Roman Empire.

Italian summers in the south are on the whole too hot, and dry, and dusty to be agreeable. Yet cloudiness is then least, the winter storms are over, and the chill of the colder season is no longer to be dreaded. Winter travellers in Italy will very likely experience a *sirocco*, a warm southerly wind, usually muggy, cloudy and rainy. It is very much like the corresponding southerly wind in the eastern United States, which here blows northward from the warm waters to the south. In southern Italy and in Sicily a hot, dry and dusty *sirocco* is well known, both disagreeable and oppressive. This is most common in spring. Palermo averages 12 days a year of *sirocco*. The "dry" *sirocco*, blowing from between S.E. and S.W., either brings no rain, or perhaps a few drops or even a short downpour. On the peninsula it is often followed by a cooler and bracing northerly wind, in the rear of a retreating storm.

The western coasts of the Balkan peninsula have a warmer, and rainier climate than that of the opposite Italian coasts, because of the greater prevalence of moist winds from the sea in the former case. The mountainous rocky shores, indented with numerous fjords and islands, are favorable for heavy rains, especially in winter, and luxuriant vegetation adds greatly to the natural beauty of the scenery. The rainfall averages over 40 inches, and locally exceeds 80 or even 100 inches. On the mountains back of Cattaro, a port of call on some Mediterranean cruises, the mean annual rainfall exceeds 180 inches—one of the heaviest in Europe. This fact should add to the interest of a visit to the beautiful Bay of Cattaro. Corfu, off the southern coast, with a tempered climate, is somewhat of a winter resort. A winter characteristic of the coast from Trieste southwards towards Albania, especially in Dalmatia, is the *bora*, a local wind which perpetuates the old name Boreas. This is a dry, cold, squally, northeasterly wind which blows down from the elevated interior. It is most violent in winter, and may rage for days at a time. This cold easterly *bora* is in striking contrast with the warm southerly *sirocco*. The eastern portion of the Balkan peninsula has little protection against invasions of winter cold from the north, and therefore has a much more severe climate than the west coast.

Greece, with its Mediterranean climate, has mild, rainy winters, and hot, nearly rainless summers. Athens has a January mean of between 45° and 50°, with a July mean over 80°. The thermometer usually falls below freezing a few times each winter, Plutarch wrote of frost occurring in April, and a short, wet snowstorm is by no means an un-heard-of greeting for winter or spring tourists. Snow comes with chilly northerly winds, but does not stay on the ground at low levels. The mountains, on the other hand, have a good deal more snow, and naturally keep it longer. The weather is changeable in the colder months. Southerly and southwesterly winds are often squally and bring rain, which, however, usually lasts only a few hours and is followed by fair, warm, sunny days. The winters of eastern Greece are colder, and the summers are warmer, than those of Italy in the same latitudes.

The tourist who extends his journey as far east as Constantinople, passing the Gallipoli Peninsula, may well recall the sufferings of the English and Anzac troops there during the Great War, first from heat and lack of water in the dry summer, and later from the cold, and the rains, and the snow.

Constantinople is far enough east to have considerable winter cold, in spite of its location with regard to neighboring bodies of water. Reference has previously been made to the snows of February, 1928. The writer has a photograph of the city taken in 1919, with the Allied fleet at anchor in the adjacent waters, which shows the roofs of the houses covered with snow. It is historically on record that the Bosphorus has been covered with ice, so that passage between Europe and Asia was made on foot. In this case it is probable that the ice had drifted down from the Black Sea; had become compacted in the narrow channel, and had then frozen.

The coast and the hill country of Syria have a climate more or less similar to that of southern Italy. The coast has January mean temperatures arranging between 50° and 55°, while on the hills the means run somewhat below 50°. The northern mountains have heavy snows. Even Jerusalem is not so far south as to be exempt. Thus in February, 1920, 40 inches of snow were reported as having fallen on the level, with drifts 10 feet or more deep. In February, 1928, as previously noted, a considerable snowfall occurred. The rainfall is most abundant on the Mediterranean slopes, where grapes, olives and cereals are raised. Smyrna figs are well known in the world's markets. Back from the coast, the Jordan Valley has a climate whose distinguishing features are its heat and aridity. Travellers in the Holy Land, observing the relatively small rainfall, may perhaps ask themselves how large a population this region can probably support, and may wonder whether many of the highly optimistic predictions regarding the agricultural future of Palestine are really within the bounds of climatic possibility.

The north coast of Africa belongs to the Mediterranean type of climate. The Atlas Mountains serve as an effective barrier to the penetration of moist northerly winds into the desert. The fact that winds from northerly directions must blow across the warm Mediterranean before they reach Africa ensures the north coast of Africa a milder winter than that found in most parts of the northern shores of that sea. Winter storms, with their spells of cool and rainy weather, are not lacking even here, but they are relatively few, and generally not severe. Frost and snow come at rare intervals at the lower levels. At the higher elevations it is colder, and snow is abundant, lasting well into the spring or even summer on the highest peaks. Of Algiers, as of many other places, it is often said that here is "the finest climate in the Mediterranean." With sufficient water supplied by the winter rains, supplemented by irrigation, abundant crops are raised on the fertile lowlands and in the valleys. The olive and the vine are abundant, and the cedar forests appeal to the traveller. In Roman days agriculture was far more generally practised in certain parts of North Africa, and the ruins of ancient irrigation works indicate a more effective use of the available water supply. Biskra, at the southern foot of the mountains, on the northern margin of the Sahara, is now visited by many tourists. Here the rainfall is about 8 inches; and there are more than 250 clear days a year.

Few tourists miss Egypt. The Nile valley early became the seat of a great civilization. The Nile floods, fed by the spring rains on the Abyssinian highlands, were in ancient times, as they have been ever since, the key to the very existence of Egypt. Here is a true desert climate. The rainfall decreases to the south, from about 8 inches at Alexandria to between 1 and 2 inches at Cairo. This rain falls during the occasional winter storms which cross the Mediterranean and bring variable winds, clouds and light rainfall. Cairo is far enough inland to be almost beyond reach of these disturbances. Still farther up the Nile they no longer have any control over the weather. With excellent hotels; a mild, dry, sunny climate, and a wealth of interesting things to see, Cairo is naturally a favorite winter resort and a center for Nile tours. Winter temperatures occasionally fall to, or very slightly below freezing. Visitors to Egypt in late winter and spring may observe the famous *Khamsin*, a strong, hot, dry, sand-laden southerly wind, blowing down the Nile valley towards the Mediterranean.

Thus, in as brief an outline as is consistent with a desire to make it useful, a portion of a proposed guide-book to the world's weather and climates is now presented, as a suggestion towards making travel more interesting and more instructive.

CAN BUSINESS BE MADE A SCIENCE?

By EMORY R. JOHNSON

Dean of the Wharton School of Finance and Commerce, University of Pennsylvania

AT THE annual meeting of this Society in 1927, Professor Cheney, speaking with his usual clearness of thought and facility of expression, discussed the question, "Is history capable of being treated as a science?" and he said:

"My answer to this question is (1) that the phenomena of history are as capable of being treated scientifically as are any other objects of observation, (2) that in the hands of its most modern representatives history is fast becoming scientific, (3) that before it becomes a true science we must deliberately divest ourselves of certain habitual conceptions concerning it."

The major part of Professor Cheney's paper was devoted to a discussion of the third part of this answer and he set forth the several requirements of scientific historical work. He did not claim that all historians had become scientific in method, but he showed "how history could be made scientific" and stated in his closing paragraph that "History as a science may, perhaps even more than other sciences, because it is more engaged with the affairs of men, habituate men to conformity to its favorite standards, extreme care in testing sources of information, caution in inference, moderation in statement, tolerance in all judgments, and so lift the general level of intellectual and social life."

All students of history will agree that Professor Cheney's own writings are as good an illustration as could be found of history that is scientific in spirit and method.

Much to my surprise it has fallen to my lot to devote some years to the problems of business education, and not unnaturally I have been and am now confronted not only with the question, what is education? but also with the question what is business? I take it that it must be axiomatic that an acceptable answer must be found for both queries before a philosophically sound program of business education can be formulated and effectively carried out. So much has been said in definition of education that I would be unwise, even if I had the time, to attempt to add anything to what has been spoken and written on that subject. I have, however, had the temerity to attempt a partial answer, not to the question what is business, but to the query, can business be made a science?

Possibly I should have been content with asking whether economics, which is generally conceded to be one of the social sciences, and which is often defined as the science of business, is in fact a science, or, if not, whether it is capable of being treated as a science. Much might be said as to the scope and limitations of the application of the methods of the physical sciences to the data of economic affairs; but, while admitting that much economic writing is not scientific, I am going to assume that economics in the hands of men of scientific spirit is like history capable of being treated as a science. The principles of inductive and deductive reasoning are applicable to economic data. The marshalling of the facts of economic affairs and their accurate interpretation require all the scientific objectivity that is demanded of the successful historian. The scientific spirit must, first of all, control in the gathering and statement of facts. The economist, like the historian, must investigate to find out what the facts really are, not to get evidence in support of a doctrine or a point of view. In the second place the data having been obtained and their scope and limitations having been determined, conclusions or principles should be derived from them by methods that are scientific in spirit and are applicable to the doings of men individually and as members of society. The data of the physical world and of human beings acting individually and collectively are dissimilar but equally real, determinable and usable as bases for scientific structures.

I would not say that economics or history or the other social sciences have attained, or will in the near future, if ever, acquire the definiteness as regards data and the formulation of controlling principles that has been reached by several of the physical sciences. The social sciences are in the process of evolution, a process that is being aided by the work of the historian, the psychologist, the economist and especially by the great business and other organizations that are spending millions each year in support of laboratories and investigations whose purposes are partly to find new sources of wealth or well-being and partly to discover principles whose application will increase the values obtainable from known sources of wealth and human happiness.

While the social sciences have in prospect a long and presumably an endless evolution, there is plenty of encouragement to be gotten from the fact that as a result of better methods and aims of scholarship and of enlarging material support, and as a consequence of the widening influence of education, this evolution is making greater progress year by year.

In this connection the status of the social sciences is of significance because if business is to become a science there must be principles by which procedure can be determined. It must be possible for the intelligent man who knows the facts of a given problem or situation to be guided in his actions by general principles rather than merely by the rule of thumb or by intuition. To avoid being misunderstood it may be well to remark in passing that should business be made a science, the value of intuition and judgment on the part of business men will not have been destroyed. Intuition is a great help in all walks of life, and is possibly of greatest assistance to the scientist who is ever seeking to establish and define new relationships and new truths. Scientific methods cannot tell a man what he ought to do. The most they can do is to inform him what will be the consequences of an action or of a failure to act. No matter how scientific business men may become, some individuals will be successful business men while a larger number, but we may hope a decreasing percentage of the total, will not be successful men of affairs. Science will not take the human factor out of the equation of conduct.

It is obvious that if business is to become a science or to the extent that it becomes scientific, it must be carried on by the application of principles to problems. There must be laws by which the business man may determine his course of action. The process of conducting business scientifically would, like other scientific procedure, start with the ascertainment of the obtainable facts, and their appropriate appraisal. When the situation or problem confronting the business man is thus revealed and a course of action is to be decided upon, his decision, if it involves a question of banking, investments or finance, will be determined by his knowledge of the principles of finance and of their applicability to the instant question, and by his ability to be guided by principle instead of being controlled by any of the emotional and other influences that often are stronger than the judgment of men entrusted with business affairs. Similarly when a decision as to a problem of business organization or personnel management is to be made well-established management principles should be known and adhered to, in so far as, and to the extent that, the principles are applicable to the particular problem. Problems in merchandising, foreign trade and other fields of business will call for adherence to the principles applying to those several kinds of business.

This is not the place to discuss the possibility of accurate business forecasting. The forces that determine the trends of business, the alternations of prosperity and depression in business generally, are, however, far better understood than they formerly were, as a result of the study that has been given to the "business cycle" during recent years. It is possibly not claiming too much to say that it is usually possible to foresee some months in advance whether the tide of business as a whole is to ebb or flow, and whether the change in the general level of business conditions is to be large or small.

It is not to be inferred from the statements just made that business conduct can be converted into a process that is mechanical or mathematical in character—a process of solving known problems merely by the use of economic rules or formulæ. The business man must first of all be a good diagnostician. To get the essential facts, to appraise them accurately and to interpret them correctly requires adherence to scientific methods; and then, knowing the facts, to decide upon the goal and to choose and to adhere to the principles of procedure that will most probably lead to that goal demand clear thinking, honesty of purpose, firmness in following a chosen line of conduct, and the ability to enlist the coöperation of others. Scientific management of business does not reduce the importance of personality, it makes it possible for the business men of strong personality to accomplish greater results, and to make fewer serious mistakes.

For business to become a science five things are necessary:

1. There must be economic principles or laws of established validity that are applicable to business management. It is the task of economists, working in many different fields, to develop the science of economics, to discover, formulate and expound economic laws and tendencies. The economists must prepare the way, or, to change the figure of speech, they must assist in laying the foundation. The designation economist is not to be understood to be limited to those who teach or write as professional workers. In industry, finance, merchandising, commerce, transportation, engineering and in many occupations, experts in plants, stores, shops and laboratories are discovering and formulating new relationships, new principles, and new evidences of tendencies. This will continue to go on indefinitely, steadily enlarging and strengthening the science that interprets business life and develops its underlying principles.

2. Scientific methods must be followed in the conduct of business if it is to become a science. These methods are well established. They involve finding and facing the facts, and a procedure in harmony therewith. The scientific method

in business does not imply the substitution of inquiry and generalization for action on the part of managers. The essence of business is action, the accomplishment of results. The aim of business is the creation of wealth by the physical and mental effort of men and by the employment of capital. Business is dynamic and constructive. The question is what shall characterize the process? Shall it be haphazard, or in accordance with a definite order of procedure and a clearly-defined managerial policy?

A query thus stated in the form of a leading question cannot be answered without qualifying reservations, the first of which is that business applies to a vast variety of activities, some small and individually conducted, others large and highly organized; some of long-standing with well-defined opportunities, aims, methods and codes, others newly established which must pioneer in hitherto unoccupied fields of endeavor. The large and well-established business can adhere to scientific methods; the small enterprise must rely more largely on the unguided judgment of one or a few men. Risks may be minimized in some kinds of business while in others more must be ventured.

3. All of the foregoing and many other facts are characteristic of business in its manifold activities, and I have no thought of suggesting that the same degree of scientific management is possible throughout the business world. What is possible and desirable, however, is that a common spirit or ideal should everywhere prevail in business. We may call this the professional and the scientific spirit. Those who are engaged in creating the wealth upon which their own and society's life, happiness and progressive well-being are dependent may feel and should feel that theirs is a calling to be ennobled by high professional ideals and is to be made of ever-increasing service to mankind by the spirit and methods of science.

The cultivation of a professional spirit in business should be a definite aim of society. Much has been and is being done. Nearly all of the leading categories of business men have their organizations or associations for the advancement of their several kinds of industry. One thinks at once of the many different organizations of farmers, of manufacturers and other classes of producers, of merchants, miners, carriers —there are hundreds of such societies—each and all doing something to advance the cause of their several vocations. Many of these organizations have formal codes governing the conduct of their respective businesses. There is convincing evidence that business ideals are rising, and it may not be out of place to say that some help in bringing this about has been rendered by the two hundred colleges and universities in the United States that have introduced business subjects into their curricula.

4. It will be accepted as axiomatic that organized research is one of the foundations upon which business scientifically conducted must rest. Research provides the data that make scientific procedure possible; research reveals the defects of present methods and the possibility of improvement; it discovers new sources of wealth and new and better processes in the mechanics, the organization and the management of production, trade, and finance. It is to research in pure science, in chemistry and physics, especially, that business is under greatest obligation; but research done to improve business methods and management, to increase production, to improve distribution has been of great service.

This has been said so often that one must apologize for repeating it, but there is no more hopeful indication that business generally may some day be conducted in accordance with scientific methods than the fact that model business organizations, such as the American Telephone and Telegraph Company, the General Electric, the General Motors, and others that might be cited, are today spending large sums for research in the field and in their laboratories. Moreover, they devote funds freely to educating the public; they are scientific and educational as well as business institutions.

Research is so essential to the development of scientific methods in business, that the large universities have a peculiar obligation and opportunity to be of service to the public by making generous provision for research both in the physical and the social sciences. Organized business research has made a start in the universities, it merits the same degree of support as is given to investigations in the physical sciences. The universities do not need to try to duplicate the research work being done by the large corporations to develop their respective businesses. What the universities especially should do is to make it possible for the members of their faculties who have the urge to investigate to carry on research work in their several fields. Some of the work thus done may not yield results of material value, some may do much to increase the wealth and happiness of mankind. It is difficult to foretell the value of research work, but happy is the university that can, or would the institution be that could, provide the specialists and experts in its faculty with ample funds to carry on their research work advantageously. Fortunately in this country what really needs to be done is ultimately provided for; and so it will be with obtaining support for university research. Indeed, wealthy foundations and men of large means have already made possible much valuable research work upon health problems and other matters of import to the public.

5. The only other influence to which I shall refer as contributory to the development of the scientific spirit and method in business is education. It is the function of education not only to impart and to extend knowledge, but to develop ideals, to influence ways of thinking and acting in all phases of life. We are fortunate in living at a time when the physical, mental and social sciences are reshaping thought and action as never before. We refer to the present as a scientific age; and while this is true in only a very relative sense, it is true that "the thoughts of men are widened with the process of the suns," and it becomes increasingly easy for men to substitute new ways for old ways. The scientific leaven is working in thought and conduct.

As is well known, increasing emphasis is being placed upon educational training in scientific methods. In schools of engineering, architecture, and business, students acquire, or should develop, the habit of investigating before deciding, of getting the facts before acting, of basing policies upon principles applicable to the given problem or situation. It need not be said that education is still less than one hundred per cent successful in accomplishing this aim, but it is nevertheless a force of great influence.

The conclusion to which this brief analysis leads is not that business generally now is, or is soon to become, a science; but that forces are operating that are developing in business —*i.e.*, in men of affairs—a greater degree of idealism and a more definite professional spirit. Moreover, there is a growing tendency to think and act in all matters more in accordance with the methods of science. This is especially observable in large business enterprises many of which are now controlled by leaders who manage their affairs in the spirit and by the methods that control in science.

The goal to be reached ere business in general may be called a science or be termed an activity that is scientific in spirit and method is some distance ahead. "But not in vain the distance beacons." Our present tasks as educators or scientists or men of affairs are to define the objective, chart the road thither, and to strengthen the impulses that will impel men to follow the road thus laid out. The *raison d'etre* of business education and its opportunity to be helpful in changing business from an art to an activity more and more scientific in spirit and method become increasingly manifest with the passing years.

DISCUSSION OF THE KINETIC THEORY OF GRAVITATION IV: CORRELATION OF CONTINUAL GENERATION OF HEAT IN SOME SUBSTANCES, AND IMPAIRMENT OF THEIR GRAVITATIONAL ACCELERATION

By CHARLES F. BRUSH

(*Read April 21, 1928*)

At the Minneapolis meeting of the American Association for the Advancement of Science in December 1910, I had the honor to outline "A Kinetic Theory of Gravitation."[1] This was followed by a "Discussion" of the theory in 1914.[2] A second "Discussion" came in 1921.[3] A third "Discussion" appeared in 1926.[4]

The latter paper contains a concise synopsis of the theory, and strong argument supporting my contention of 1910, that the energy acquired by falling bodies is derived from the ether.

This third "Discussion" (1926) contains in its title "Some Experimental Evidence Supporting Theory; Continual Generation of Heat in Some Igneous Rocks and Minerals. Relation of This to the Internal Heat of the Earth and Presumably of the Sun." Quoting from this paper: "Gravitation Waves and Heat.

"Heat is often defined as an agitation of atoms and molecules of matter, and measured by the total kinetic energy of such agitation. The agitation consists partly in internal vibrations of the elastic atoms and molecules and spinning about their various axes, and partly in a very rapid translatory motion among themselves. Thus they are sup-

[1] *Science*, March 10, 1911; *Nature*, March 23, 1911.
[2] *Proc. Am. Phil. Soc.*, Vol. LIII, No. 213, January–May 1914.
[3] *Proc. Am. Phil. Soc.*, Vol. LX, No. 2, 1921.
[4] *Proc. Am. Phil. Soc.*, Vol. LXV, No. 3, 1926.

posed to dart about in every conceivable direction, constantly colliding with each other and rebounding or glancing in new directions. The kinetic energy of this translatory motion constitutes *sensible* heat (not total heat) and is the measure of *temperature*. Anything (such as absorbed radiation) which stimulates the internal vibration of atoms or molecules likewise increases their translatory velocities by the increased violence of rebound after collision, and thus increases their temperature; and *vice versa*.

"All the above is known to be true of gases and vapors (kinetic theory of gases), and is generally believed to be true of liquids and solids.

"The '*mean* free path' and the '*mean* velocity' between collisions of the molecules of many gases under stated conditions have been computed. But it has also been shown mathematically that the higher and lower velocities, and the longer and shorter paths, differ greatly from the means, and may in each respect vary twenty or more times in amount. Doubtless this is true also of liquids and solids.

"Certainly the 'free path' of atoms and molecules in solids must be very short, but quite as certainly there must be some space for movement because we know that the atoms or molecules of solids are not in contact. And diffusion of contiguous metals into each other proves migration of the atoms or molecules just as in gases, though vastly slower, as we should expect from the extreme shortness of their free paths.

"From the fortuitously wide variation in velocities and free paths of the billions of vibrating atoms or molecules in their heterogeneous movement, it follows that collision frequencies must also vary greatly, from instant to instant, everywhere in a body of matter.

"Probably the postulated gravitation waves are not confined to one frequency, but have a wide range of frequencies as do the well-known X-rays.

"With the foregoing in mind it is easily conceivable that some kinds of matter may have atoms or simple molecules or complex molecules of occasional vibration frequency corresponding with some gravitation wave frequency, whereby fortuitous resonance can, for brief instants, be established at various points. This would result in a slight increase of vibrational activity and a cumulative rise of general temperature, perhaps sufficient to be detected.

"A body of such matter, with some thermal insulation, would become and remain permanently warmer than a neighboring body similarly circumstanced, but not endowed, or less endowed with the permissive heat-generating quality."

The foregoing hypothesis had been my guide in a very lengthy search for some material exhibiting continual generation of heat in observable amount.

A carefully designed calorimeter is illustrated and described in the paper (1926), and details of many experiments given. These resulted in the discovery that some rocks and minerals did generate an easily observable amount of heat. But in some instances this declined materially in the next few months. The cause of the decline, or rather the cause of the abnormally large generation of heat in recently crushed minerals, has been ascertained, and is referred to in this and the succeeding paper (1927).

In April 1927 I presented another paper on "Persistent Generation of Heat in Some Rocks and Minerals."[1] This is a continuation of the 1926 paper. It describes a new and different calorimeter, built in the spring of 1926, and since known as the "Ice Calorimeter." It has been in almost continuous use down to the present time (April 1928) and has proved very satisfactory. With this calorimeter it has been found that some of the natural heat-generating materials, and some of the artificial silicates hereafter described, have retained their heat-generating activity unimpaired; and none of these substances is more than minutely radioactive. Quoting from the 1927 paper:

"It is notable that all the materials which appear to be endowed with persistent heat generating activity are very

[1] *Proc. Am. Phil. Soc.*, Vol. LXVI, 1927.

complex silicates. And it seemed highly probable that some silicates may be very much more active than others. This now appears to be true.

"With this thought in mind I commenced last summer a systematic campaign with artificial silicates. This is still in progress."

There follows a description of the preparation, in the wet way, of many complex silicates, and their preliminary testing for heat generation. A silicate of the protoxides of nickel and cobalt showed very large activity, larger than either silicate alone; and this now appears to be permanent. Nickel and cobalt are almost identical in atomic weight, and differ but one unit in atomic number. Quoting again from the 1927 paper:

"In the absence, at present, of other explanation, it is thought that persistent heat generation in some rocks and minerals is due to isotropic ether waves of great penetration; very great indeed, if the generation goes on in the interior of the sun and planets as it does at the surface of the earth."

It is now thought that the class of isotropic ether waves postulated as the cause of persistent generation of heat in some substances, is the same class, perhaps of very wide range of frequency, postulated as the cause of gravitation.

Conversion into heat of some of the energy of the gravitation ether-waves, however little, might be expected to impair to some extent the falling velocity of a heat generating substance; *and all such substances thus far tested have clearly shown impairment.*

I have yet found no exception to this remarkable phenomenon, though I have already tested many natural and artificial minerals. Substances which have shown no generation of heat in the calorimeters show no impairment of their falling velocity when compared with lead. Substances exhibiting small, moderate or large generation of heat have shown comparatively small, moderate or large impairment of their gravitational acceleration. I aim to continue this research until the quantitative relationship of the two phenomena is ascertained. This may open a fertile field for mathematical exploration.

In making the above indicated comparisons of falling velocities I have largely used the method and apparatus described and illustrated in my 1923 paper on "Some New Experiments in Gravitation."[1] (See also 1924 paper of same title.)[2]

Two aluminum containers are used, alike in size, shape, weight and smoothness of surface, and dropped *simultaneously*, side by side, though exactly the same distance (about 122 cm.).

Each container, at the end of its journey, breaks an electric circuit. But the breaks of both containers are in series in the same circuit, so that the break which occurs first produces a bright spark, while the belated break gives no spark because its circuit is already open.

When the containers are equally loaded with the same metal, there is no visible spark at either break or a very feeble spark at one or the other indifferently. But when they are equally loaded with certain different metals, one container persistently produces a bright spark, though the containers are always reversed in position for each trial. From this it seems clear that the container giving the spark falls a little faster than the other. This sparking condition is clearly manifested when the faster container reaches the end of its free path as little as .0125 mm. (.0005 inch) in advance of its neighbor. This indicates a time difference about 1/400,000 second. (During their half second of falling the containers acquire a velocity about 490 cm. per second.)

The 1923 paper also describes how approximate quantitative measurements are made. These are very tedious, especially when falling velocity differences are large.

To facilitate estimation of the larger falling velocity differences I am perfecting a photographic method of observation. After falling about 110 cm. the small lower ends of

[1] *Proc. Am. Phil. Soc.*, Vol. LXII, No. 3, 1923.
[2] *Proc. Am. Phil. Soc.*, Vol. LXIII, No. 1, 1924.

the containers are photographed in silhouette against a white background having many horizontal black lines, and illuminated by a very bright electric spark.

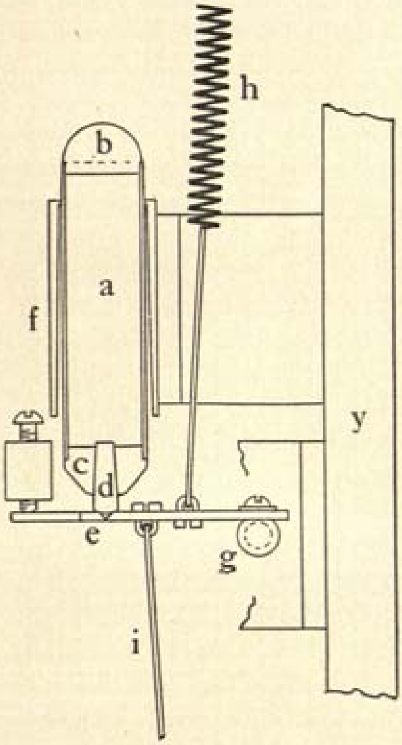


FIG. 1

To make clear to the reader the whole photographic outfit and method without bothering him to refer back to the 1923 paper, I shall here introduce some parts of that paper as follows:

Plate I shows the releasing mechanism with the loaded containers in place, ready to start on their downward journey. A cast-iron plate 30.5 cm. square and 2 cm. thick, with machined surfaces, is firmly bolted to a thick brick wall, with its face truly vertical. It carries the mechanism for holding, guiding and releasing the containers. Below the plate may be seen the starting device ready to be tripped by pulling a cord hanging from it.

Plate II shows the same mechanism after the containers have been released and have fallen. It also shows the starting device locked against rebound.

Figure 1 is a diagram of the dropping, or releasing mechanism, in vertical side section through axis of the right-hand container shown in Plate I. The container *a* is made of aluminum tubing 8 cm. long and 2.2 cm. and 2 cm. outside and inside diameters. The upper cm. of the tube is tapered smaller, and the tube is capped by a snugly fitting dome-shaped plug of hard rubber *b*, easily removable for loading

PLATE I

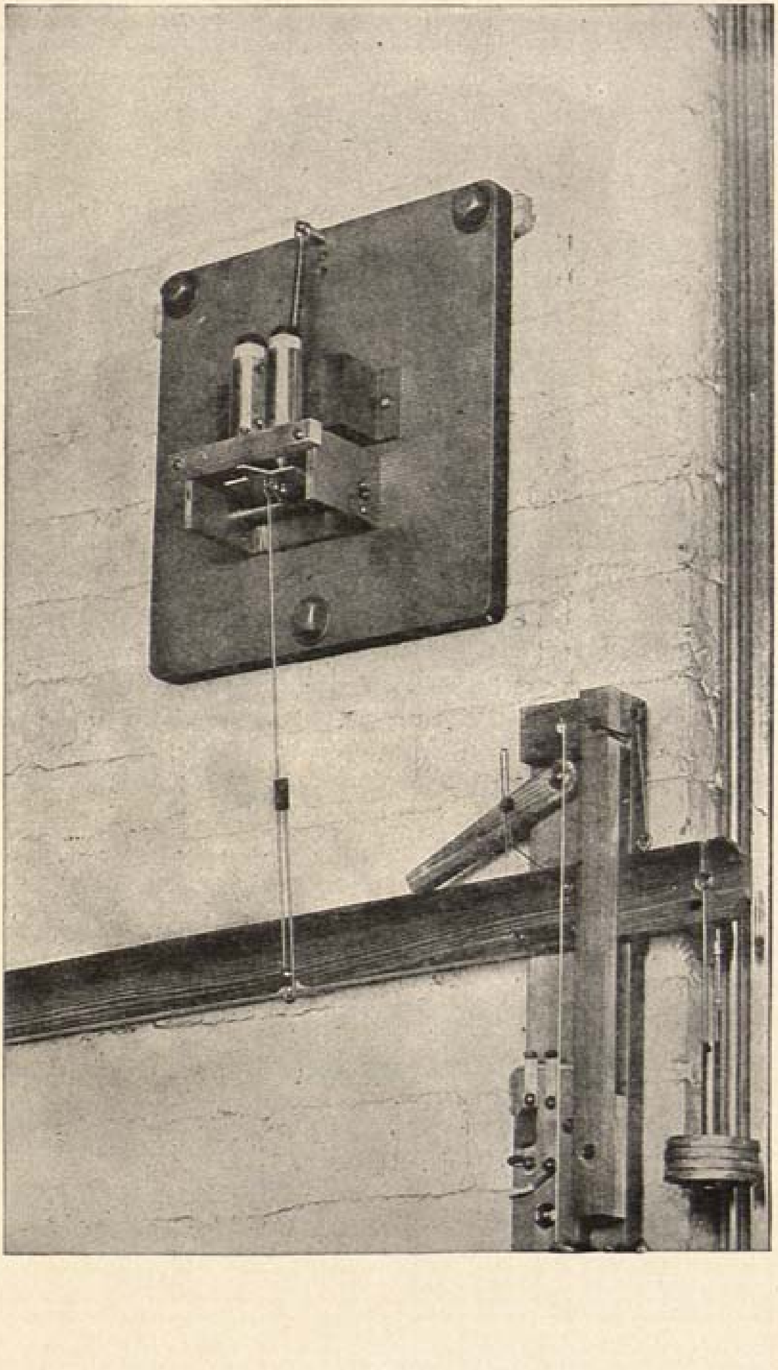
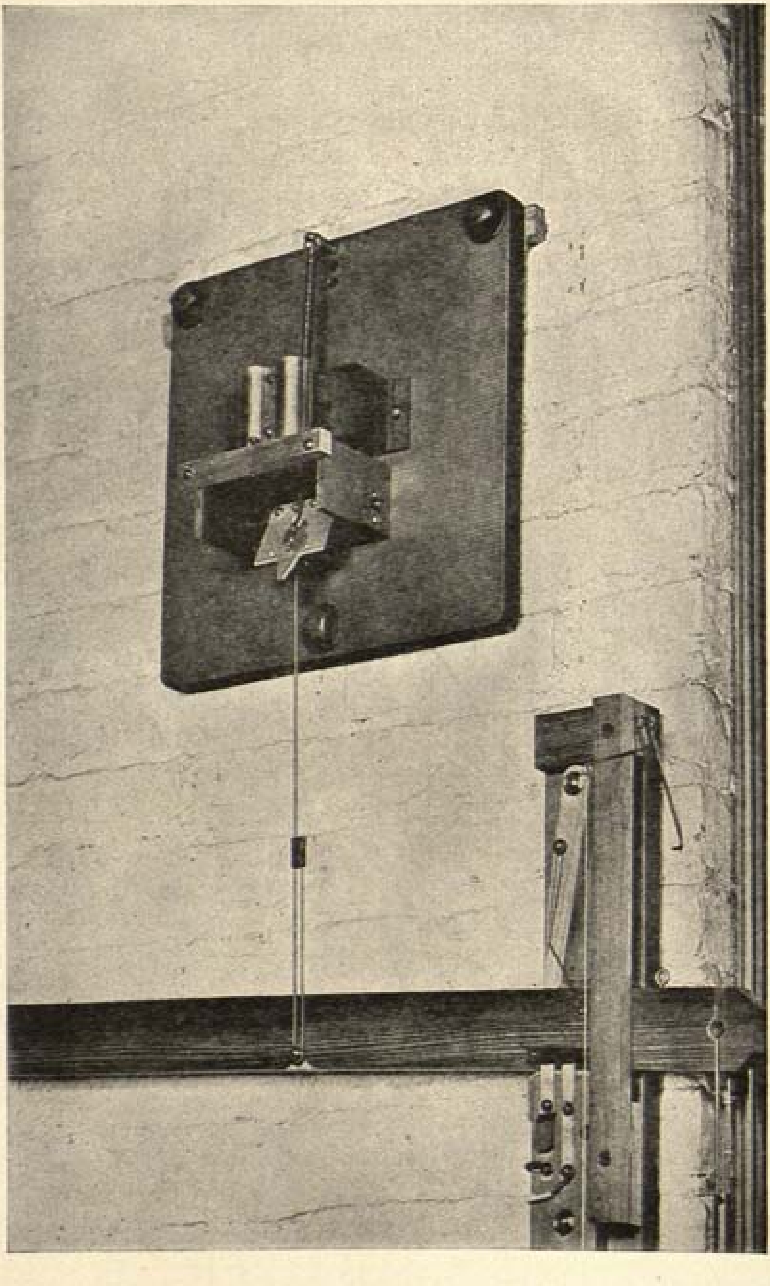


PLATE II



the container. The lower end of the tube is permanently closed by a forced-in aluminum plug *c*, through which passes the firmly driven conical brass stud *d*. The lower end of brass stud *d*, after machining, to center it, was ground and polished like a lens, to a radius of 6.5 mm. It rests on the open end of a smaller conical cavity in the thick aluminum plate *e* permanently adjusted to horizontal position. The open end of the conical cavity is ground to the same radius as the brass stud, so that the latter contacts with the plate in a narrow ring.

The container *a* is held in truly vertical position by the guide tube *f*, which is slightly conical inside and, at the last mm. of its upper end, just freely fits the container at the point where the latter begins to taper. The inside of the guide tube at its upper end, as well as the container, are polished; and as there is no side thrust on the container, friction between it and the guide tube is virtually zero. Moreover, owing to its tapered upper end, the container is absolutely free after the first mm. of its fall.

The plate *e* is firmly attached to the brass axle *g* by 4 screws. The axle has smaller end bearings pulled firmly upward against inverted *V* bearings by the stiff spring *h*. This spring also holds up both loaded containers with a safe margin to spare. Thus the mounting of the axle *g* is like that of a transit instrument inverted, and there is no lost motion in its bearings.

When it is desired to leave the containers free to fall, the starting mechanism shown in Plates I and II is tripped and the plate *e*, common to both containers, is jerked suddenly downward and then to the right by the rod *i*, much faster than the containers can follow by gravity. This is because the starting lever and weights at its right-hand end fall some distance and acquire considerable velocity before taking up the slack in the link at the lower end of rod *i*.

The guide tubes *f* and the mechanism of plate *e* are mounted on separate brass cradles very firmly screwed to the heavy cast-iron plate *y*. Hence any jar due to the sudden jerking downward of plate *e* is not directly communicated to the guide tubes.

The white surface with horizontal black lines against which as a background the lower rounded ends of the containers are photographed in silhouette, is adapted to have horizontality of the black lines adjusted by a fine-threaded screw.

A horizontal brass sparking post is located just above the central upper edge of the white surface, with its polished blunt end projecting about .7 cm. in front of that surface. A long horizontal fine steel wire, a prolongation from a coiled spring of the same wire, has its free end located about .8 cm. in front of, and about .7 cm. diagonally above, the end of the sparking post. The wire is held in place by a suitable guide, and bears against its upper stop with very slight pressure.

The conical shoulder of one of the containers strikes the steel wire 1.5 cm. from its end, and when the latter is carried downward 2 or 3 mm. the spark occurs between the free end of the wire and the sparking post. Thus the spark illuminates the white surface obliquely, and is itself out of the picture. The duration of the spark is estimated to be much less than a millionth part of a second, and hence the container tips are practically motionless while being photographed.

The very slight retarding effect on the container by its impact with the fine steel wire is cancelled at the next exposure, when the containers are reversed in position.

The spark is provided by a large condenser charged by an induction coil having a spark gap in each arm of its circuit. The length of one of these gaps is regulated by a screw easily turned while the coil is in action. Thus the potential charge of the condenser can be controlled. A suitable electroscope is connected in parallel with the illuminating spark gap, and when an adequate potential is indicated the camera lens is opened and the containers released. In this way successive exposures may be made almost as rapidly as the containers can be replaced and the photographic plate moved to new positions.

The camera lens is located about 37 cm. in front of the white surface, and the photographic plate about an equal distance behind the lens; so that the image is about equal in size to parts photographed.

The plate holder moves vertically in guides, and rests on a pin in one of eight equally spaced holes 1.6 cm. apart in the back-board of the camera. This back-board has a horizontal opening 1.4 cm. wide, which limits the exposed portion of the plate to a strip of this width. Thus eight pictures of the falling container tips are made on one plate. The containers are reversed in right and left position after each exposure.

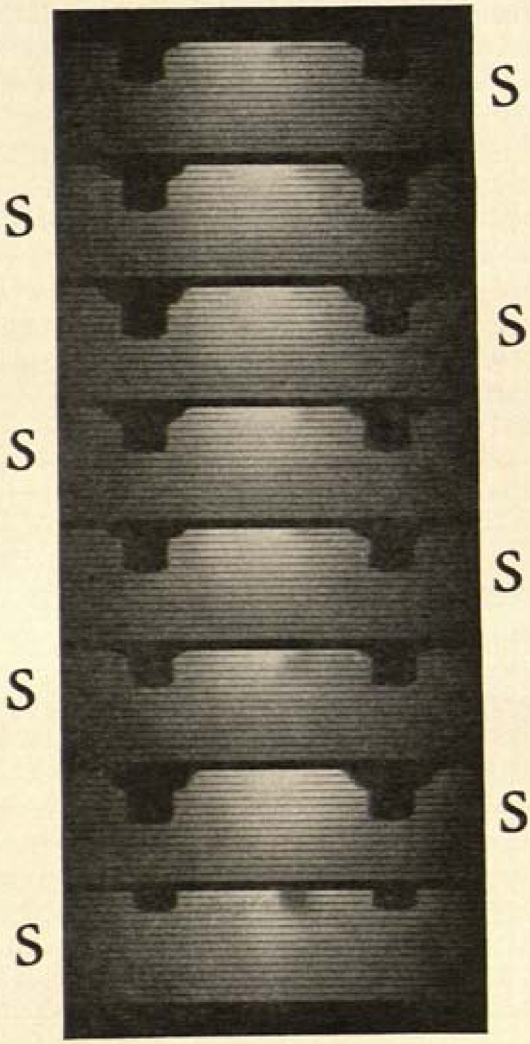
Plate III shows such a series of photographs. Each container weighed approximately 30.6 grams. One was marked with a white spot on its top for identification. This one, lettered *S* on the plate, was filled with silicate of nickel and cobalt, which weighed 13.6 grams, or about 30.8 per cent of the total weight of the loaded container. The unmarked container was loaded with lead sawdust, held tightly in the lower end by a short closely fitting cork above it, until it very closely equaled the marked container in weight.

Each of the eight photographs on the plate, when magnified, clearly shows the *S* container (Silicate) slower than its companion. Six more similar plates have been made with the same loaded containers, and all show the same effect. It will be noticed that the amount of retardation of the *S* container varies considerably in the eight exposures of Plate III. This was principally due to small lateral air currents in the room which acted unequally on the two containers when one shielded the other; as was demonstrated with another plate by purposely increasing the lateral air currents. I shall eliminate lateral air currents in future work.

Of course I tried exchange of loads in the containers, but without observably affecting the result; the container holding the silicate was always slow.

The observed retardation of the silicate container must be due to impaired gravitational acceleration of the silicate

PLATE III



as compared with the lead sawdust in the other container; and as the silicate constitutes only 30.8 per cent of the total mass undergoing acceleration, we must multiply the observed retardation by 3.25 to find the full impairment of the silicate alone.

In the apparatus as set up, the centers of the container tips are about 1.6 cm. in front of the lined background; hence tips and lines cannot both be sharply in focus of the camera lens. In Plate III the focal plane of the camera was about half way between the tips and the lines. Sharpness of lowest part of the curve of the tips was very greatly enhanced by permanently covering all of the camera lens except a horizontal strip 2 mm. wide across its center.

The comparison lines in Plate III are spaced one mm. apart between centers. I am installing another white background with very much finer lines spaced only half a mm. apart, and far better adapted to micrometer measurement of container tip differences of level.

It is my aim in the near future to ascertain, as nearly as possible by the photographic method, the true value of gravitational impairment in the silicate; and to check this by the earlier method of spark detection already referred to. Shall do the same with Sandusky clay. In the meanwhile measurement is being made of the *rate* of heat generation in these substances. When these data are in hand we shall begin to know something of the quantitative relation of the apparently correlated phenomena.

The Bureau of Standards, with a calorimeter of its own designing, is working with some of the heat generating substances for the purpose of checking my findings.

Correlation of continual generation of heat in some substances and impairment of their gravitational acceleration, is regarded as very strong evidence in support of the kinetic theory of gravitation; and we seem now well started on the way of finding out something definite about the nature of gravitation, which is by far the greatest of all outstanding physical problems.

Cleveland, April 1928

REPORTS OF THE PRINCETON UNIVERSITY EXPEDITIONS TO PATAGONIA, 1896–1899

By WILLIAM BERRYMAN SCOTT

(*Read April 20, 1928*)

WHEN I sent the title of this paper to the Program Committee in October, 1927, I did so in the confident expectation that I should be able to report to the Society the completion of this long delayed series, and to exhibit all of the volumes. Unfortunately, however, this hope has been disappointed. Partly owing to delays of the authors (of whom I am one) and printers, the work is still unfinished; and after so many disappointments, I hesitate to fix a term for its completion. It is, nevertheless, possible to give an approximate statement of what the completed volumes will contain.

Like everything else in connection with these expeditions, the plan of the series of reports was due to Mr. Hatcher, the leader of the explorations. So great were the collections in all departments of natural history, that he felt it would be a great misfortune to have the results scattered through many publications—journals and transactions and proceedings of learned societies; and he therefore proposed to me that I should endeavor to finance the independent publication of these remarkable results. The late Mr. J. Pierpont Morgan gave me $24,000 for the work; and, so far as could be foreseen at that time (1900), this sum should have been sufficient, as the plan called for only eight quarto volumes. Unfortunately, however, every contributor far exceeded his estimates as to the amount of text and the number of plates which he would require, and the nominal eight volumes have expanded to fourteen. The additional sums necessary for the publication have been obtained partly from the Carnegie Institution of

Washington ($1,500) and Princeton University ($6,000), and from sales of the work to subscribers.

Volume I contains the narrative of the expeditions and the geography of Patagonia by Mr. Hatcher, whose untimely death prevented his taking any further part in preparing the Reports.

Volume II (Ornithology) was mostly written by the late Messrs. W. E. D. Scott, of Princeton, and R. B. Sharpe, of the British Museum, both of whom died in 1910, leaving that volume unfinished. Their remaining manuscript was taken by Dr. Witmer Stone, of the Philadelphia Academy of Natural Sciences, and the final part was entirely written by him.

Volume III (Zoölogy) is due to a number of hands. The late Dr. J. A. Allen, of the American Museum of Natural History in New York, wrote the chapters on the Mammals; Dr. L. Stejneger, of the U. S. National Museum, prepared those on the Reptiles and Amphibia; the late Dean C. H. Eigenmann, of the University of Indiana, wrote the report on Fishes; the late Dr. A. E. Ortmann, formerly of Princeton and then of the Carnegie Museum in Pittsburgh, reported on the Crustacea; Professor Moore, of the University of Pennsylvania, wrote the part on the Leeches; and Dr. Pilsbry, of the Academy of Natural Sciences, the report on the non-marine Mollusca.

Volumes IV to VII, inclusive, were devoted to Palæontology. Volume IV contains the reports of Dr. T. W. Stanton of the U. S. National Museum on the Cretaceous Invertebrates, by Dr. Ortmann on the Tertiary Invertebrates, and finally the report by Dr. W. J. Sinclair, of Princeton, on the Marsupials of the Santa Cruz formation. Volume V was entirely written by myself, and contains the descriptions of the Edentata and Glires (Rodentia) of the Santa Cruz. Volume VI has Dr. Sinclair's chapters on the Santa Cruz Typotheria, and mine on the Toxodontia and Entelonychia. The remaining part of this volume, my report on the Astrapotheria and the Monkeys of the Santa Cruz, is now in the printers' hands, and will, I trust, appear in a few weeks. Volume VII contains my chapters on the Litopterna. Dr. M. S. Farr, of Princeton, is preparing the report on the fossil birds of Patagonia, and that will go to the printer as soon as the Astrapotheria are completed. And, finally, is a brief summing up of the Santa Cruz fauna and of Patagonian geology by myself. This will contain nine plates in heliotype reproduction of Mr. Charles Knight's restorations of Santa Cruz mammals, 13 different genera; these plates are now making in Boston by the firm of E. O. Cockayne. I am particularly glad to publish these restorations, because of the great interest which Mr. Morgan took in them. One of the first things he said, on agreeing to furnish the funds, was that we should immediately turn to Knight for a series of restorations.

Volume VIII, and a supplementary volume, are devoted to Botany. The great bulk of this Flora Patagonica was prepared by the late Professor George Macloskie of Princeton, with the most valuable criticism and assistance of the eminent Swedish botanist, Pér Dusen, who also wrote the chapters on the Patagonian mosses. The report on the Hepaticæ was written by Professor Evans of Yale University.

I can already give a very close approximation to the number of pages of text and of plates which the finished work will contain, as all of the plates are either finished, or in the hands of the engravers. The great majority of the plates are lithographs, which were made by the firm of Werner and Winter of Frankfurt, in Germany; but some photographic processes were made in this country, partly in New York, and partly in Boston. The water color drawings for the modern birds were done by Mr. Keulemans, who was so long associated with Dr. Sharpe in the work of the British Museum. The botanical plates were mostly prepared in London, as Dr. Macloskie, through his connections there, was able to exercise a closer supervision than he could have done in Frankfurt. The other drawings, zoölogical and palæontological, were the work of Mr. Bruce Horsfall and the late Mr. F. von Iterson. The total number of quarto pages of text is approximately 4,880 pages, of preliminary text 107 pp., and of plates 421, of which 37 are colored.

The long delay in the completion of the work has been due partly to the war, and partly to the period of extravagant prices which followed. This inflation affected especially everything connected with the making of books, having more than doubled the cost per unit of the parts which remained to be issued after the war. The end is now in sight; and I cannot but esteem myself most fortunate in having been able to see the great work thus far on its way. Even should I not live to see the actual completion, there remains but very little for me to do.

A METHOD FOR DETERMINING THE CONSTANTS OF AN ELECTROMAGNETIC OSCILLOGRAPH

By A. E. KENNELLY

(*Read April 21, 1928*)

An oscillograph photographically records the wave form of electric currents or discharges passed through it. There are, at present, two types of oscillograph:

(1) Electronic oscillographs.

(2) Electromagnetic oscillographs, including also electrothermic.

Type (1) may be regarded as free from the effects of inertia of moving parts, and is not here considered. This discussion relates to type (2).

Oscillographs are employed to record:

(1) Periodic currents.

(2) Non-periodic or single discharges.

This discussion relates essentially to records of periodic or alternating currents.

Owing to the effects of inertia in the moving parts of its vibrator, the response of an oscillograph to alternating-current impulses of different frequencies is not the same. If the oscillograms produced by the instrument are analyzed into a series of Fourier components of different frequencies, it is known that a correction factor should be applied to each component, to eliminate the difference due to inertia, the magnitude of the correction depending on the frequency.

It has been recognized [1] that if the resonant frequency of an oscillograph vibrator could be identified experimentally, and likewise its "quadrantal frequencies," the correction factor for any recorded frequency could be evaluated. More recently, however, improved facilities for supplying a wide

[1] "Electrical Vibration Instruments," A. E. Kennelly, Chapter XXI.

range of alternating-current frequencies to an oscillograph for testing purposes, have simplified the means available for arriving at the desired correction factor.

There are three characteristic constants of an oscillograph, from which its behavior at any or all frequencies is completely determinable; namely,

(1) Its resonant frequency f_0, in cy.p.s., or the frequency to which its vibrator is resonant, as a vibrating mechanism.

(2) Its "specific deflection" θ', or deflection per unit of testing current, usually expressed in millimeters amplitude of scale deflection per rms. ampere of single-frequency alternating current, taken at some convenient frequency of reference, such as the "initial" frequency, or a relatively very low frequency, such as 60 cy.p.s.

(3) Its "bluntness of resonance" B_0 or B_s, this bluntness being a simple numeric, the reciprocal of its "resonant sharpness." The bluntness B_0 is its bluntness to decaying oscillations, and B_s its bluntness to sustained oscillations. B_0 is just half of B_s. In a "dead-beat" or strictly aperiodic vibrator, $B_0 = 1$.

The correction factor C to be applied to an observed deflection at frequency f, in order to reduce the same to what should be expected at the initial frequency f_s is

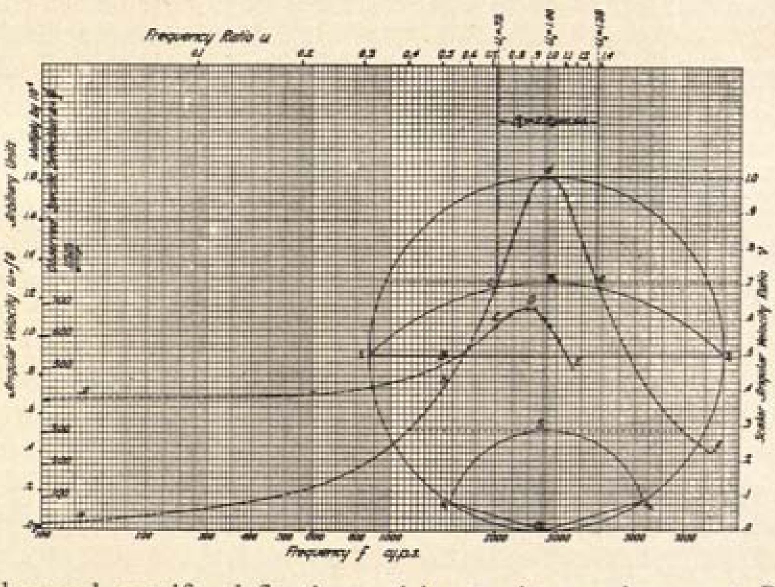
$$C = (1 - u^2) + jB_s u = (1 - u^2) + j2B_0 u \quad \text{numeric} \angle \quad (1),$$

where $u = f/f_0$, and $j = \sqrt{-1}$.

There is ordinarily no difficulty in finding the specific deflection at a very low initial frequency f_s. The problem is to find the resonant frequency f_0 of the instrument; because, as is well known, the maximum specific deflection is not obtained at resonant frequency; but at the "maximum admittant" frequency, which may be very different.

The following procedure may be followed. Connect the oscillograph to an electron-tube oscillator, capable of supplying a reasonably pure alternating current of suitable strength to be measured, over a satisfactory frequency range. Observe the deflection amplitude d_s, produced by a measured alternating current I, through the instrument, at a very low frequency. The ratio d_s/I is the specific deflection θ_s', at initial or nearly zero frequency. Now pass the same, or nearly the same, strength of current through the instrument from the oscillator, at increasing frequencies, until the maximum specific deflection θ_m' is obtained. Continue to make a few observations of specific deflection at frequencies a little higher than that of maximum θ'.

Plot these specific deflections as ordinates against frequencies as abscissas, on semi-log paper, as indicated in the accompanying figure. The curve *ABCDE* is the graph of



observed specific deflections, rising to its maximum at *D*, 690 mm. per ampere. Observations are carried on from *C*, at 2,000 cyps, to say *E*, at 3,200 cyps. Then multiply the ordinates on the curve *ABCDE* by their respective abscissa frequencies, and so construct the angular-velocity curve *abcdef*. This is the resonance curve of the instrument, and, being drawn on semi-log paper, should be symmetrical about its maximum ordinate Qd, which locates the resonant frequency f_0, as 2,800 cyps in this particular case. If the same process of construction is carried out on a sheet of ordinary uniformly ruled paper, the resonance curve *abcde* cannot be expected to be symmetrical about its maximum ordinate.

Having ascertained the resonant frequency f_0 in the manner just described, the bluntness B of the instrument can be found from the relation

$$B_s = 2B_0 = \frac{\theta_s'}{\theta_0'}. \qquad \text{numeric (2)}$$

In the case illustrated, $\theta_0' = 640$ and $\theta_s' = 410$; so that their ratio $B_s = 0.64$, and $B_0 = 0.32$. This means that the mechanic resistance to motion in the vibrator is nearly one third of the amount necessary to make the system aperiodic or dead-beat. By using these values for B_s or B_0 in formula (1), any observed deflection at a measured frequency ratio u, can be reduced to the magnitude and phase obtainable at the initial frequency.

Thus, at $f = 1,680 \sim$, or frequency ratio $1,680/2,800 = 0.6$, the two curves $ABCDE$ and *abcde* are near their point of intersection, and $\theta' = 560$. Using formula (1), we have $C = (1 - 0.36) + j0.64 \times 0.6 = 0.64 + j0.384 = 0.747 \angle 31°$. This means that we must reduce the apparent amplitudes of the oscillogram, taken at this frequency, in the size ratio of 0.747, and advance the phase of the waves 31° of their cycle, to restore parity to initial-frequency conditions. In fact, $0.747 \times 560 = 418$, which is in fair agreement with the initial-frequency specific deflection of 410 mm. per ampere.

Again, at the resonant frequency $f_0 = 2,800$, $u = u_0 = 1.0$. Here, as we have seen, $\theta_0 = 640$. Substituting $u = 1$, in formula (1), we have $C = jB_s = j0.64$. This means that the apparent amplitudes of oscillograms at this frequency must be reduced in the ratio 0.64, and their phase must be advanced by j, or a quarter of their own cycle, to bring them into parity with oscillograms at the initial near-zero frequency. In fact, $0.64 \times 640 = 410$, which is in agreement with the observed specific deflection θ_s at initial frequency.

An alternative method for determining B_s, without reference to θ_s, is as follows:

Referring to the figure, and having found the resonant or peak frequency f_0, as already described, in this case 2,800, draw a scale of ordinates to scalar angular-velocity ratio ν, as indicated on the right hand side of the diagram, using the peak height of the curve *abcde* as unity. Also draw a new scale of abscissas to frequency ratio $u = f/f_0$, as indicated along the top of the diagram, using f_0 as unity. To these new scales, the resonance curve *abcde* becomes a curve of "specific resonance." In forced electrical or mechanical vibrations, specific resonance curves differ only in the particular quantity of bluntness. Through R, a point on the peak ordinate at an elevation of $0.707 = 1/\sqrt{2}$, on the new vertical scale, draw the horizontal broken line cRe. On the peak ordinate Qd as diameter, draw the resonance circle $Q1d2$. With center Q and radius $QR = 0.707$, draw the circular arc $1R2$, which will intersect in the diametrically opposite points 1, 2. These are the two "quadrantal points" on the resonant circle. Draw a horizontal line cRe through the point R, intersecting the resonance velocity curve in c and e. Draw verticals from these points to the axis of frequency ratio, intersecting that axis at u_1 and u_2, respectively, in the case illustrated at 0.72 and 1.38. These are the quadrantal frequency ratios, corresponding to 2,020 and 3,870 cyps respectively. The phases of these quadrantal angular velocities with respect to the actuating current, are indicated by the vectors $Q1$ and $Q2$ with respect to the peak ordinate Qd as standard; *i.e.* u_1 at 45° in advance of the current, and u_2 at 45° behind the current. The phases of the displacements at the same quadrantal frequencies, with respect to the actuating current, are indicated by the same two vectors $Q1$ and $Q2$ with respect to the axis QP as standard; *i.e.* θ_1' at 45° behind the current, and θ_2' at 135° behind the current.

The difference between u_2 and u_1 should be equal to B_s, so that in the case illustrated, $1.38 - 0.72 = B_s = 0.66$, or

$B_0 = 0.33$, which is in fair agreement with the values of B found by the preceding method. Moreover, the product $u_1 \cdot u_2$ should be unity. In this case, it comes out $0.72 \times 1.38 = 0.994$.

The phase of any displacement θ' may be similarly obtained at any frequency, by marking off the ordinate corresponding to this frequency from the velocity curve *abcde*, on the central axis *Qd*. Let *S* be such a point. With center *Q* and radius *QS*, draw a circle intersecting the circle *Q*1*d*2 in two points say s_1 and s_2. Then the vectors Qs_1 and Qs_2 with respect to *QP* as initial line, will indicate the phase retardations of the deflections behind the alternating current in the oscillograph, at the frequencies corresponding to the ordinate *QS*, one below and the other above the resonant frequency.

It is evident that a complete series of deflection amplitudes versus frequency, over the full range covered in the use of an oscillograph, so as to enable a complete curve *ABCDE* to be drawn, would render all computations of correction factor unnecessary. In other words, the correction factor could be read directly from a curve so drawn. It often happens, however, that an observer, using an oscillograph to record some particular electrical event, can only spare time for a brief calibration of the instrument, and in that case he can avail himself of the method here outlined. Moreover, a complete curve of deflection θ' versus frequency f, does not directly reveal the phase correction.

The particular instrument whose behavior is shown in the illustration is somewhat more resonant than usual. The ordinary bluntness B_0 is about 0.5 ($B_s = 1.0$).

The author is indebted to Mr. F. R. Siskind and Mr. M. Samoiloff for assistance in making the tests and records appearing in the illustration.

WERE THE ANCESTORS OF MAN PRIMITIVE BRACHIATORS?

By WILLIAM K. GREGORY

(*Read before the Galton Society, New York, March 2, 1928; and at the meeting of the American Philosophical Society, April 20, 1928*)

In his memoir of 1911–12 on the Chapelle-aux-Saints skeleton Professor Marcellin Boule (1) took exception to the view that the Neanderthal race was in any marked degree anthropoid ape-like. He argued that, on the contrary, the real relationship between man and anthropoids was much more remote than had been generally assumed. He gave some careful consideration to the mass of evidence from physiology and morphology indicating close kinship between man and anthropoids, but in view of positive palæontological evidence afforded by the Neanderthal skeleton he thought it more prudent to attribute many of these resemblances to parallelism. Boule further contested the view that man had been derived from a form with long arms and short legs like the anthropoids and suggested that the remote ancestors of man might rather have resembled more nearly the quadrupedal cynomorph monkeys, especially in the proportions of the fore and hind limbs. Shortly after the discovery of the Piltdown fossils Boule (2) objected to the term *Eoanthropus*, or Dawn-man, applied by Smith Woodward to a Pliocene stage that was already essentially a man and suggested that the real *Eoanthropus*, or Dawn-man, would be as much older than the Pliocene as *Eohippus* is older than *Pliohippus*. He also predicted that the real *Eoanthropus* would some day be found to be a small upright-walking creature with a very large brain.

In 1916, in the course of a paper dealing chiefly with the evolution of the dentition of recent and fossil primates (3), I ventured to take issue with M. Boule and to defend the following theses:

1. That the cumulative evidence from comparative anatomy, embryology and physiology had already sufficiently established the fact that the anthropoid apes are man's nearest existing relatives;

2. That the gorilla and chimpanzee, in spite of evident and marked specializations, were on the whole nearer in structure to the common stem form than is the modern *Homo sapiens;*

3. That the human foot in spite of its superficial difference from a gorilla foot was internally constructed on the basic primate plan, in which the hallux forms the inner branch and the remaining digits the outer branch; and that the human foot showed evident signs of having been derived from a gorilloid type of foot by quite moderate readjustments in proportions of its several parts, consequent upon long continued terrestrial bipedal habits.

4. That during the millions of years since the Lower Miocene man had become so thoroughly adapted to bipedal terrestrial life that a process of marked differential growth had brought about a positive lengthening of the femur, a relative shortening of the forearm, fingers and toes, and a progressive lengthening of the thumb and great toe.

In 1920 in the work on "*Notharctus*, an American Eocene Primate," it was shown (4) that evidences of present or past arboreal adaptations were conspicuous in various parts of the skeleton of all known primates of every family and that the palæontological data strongly suggested a very ancient and remote origin for these characters.

As far back at least as Middle Eocene times and probably much earlier, the arboreal stamp had been thoroughly impressed upon the hind feet in the three dominant families of lemuroids and tarsioids in which these parts are known. No palæontological or morphological evidence known to me has ever been brought forward to prove the existence of any infrahuman primates either of the Eocene or of later ages that did not have clear marks of present or past arboreal adaptations in the hind foot. Permit me to develop this important point somewhat in detail.

My earlier studies on the foot structure of all the known recent and fossil families of primates (summarized in the work on *Notharctus*) led to the conclusion that perhaps as far back as basal Eocene or even Upper Cretaceous times the order of Primates was already set far apart from its ground-living placental relatives by the possession of what may be called a biramous type of hind foot (Fig. 1). In this type of foot the powerfully developed great toe (already foreshadowing the great toe of man in its basic anatomy) was set off as the inner fork of a biramous clasping organ; in climbing movements the friction pads on the volar surface of this inner division of the foot were pressed against the bark of trees on the inner side, while the volar surfaces of metacarpals and digits II, III, IV, V, were pressed against the bark on the outer side.

In this pincer-like or clamping type of foot, preserved today in the living lemurs, the widely divergent, extended hallux cannot be drawn around parallel to the other digits for the reason that its motion is limited by a great olecranon-like process that projects from the proximal end of the first metatarsal and is received into a deep bony slot or cleft lying on the volar surface of the tarsus. To the back of the process itself is attached the thick tendon of the powerful peroneous longus muscle, as it is in all known primates, including man.

In the same work it was shown that as we pass from the Eocene and recent lemuroids and tarsioids to the South American monkeys, Old World monkeys, anthropoids and man, we observe on the whole a progressive reduction in the prominence of the peroneal process of the hallux and a corresponding improvement in the ability to draw the hallux from a position of wide divergence to a position more nearly alongside the outer digits. Of course one might assume arbitrarily that the series ran the other way, that the human foot is the most primitive and the Eocene *Notharctus* foot the most advanced; but such an assumption besides being dead against the palæontologic record as it stands, must ignore all

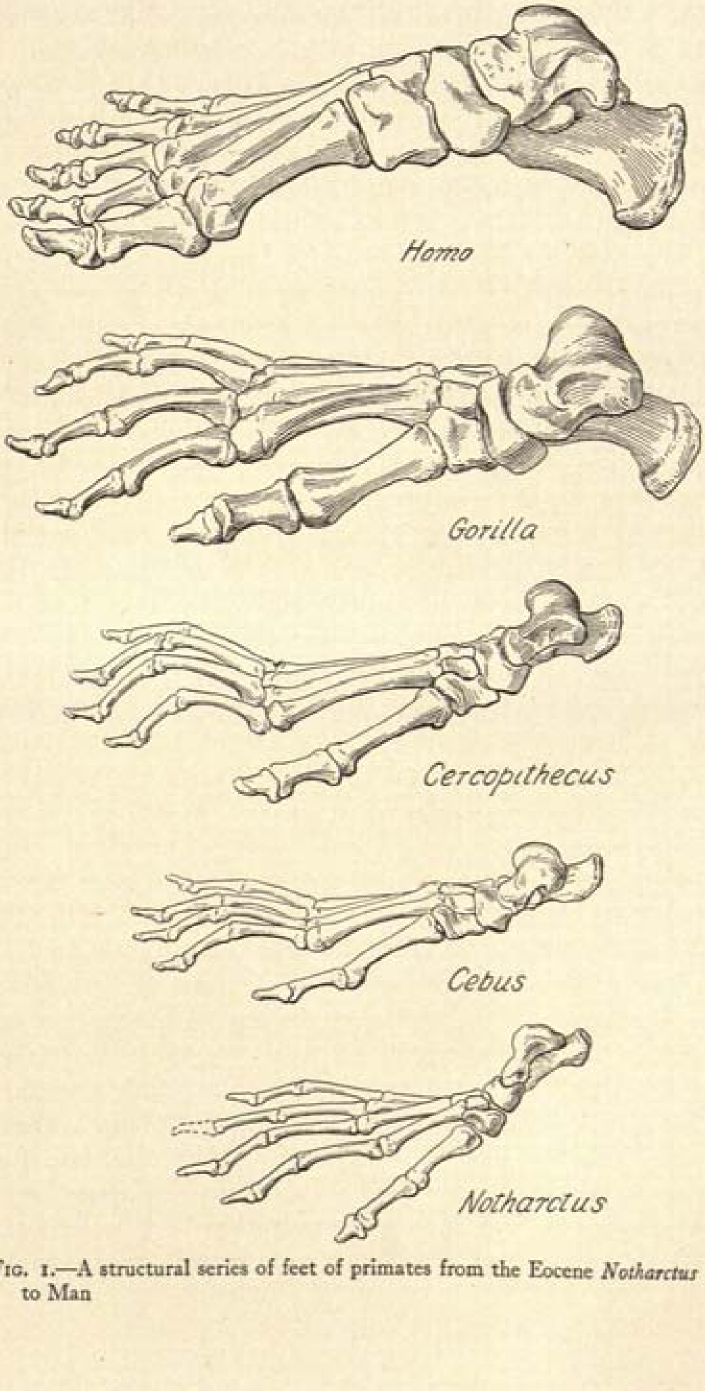


Fig. 1.—A structural series of feet of primates from the Eocene *Notharctus* to Man

the other evidence tending to show that the general progress in the evolution of the teeth, jaws, skull, brain, reproductive organs and many other parts, has been from Eocene lemuroids to primitive monkeys to primitive anthropoids to man. In short, all the facts known to me at the present time support the conclusions of 1916 to 1920 that the Primates as an order stand far apart from the terrestrial placental mammals, that the biramous type of hind foot was first evolved in the very remote tree-shrew-like ancestors of the primates in Upper Cretaceous times, that this biramous hind foot became the starting-point for the extensive deployment or adaptive radiation of the feet in response to the many different methods of locomotion assumed in lemuroids, tarsioids, New World monkeys, Old World monkeys, anthropoids and man.

To come at once to the main issue, the human foot itself appears to bear indelible traces of remote arboreal origin. What other valid explanation has been offered of the fact that in spite of millions of years of later terrestrial adaptation the foot of man is still from a morphological viewpoint distinctly biramous in the arrangement of its musculature, in the length and dominance of the great toe, in the presence of flat nails on all the digits, in the transmission of the weight of the body along two diverging streams, the scaphoid stream on the inner and the cuboid stream on the outer side of the foot? I therefore can find no logical alternative to the conclusion that man like all other known primates is a descendant of forms with a typically primate biramous type of hind foot which was evolved during the enormously long ages preceding the stage of terrestrial bipedal progression.

But it may still be asked why may we not derive directly the human foot from the general type displayed in primitive placental mammals of the Lower Eocene, without having to carry it through the elaborate and very indirect course of evolution suggested above? In response to such a suggestion I would submit a two-fold answer: first, the foot of man is assuredly far more like that of the gorilla, both internally and externally, than it is like that of any known non-primate foot of Eocene or later times; secondly, the wholly independent origin of the human foot could be true only in case the group of Bimana recognized by the old French naturalists were an entirely independent order not derived from any primate and of wholly unknown affinities. But here the evidence from all parts of the anatomy and physiology lies mountain high on the side of Linnæus's concept that man is a very primate of the Primates, and even more specifically in favor of Darwin's view (5, p. 206) that man is an offshoot from some primitive member of the anthropoid stock. In support of this statement I submit the evidence already at hand in the vast literature of the subject and in particular, the entire series (6) of papers by Keith, Elliot Smith, Tilney, Huntington, Wingate Todd, McGregor, Morton, Schultz, myself and many others. But the idea of the independent terrestrial origin of man reappears in the literature whenever the fundamental fact of man's intimate anatomical kinship to the brachiating anthropoids is forgotten. Let us consider then the necessary implications of the idea that the ancestors of man had been ground-living creatures long before the erect bipedal stage had been reached.

If the ancestors of man, during the enormously long reaches of time of the Eocene epoch, avoided the arboreal specializations of the entire order and if further, during the same period they were terrestrial or only partly arboreal animals derived directly from the primitive placental stock, how by escaping the Scylla of arboreal specialization, could they have avoided the Charybdis of terrestrial quadrupedal specialization? If they were terrestrial freely-moving quadrupeds for long periods would they not have suffered some of the numerous specializations and reductions that we commonly observe in many such cases that are known to palæontologists? Would they not, for instance, have lost the contact between the clavicle and acromion? Would they not have acquired more or less definite specializations in the limbs, such as the digitigrade posture, the restriction of limb movements largely to the antero-posterior plane, together with the well-known osteological marks that are commonly indicative of such habits? Why should the oldest known human skeleton have such large-sized thumbs and great toes if its ancestors had for long earlier periods been terrestrial cursorial quadrupeds? Is not the human skeleton famous for its complete avoidance of just such quadrupedal cursorial specializations? All of its five digits on hands and feet are undiminished, it shows no trace of digitigrade or unguligrade adaptations, its clavicle and acromion are intact, it retains the power of freely supinating the forearm, the proximal segments of its limbs are long while the metacarpals and metatarsals are short—the very opposite characters from those of cursorial quadrupeds. And indeed the human skeleton retains so many primitive mammalian characters that several authors have tried to turn the clock of evolution backward and make man the father of the lower mammals.

But if all the osteological evidence is definitely against the view that the early Tertiary ancestors of mankind were terrestrial cursorial quadrupeds, then, unless we seek for aquatic ancestors of man, must they not obviously have been either arboreal quadrupeds or arboreal bipeds? If they were arboreal quadrupeds and if the erect position was assumed after the animals came down out of the trees, then the human foot ought to be more like that of the baboon, in which there is a strong tendency for the middle digit to become the main axis. But Dr. Morton (7) has well shown the profound contrast of the human foot with the mesaxonic quadrupedal types and its equally profound agreement with the brachiating anthropoid type.

That the bipedal erect posture of man represents an old and deep-seated set of adaptations stretching well back into Tertiary time, is admitted by all. The very characters which man now retains but which he would surely have lost if he had become specialized as a terrestrial quadruped, such as the complete, well-developed clavicle, the broad round chest, the marked ability to rotate the humerus and supinate the forearm, all these and many others are found together elsewhere among the primates only in the brachiating apes. Indeed Keith (8) and others have not failed to draw the inference that man owes many of his deeper bipedal adaptations to erectly-moving, bipedal arboreal ancestors, namely the brachiating anthropoid stock. In fact the greatest contribution to the theory of man's origin since Darwin's time has been made not as yet through palæontological discoveries, however valuable these may be, but through the demonstration by Sir Arthur Keith and many others that all the basically human arrangements of the viscera, all the essentially human foundations of the brain, have been already achieved by those erectly-moving arboreal bipeds, the gibbons. In this connection Keith's admirable but technical summaries published in the *British Medical Journal* of March and April, 1923, have scarcely received the wide-spread attention they so well deserve.

One of the principal objections that has been raised by Professor Osborn (9) against the derivation of man from primitive brachiators may be stated fairly as follows: Brachiation, it is said, leads to the reduction and loss of the thumb; therefore as evolution is irreversible and as man's thumb is not reduced, he cannot be derived from a brachiating anthropoid.

An inspection of Fig. 2, however, will show that even after millions of years of extreme brachiating life the siamang's thumb is vigorously developed and the Mountain Gorilla's thumb is a powerful though somewhat short digit. It is only in the excessively specialized orang and to a less extent in the chimpanzee that brachiation is associated with severe reduction of the thumb. Thus in this siamang the thumb length is 56.6 per cent of the total hand length from the tip of the middle finger to the proximal end of the palm, while in this orang the thumb length drops to 44 per cent.

In this man the thumb length rises to 67.7 per cent of the total hand length. Now all admit that the thumb is a functionally dominant structure in man's hand and that its dominance is somehow connected with man's increased brain

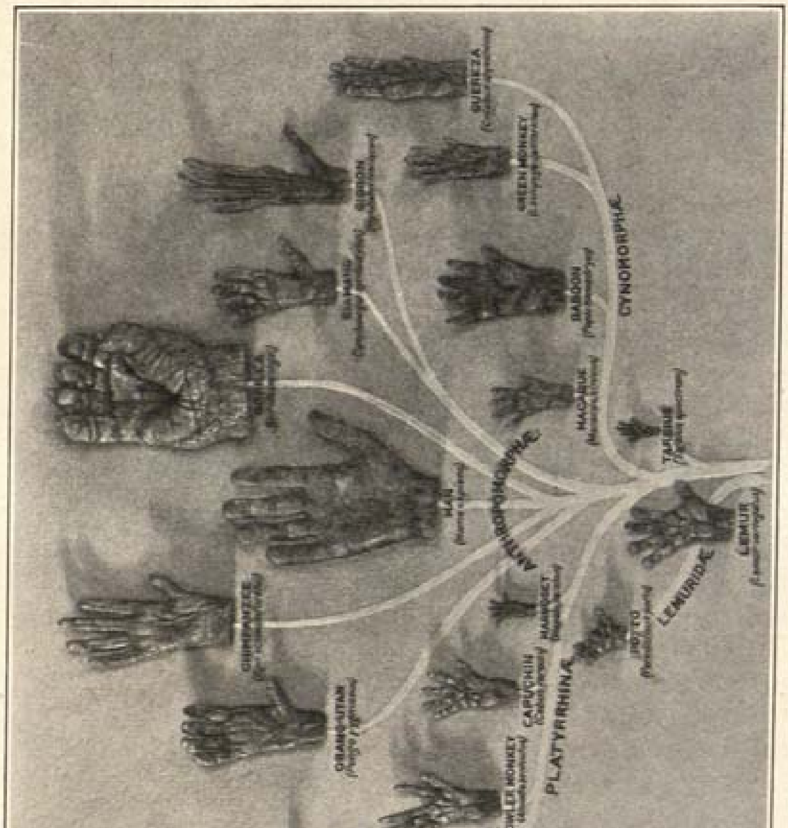


Fig. 2.—The hands of representative primates. Casts from specimens in American Museum of Natural History

power. Has any valid evidence been presented against the view that man's thumb, like his brain and his great toe, has enjoyed both a relative and an absolute increase in size?

The resemblance between the human hand and the hands of the chimpanzee and gorilla is by no means a superficial one. Professor J. E. von Boas (10), Sonntag (11) and other anatomists have exposed the many peculiar and significant morphological agreements in the anatomy of the hand between man and the chimpanzee and gorilla. In spite of the fact that the chimpanzee and the gorilla now walk on bent knuckles, their hands are unmistakably true hands and not merely front feet; and the brachiating anthropoids are the only known primates which closely approach man in this respect. And by as much as the human hand resembles those of the essentially brachiating chimpanzee and gorilla, internally as well as externally, by so much does it differ from the hands of the primitive pronograde monkeys, either of the Old World or of the New World. In the fœtus it is true the human hand is shorter than the gorilla hand (12), but that shows only that many generic differences arise in the fœtal stages, as modern embryologists well understand (13).

The derivation of the human hand from a primitive brachiating stage requires only a progressive increase in the functionally dominant thumb, a relative widening of the palm and a relative shortening of the other digits. In fact the differences between the human hand and the gorilla hand are far less profound than the differences between the human foot and the gorilla foot.

What is the necessity then for resorting to entirely hypothetical unknown families in tracing the origin of the human hand, when even the still surviving known forms of primates present such clear-cut successive stages from the pronograde forefoot of the lemurs to the brachiating true hand of the stem anthropoid, thence to the tool-manipulating hand of man? To put it another way, man's hand as a whole differs conspicuously from that of carnivores, rodents and members of all other orders and agrees in its general ordinal characters with the arboreal primates from the Eocene *Notharctus* up to *Gorilla*. More in detail, in every bone it shows unmistakable marks of close affinity with the hands of the brachiating great apes. This can easily be verified by inspection of the original specimens.

To Professor Osborn the habit of brachiation even in its earlier stages, by the law of irreversibility of evolution led the whole race of anthropoids into a hopeless cul-de-sac. To Keith, Morton and myself the habit of brachiation, at least in its earlier stages, proved to be the one way of turning the vertebral column at right angles to its former horizontal position and thus of opening the possibility of erect progression on the ground (14). Is there no way in which this unhappy deadlock of contending theories can be broken?

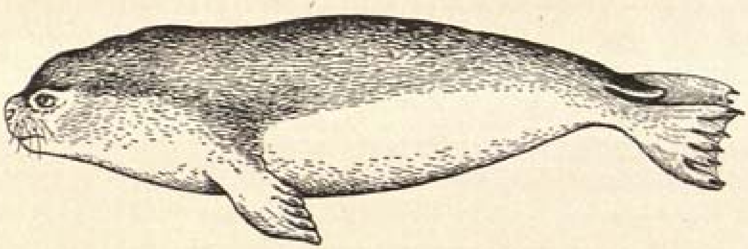


FIG. 3.—A harbor seal (*Phoca*). After Abel and from original specimen. To show the modifications of the hands and feet correlated with the change from terrestrial to aquatic life

In the light of Anton Dohrn's classic doctrine (15) known as the "change of function" ("Das Prinzip des Functionswechsels") we may I think see through the mist of the law of the irreversibility of evolution as it is nowadays so frequently misapplied. A single instance must suffice but examples could be multiplied. Inspection of Fig. 3 will show that in the forefoot of this seal (*Phoca* sp.), now transformed by change of function into a flipper, the thumb is far longer than the other digits in order to support the border of the paddle. Similarly in the hind foot both the great toe and the fifth digits have been greatly increased in length and strength. Now although the earlier palæontological history of the seals is a complete blank, the evidence from the brain and many other parts of the anatomy sufficiently proves that the seals are descendants of terrestrial placental carnivores of normal quadrupedal construction. But this in turn clearly implies that in the terrestrial ancestors of the seal the thumb and great toe were shorter than the other digits, as they are in all known carnivores. The "irreversibility of evolution" has therefore not prevented a profound remodelling and change of proportions in the relative lengths of the digits following upon a change of function. In man the increased length of the thumb and great toe appears to me to be just as secondary as it is in the seal.

If any part of man's anatomy should bear testimony as to the possibility of man's origin from brachiating ancestors, it ought to be his entire pectoral limb. In the brachiating apes the fore limbs subserve primarily the function of locomotion, but in man they serve chiefly for the manipulation of tools and weapons and the carrying of loads. Consequently whatever resemblances they may show are in spite of different functions.

In the work on *Notharctus* already mentioned (4) it was shown (Figs. 4, 5) that if we place the humeri of relatively primitive terrestrial quadrupedal mammals at the left of a comparative series then the humerus of *Notharctus* will come next to these, since it retains clear traces of a quadrupedal habitus but clearly foreshadows the primate types in some respects. Next toward the right must be placed the humeri of existing lemuroids, which exhibit varying degrees of elongation of the shaft, combined with the primitive quadrupedal features of an entepicondylar foramen and a dorsally produced supinator crest; next in line come the humeri of the South American monkeys, which retain the entepicondylar foramen but have lengthened the shaft in correlation with their greater reach in climbing. Then follow the humeri of the Old World monkeys, which have wholly lost the entepicondylar foramen. Of these the more arboreal forms (Pl. I,

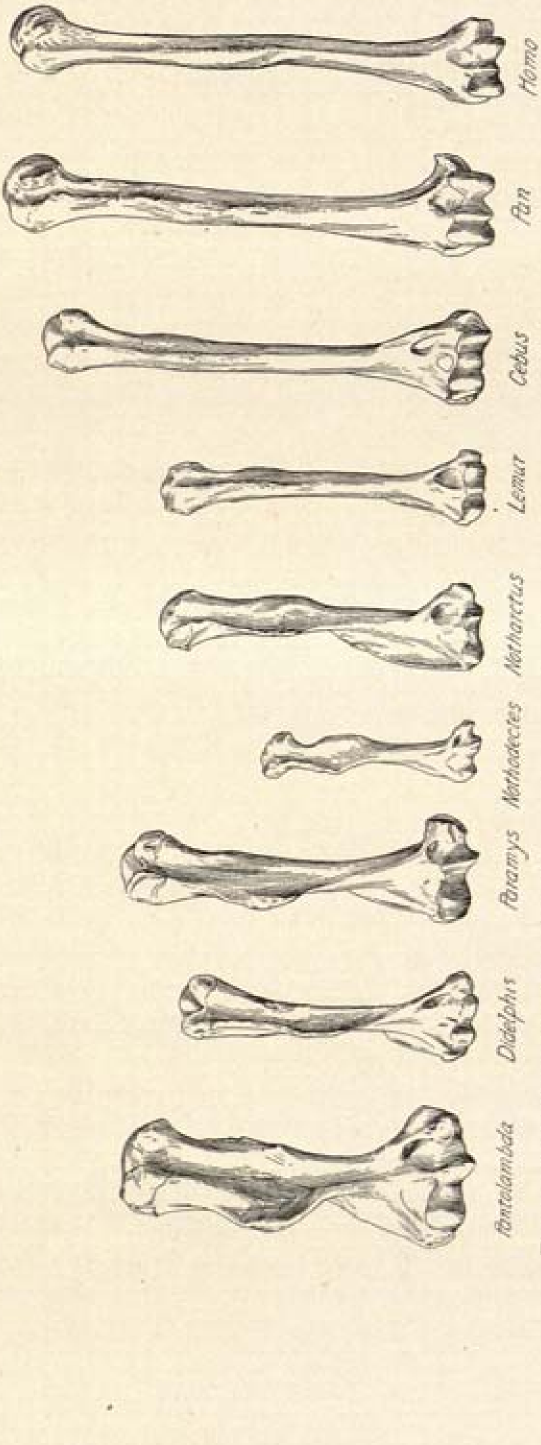


FIG. 4.—Humeri of pronograde terrestrial mammals, pronograde-arboreal lemuroids and monkeys, brachiating anthropoid and man. Front view

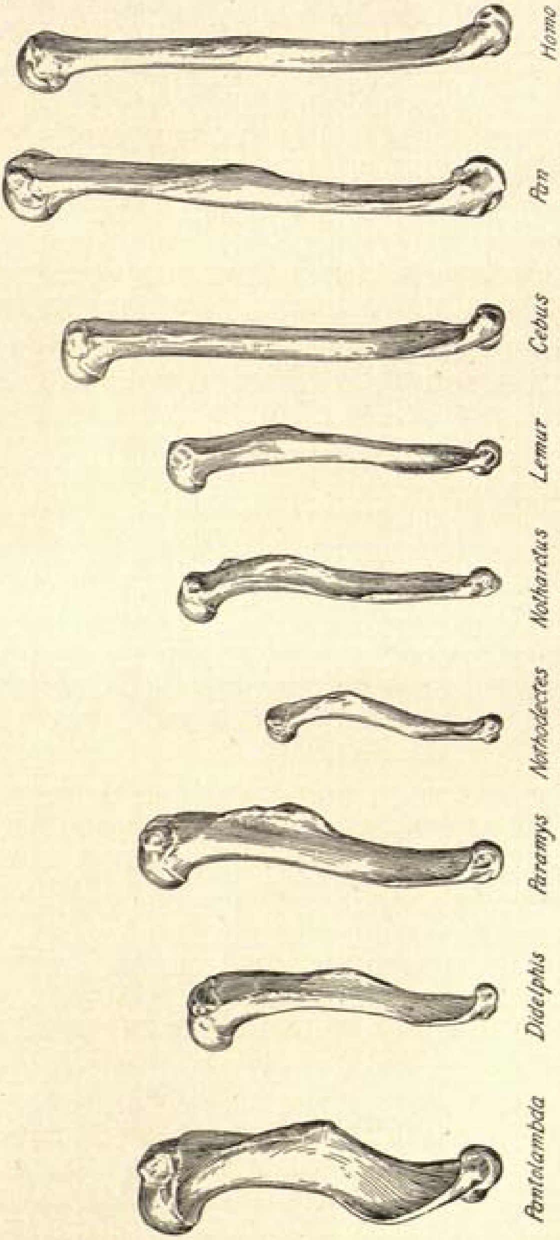


FIG. 5.—Same series. Side view

H) carry the line of evolution toward the anthropoid types, but in the baboons (Pl. I, G), which are secondarily terrestrial, the humerus is distinguished by the enlargement of the deltopectoral crest and the swollen form of the greater and lesser tuberosities. Then come the anthropoids, headed by the gibbon (Pl. I, F), in all of which the shaft of the humerus is long and cylindrical, the head large and globular, the deltopectoral crest reduced and the great tuberosity reduced to a peculiar low protuberance, which with its fellow the lesser tuberosity flanks the bicipital gutter. At the distal end there is no trace of the entepicondylar foramen and the short supinator crest is directed obliquely upward toward the shaft. Suppose now we pick up a human humerus (Fig. 4 and Pl. I, A) and compare it with each one of this series. It is assuredly very different from all the primitive quadrupedal types; it differs conspicuously from the secondarily quadrupedal baboon in its long cylindrical shaft, globular head and reduced tuberosities. When we come to the gibbon's humerus, however, we see a marked increase in resemblance to the human specimen, and when we compare the latter point by point with those of the chimpanzee and the gorilla, we recognize in the proximal end and shaft surprisingly close agreements; only the distal end shows a thinner outer border of the supinator crest, a less pronounced ridging of the trochlea and a few other rather inconspicuous but perhaps important points of difference.

In preparation for the present paper I took the few anthropoid humeri and the first human humerus that happened to come to hand and laid them out in the series shown in these photographs (Pls. I–IV). I think there can be no question of the strong structural resemblance of this human humerus to the brachiating types of the recent anthropoids. The measurements of these specimens (Table I) show this human humerus to be surprisingly close to that of this particular chimpanzee. In other words, although no longer a brachiating animal, man retains a humerus of brachiating habitus so nearly allied in structure to those of the anthropoid apes, especially the gorilla and the chimpanzee, that the ascription

TABLE I

	Gibbon	Orang	Gorilla	Gorilla	Gorilla	Chimpanzee	Man	ANTHROPOID APES Extremes	ANTHROPOID APES Mean	Chimpanzee	Man	Baboon	Macaque	*Cebus*	Lemur	*Nothardus*
Humerus length (l)	230	427	450	472	409	330.5	314	230 –472	351	330	314	237	123	113	107	70.5
Breadth across ento- and ecto-condyles (b)	27	72	100	119	94	65	63	27 –119	73	65	63	41	19	21	25	19.5
Index $\frac{b \times 100}{l}$	11.7	16.8	22.2	25.2	22.9	19.6	20	11.7– 25.2	18.4	19.6	20	17.3	15.4	18.6	23.3	27.6
Breadth mid-shaft (b')	9	22	35	40	30	21	20	9 – 40	24.5	21	20	17	8	7	7	6
Index $\frac{b' \times 100}{l}$	3.9	5.1	7.7	8.4	7.3	6.3	6.3	3.9– 8.4	6.1	6.3	6.3	7.1	6.5	6.2	6.5	8.5
Width across head to greater tuberosity (w)	20	56	70	80	63	48	50	20 – 80	50	48	50	41	18.8	16.5	17	11.5
Index $\frac{w \times 100}{l}$	8.7	13.1	15.5	16.9	15.4	14.5	15.9	8.7 – 16.9	12.8	14.5	15.9	17.3	15.3	14.6	15.9	16.3

of these resemblances to convergent evolution would be a bold *petio principii*.

In conclusion, it has been shown by many investigators that in the anatomy of the locomotor apparatus as a whole man is indubitably nearer to the brachiating chimpanzee and to the formerly brachiating gorilla than to the pronograde or arboreal quadrupedal monkeys. The observations on the comparative osteology of the humerus recorded in the present paper tell in the same direction.

That man now differs from the chimpanzee in the relative lengths of his limb segments is not to be wondered at, in view of millions of years of divergent specialization. His long legs, short arms, highly progressive thumb and perfected foot belong to his long continued and relatively recent terrestrial habitus, but the biramous internal construction of his foot, the essential musculature of his foot and hand and a host of adjustments to the upright gait in brain, in skeleton and in viscera, are part of his heritage from primitive brachiating ancestors. These characters, along with many striking peculiarities of the dentition and of the brain, to say nothing of physiological tests, tie him irrevocably to the primitive brachiating apes and at the same time separate him from all the known pronograde monkeys.

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* See also Straus, W. L., Jr., 1927, "Growth of the Human Foot and its Evolutionary Significance," Contrib. to Embryol., No. 101, Vol. XIX, Carnegie Inst. Washington, Publication No. 380, pp. 93–134; and McMurrich, J. Playfair, 1927, "The Evolution of the Human Foot," *Amer. Jour. Phys. Anthrop.*, Vol. X, No. 2, pp. 165–171. Weidenreich F., 1921–22, Der Menschenfuss. *Zeitschr. f. Morphol. u. Anthropol.*, Bd. 22, Heft 1–2, S. 51–282.

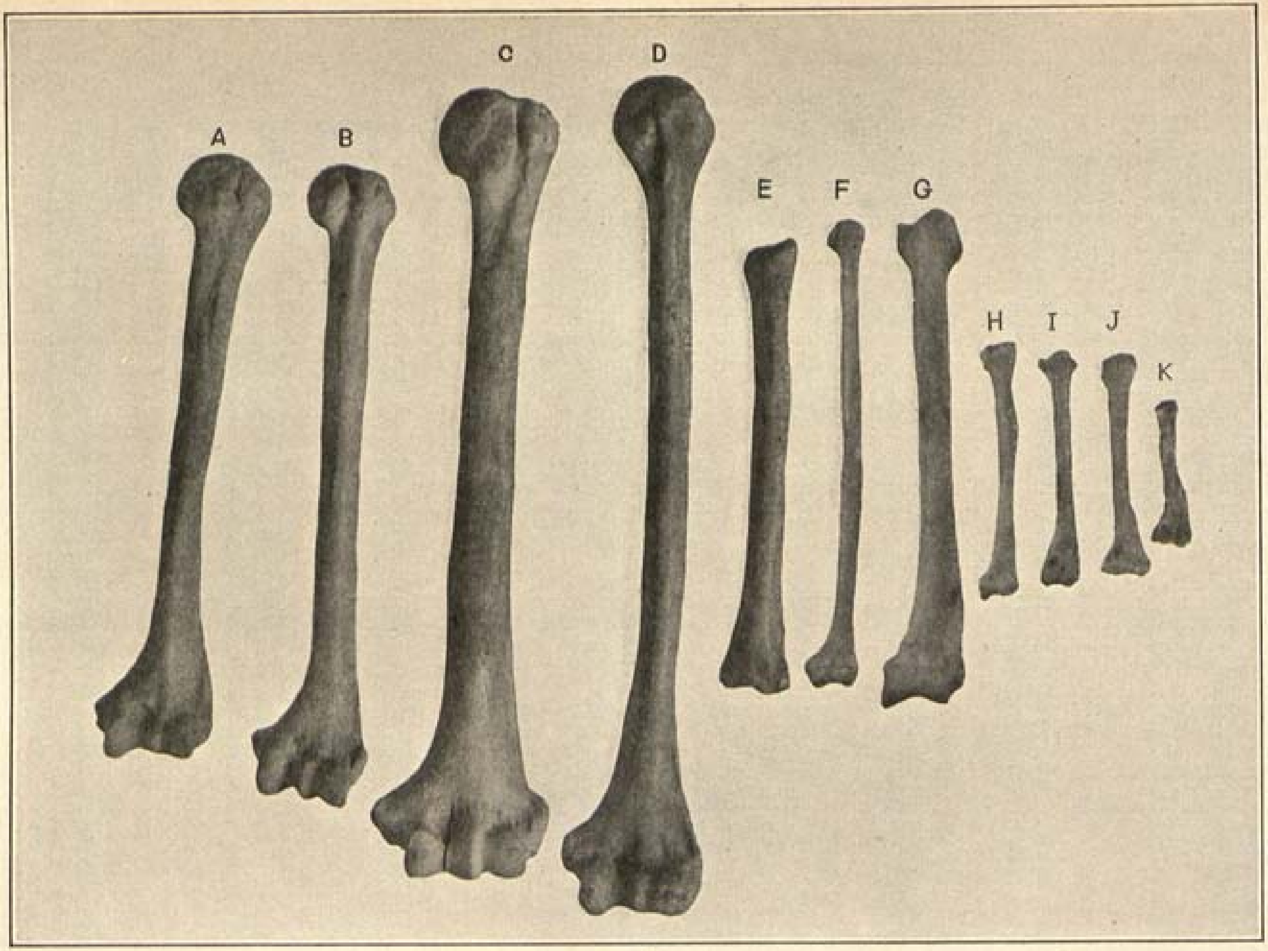

PLATE I.—Series of humeri of primates. Front View. A, Man; B, Chimpanzee; C, Gorilla; D, Orang; E, *Dryopithecus* (cast); F, Gibbon; G, Baboon; H, Macaque; I, *Cebus;* J, Lemur; K, *Notharctus*

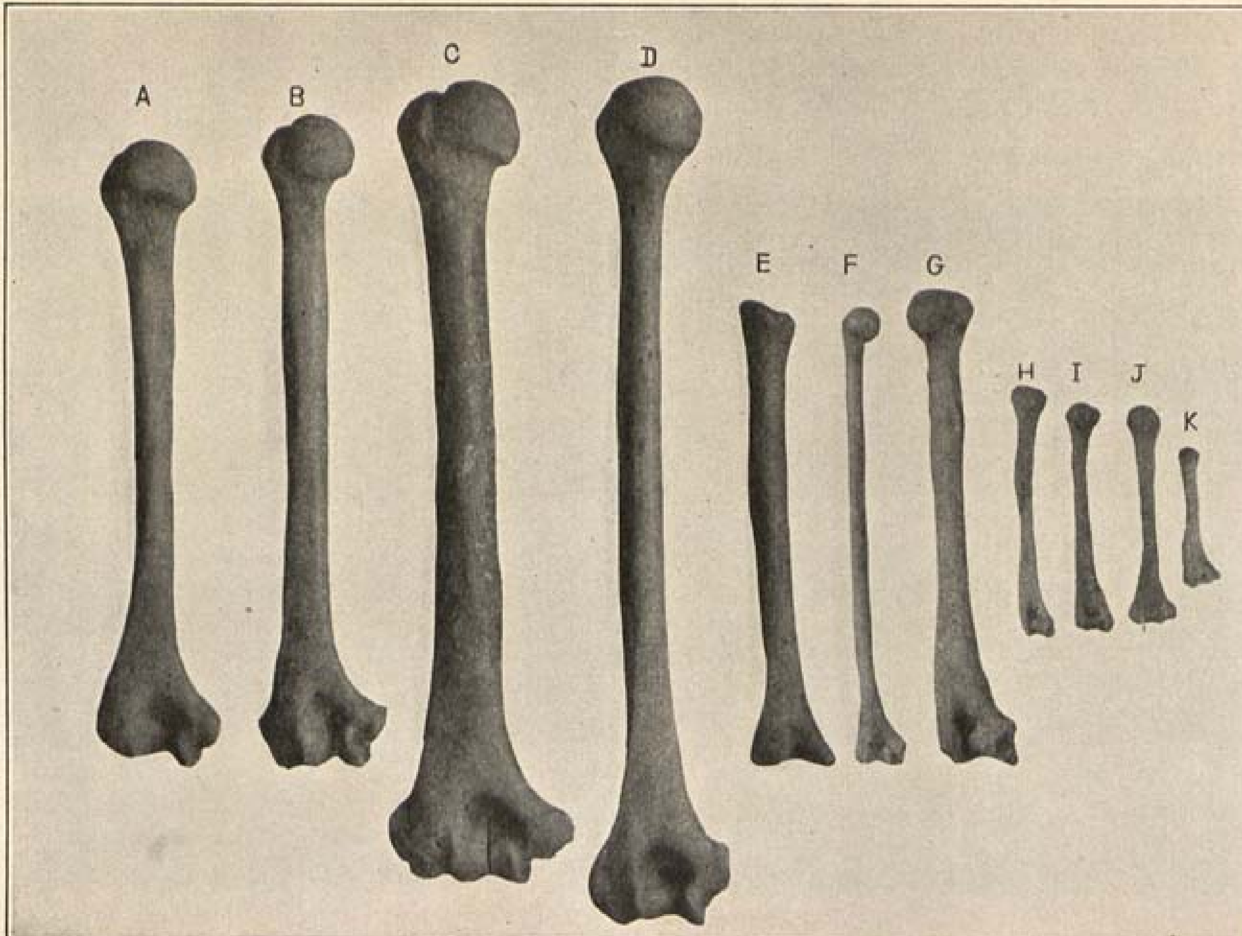

PLATE II.—Same series. Back view

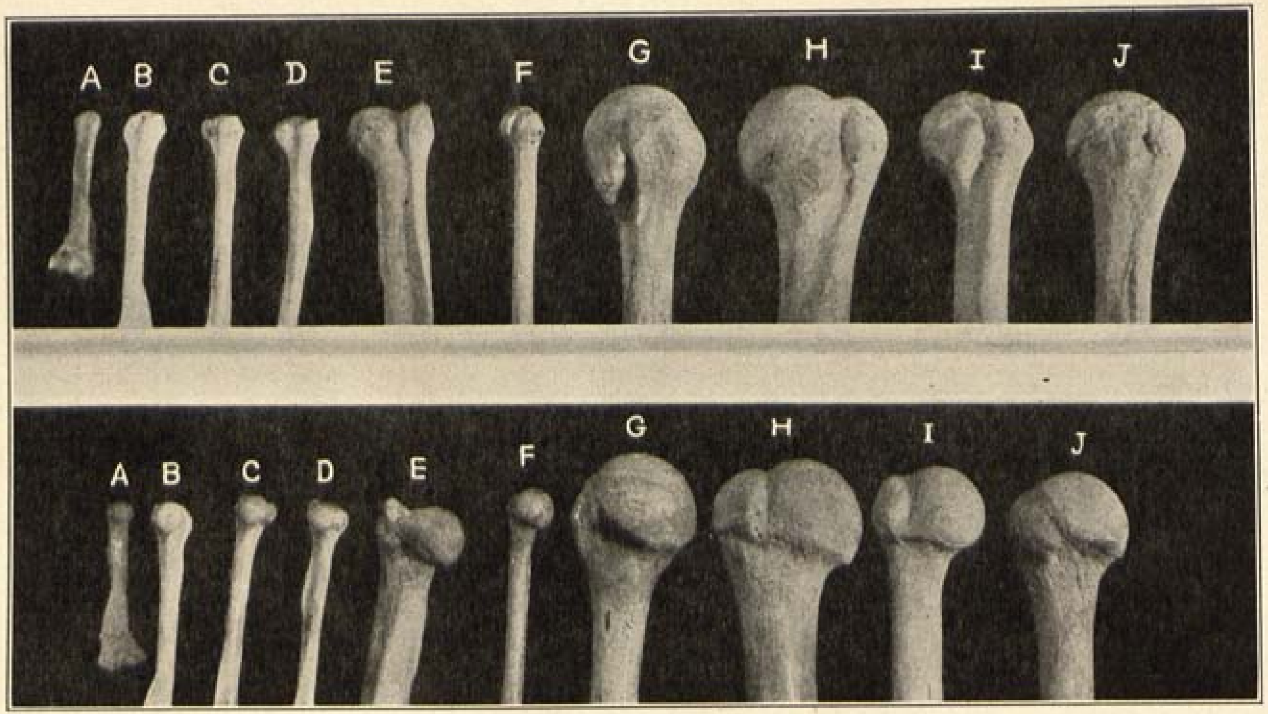

PLATE III.—Series of humeri of primates. Upper half, front view. A, Notharctus; B, Lemur; C, *Cebus;* D, Macaque; E, Baboon; F, Gibbon; G, Orang; H, Gorilla; I, Chimpanzee; J, Man. Same series. Back view

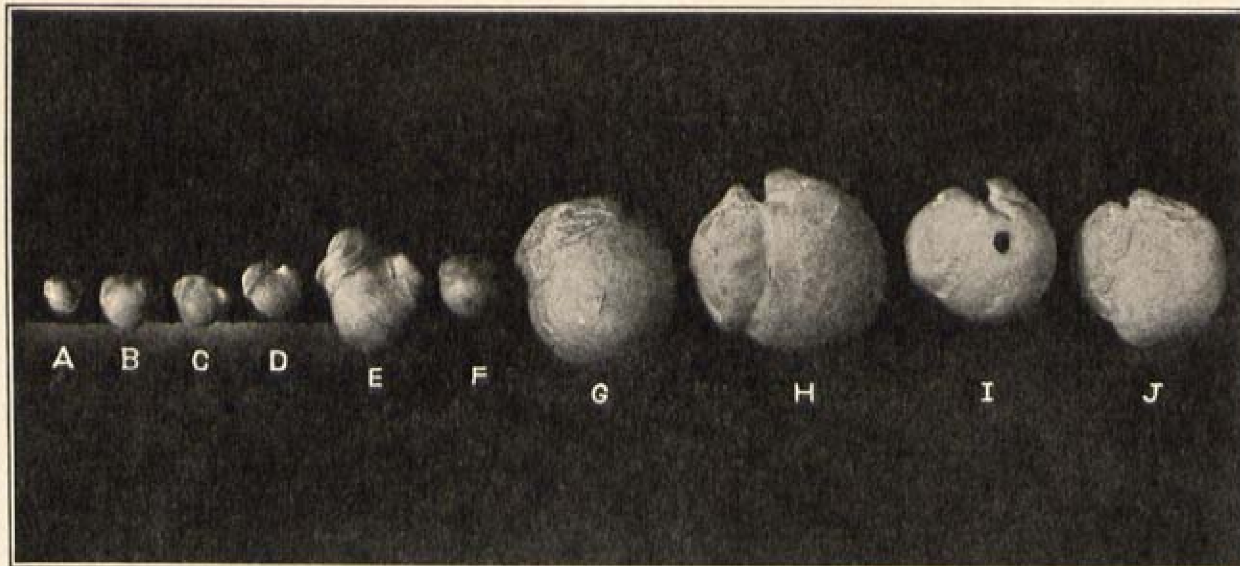

Plate IV.—Same series. Proximal view

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PRESENT STATUS OF THE PROBLEM OF HUMAN ANCESTRY

By HENRY FAIRFIELD OSBORN

Vice-president, American Philosophical Society; Senior Geologist, United States Geological Survey; Research Professor of Zoology, Columbia University

(*Read April 20, 1928*)

Before the American Philosophical Society April 20, 1928, Professor Osborn renewed his attack upon the ape-man theory of human ancestry which he began at the bicentenary meeting of the Society in April, 1927. Referring to the Central Asiatic Expedition of the American Museum of Natural History which left Peking on April 14 bound for the previously discovered fossil horizon of Oligocene age, he asked the anatomists present what kind of Primate was likely to be found in these beds. In the opinion of all the opponents of the new-Dawn Man theory, including Sir Arthur Keith and probably all students of human evolution in America, the Central Asiatic Expedition should find a primitive fossil ape much more generalized and less specialized than the living chimpanzee but still unmistakably an ape. On the contrary, according to the pro-Dawn Man theory which Professor Osborn is now advocating, the Oligocene rocks of central Asia will sooner or later reveal the existence at that remote period, now estimated as fifteen million years ago, of a distinctly prophetic pre-human type only remotely resembling any fossil or existing form of ape, but exhibiting in the limbs, in the hands and feet, and especially in the structure of the brain the *potentiality* of developing into a highly progressive human type.

In this address Professor Osborn continues as follows:

Grounds for this pro-Dawn Man theory are directly derived in part from existing embryological, anatomical, and zoölogical evidence, in part from certain principles of animal descent or phylogeny which were entirely unknown in the period when Charles Darwin published his classic work, "The Descent of Man" (1871), and the shortly succeeding period when Huxley wrote his famous essay "Man's Place in Nature." Although following Lamarck, who early in the nineteenth century sketched the apes as human ancestors, Charles Darwin required great courage to draw in 1871 the following picture of our ancestors:

> The early progenitors of man must have been once covered with hair, both sexes having beards; their ears were probably pointed, and capable of movement; and their bodies were provided with a tail, having the proper muscles. Their limbs and bodies were also acted on by many muscles which now only occasionally reappear, but are normally present in the Quadrumana. At this or some earlier period, the great artery and nerve of the humerus ran through a supra-condyloid foramen. The intestine gave forth a much larger diverticulum or cæcum than that now existing. The foot was then prehensile, judging from the condition of the great toe in the fœtus; and *our progenitors, no doubt, were arboreal in their habits, and frequented some warm, forest-clad land.*

I take the liberty of italicizing the two most salient lines in this oft-quoted passage because they give the key to the thought of Darwin and of subsequent advocates of the ape-man theory, down to the present time. Over against such a habitat which has framed the structure of all anthropoid apes may be placed the conclusion theoretically reached by the brilliant geologist Joseph Barrell in 1917 and independently reached by myself by direct observation during my journey of 1923 into the heart of the desert of Gobi. To my knowledge Barrell was the first to formulate what may be called a semi-arid plateau theory of the origin of man, as recently quoted by Charles Berkey in "Geology of Mongolia": [1]

> . . . Among the many suggestive thoughts offered by Joseph Barrell (1917) as guiding hypotheses for our explorations in central Asia, his idea about primitive man is especially ingenious. Man's strong padded foot, his relatively long leg and his erect posture,

[1] Charles P. Berkey and Frederick K. Morris, "Geology of Mongolia, Natural History of Central Asia," Vol. II. Published by the American Museum of Natural History, New York, 1927.

are all distinct departures from an adaptation to life in the trees, and tend, instead, to fit him for running and for tramping long distances; in short, for life on open plains where trees grow in patches along the stream courses, rather than for life in a dense forest. Granting that the more distant ancestors of men lived in trees and in jungles, it seems probable that they would have remained arboreal in an environment of jungle and forest. But in a region where forests were thinning, where open, treeless plains were beginning to appear, and where the climate was changing toward cooler and more arid conditions, it seems probable that arboreal types must adapt themselves to the plains, or become extinct.

I am not conscious of having seen or heard of Barrell's generalization prior to my own discovery of the same principle, which I enunciated before a gathering of geologists and natural philosophers in Peking:[1]

Mongolia was probably not a densely forested country—this is indicated by the animal remains found there in the earlier deposits. An alert race cannot develop in a forest—a forested country can never be a center of radiation for man. Nor can the higher type of man develop in a lowland river-bottom country with plentiful food and luxuriant vegetation. It is upon the plateaus and relatively level uplands that life is most exacting and response to stimulus most beneficial. Mongolia always has been an upland country, through the Age of Mammals and before. It was probably a region forested only in part, mainly open, with exhilarating climate and with conditions sufficiently difficult to require healthy exertion in obtaining food supply. . . .

In the uplands of Mongolia conditions of life were apparently ideal for the development of early man, and since all the evidence points to Asia as the place of origin of man, and to Mongolia and Tibet, the top of the world, as the most favorable geographic center in Asia for such an event, we may have hopes of finding the remote ancestors of man in this section of the country. However, this Mongolian idea must be treated only as an opinion; it is not yet a theory, but the opinion is sufficiently sound to warrant further extended investigation.

No pro-human habitat could present a wider contrast to Darwin's "warm, forest-clad land." All recent ethnologic

[1] H. F. Osborn, "Why Mongolia May be the Home of Primitive Man," *The Peking Leader*, October 10, 1923.

and physiographic evidence points in the same way, namely, that intelligent, progressive and self-adaptive types of mankind arise in elevated upland or semi-arid environments where the struggle for food is intense and where reliance is made on the invention and development of implements as well as weapons. On the contrary, there is no premium on invention, intelligence or self-adaptation in animals of any kind living in warm forests.

Granting all the very strong *circumstantial* evidence in favor of the ape-man theory, which has been piled mountain high by investigators since the time of Darwin and has been recently revived and stimulated to new force by the attacks of the fundamentalists on the whole evolution theory, we must look for the direct evidence which can come only from geology and palæontology. The final solution of this problem of problems therefore rests with the fossil hunter and explorer, whose task is an extremely difficult one because fossil remains of Primates, always scarce, are becoming increasingly scarce as the Primates rise in the scale of intelligence. I do not know the exact figures, but I think it is safe to say that 50,000 to 1 is about the ratio of probability of discovery of the fossil remains of lower orders to the remains of Primates of Tertiary time.

Meanwhile, the circumstantial evidence of geology and of geography is all in favor of the theory that the pro-man stock was well established in Oligocene time, now conservatively estimated at 16,000,000 years ago. At this time occurred the first *modernization* of the entire mammalian kingdom. So far as we can observe geologically, this modernization was due to the first great wave of aridity concurrent with the complete elevation of great continental plateaus, especially in central Asia and in the western region of North America. This wave of aridity and the elevation caused a profound cleavage in the mammalian world, the first great natural divorce between the warm forest-loving types developed during the preceding Eocene period and the temperate plains and plateau-loving types which apparently invaded the great

Oligocene belt of the 40th parallel from the North. This cleavage profoundly affected the whole mammalian world of this region; not only the horses, rhinoceroses, tapirs, and even-toed animals like the progenitors of the deer, the cattle, and the camel families had to make their choice between forest regions and the plains, but the carnivorous enemies—wolves and foxes and the progenitors of the greater carnivores in the cat family—were compelled to go forest-ward or plains-ward. It is not at all probable that the Primates—lemurs, North and South American monkeys, apes, and the hypothetic division of pro-man—were exempt from this compelling and fateful decision. Why was it postponed by the progressive progenitors of man when adopted by all the progressive elements in the remaining mammalian world? Why theoretically postpone this fateful decision on the part of our primate ancestors to Miocene or Pliocene time as is still done by many conservative writers who continue to adhere to the abandoned conceptions of the period of Charles Darwin's speculation partly because of loyalty to him and reverence for his classic contribution to anthropology?

This concludes the seventh address which I have devoted to this absorbing subject. In the succeeding or eighth address I shall continue the attack and try to demonstrate that while the anatomical and embryological evidence for the *kinship* of the apes to man is overwhelming, the same evidence, when closely analyzed and subjected to conditions of modern principles of phylogeny discovered since Darwin's time, compels us to replace the ape-man theory hypothesis by the new pro-Dawn Man theory.

RACIAL CHARACTERS IN HUMAN DENTITION

Part I

A Racial Distribution of the Dryopithecus Pattern and Its Modifications in the Lower Molar Teeth of Man

By MILO HELLMAN

Research Associate in Physical Anthropology, American Museum of Natural History; Professor of Comparative Dental Morphology, New York University College of Dentistry

(*Read April 20, 1928*)

Gregory's works "Studies on the Evolution of the Primates" (1916) and "The Evolution of The Human Dentition," (1922) and Gregory and Hellman's work "The Dentition of Dryopithecus" (1926) have given a new impetus to the study of tooth forms. The facts brought to light add new significance to the morphological characters of the human teeth, and furnish reliable means for disentangling many confusing elements that usually enter into the general problem of racial distinctions. Applying these results to the study concerned in "A Racial Distribution of the Dryopithecus Pattern and its Modifications in the Lower Molar Teeth of Man," it is possible to point out:

1. That the fundamental pattern of the human lower molar teeth is essentially primitive, that is, of anthropoid origin;
2. That the pattern is undergoing a gradual and progressive change in modern man;
3. That the progressive nature of this change may be recognized by certain well-defined stages;
4. That the change is taking place in many races of mankind; and
5. That the degree of progressive modification may be measured by the frequency with which the advanced stages are reached by different racial groups.

1. The Fundamental Pattern of the Human Lower Molar Teeth

The fundamental pattern of the lower molar teeth of man is not specifically human. It is shared equally by all anthropoid apes and is derived from a primitive stem form commonly related to both man and apes. In order to give a general idea of this pattern I shall direct your attention to those features which render it primitive at first, and which by subsequent changes transform the primitive into the more advanced [1] type. Fig. 1 presents the crown view of a human lower first molar tooth. In its outline it is somewhat rhomboid in form. The masticating surface, as shown in the illustration, presents elevations and depressions. The elevations are the cusps. There are five of them in the human lower first molar, three on the outer side and two on the inner side. A cusp terminology was worked out by Professor Osborn in 1888. These terms name each cusp in accordance with its first appearance. It requires some technical knowledge and skill, however, to be able to understand these terms with some degree of confidence. On this account and also for the purpose of making the matter more simple and expedient, Professor Gregory suggested that the cusps should be designated by numbers. The outer cusps are thus numbered 1, 3 and 5, and the inner 2 and 4.

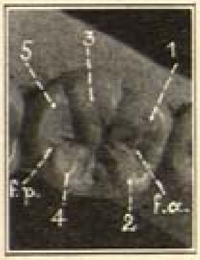


Fig. 1.—Lower first molar tooth of American Indian from Tarasco, Mexico; Showing various features entering into the composition of the "Dryopithecus Pattern"

The cusps, however, are not all alike. There are some differences to be noted in their form. The outer cusps, for example, are low and rounded, while the inner are higher and pointed. It should also be noted that the outer cusps are separated from the inner, and all cusps are separated from

[1] The term "advanced" is used in the sense of being furthest removed from the primitive condition. It is not meant to imply that the change is either beneficial or detrimental.

each other by valley-like depressions called sulci. At the bottom of these valleys or sulci there are cleft-like furrows or grooves to be observed. The groove extending lengthwise in the valley between the outer and inner cusps, begins in a transverse depression in front of the cusps 1 and 2, called *fovea anterior*, and ends in a depression behind cusp 4, called *fovea posterior*. The shorter transverse grooves, two on the outer side and one on the inner side, branch out from the longitudinal groove and extend cheekward and tongueward. In a typical human lower first molar crown, these grooves

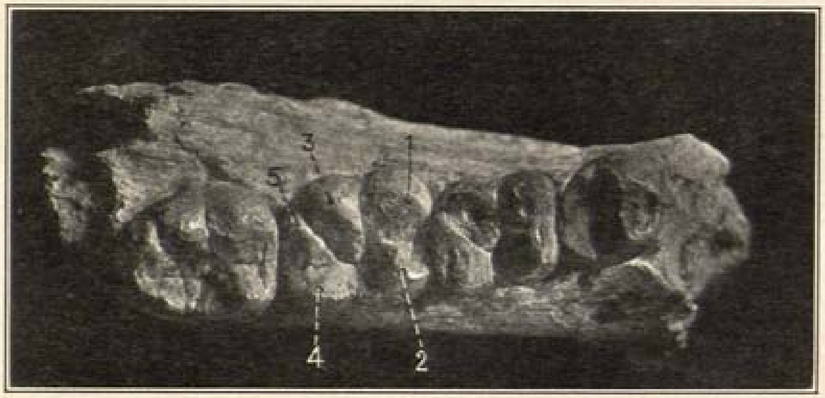


Fig. 2.—Left side of mandible of *Dryopithecus frickæ*, of the Miocene of India, showing the earliest manifestation of the "Dryopithecus pattern"

are distributed in a definite way. As the two outer grooves extend from the outer toward the inner part of the tooth, they converge meeting the inner groove at about the center of the crown. The figure thus formed by the three grooves resembles quite closely that of the letter Y, the outer furrows forming the diverging limbs and the inner the stem.

There are other features contributing to the formation of this groove system. These are the crests of the four main cusps (1, 3, 2 and 4) of the molar crowns. As may be seen in the illustration, there are prominent crests extending from the tips of the cusps toward the longitudinal furrow. Some of these crests approach each other in a definite way. For

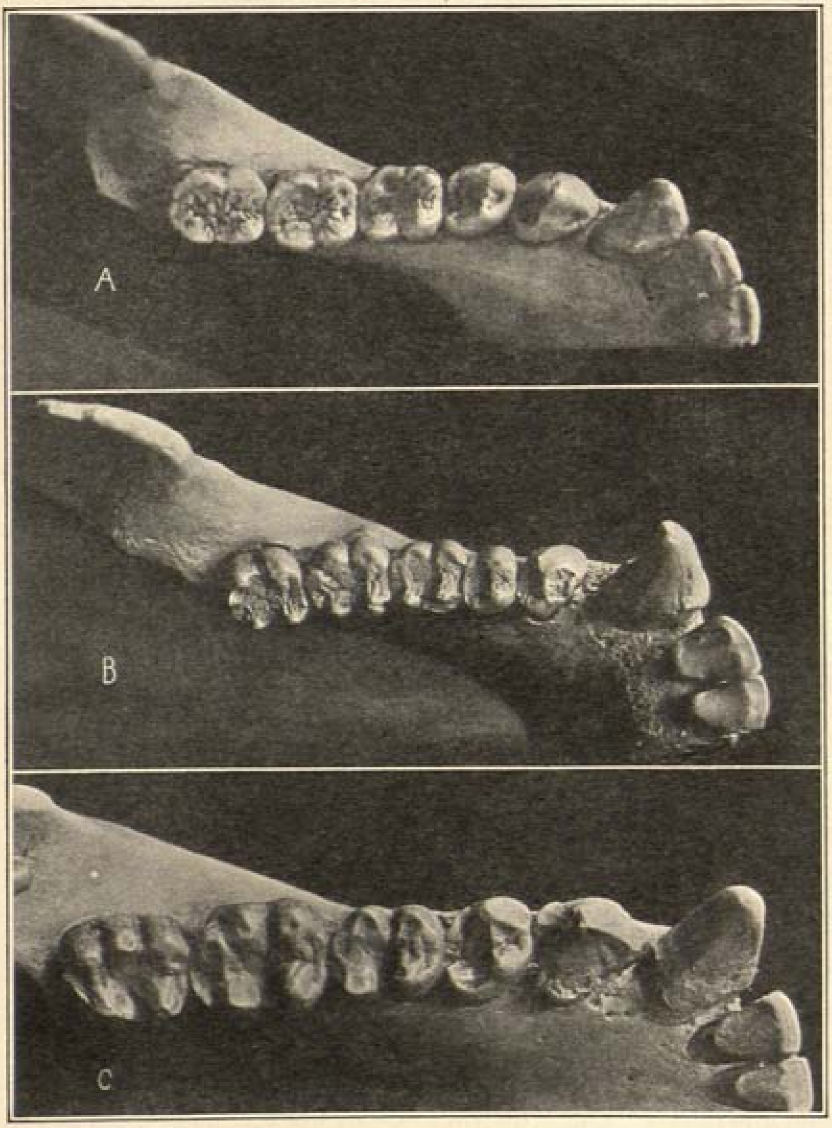


FIG. 3.—Left half of jaws of Orang (A), Chimpanzee (B), and Gorilla (C), showing presence of "Dryopithecus pattern" on all lower molar teeth. Diverse modifications in Orang and Chimpanzee in form of enamel wrinkles tend to obscure it, but the general outline is clearly discernible

example, it will be seen that the base of the crest of cusp 2 comes into contact with the base of the crest of cusp 3, the bases of the crests of cusps 1 and 4 remaining separated. *This whole pattern of five main cusps, the longitudinal groove with its terminal depressions, the foveæ anterior and posterior, and the Y-shaped system of transverse grooves was named by Gregory in 1916 the "Dryopithecus pattern"* of the lower molars. To this must also be added the contact of cusps 2, 3.

This pattern is found fully established in the lower molar teeth of all known species of the extinct ape named *Dryopithecus* and in the allied genera from the Miocene of Europe and of India. Fig. 2 presents the left half of a lower jaw of the fossil anthropoid *Dryopithecus* collected by Barnum Brown of the American Museum Expedition to the Siwaliks of India. The molar pattern described above may be seen

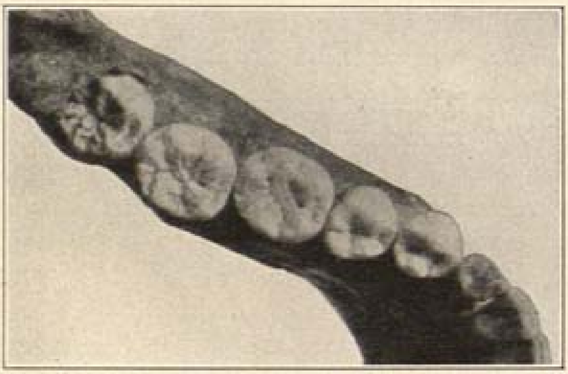


FIG. 4.—Left side of jaw of Mousterian Youth, showing presence of "Dryopithecus pattern" on all lower molar teeth

in every detail in all three molar teeth. That this pattern is also found intact on the lower molar teeth of all genera of the modern anthropoid apes, may be seen in Figs. 3, C, Gorilla; B, Chimpanzee, and A, Orang. In Gorilla the pattern is as clearly defined in each of the molars as in *Dryopithecus*. There are, however, certain diverse modifications in the form of enamel wrinkles seen in Chimpanzee and Orang tending to obscure the groove system; the fact, nevertheless, remains that the fundamental pattern as a whole is present and noticeable in all the teeth of the anthropoids as originally inherited from *Dryopithecus*.

In the extinct hominidæ too this pattern is found on all lower molar teeth. Examples of this may be seen in the lower molar teeth of Piltdown Man, of Heidelberg Man, in the Eringsdorf Child. The Mousterian Youth, Fig. 4, furnishes the clearest evidence of the presence of the *Dryopithecus* pattern on all lower molars. This evidence lends support to the view that this pattern is fundamentally primitive, meaning, of course, as originally inherited from *Dryopithecus*, and like many other structural characters is present in early man and in modern anthropoids.

2. The Gradual and Progressive Changes of the Dryopithecus Pattern in the Lower Teeth of Modern Man

In modern man this pattern is undergoing a marked change. This change is at once manifest when the three lower molar teeth of a modern human mandible are compared. Note for example Fig. 5, A and B, illustrating the lower molar crowns of a modern Hindu male and a Bedouin female. While the first molar crown retains the fully formed Dryopithecus pattern, the second and third molars are changed. The changes taking place are as follows: the general contour becomes more rounded, both in the second and third molars; the antero-external corner is flattened and not as angulate as it is in the first molar. In the third molar the primitive number of cusps is either retained as in Fig. 5, A, or reduced as in Fig. 5, B; but the system of grooves is changed in both instances. In the second molar both the number of cusps and system of grooves are changed. The cusps are reduced from 5 to 4 and the groove system is changed from the Y to the cross. There is another change occurring in which the cusp number is reduced but the primitive system of grooves is retained; *i.e.*, the Y system persists despite the fact that the posterior limb has disappeared. Fig. 7, B, illustrates this change quite clearly, the five original cusps being reduced but the system of grooves is unchanged, excepting the loss of the posterior limb of the Y-system of grooves.

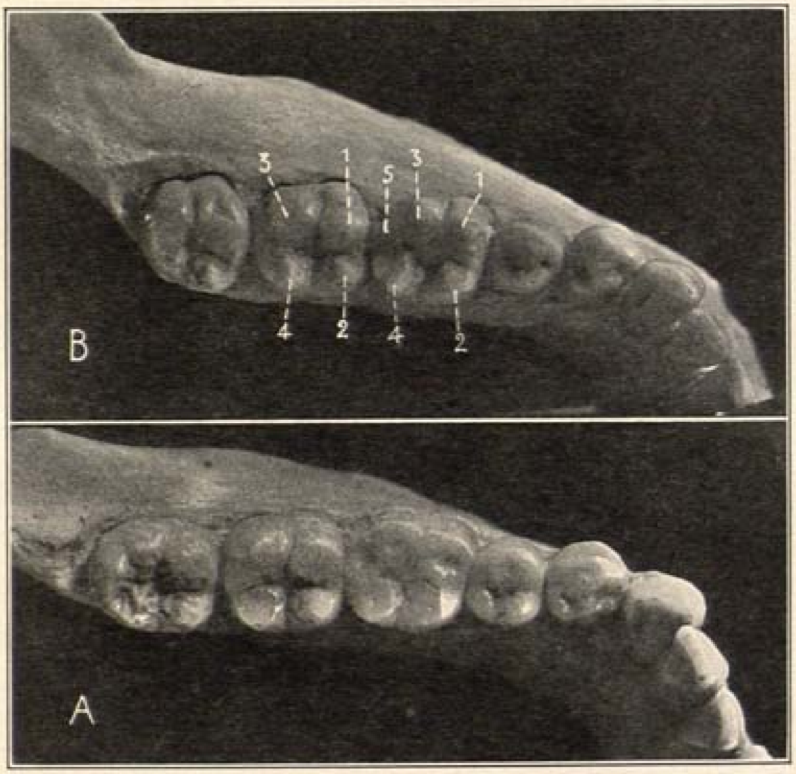


FIG. 5.—Left side of jaws of Hindu Male (A) and Bedouin female (B), showing changes in pattern of lower molars in modern human beings

There is, however, another change taking place in which the cusp number is increased. In this change certain cusps are added on to those already present. Thus in Fig. 6, representing the lower teeth of an American Indian, there is an extra cusp present. This cusp is denoted by the number 6. In the illustration it may be seen that in addition to the original 5 cusps on the first and the third molar there is also a cusp 6. Also in the second molar this cusp 6 is present despite the fact that the primary number of cusps has been reduced to four. An incipient cusp 7 may also be seen between the cusps 2 and 4 in these teeth. Certain odontographers

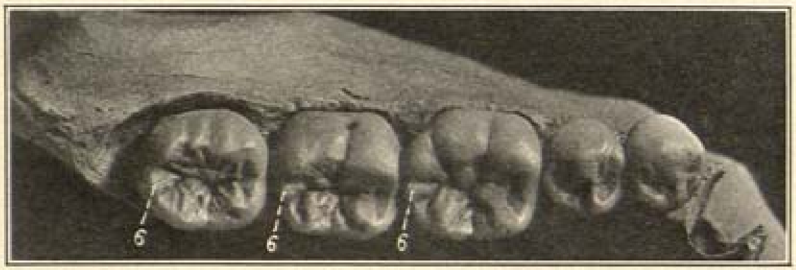


Fig. 6.—Left side of jaw of American Indian, showing additional cusp six to primitive and modified molar patterns

accustomed to count cusps just according to number add much confusion to this situation. By the mere count of cusps the fact is overlooked that while some of them are homologous, others are only analogous. The examples thus shown point out the typical changes as they occur in different teeth.

3. Stages of Pattern Changes in the Lower Molar Teeth of Man

By a systematic arrangement of these changes as they occur in the three lower molar teeth of man, it is found that they fall into four well-defined stages. Fig. 7, A, B, C and D, illustrates these stages.

Stage I (Fig. 7, A) is that representing the original *Dryopithecus* pattern.

Stage II (Fig. 7, B) is that in which the primitive cusp formula is reduced from 5 to 4 but the groove system still retains the Y form with the loss of the posterior limb, *i.e.*, the basal contact of cusps 2 and 3 is retained.

Stage III (Fig. 7, C) is that in which the primitive cusp formula is retained but the groove system is changed from the Y to the +, the basal contact of cusps 2 and 3 has been lost.

Stage IV (Fig. 7, D) is that in which both the cusp formula and the groove system is changed, thus producing the most

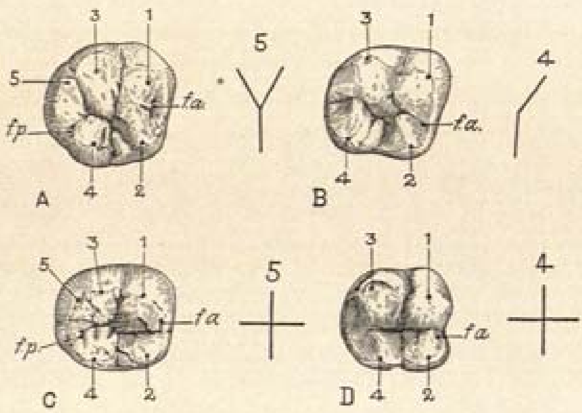


Fig. 7.—Four stages of development of lower molar teeth, illustrating the different changes from the Dryopithecus to the most advanced pattern. A, Dryopithecus pattern indicated by Y5 meaning primitive system of grooves and cusp formula. B, modified Dryopithecus pattern indicated by Y4 meaning cusp formula reduced to four and groove system primitive with the loss of the posterior limb of the Y. C, Primitive cusp formula retained but groove system changed to cruciform, the sign being +5. D, Cusp formula and groove system changed, sign +4

advanced lower molar pattern which consists of four cusps and a cruciform groove system.

4. Pattern Changes of the Lower Molar Teeth in Various Races of Mankind

If the lower molars of a series of skulls of a certain racial group are sorted out according to this series of changes the tendency of their variability in pattern becomes evident at once. Thus, in Table I the lower molar teeth of sixty-one (61) White males are tabulated. In this table it is noticed that of the ninety-eight (98) first molar teeth (M_1) present

87% retain the primitive Dryopithecus pattern, 7% retain the primitive groove system with the loss of cusp 5, 2% retain the primitive cusp formula with a change in the groove system and in 4% both the cusp formula and groove system

TABLE I

PERCENTAGE DISTRIBUTION OF LOWER MOLAR PATTERN IN M_1, M_2 AND M_3 IN WHITE MALES

W. R. U. Whites (61)

	N	Y5	Y4	+5	+4	c6	c7	CM	Im
M_1......	98	87	7	2	4	—	3	—	—
M_2......	110	—	5	1	94	—	2	—	—
M_3......	74	4	—	34	62	3	—	20	9

Legend: N, number of Molars; Y5, Dryopithecus pattern; Y4, Dryopithecus pattern less cusp 5; +5, primitive number of cusps but groove system changed to +; +4, cusp formula reduced to 4 and groove system changed to +; c6, cusp 6; c7, cusp 7. CM, congenitally missing; Im, impacted.

are changed. In the second molars (M_2) the changes are greater. None of the hundred ten (110) second molars retain the Dryopithecus pattern, 5% retain the primitive groove system with the loss of cusp 5, in only 1% is the primitive cusp formula retained with the changed groove system and in 94% there is a change both in cusp formula and groove system. In the third molar 4% of the seventy-four teeth retain the primitive Dryopithecus pattern, none of the third molars retain the Y system when the primitive cusp formula is reduced, 34% retain the primitive cusp formula with a change in the groove system and in 62% both the primitive cusp formula and groove system are changed. These figures simply point to the fact that of the three lower molars the first is most primitive, because it still retains the Dryopithecus pattern in the highest number of cases. The lower second molar is most advanced, because it attains the most advanced form in the highest number of cases. The third molar is intermediate, because it has not entirely departed from either or both of the primitive features entering into the original Dryopithecus pattern and reaches the most advanced stage by a smaller percentage of cases than does the second molar.

These facts are also brought out in similar analyses of other racial groups. Thus in Table II the lower molar teeth

TABLE II

PERCENTAGE DISTRIBUTION OF LOWER MOLAR PATTERN IN MONGOL MALES

Buriats (21)

	N	Y5	Y4	+5	+4	c6	c7	CM	Im
M_1	36	100	—	—	—	—	11	—	—
M_2	39	—	5	31	64	15	—	—	—
M_3	31	—	—	77	23	29	6	17	3

Legend—See Table I.

of twenty-one (21) Mongols (Buriats of Central Mongolia) are tabulated. If carefully examined the figures point to the same facts. The distributions are somewhat different but the general facts are the same. In Table III the lower molar teeth of forty-nine (49) West African Negro male skulls are tabulated. The story again is the same although the detail is different.

TABLE III

PERCENTAGE DISTRIBUTION OF LOWER MOLAR PATTERN IN WEST AFRICAN NEGROES

West African Negroes (49)

	N	Y5	Y4	+5	+4	c6	c7	CM	Im
M_1	97	99	1	—	—	3	2	—	—
M_2	96	17	12	8	63	6	5	—	—
M_3	88	20	3	59	17	16	6	—	1

Legend—See Table I.

5. PROGRESSIVE MODIFICATION OF THE LOWER MOLAR TOOTH PATTERN INDICATED BY THE FREQUENCY WITH WHICH THE ADVANCED STAGES ARE REACHED BY DIFFERENT RACIAL GROUPS

If a comparative study is made of homologous teeth of different racial groups certain facts become obvious. Namely, the differences tend to indicate the progressive nature of the changes observed. In Table IV the lower first molar of the European Whites, Asiatic Mongols and West African Negroes is tabulated. It will be noted that of the three races the Whites are more progressive, because their first

TABLE IV

Percentage Distribution of Lower Molar Pattern in European White, Asiatic Mongol and W. African Negro (Males)

Molar 1

	N	Y5	Y4	+5	+4	c6	c7
White	98	87	7	2	4	—	3
Mongol	36	100	—	—	—	—	11
Negro	97	99	1	—	—	3	12

Legend—See Table I.

molars retain the primitive Dryopithecus pattern in the lowest percentage of cases. Some of their first molars have reached even the most advanced stage. Both the Mongols and the Negroes retain the Dryopithecus pattern in all their first molar teeth not advancing to any of the succeeding stages at all with the exception of one case among all Negroes.

TABLE V

Percentage Distribution of Lower Molar Pattern in European White, Asiatic Mongol and W. African Negro (Males)

Molar 2

	N	Y5	Y4	+5	+4	c6	c7
White	110	—	5	1	94	—	2
Mongol	39	—	—	19	81	15	—
Negro	96	17	12	8	63	6	5

Legend—See Table I.

The second molar as indicated in Table V shows changes of a different kind. Whereas the Whites and the Mongols reach the most advanced stage in a higher number of cases than do the Negroes, the Whites show a higher percentage than the Mongols. Moreover, since the Mongols show 19% still in the transitional stages and the White only 6% it is quite clear that the former are intermediate in the progressive modification of the second molar pattern, while the Negro is most backward in this respect.

Table VI too shows the same progressive changes in the lower third molar. But this table also indicates to what extent the primitive features are retained in the Negro. By combining both the primitive cusp formula and the primitive groove system, it is seen that the Negro retains either or

TABLE VI

Percentage Distribution of Lower Molar Pattern in European White, Asiatic Mongol and W. African Negro (Males)

Molar 3

	N	Y5	Y4	+5	+4	c6	c7	CM	Im
White	74	4	—	34	62	3	—	20	9
Mongol	31	—	—	77	23	29	6	17	3
Negro	88	20	3	59	17	16	6	—	1

Legend—See Table I. CM, congenitally missing;
Im, impacted.

both of the primitive features in the highest number of cases reaching the most advanced stage in only a very small percentage of cases. The Whites again though retaining the primitive pattern in 4% of cases and the primitive cusp formula without the groove system in 34%, the most advanced stage is reached by the highest number. The Mongols are again intermediate. Although they lose one of the primitive features, the Y groove system, entirely in this tooth, they still retain the primitive cusp formula in 77% of cases reaching the most advanced stage by only 23%.

Comparisons of these characters in three different groups of Whites comprising a group of American Children, the above mentioned European Whites and an ancient group of Whites from Hungary of the ninth century, the following differences were noticed. The lower first molar, Table VII,

TABLE VII

Percentage Distribution of Lower Molar Pattern in American Children (85), European Whites (61), and Ancient European Whites (42), All Males

Molar 1

	N	Y5	Y4	+5	+4	c6	c7
American	164	74	12	13	1	—	2
European	98	87	7	2	4	—	2
Ancient	54	83	11	—	6	—	2

Legend—See Table I.

of the American Children is more advanced than the Recent and Ancient Europeans. In the second Molar, Table VIII, the American Children and Recent Europeans are more

TABLE VIII

Percentage Distribution of Lower Molar Pattern in American Children (85), European Whites (61), and Ancient European Whites (42), All Males

Molar 2

	N	Y5	Y4	+5	+4	c6	c7
American	95	—	4	6	90	2	—
European	110	—	5	1	94	—	2
Ancient	54	2	9	11	77	2	—

Legend—See Table I.

advanced than the Ancient Europeans. In the third molar, Table IX, the Recent Europeans are again more advanced.

TABLE IX

Percentage Distribution of Lower Molar Pattern in American Children (85), European Whites (61), and Ancient European Whites (42), All Males

Molar 3

	N	Y5	Y4	+5	+4	c6	c7	CM	Im
American	—	—	—	—	—	—	—	—	—
European	74	4	—	34	62	3	—	20	9
Ancient	35	6	11	34	49	3	—	35	—

Legend—See Table I. CM, congenitally missing; Im, impacted.

The Americans being young subjects had no third molars as yet.

Comparisons of the Mongols with some Chinese, Eskimos and American Indians bring out the following differences. In the first molar, Table X, they are all alike primitive.

TABLE X

Percentage Distribution of Lower Molar Pattern in Buriats (21), Chinese (19), Eskimos (30), and Am. Indians (55). (Males.)

Molar 1

	N	Y5	Y4	+5	+4	c6	c7
Buriats	36	100	—	—	—	—	11
Chinese	26	100	—	—	—	—	—
Eskimos	29	97	—	3	—	1	—
Amerinds	97	100	—	—	—	6	—

Legend—See Table I.

TABLE XI

Percentage Distribution of Lower Molar Pattern in Buriats (21), Chinese (19), Eskimos (30), and Am. Indians (55). (Males.)

Molar 2

	N	Y5	Y4	+5	+4	c6	c7
Buriats	39	—	5	31	64	15	—
Chinese	21	—	—	19	81	—	5
Eskimos	30	6	7	37	50	20	—
Amerinds	99	1	—	31	69	16	—

Legend—See Table I.

In the second molar, Table XI, the Eskimos are most primitive while the Chinese are most advanced, the Buriats and American Indians being intermediate. In the third molar, Table XII, the American Indians and the Buriats are most primitive, while the Chinese and Eskimos are most advanced.

TABLE XII

Percentage Distribution of Lower Molar Pattern in Buriats (21), Chinese (19), Eskimos (30), and Am. Indians (55). (Males.)

Molar 3

	N	Y5	Y4	+5	+4	c6	c7	CM	Im
Buriats	31	—	—	77	23	29	6	17	3
Chinese	16	—	—	50	50	—	—	32	4
Eskimos	29	—	—	52	48	28	—	28	9
Amerinds	84	—	5	75	19	32	6	13	7

Legend—See Table I. CM, congenitally missing;
Im, impacted.

Comparisons of Blacks comprising the West African Negroes, a group of American Negroes and a group of Australian aboriginals also shows some interesting differences. In the first molar, Table XIII, the Blacks like the Yellows

TABLE XIII

Percentage Distribution of Lower Molar Pattern in African Negroes (49), American Negroes (119), and Australian Aboriginal (20). (Males.)

Molar 1

	N	Y5	Y4	+5	+4	c6	c7
Af. Negro	97	99	1	—	—	3	12
Am. Negro	179	98	1	1	—	2	22
Australian	18	100	—	—	—	8	—

Legend—See Table I.

TABLE XIV

Percentage Distribution of Lower Molar Pattern in African Negroes (49), American Negroes (119), and Australian Aboriginal (20). (Males.)

Molar 2

	N	Y5	Y4	+5	+4	c6	c7
Af. Negro........	96	17	12	8	63	6	5
Am. Negro.......	218	6	21	24	49	8	4
Australian.......	21	5	—	43	52	43	—

Legend—See Table I.

are all primitive. In the second molar, Table XIV, the West Africans show a higher percent in the first and last stages while the American Negroes and Australian aboriginals reach a higher percent in the transitional stages. In the third molar, Table XV, the West African Negroes are most primitive, the American Negroes least and the Australians intermediate.

TABLE XV

Percentage Distribution of Lower Molar Pattern in African Negroes (49), American Negroes (119), and Australian Aboriginal (20). (Males.)

Molar 3

	N	Y5	Y4	+5	+4	c6	c7	CM	Im
Af. Negro....	88	20	3	59	17	16	6	—	1
Am. Negro...	187	4	3	63	30	33	6	11	13
Australian....	23	14	—	72	14	54	7	13	—

Legend—See Table I. CM, congenitally missing;
Im, impacted.

Comparisons of the males and females in several groups revealed the fact that among the more progressive races, as the American Children, for example, the females are more advanced. In the Ancient Europeans the females and males are almost alike in the second molar. But, in the first and third molars the females are more conservative. Among the Buriats the females are more advanced in the second molar, alike in the first, and more conservative in the third molar. Among the Australians both sexes are alike primitive in the first molar but in the second and third molars the females are more progressive. The West African females are somewhat conservative in the second molar and advanced in the first and third.

The additional cusps, 6 and 7, are found more in evidence among the colored races. Among the Whites, Tables I, VII, VIII, IX, these cusps appear in such small proportions as to be negligible. Among the Mongols, Tables II, X, XI, XII, the American Indians and the Eskimos show the greatest number of cusp 6, the Buriats following them in a less extent, while the Chinese have none, the third molar showing a higher percentage of this cusp than the other molars. Cusp 7 in the first and third molars is frequent only in the Buriats (11% and 6%), in the second molar in the Chinese (5%) and in the third molar only in the Amerinds (6%). Among the Blacks, Tables III, XIII, XIV, XV, the percentage of cusp 6 in the first and second molars is three to five times as high in the Australians as in the Negroes. On the third molar, however, this cusp appears so often in the Negroes as to increase their ratio 1 : 2. Cusp 7 on the other hand appears only in the Negro first and second molars. In the third molars it is as frequent in the Australians as in the Negroes.

Observations have also been made on the congenital absence of the third molars. With the exception of the West African Negroes it appears in a rather high percent in all races examined. (See Tables.) The argument favoring the idea that the congenital absence of the third molar is due to the reduction in size of the jaw and the crowding of the dentition, cannot be corroborated. The American Negro, the Eskimo, the Buriat and the Australian Aboriginal do not show any lack of room, due to diminution in size of the jaw, and yet they all present a very high percent of congenitally missing third molars. Missing third molars have also been observed in Chimpanzee. Also the impacted third molars seem to have no relation to lack of space. They seem to be associated rather with the position of the tooth crown during the formation period than with the room needed for its accommodation.

Summary

To sum up the main points, it may be stated:

1. That even with the widest range of variation the pattern of the lower molar teeth of man may be traced to a fundamental type named by Gregory the "Dryopithecus pattern."

2. That this pattern is recognized by a distinct number of cusps which must be accompanied by a definite system of grooves.

3. That certain changes appear which tend to modify the fundamental pattern in the lower molar teeth of man by a gradual and progressive process of evolution.

4. That these changes occur in four well-defined stages depending upon the modification of the system of furrows, the reduction in number of the main cusps or both.

5. That the different molars in the same dentition show changes different in kind and that like teeth in different racial groups show changes that are alike in kind but different in degree.

6. That as shown by the changes in crown pattern of the lower molar teeth, the most advanced stage is attained by the modern Whites; the most primitive stage is retained by the West African Negroes, the Mongols being intermediate.

This report is but a general survey of the extensive problem involved, and presents just a beginning of the studies to be pursued. A more detailed account will follow the completion of further investigations.

Acknowledgment

In conclusion I wish to acknowledge my indebtedness to the American Museum of Natural History for the privileges given me to examine the von Luschan collection from which the West Africans and the Ancient Europeans were chosen for this study. Also to the Western Reserve University Medical School I am grateful for having had access to their skull collection of European Whites and American Negroes. At the United States National Museum I examined the Eskimos for which I am under great obligations. For all these privileges I can but express my sincerest thanks.

THE PRESENT STATUS OF RESEARCH IN EDUCATION

By FRANK PIERREPONT GRAVES

President of The University of the State of New York and New York State Commissioner of Education

(*Read April 21, 1928*)

IN THESE days of scientific investigation it seems unfortunate and inconsistent that education, the basal science through which an understanding of all other lines is advanced, should have received so little recognition as a field of research. Not only has progress in this direction been given little encouragement; it has actually been met with ridicule, scorn, and deliberate hostility. Opposition has amounted almost to hysteria or obsession, as indicated by wholesale condemnations in recent magazine articles and by frantic efforts on the part of certain faculty groups to prevent the selection of an Education man for the vacant presidency in any university. Much of this bitter antagonism is due to pure jealousy, for Departments and Schools of Education do largely occupy the center of academic attention at the present day, and professors of Education have of late years been selected as chief executives in a score of colleges and universities. Moreover, this is likely to continue until professors of the older subjects generally undertake to learn something about the aims and principles of education as a whole and to view academic questions from a more rational standpoint than that of mere tradition or their own narrow specialties. But it must be confessed that a goodly share of this abuse has been deserved, and that the educational guild today is only doing penance for past charlatanism, misdeeds, and overweening conceit. Cannot education somehow obtain a limited absolution and rehabilitate itself as a communicant? The day of the passé school superintendent in the West and of the 'white rat' psychologist in the East as an incumbent of the chair of

Education has long since passed, and there are hundreds of well-trained men now endeavoring to place educational research upon a safe and sane basis.[1] It is high time to drop mountebankism and obstructionist tactics alike and to consider what can be done to make the subject reputable and strengthen its scientific methods.

Our first step is to note where the obstacles lie. What is it, then, that hinders this reputed pseudo-science from becoming genuine? If we compare the present situation of educational research with that of the recognized sciences, it will be seen that the field of research in education bristles with difficulties. Not the least of these is the fundamental fact that human intelligence, which conditions both the data and procedure of education, is exceedingly complicated. It does not lend itself to pigeon-holing and classification as readily as temperature, weight, or the force of the wind, and it cannot be meted and charted by the stethoscope, slide rule, or cymograph. Likewise the environment—physical, intellectual, and social—in which education has to "live, move and have its being," is varied, complicated, and filled with great differences.

Hence, in all his endeavors to fashion instruments of measure, the worker in educational research is "sore let and hindered" by obstacles of a degree and kind not encountered in other sciences. Yet the development of any science is dependent upon precision in measuring. Astrology never became astronomy, nor alchemy chemistry, until accurate standards and means of measurement were devised in their procedure. Geodetic surveying would never have been evolved, had we continued to measure the coast line with the revolutions of a cartwheel, as the Egyptians did, and the use of wampum as a medium of exchange indicates a very early

[1] For some account of such efforts, read the addresses at the fiftieth anniversary conference of the Johns Hopkins University by George D. Strayer on "The Scientific Approach to the Problems of Educational Administration," and by William H. Burnham on "Scientific Progress in Education in the Last Fifty Years," and the address by Otis W. Caldwell, the chairman of Section Q, at the American Association for the Advancement of Science, at Philadelphia, on "The Scientific Study of the Curriculum" (*School and Society*, 24: 623, 625, and 631).

stage in the march toward a science of finance. Until we know more definitely what education seeks, and until we can acquire greater accuracy in measuring intelligence and in measuring the products of education, we shall have great difficulty in securing for educational research a recognized position in the sisterhood of sciences.

As a result of the labors of such scientists as Terman, Haggerty, Dearborn, and Otis,[1] numerous systems of intelligence tests have been formulated and standardized, and have proved of great value in the promotion and classification of pupils and in educational and vocational guidance. But we are not yet sure that we know just what factors enter into intelligence and need to be tested. In his "Educational Determinism" Bagley[2] tells us that all existing tests of intelligence fail to evaluate some very essential ingredients, such as moral qualities—integrity, industry, perseverance, courage, loyalty, and 'human' abilities—sense of humor, tact, sympathy, sociability. Link,[3] on the other hand, in describing intelligence, holds that too much is being measured. He shows that 'intelligence,' or capacity to learn, is being measured by what we have already learned—that is to say, by the results of original intelligence *plus* something acquired, and that the Stanford tests, so widely used, fail of maximum validity because the opportunities for learning are in many states far inferior to those of California, where the tests were standardized.

We have also endeavored to make precise measures for achievement in various school subjects through 'scales' and 'tests.' A scale is a species of educational yardstick upon which samples of achievement in the skill or knowledge being measured take the place of the inch or half inch marks. These samples represent different degrees of ability, exactly ascertained and arranged, so that each is equidistant from

[1] *Stanford Revision of the Binet-Simon Tests* (Houghton Mifflin, Boston); *Haggerty Intelligence Examinations*, *Otis Group Intelligence Scales* (World Book Co., Yonkers); and *Dearborn Group Test of Intelligence* (Lippincott, Philadelphia).

[2] Bagley, William C.: "Determinism in Education" (Warwick, York, Baltimore).

[3] Link, Henry C.: "What is Intelligence?" (*Atlantic Monthly*, September 1923).

the other by a definite degree of value. Thus the pupil's attainment can be indicated by position on the scale. Educational tests, on the other hand, consist of things to be done, of equal difficulty or of a relative degree of difficulty that has been previously established. Here the measure of a pupil's performance is determined by the number of units accomplished within a given time. Through these two devices Thorndike, Ayres, Courtis, Buckingham, Gray, Monroe,[1] and others have enabled us to measure handwriting, arithmetic, spelling, reading, English composition, drawing, literature, sciences, and foreign languages with some degree of success.

The difficulties in securing accuracy are enormous. Take, for example, what seems to be the simple matter of constructing a scientific scale or test in spelling. Not only have many remarkable minds[2] found in the difficulties which beset this task a foeman worthy of their steel, but it is to be admitted that the outcome has not yet been entirely satisfactory. In selecting the important words for measuring capacity, it is found that a list which proves an excellent measure in one community will scarcely serve at all in another. Though perhaps teaching the same list of words as a whole, communities do not teach the different parts of the list in the same order or at the same time of year. Moreover, the list that will test the reliability of a school or class will not answer at all for a given individual. Thorndike holds that one or two words from a standardized spelling list may be sufficient to test the capacity of a group of a thousand pupils, but to measure an individual's ability, fifty to one hundred words at least will be needed. To meet this situation, both

[1] See *Thorndike Handwriting Scale*, *Stone Reasoning Test* (Teachers College, New York); *Ayres Measuring Scale for Handwriting* (Russell Sage Foundation, New York); *Courtis Standard Research Tests* (published by author, Detroit); *Cleveland-Survey Tests in Arithmetic*, *Buckingham Scale for Problems in Arithmetic*, *Buckingham Extension of Ayres Spelling Scales*, *Monroe Timed Sentence Spelling*, *Gray Standardized Oral Reading Paragraphs*, *Monroe Standardized Silent Reading Tests* (Public School Publishing Co., Bloomington, Ill.), etc.

[2] *Ayres Spelling Scale* (Russell Sage Foundation, New York); *Iowa Spelling Scales* (Univ. of Iowa, Iowa City); *Buckingham Extension of Ayres Spelling Scales*, *Monroe Timed Sentence Spelling Tests* (Public School Pub. Co.); *Courtis Standard Dictation Tests* (Published by author, Detroit).

Ayres and Ashbaugh recommend that we now construct a standardized list with a large number of words and out of this material select as a test the words that have been taught in class.

If spelling, then, is so difficult a matter, what are we to say about a scale or test in geography, history, composition, or literature, where not only the same problems arise but where there is so little agreement as to essentials? Nor do our troubles end here. It is particularly difficult to isolate the factors contributing to an educational product, because the social situation with which education deals is so much more complex than is that of any other science. Take such a field as character education. Our problem here is to analyze character into its component abilities and discover methods for training them. As yet "character" is almost completely unanalyzed. Any number of devices have been suggested as means of development, such as having school children repeat the Ten Commandments, the Golden Rule, or a Babbitized form of the Categorical Imperative, or commit certain formalized principles and strive to perform certain types of prescribed activities each day. All of these have been found by the recent investigations of Dr. Mark May [1] to lead to hypocrisy and other results quite antithetical to character building. And even after this concept of character shall have been analyzed and a scientific process of education agreed upon, there is no scientific surety that much of our present school and class organization does not point toward demoralization. With the formal school material so remote from life, any bright boy of the present day may come almost unconsciously to simulate the externals of attention by looking the teacher down into the roots of her eye while his mind is really miles away. So, too, with the general heterogeneous groupings in our public schools, the dull youngster speedily develops a technique for keeping up appearances, while the exceptional child acquires equally cunning devices to conceal his want of concentration and industry.

[1] May, Mark A.: "Studies in Deceit" (Yale University, Department of Education, 1928). A series of studies in character traits.

Nor do our tribulations cease, even assuming the determination of the diverse elements to be measured. Educational research has also met with trouble in developing a technique of its own—a factor essential to the progress of any science. Until recently it has borrowed its techniques from other sciences, especially statistics and psychology, and these were found not altogether usable. They are largely methods of procedure adapted to the artificial conditions of the scientific laboratory, whereas the laboratory for educational problems is the public schools, and any technique that separates experiment from life must fail to yield conclusive results.

Then, too, even when distinctive techniques have been developed, they have been subject to considerable criticism. Perhaps the best example of a new technique and its drawbacks is found in the case of control groups, as in the Bagley-Ruediger [1] experiments on the transfer of the habit of neatness from one study to another, and in Coxe's test of the influence of Latin on the spelling of English words, and Stevenson's study of the optimum size of a class. In all of these investigations one set of pupils serves as a control group, in which normal conditions are maintained; another set serves as an experimental group, in which one of the conditions is changed. Such a technique marks a real advance, but, in setting up the experiment, it has been found difficult, if not impossible, to select groups and teachers that are exactly equal in ability, and to secure enough schools or pupils to make the results trustworthy, and to eliminate all variables except the one it is desired to employ.

Another technique especially adapted to Educational research is that of index numbers. The first notable instance of the use of such a method is seen in the attempt of Ayres [2] to measure the relative efficiency of education in the various

[1] Ruediger, William C.: "The Principles of Education" (Houghton Mifflin, Boston), Chapter VI, pp. 108 ff.; Coxe, Warren W.: "The Influence of Latin on the Spelling of English Words;" and Stevenson, P. R.: "Smaller Classes or Larger?" (Public School Publishing Company, Bloomington, Ill.).

[2] Ayres, Leonard P.: "The Index Number for State School Systems" (Russell Sage Foundation, New York).

States of the Union. To determine the position attained by each state, Ayres used ten items—per cent of school population attending daily; average days attended by each child of school age; average number of days schools were kept open; per cent that high school attendance was of total attendance; per cent that boys were of girls in high schools; average annual expenditure per child attending; average annual expenditure per child of school age; average annual expenditure per teacher employed; expenditure per pupil for purposes other than teachers' salaries; expenditure per teacher for salaries. A similar technique has been used by many other investigators not only in determining the ranking of states and counties according to educational efficiency,[1] but in deciding what method of administrative or financial control is best for cities.[2]

Against all these investigations the criticism has properly been urged [3] that the items are too few and simple, especially when considered of equal value and without weighting, to reflect significant differences in connection with a problem so complex as the achievement of satisfactory education. The technique assumes, for example, that greater expenditures necessarily secure better education and that there is a causal relation between a certain kind of organization or equipment and certain results. Moreover, it fails to take account of the social and economic conditions in a community—educational atmosphere, government and social tradition, character of population, and also of the newer and improved forms of educational service—curriculum revision, individual differences, mental and physical handicaps, health, and recreation. In short, this clever technique would seem to stress too much

[1] Schrammel, Henry E.: "The Organization of the State Department of Education" (Ohio State University Press); Phillips, Frank M.: "Educational Rank of the State" (*American School Board Journal*, April 1926); Annual Report of the Superintendent of Public Instruction, Commonwealth of Virginia, 1923–4 and 1924–5.

[2] Frazier, Geo. W.: "The Control of City School Finances" (Bruce Pub. Co., Milwaukee); McGaughy, J. R.: "The Fiscal Administration of City School Systems" (Macmillan, New York); Harrell, C. Adair: "Educational Measurements" (Special Joint Committee on Taxation and Retrenchment, State of New York, 1927).

[3] Gulick, Luther: "Fiscal Problems of City School Administration" (Special Joint Committee on Taxation and Retrenchment, State of New York, 1928).

the material and mechanical and too little the functional and educational phases, which, while complicated and difficult to formulate, are the more important.

There are several other obstacles in the pathway of educational research, which must be removed before it can progress. Because the necessity for investigation arises almost altogether from actual difficulties encountered by schoolmen in the administration of schools or by teachers in handling their classes, this procedure is to be classified as *practical* research, rather than that which is so common in most sciences, *pure* research. Such a type of investigation has the advantage of dealing with real situations and aims to produce immediate adjustments,[1] but nevertheless the fact that education has naturally and almost of necessity so largely neglected pure research has prevented it from establishing fundamental theories and formulating general laws and has consequently delayed it in breaking its scientific chrysalis.

Again, the practical nature of education tempts its exponents to become administrators, rather than investigators, and, if they undertake research at all, their work runs a risk of being dangerously diluted. An administrator cannot easily be metamorphosed into a researcher. The procedure and habits of the two are polar. It is the business of the research man to keep his mind on the alert for new facts and never to settle things, except tentatively. The man in administration, on the other hand, finds his true function in issuing a *fiat*, even if it afterward develops that his decision was wrong. He may upon occasion accomplish considerable in research, provided that he can keep himself in two pockets, as it were, but an administrator *qua* administrator can no more be a research man than black can be white. Happily we are now beginning to create research professors in education, remote from all virus of administration, and to establish institutes of research in schools of education, bureaus of research in city school systems, and divisions of research in State Education departments.

[1] See Charters, W. W.: "Pure Research and Practical Research" (*Journal of Educational Research*), Vol. XII, No. 2, pp. 100 f.

Somewhat akin to this temptation for the educationalist to succumb to the administration lure, is the very strong urge for premature publication of research findings in education. This often leads to work poorly done. There are various causes underlying this unscientific tendency toward haste in publicity. The material, of necessity, deals with people and social situations, which are continually changing and shifting. Where results are too long delayed, the educational researcher is likely to be accused of antiquarian interests or of a *post mortem* diagnosis. Likewise the financial reward for crowding into print and having a pyramid of books and articles to one's credit has proved the downfall of many a promising investigator. This undue emphasis upon fertility, regardless of the merit of the production, characterizes the over-ambitious university man in general, but, owing to the newness of the field, it is a little stronger with workers in education than in other lines. It represents a tendency that will largely disappear as equipment and facilities multiply and the youthful aspect of the subject wears off.

Lastly, there has until recently been pronounced opposition to using schools as laboratories, on the ground that education deals with immortal souls and is something too sacred for profane experimentation. This interesting outcrop of obscurantism seems to be disappearing as we proceed, and schools are now rather inclined to feel proud when selected for laboratory efforts. Yet such hesitancy has seriously delayed the possible development of educational research, and, while school authorities no longer interpose any objection, this is not altogether true of the general public and their legislative representatives.

Such are some of the hurdles over which research in education must pass before arriving at the goal of recognition. Nevertheless, it may now reasonably claim to have somewhat approached the stage of becoming a science, since it has certainly developed various means of measuring many of the products and processes of education. While it is most difficult to apply a yardstick to such undefined and complicated entities as intelligence, personality, and character, since we have as yet only a very inadequate knowledge of humanity, a good start has been made, and the present instruments, imperfect as they are, mark a long advance over the old stage of preconceptions, prejudices, and unsupported opinion, through which research formerly groped in judging and comparing the achievements of school systems. Research has also devised techniques whereby educational situations may be brought under control while striving to measure what takes place under different conditions.

Investigators are constantly learning better how to set up control groups and hold the conditions constant while applying the variable. They are also gradually acquiring the necessary skill in analysis to determine the value of various items to be utilized in comparisons of efficiency, methods of administration or finance, and other like problems. A host of research men, emancipated from all administrative and even class duties, are springing up;[1] and, with the growth of a corps of professional assistants, statistical aids, and technical apparatus, prompt and reliable results can be had. Pure research will soon become more common and general laws will be evolved, while, on the other hand, practical experimentation will no longer be regarded as a dangerous proceeding either for schools or pupils.

If up to the present research in education has not reached the precision of physical and biological sciences, it has at least passed the stage of ridicule and should meet with hearty encouragement from scientists. Its efforts to develop standards and formulate procedures indicate that it is now making a successful struggle toward scientific standing. Yet it has as yet been granted little or no general recognition. Amongst the thousand most noted men of science in Cattell's inventory,[2] not a single one has been awarded his place because

[1] For their achievements, methods, and bibliography, see Monroe, Walter S., and Engelhart, Max D.: "The Techniques of Educational Research" (University of Illinois, 1928).

[2] See Cattell, J. McKeen (Editor): "American Men of Science," Fourth Edition (The Science Press, New York, 1927).

of achievements in educational investigation, and, while the American Educational Research Association is permitted to send a representative to the National Research Council, he is admitted only as a member of the section in Psychology or in Anthropology.

This situation must change. There is a crying need for research in education. An interest which involves the expenditure of more than two billion dollars each year, and, what is of far greater importance, concerns the welfare and future of thirty million boys and girls, should not be subject to the hazards of guess-work and waste. Where the demand for school efficiency and educational results is so great, the means for satisfying it must soon be found and due credit be given those who carefully conduct the search. The day cannot be far distant when the work of men like Ayres and Buckingham, Courtis and Charters, Terman and Thorndike, will bear its legitimate fruit. The influence of hundreds of young men who are each year turning their attention for a time at least to research in education is bound to be cumulative and will eventually be felt.

TEXTUAL CRITICISM OF THE GREEK OLD TESTAMENT

By MAX L. MARGOLIS

(*Read April 19, 1928*)

"The more frequently a text has been copied," says Otto Stählin,[1] "the greater will have been the influence exerted by the copies one upon the other, resulting in cross relationships and the repeated rise of new mixed texts. Hence given a large number of manuscripts it is often impossible to find any one which is a direct transcript of a 'copy' (Vorlage) still extant. Such is the case for instance with the numerous manuscripts of the Septuagint, among which, in the judgment of P. de Lagarde, no single one is entirely without value, but at the same time none is quite free from grave corruptions. Under such conditions it is impossible to establish a stemma (family tree, pedigree); it is quite feasible, however, to assemble manuscripts within groups of common descent by reason of identical corruptions (in this connection additions and omissions are particularly significant)."

2. A glance at the farrago of readings in the laboriously compiled apparatus, whether in the Oxford (1798–1827, Holmes-Parsons) or in the Larger Cambridge Septuagint (1906–, Brooke-McLean-Thackeray), will reveal, on the one hand, readings which are singular, *i.e.*, found among the witnesses examined by the editors just in one to the exclusion of all others, and, on the other hand, readings which are held in common by two or more witnesses. Singular readings answer to one of two tendencies marking the course of textual transmission. It is the tendency to alteration. In the singular readings are revealed the habits and idiosyncrasies of scribes, their physical and mental failings, and even their moral delinquencies, as when in sheer haste they pass over portions of the text or wilfully indulge in contraction. Gen-

[1] Editionstechnik, 1909, 17.

erally speaking, singular readings are so much refuse which must needs be swept up and constitute the subtrahend in the operation thrust upon the student who is always far more interested in the antecedent copy, now lost, than in the extant transcript.

3. For the sake of illustration, let us take in the Book of Joshua [2] codex p (Petrograd Gr. 62 and the concluding portion —24. 26 to the end—in the Bodleian Auct. T. inf. 2. 1, designated as a_2 in Brooke-McLean). It has 380 singular readings. The scribe [3] slipped up in the uncial script which on occasion he reproduced inaccurately. The greater number of graphic errors consists in the confusion of ΑΔΛ, ΕCΘΟ, ΓΙ, ΗΝ. Thus 19.38 ΔΑΛ is transformed into ΑΔΑ, μαγδαλιηλ into μαγαδαιηλ. 15. 21 καβσενα for καβσεηλ with Α for Λ and Ν for Η. The worst example is 19. 13 επι ποαγκασειμ; apparently the antecedent copy read επι πολ⸢ κασειμ—Α for Λ requires no comment, but ⸢ (sign of abbreviation for ιν [4]) was misread as Γ.[5] When the scribe found in his copy a δ originating in θ (mispronunciation !), he replaced it by λ as 15. 58 βελ < βεδ < βεθ 19.39 νεφλαλειμ < νεφδ. < νεφθ.; the reverse 15.58 αουθ < αουδ < αουλ. ε for C: 11. 22 αενδωθ for ασηδωθ (at the same time with Ν for Η). ο for ε: 15. 6 βοθ. ο for θ: 15. 27 βαιο. ο for σ: 15. 3 καδηο. θ for σ: 13. 13 γεθσουρ for γεσσουρ (γεσουρ); conversely 9. 17 βηρως for βηρωθ. Ι for Γ: 15. 37 ιαδ. Π for Τ: 23. 13 επι. ρ in the place of Β: 19. 29 αζχειρ (at the same time χ and ζ change places). Μ for Λ (also liable to confusion in the cursive script): 21. 18 αμμων for αλμων; Ν for Λ (as above): 17. 3 μαανα. Μ for Ν (graphically similar in both scripts, but also subject to indistinct pronunciation): 19. 19 αμαρεθ.

Cursive characters confused: ꞑ ꞕ μ. ꞑ (β) for ꞕ (κ): 15. 13

[2] A critical edition of the Greek text, with an apparatus containing the divergences in four principal recensions and their attestation within each as well as the variations in the sub-recensions and individual group members, has been prepared by the writer and is now in course of publication.

[3] The errors need not all have been his own. Some he may have taken over from copies no longer extant (or at present uncollated). In the end they all represent aberrations by individual scribes.

[4] Thompson, "Handbook of Greek and Latin Palæography," 94.

[5] Comp. similarly 15. 3 ακραβοy for ακραβειν.

εναβ for εvακ; conversely 15. 24 κωλαθ (at the same time ω and α change places) for βαλωθ; 18. 17 και ων for βαιων (with false division of words). For confusion of κ and μ comp. 13. 17 μαβωθ for βαμωθ. Through β, μ (then ν) and ρ are confused: 19. 37 ασομ for ασωρ 13. 9, 16 μισων for μισωρ.

The confusion of δ and ρ calls for especial attention. Recourse to the Hebrew is inconceivable. A scribe guilty of such miswritings as have been cited surely had no knowledge of the Hebrew tongue. I know at present of no form of the script, uncial or cursive, in which the two letters might have been confused. Examples: 12. 2 αροηδ for αροηρ; the reverse: 15. 34 αριαθαιμ for αδιαθαιμ 19. 36 αραμει for αδαμει. The identical interchange meets us in singular readings of other manuscripts, and where the same confusion is encountered in the most ancient form of the text it need not at all ascend to a misconception of the Hebrew by the translator but may constitute an inner-Greek error.

Confusion of similar sounds (through mishearing): ρ for λ (the confusion would be most natural with Copt scribes [6]): 18. 20 ιεφαηρ[7] for ιερφαηλ. φ for β (spirantic pronunciation): 19. 11 δαφασθαι. φ for θ [8]: 19. 14 ιεφφαηλ for ιεφθαηλ. χ for γ (spirantic pronunciation [9]): 15. 19 χολα (incidentally with final θ dropped). I leave on the side confusion of vowel sounds.

Interchanges of consonants are particularly frequent at the end of a word when neither script nor sound confusion seems to be involved. θ is the favorite final—whether for β (11. 1 ιωβαθ) or λ (19. 27 χαβωθ, possibly through χαβωδ) or ρ (19. 34 θαβωθ) or μ (15. 34 ηνεθ, *i.e.*, ηναιμ) or ν (13. 17 δεβωθ; conversely 16. 6 θηναν for θηναθ); we also find μ for σ (10. 5 λαχειμ). It is possible that in all these cases the final was abbreviated in the antecedent copy. Elsewhere the scribe may have imagined the sign of abbreviation to exist in the

[6] "The two sounds are more closely akin in Egyptian than in any other language," Stern, "Koptische Grammatik," 28.

[7] Incidentally with medial ρ dropped, comp. 15. 30 εμα for ερμα.

[8] Jespersen, "Lehrbuch der Phonetik," 1904, 36. But graphic similarity may be a further contributory cause.

[9] Mayser, "Grammatik der griech. Papyri," I, 164, rem. 2.

'copy.' At any rate the scribe loves to tack on final consonants where they had no place. Here again θ is the favorite ending: 15. 3 σενναθ and other examples. Next in favor is ν, but we find tacked on also μ, β or ρ. Likewise superfluous vowels are indulged in at the end of a word.

Just as imaginary signs of abbreviation led to all sorts of consonantal finals where they did not exist, so real marks of abbreviation were overlooked and as a result the final was dropped. So 1. 3 αυτο for αυτον 10. 38 αυτη for αυτην 39 χεβρω for χεβρων 15. 34 ραμε for ραμεν 18. 25 βηρω for βηρωθ 19. 5 ασε for ασερ 21. 4 πολει for πολεις and other examples. Similarly vowels are dropped at the end.

Other examples of curtailment: 15. 11 αχχωνα for αχχαρωνα (the loss of initial σ, due to haplography, it shares with others) 19. 38 θαμους for θασμους 21. 34 ιεναaμ for ιεκναaμ 22. 9 γααδ for γαλααδ (conversely 12. 6 γαλααδ for γαδ).

Deeper seated corruptions: 9. 17 βερειμ for ιαρειμ. 10. 40 γαβερ apparently for ναγεβ through the intermediary ναβεγ, but initial γ remains to be explained. 13. 5 χασβι for γαβλι—χ for γ calls for no explanation, see above; β and λ are apparently transposed; but σ for λ is peculiar unless we postulate θ (out of δ) as intermediary; so also 18. 17 χαλισωθ for γαλιλωθ. 15. 44 αχβειρ apparently (through αχβειβ) for αχζειβ. 17. 17 s. εγω for εἶς· ὁ—certainly a baffling corruption. 19. 3 ασοδολ for ασομ. And so on.

Omissions: Due to haplography: 1. 8 του for τουτου 13. 19 σεβα for σεβαμα (μα looked to the scribe like βα; see above). Or per homoioteleuton: 2. 19 εν τη οικια σου omitted after σου 8. 1 και την γην αυτου after και την πολιν αυτου and so forth.

Transposition of letters: 18. 23 εφαρ for εφρα 19. 38 ιαειρων for ιαριων. But also of textual elements: 2. 2 τω βασιλει λεγοντες ιεριχω for τω βασιλει ιεριχω λεγοντες—in all likelihood the antecedent copy had λεγοντες in margine or supra lineam, hence it was omitted in textu, *i.e.*, the textual reading squared with **C**.[10] 6. 19 αγιον εσται τω κυριω appears after (instead of before)

[10] **C** is the Constantinopolitan recension. In the present instance (as in so many other cases) the starting-point is a **P**(alestinian) text, *i.e.*, the recension prepared by Origen (Hexapla-Tetrapla), which read: λεγοντες ※ ιδου: (the asterisk marking an

εις οικον [11] *κυριου εισενεχθησεται*—here again the element appearing now at the end must have stood in margine because it had been dropped in textu, and that for the reason that in a still anterior copy, written in uncials in lines of 14 letters each, the element dropped in transcribing that codex must have occupied a line by itself:

ΑΓΙΟΝΕСΤΑΙΤΩΚΩ
ΕΙСΟΙΚΟΝΚΥΡΙΟΥ
ΕΙСΕΝΕΧΘΗСΕΤΑΙ

Accordingly the reading *οικον* (see note 11) was inherited.

Textual elements are supplanted by others introduced from the immediate vicinity through aberration of the eye. Thus 11. 21 in the place of *συν ταις πολεσιν αυτων* the scribe wrote *συν ταις πολεσιν ιουδα*, because *εκ παντος ορους ιουδα* immediately precedes. 21. 17 *και γαβε και τα αφωρισμενα τα προς αυτη* has been displaced by *και βαιθσαμες και τα αφορισμενα τα προς αυτην πολεις εννεα παρα των δυο φυλων τουτων* repeated from the end of the preceding verse. Through reminiscence a plus is introduced from another context. Thus 1. 15 *ο θεος* is enlarged by *των πατερων*, apparently from verse 11.

Omissions may be due to conscious condensation, as when pronouns (*e.g.*, 1. 2 *εγω* in front of *διδωμι* 2. 14 *και ειπεν* for *και αυτη ειπεν* 6. 26 *εκεινην*), particles (14. 13 15. 19 23. 2 *και*), *λεγων* (18. 8), introductory verbs (like *αναστηθι* 7. 10 *αναστας* 8. 1) are sacrificed. A double divine name is curtailed (7. 13 *ο θεος* for *κυριος ο θεος*) and a single one just as readily enlarged (6. 16 10. 32 *κυριος ο θεος* for *κυριος*).

Substitutions. The pronoun replaces the noun (8. 17 *αυτων* for *ισραηλ*) and conversely the noun the pronoun (21. 41 *την γην αυτων* for *αυτην*). The singular is put for the plural (8. 10 *ανεβη* in agreement with *αυτος*);[12] the reverse 11. 10 *κατελαβοντο*.[13] The cases are interchanged: 16. 10 *βασιλεως* (for

addition, in accordance with the Hebrew, wanting in the Greek). By a dislocation of the signs the text before C read: ⁜ *λεγοντες* : *ιδου*, hence (because of the wont of C to discard asterized elements) *λεγοντες* was omitted and *ιδου* retained.

[11] So p singularly for *θησαυρον*. See the variants in verse 24.

[12] But 21. 2 *λεγων* for *λεγοντες* is a blunder due to ignoring the compendious manner in which *λεγοντες* was written in the antecedent copy.

[13] But 3. 15 *επληρουν* is senseless.

βασιλευς) is sheer thoughtlessness; similarly thoughtless is 1. 1 υιου for υιω; 1. 11 ενετειλατο του λαου, by influence of the preceding παρεμβολης του λαου. The dative is made to do service for προς c. accus. (9. 15 αυτοις); conversely δια c. accus. replaces the mere accusative (18. 4 δι αυτην, conformed to διελθειν). 16. 3 the scribe writes η θαλασσα for επι την θαλασσαν. The middle voice is substituted for the active (5. 14 προστασση, unless it is merely a case of dropped final); the indicative for the subjunctive and the reverse. For the present tense the perfect is written (10. 20 -σεσωσμενοι for -σωζομενοι); for the finite verb participial construction is indulged in (9. 16 ακουσαντες) and the reverse (24. 33 ελαβον). The compound verb is preferred to the simplex (3. 6 απηραν 14. 11 ενισχυων) and conversely (19. 27 στρεψει for επιστρεψει). The preverb may be enlarged by another: 19. 47 προ-κατελαβοντο; or one preverb may be placed for another: 24. 17 απηγαγεν for ανηγαγεν (probably graphic: Π for N) 8. 3 διαβηναι for αναβηναι 21. 35 παρασπορια for περισπορια. In like manner the prepositions are interchanged: 21. 1 εις for εκ (a graphic blunder: ιc for κ) 6. 15 επι for εν 8. 6 εκ for απο 13. 32 απο for κατα. For the adversative αλλα we find 3. 4 the conjunctive και. Less familiar words are replaced by more familiar ones: 9. 5 κοιλια for κοιλα 13. 3 σατραπαις for σατραπιαις 16. 5 γαζα for γαζαρα; or generally through reminiscence from other contexts, as 9. 15 παρεμβολης for συναγωγης and so forth.

Peculiar substitutions: 4. 18 εξεθαμβηθησαν for εξεβησαν 17. 9 κατοικησεται for καταβησεται 24. 3 τον πατερα υμων και την μητερα for τον πατερα υμων τον αβρααμ and so on.

A doublet is 15. 59*a* και κωλω και κουλον—the first element must have stood in the margin of an antecedent copy pointing to the variant reading κωλω (so codex i). Conflation underlies such a reading as 19. 18 ιεσζραελ from ιεσραελ and ιεζραελ (both these readings are attested).

4. As with p, so it is practically with all other manuscripts. Singular readings may be more numerous in one and less in another, but they all have them, whether a manuscript be recent or ancient. Among the manuscripts of the fourth and fifth centuries codex A (the Alexandrinus) has 97 singular

readings and B (the Vaticanus) just 30. When 6. 11 B stands alone with its omission of την πολιν, the assumption is justified that we are dealing with a delinquency of the scribe of B or of some ancestor of it which we have no reason to burden the Greek original with. Singular readings are instructive enough as showing the range of variation to which transcripts become liable, but in themselves these readings are valueless and lead us away from the original which it is our aim to restore.

5. There are singular readings which are extremely valuable, but in one case only, namely when the manuscript in which they occur happens to be the sole representative of a recension. The **P**(alestinian) recension, to which reference was made in note 10, existed in two forms (sub-recensions): $\mathbf{P}_1$ and $\mathbf{P}_2$, the former being the first or hexaplaric edition of Origen's revision of the Septuagint, and the latter the second or tetraplaric edition.[14] This distinction is not always maintained, because the codices extant of either group are more or less eclectic. However, the sole Greek manuscript representative of $\mathbf{P}_2$ which is extant is codex b (= x Brooke-McLean, Curzon 66 of the British Museum, of the beginning of the 11th century). The so-called Syrohexaplar version (*i.e.*, the Syriac translation of Origen's revision of the Septuagint) may be found in Joshua to tally with codex b, provided we succeed in retroverting the Syriac into Greek which really becomes possible only with b before us. There are points which the Syriac was unable (at least unmistakably) to reproduce despite its manifest effort to imitate the Greek very closely. Thus readings like τον παντα λαον (7. 3), την πασαν συγγενειαν (6. 23), τους παντας βασιλεις (10. 42 11. 17) τους παντας εχθρους (21. 42), τας πασας πολεις (11. 12), τον κυριον θεον (22. 5) are confined to b. Not only is this repeated placing of the article at the front a mark of design—the Greek article is meant to do service for the Hebrew nota accusativi את—but other examples are available in which the reading has been carried from $\mathbf{P}_2$ into other manuscripts outside **P** or

[14] See my paper on "Hexapla and Hexaplaric," *American Journal of Semitic Languages*, 32 (1916), 126–140.

else is common to the entire group **P**. In other words, the principle of expressing the Hebrew nota accusativi was present to the mind of Origen when he started his work of revision; where he failed to live up to it in the earlier edition he made good the omission in the subsequent recension, and of that b happens to be the sole representative.

6. Readings which are common to a number of manuscripts may represent a singular reading of the archetype from which they all descend. This is the other tendency in the history of textual transmission, the tendency to follow copy by which its imperfections are perpetuated. Accordingly such community readings restricted to a definite number of manuscripts furnish the key to establish their common descent. Take for example the group which I designate as $\mathbf{S}_b$. It consists of seven manuscripts in my apparatus: $t_1 = 84$, $t_2 = 134$, $\mathit{t}_1 = 74$, $\mathit{t}_2 = 76$, $f = 106$, $\mathit{f}_1 = 610$, $\mathit{f}_2 = 44$ (the figures are those of Rahlfs). A characteristic reading by which this group is marked is found 17. 3. The text says that Zelophehad had no sons but daughters and then proceeds to enumerate the daughters with the introductory formula, These are the names of his daughters. But in all the seven members of the $\mathbf{S}_b$ group this introductory formula reads, These are the names of his *sons*.[15] This is but one out of 176 readings found exclusively in this group, constituting accordingly the singular readings of the archetype. Or take the large class **C** representing the Constantinopolitan recension. All its members without exception, and none outside this group, omit 19. 26 "and Allammelech." These names (with "and" preceding) are often written in the uncial manuscripts in short lines; accordingly the scribe of the ancestral copy passed over a short line standing over and below other such short lines all beginning with "and." Suffice it to say that inherited singular readings furnish the key to the grouping of manuscripts. I have presented my grouping of the codices of the Greek Joshua in a paper, "Specimen of a New Edition of the Greek Joshua," in the Israel Abrahams Memorial Volume, Vienna 1927.

[15] Two of the manuscripts have been corrected here by a later hand. But the original reading concerns us, and not the correction.

7. As I pointed out in the paper just mentioned, the individual manuscripts frequently become untrue to type. That is the result of mixture which the texts have undergone since antiquity. When ranging a text within a certain group, large or small, the eye must be on the basic character. The eclecticism is annoying, to be sure, but so long as it is possible to unravel the threads we shall be in a position to lead back every element to its source. On the road to the original text we must pass by the devious paths which it is a painful duty to tread only to regain the main thoroughfare. If we enter by means of the text prepared by Andreas Masius (Andrew Du Maes, died 1573) [16] we shall be obliged to discard phraseology framed by the editor himself and constituting examples of "Dutch" Greek.[17] Similarly, we shall brush aside the "Spanish" Greek of the Complutensian text.[18] This freedom on the part of the printers can be matched in manuscript age by examples of "Byzantine" Greek.[19]

8. As we ascend to the principal recensions of which the most significant is the Palestinian by Origen we meet with all sorts of accretions, omissions, transpositions, substitutions, and corrections in the form of proper names, all with a view to bring the Greek into consonance with the Hebrew. It is clear that all these modifications must be removed and the unrevised reading restored if we are to obtain what the original translator wrote. Where Hexapla and Tetrapla differ, we

[16] The Greek text, with an accompanying Latin translation and copious notes, was based primarily upon a codex (since lost) of the Syrohexaplar version (see above, 5), but was dependent for the Greek wording on the two printed editions, the Aldina (Venice 1518/19) and Complutensis (Alcalà 1517, published 1521). In a monograph which is being published in the Harvard Theological Series the manner in which the learned Belgian scholar constructed his text has been most minutely discussed.

[17] For example 10.26 *επειδη εθανατωσεν αυτους* for *μετα το θανατωσαι αυτους* 19.49 *και συνετελεσαν διελειν κληρω την γην εν οριοις αυτων* for *και συνετελεσαν του κληροδοτησαι την γην εις τα ορια αυτης*.

[18] Comp. 1. 4 *πασαν γην εταιων* for *πασαν την γην του χετταιου* 2. 4 *και ουκ οιδα ποθεν ησαν* in the place of *και ουκ εγνων ποθεν εισιν* and so forth; then in the forms of proper names, as *e.g.*, 7. 2 *βηθαυεν*, reproducing the pronunciation of Spanish Jews, for *βαιθαυν*.

[19] Take for example the dative for the modal or temporal accus. in $f(=f_1f_2)$ 1.3 *τροπω ω* for *ον τροπον* 11.6 *τη ωρα ταυτη* for *την ωραν ταυτην*, etc. or the substitution of *δε* for *και* at the beginning of chapters in f_2: 3. 1 *ωρθρισε δε* for *και ωρθρισεν*, etc. or the habit of $\alpha(=121)$ to sum up the phylæ by number in each tribe in ch. 19: verse 22 *ομου φυλαι δεκα εξ*, 27 *ομου φυλαι ασηρ ενδεκα*, etc.

are fortunate in possessing in the earlier edition the unrevised form which the later edition displaced or modified. It appears that Origen had before him divergent copies of the Greek text current in his days. His notion that the 'Hebrew truth' must at all hazards be taken as authoritative caused him tacitly to accept as between variations that form which squared with the Hebrew. But Origen modified the text even in matters which were purely formal in Greek and on which the Hebrew had no bearing whatsoever. Take for instance the Attic ending *-ον* for the Hellenistic *-οσαν* in the aorist. The correction of the Hellenistic Greek in form and style is still more marked in the Syrian recension. The procedure must clearly be to eliminate all changes that bear the earmarks of conscious modification and reintroduce the original. The Egyptian group, represented by the famous Vatican codex and its few satellites, appears on the whole to have been the basic text which the recensions set about to modify. Nevertheless bits of the original text may be found outside the group.

9. The remedy is supplied by a thoroughgoing study of the translator's manner. On the whole he handled his Hebrew freely, repeatedly curtailing the text, but in general not committed to slavish literalness in the phrasing of the Greek. Still it was a translation, and while here and there the translator read a slightly different Hebrew compared with the received Hebrew, substantially the Hebrew and the Greek ought to tally and they actually do tally. Hence the Hebrew becomes a means of correcting errors introduced in the course of transcription which had no place in the original translation. One example out of a number that have come under my observation will suffice. 5. 6 the Hebrew reads אשר נשבע יהוה לאבותם לתת לנו, English Version: which the Lord sware unto their fathers that he would give us; the Greek in **E**(gyptian text): *ἣν ωμοσεν κυριος τοις πατρασιν ημων δουναι* "which the Lord sware unto our fathers that he would give," for which **S**(yrian): *ην ωμοσεν κυριος τοις πατρασιν ημων δουναι αυτοις*, $\mathbf{P}_1$ (= Hexapla) *ην ωμοσεν κυριος τοις πατρασιν ημων δουναι ημιν* and

P$_2$ (= Tetrapla) **C**(onstantinopolitan text) *ην ωμοσεν κυριος τοις πατρασιν αυτων δουναι ημιν*. In truth the original Greek will have read: *ην ωμοσεν κυριος τοις πατρασιν ημιν δουναι* "which the Lord sware to the fathers that he would give us," the translator merely in accordance with his free manner placed the dative in front of the infinitive and just as freely omitted to render the pronoun "their" fully implied in the Greek article (*τοις πατρασιν* = their fathers). A scribe changed *ημιν* to *ημων* which was retained in the recensions barring **P**$_2$**C** which boldly and slavishly follow the Hebrew by substituting *αυτων*.

SOME ECONOMIC IMPLICATIONS OF AMERICA'S CHANGING WORLD STATUS

By ERNEST MINOR PATTERSON

(*Read April 21, 1928*)

It is not possible to discriminate sharply between changes due to the late war and those that were already occurring. America's position in world affairs was altering rapidly before 1914 and much that has happened since that date would have transpired even if there had been no war. For the most part developments have merely been hastened yet some of their significance has been enhanced by this acceleration. Four of these changes in the economic field may be briefly stressed.

First is our modified attitude toward immigration. For a century the United States was changing. A country with a small population and a meager supply of capital but with large undeveloped resources was becoming more densely populated and more independent of foreigners who had capital to lend. Some resources were showing signs of exhaustion while nearly all were more fully utilized. With the annual influx of immigrants increasing and with greater difficulties in assimilating them we have put up the barriers.

But this has not settled the matter. Instead it has merely entered on a new phase. Last summer Albert Thomas, Director of the International Labor Office, pointed out that two problems must soon be faced and an answer secured. May the people in densely populated areas claim the right to multiply as they see fit and then demand that their excess numbers be received freely in regions where there are a fewer number per square mile? On the other hand may those people now occupying the sparsely settled areas insist that they may retain control over those areas to the exclusion of others? These two issues will probably become more acute as the years pass. From time to time they appear in international conferences where invariably the representatives of densely populated countries insist that their people should be allowed to migrate freely. This view is just as regularly opposed by delegates from sparsely settled areas. The latest expression of these differences came only a few days ago from Havana.

Another sign of the growing significance of the question is the meeting last summer in Geneva of the first Conference on World Population, a gathering which discussed dispassionately the available facts regarding population growth as found and analyzed by biologists, statisticians, sociologists and economists. At the end of their sessions an organization for permanent study was created. Population growth and the migration of peoples furnish an issue which may soon become acute in international policies.

The problem presented is not easy of solution. Thus far Americans have taken refuge in the easy but superficial contention that immigration is a purely domestic question and that we may decide it as we see fit. But is it? The same argument has been pressed in support of our tariff policy, yet slowly and surely people of all parts of the world are coming to realize that restrictions on the importation and exportation of commodities are facts of international significance. What is done by one country is a matter of concern to all. To read the report of the World Economic Conference held a year ago at Geneva is to learn of the unity and interdependence of the economic world.

Our immigration policy will be challenged and vigorously. The issues raised are becoming more insistent and the answer is not easy. Mere assertion of so-called rights will be inadequate. Americans are not willing to concede unqualified rights to the people of other countries. Many of us have argued that the Chinese should not raise their tariffs; that the Mexicans should not modify their land laws; that the producers of rubber in the British Empire ought not to stabilize prices with the aid of the Stevenson plan; that the

Brazilian government should not attempt to influence the price of coffee. In some cases we urge treaties, previous legislation or the establishment of vested interests in support of our contentions but the point is that we are not willing to concede that the nationals of other countries have an unqualified right to do as they please. It happens that I favor the restriction of immigration into the United States but there is no denying the existence of the problem. How can we claim the right to do as we please with our large land area still sparsely settled and protest the rights of others to do as they please with their coffee, their oil and their rubber?

A second change is the rapid growth of large scale production and the existence of huge plants for the building of ships, the manufacture of chemicals, steel and textiles, the mining of coal. Years ago only limited areas in the world were devoted to manufacturing. Now the vast majority of the people of Western Europe live in cities and in the United States over half of our population is now urban. Even Japan, India and China are rapidly industrializing. Huge factories have been built in each country, many of them with little regard for construction elsewhere, until the aggregate productive capacity is in many lines far in excess of the readiness of the world to buy.

The test is the present capacity of the market to purchase the output. The cotton spindles of the world are about 15 per cent more numerous than in 1913 but the world demand for cotton cannot keep them busy. There is acute depression in many countries, among the worst sufferers being old England and New England. World coal output is eight or ten per cent greater than in pre-war days and the coal markets are clogged. The British coal industry has had two serious strikes since the war. Although the strikes have ended the problem has not been solved and another crisis is now developing. Continental output has been growing and in the United States the strike in the bituminous fields is a reflection of an overdeveloped industry.

Similar data can be given for other leading industries, among them shipbuilding and steel. Demand for steel fluctuates rapidly but production in England, Germany, Poland, the United States and elsewhere has during the last year or two been only from 50 to 75 per cent of capacity during much of the time.

Excessive equipment in many lines has not been unknown in the past but has now appeared on so huge a scale as to be a new sort of problem. It is being met in part by private agreements between the producers of different countries. In part it is not really faced at all with the result that there are huge losses suffered by many investors and the still worse fact of persistent unemployment and suffering for hundreds of thousands of workers.

Years ago we would have left it all to a magic "law of supply and demand." We would have expected capital and labor to shift from crowded fields to others where the demand might be greater. Today the difficulties of such adjustments are so serious that attention by governments or by international action is imperative. We are finding that unrestrained private competition is an inadequate remedy when labor and capital function in such huge units and are so immobile.

A third change is our sudden shift from debtor to creditor status, accompanied by alterations of the reverse kind in Europe where creditor groups have become debtors. In 1914 foreigners owned the stocks and bonds of American corporations, municipalities and states and we were a debtor group to the extent of several billion dollars. At the same time we were allowing foreigners to carry our trade in their vessels, were insuring much of our property through their companies and financing many other transactions through their bankers. To meet these and other annual payments our exports exceeded our imports by about $500,000,000.

Today American citizens own perhaps $14,000,000,000 of foreign stocks and bonds in addition to the claims of our government against foreign governments with a nominal value of about $10,000,000,000 more. We are expecting these foreigners to pay the charges on these public and private debts and are urging them to go on purchasing an increasing volume of exports. At the same time we are making it more difficult for their laborers to work it out by coming to America. We are insisting too that we desire to carry our foreign trade in our own vessels and that we prefer to finance our own business transactions with the aid of our own bankers. Moreover our tariffs have been raised since the war.

This curious contradiction in policies has been maintained by our willingness to accept each year more promises of foreigners to pay at some future date. The American market has been open for the sale of huge quantities of securities. This is presumably a movement that will in time diminish somewhat in volume. If so, we must submit to default on a substantial amount of these foreign loans, perhaps sacrificing first the amounts due our government or else we must readjust our policies in such a way as to permit our receiving a larger amount of foreign goods and services in annual payment. Shipping policies and the tariff must ultimately be reconsidered.

Fourth and last in this short list of changes may be mentioned the money problem. America is a gold producing country and should normally export or at any rate import very little gold. Yet for some time gold has been imported in large amounts and we now hold something over 40 per cent of the world's supply. For several years our efforts have been directed toward preventing a huge expansion of credit on the basis of these holdings, the policies of our Federal Reserve officials being designed to this end. This has been done so fully that observers abroad are alleging that the world now operates on a "dollar" standard instead of the old gold standard; that their price levels will be determined by the amount of gold we hold or release to them; and that our attitude and their monetary fate are settled by a small group of American banking officials.

This characterization may be somewhat overdrawn and it describes a condition that may not persist. In fact gold is now leaving the United States in large quantities. Nevertheless our position has permanently altered. New York is now a great financial center and our interest rates and our monetary and banking policies have become a matter of world concern. What other people do affects us more than ever before and what we do will influence them to a far greater degree than a few years ago.

The situation in the money markets during the last year or so illustrates this. For local reasons the Bank of Germany lowered its discount rates early in 1927. Funds had been fairly abundant in Germany but this action, coupled with others, started an outward movement and in five months the Bank of Germany had lost about $250,000,000 of its holdings. The discount rate was raised in June and again in October and the money market has been tense ever since.

Before July 1926 funds were leaving France—there was a rapid flight from the franc. Since that time with Poincaré as premier the movement has been reversed—so much so that his problem has been to prevent the franc rising too high. In May 1927 as one step in meeting the awkward situation a large amount of gold was drawn from England to the great distress of that country. It seemed for a time that interest rates in England would be forced up with resulting higher costs for British business. This would have been serious in a country struggling against acute depression and with a million or more unemployed. Thereupon the United States came to the rescue. By agreement discount rates were lowered here, an action that at once relieved the tension on England.

Today, however, a new problem exists. For years we have prevented fairly well the inflation that usually comes with the possession of a large stock of gold. But recently a burst of activity on our stock market has shown that control is not easy. Our discount rates have been raised to four per cent and other steps taken by the Federal Reserve banks but as yet without apparent results. Another increase in the discount rates is being discussed as a means of exercising market control. But if we raise our rates gold may be withdrawn from England and discount rates in England may rise. British industry now reviving slightly may experience a further setback and British markets for our goods reduced. What should we do?

America's position has permanently changed and the new wine is already bursting the old bottles. Old formulæ are inadequate, the old shibboleths are unsuited to the new conditions. Policies and doctrines of the past must change. One expression of this is in the title of a recent French volume —"Political Myths and Economic Realities."

Just how our position should be adjusted on even the four issues mentioned calls for a book rather than a twenty minute paper. Only one broad conclusion can be drawn. Whatever position we may take must be influenced by the attitude of the people of other countries. Interdependence is now too great for us to be indifferent to their views even though we may disagree. Great Britain's "splendid isolation," as it was called, is over. Ours too is ended. In some way and somewhen international coöperation must occur. In practice it will of course be secured in many ways and through numerous agencies.

AN ENACTMENT OF FUNDAMENTAL CONSTITUTIONAL LAW IN OLD SOUTH ARABIA

By JAMES A. MONTGOMERY

(*Read April 21, 1928*)

THE subject of the present communication is germane to the encyclopædic character of the Society, in as much as it concerns not only the restricted field of the philologist, but also the interest of the broad groups of the historian and the student of law. The subject is herewith presented, not so much for any original contribution the writer may give, but in order that an obscure field may be interpreted in its outlines to those wider circles.

My subject matter is an inscription from South Arabia, the modern state of Yemen. It was obtained, in squeeze, by the most distinguished of the hardy band of South Arabian explorers, the late Eduard Glaser, in one of his four explorations in that land, 1884–1904. He published it in his *Altjemenische Nachrichten*, Munich, 1906, pp. 162–191, with translation and notes, and it has been edited again by N. Rhodokanakis, in his *Grundsatz der Öffentlichkeit in den südarabischen Urkunden*, pp. 33–49, Abh. 1 of Vol. 177 of the *Sitzungsberichte*, phil.-hist. Klasse, of the Vienna Academy, 1915.

South Arabian history may be traced back by its monuments, with occasional cross-references from other historical fields, at least into the first half of the first millennium B.C. This is a cautious estimate in view of the fact that many of the South-Arabists would date the earlier monuments well back into the second millennium, and no consensus of opinion has as yet been obtained. Our inscription hails from Kataban, one of the several states which figure in the complicated course of South Arabian history, lying between the present capital of Yemen, Sana'a, and the Red Sea. Its age is involved in the larger vexed chronological problem. The state of Kataban, it is held, disappeared from history at 115 B.C. But the scholars who hold for the higher chronology would synchronize this inscription with the close of the domination of the country by the state of Maʿan, the Minæans of the Greeks, *i.e.* before 700 B.C. Without involving ourselves in this profound chronological dispute, we may simply assign the inscription to the pre-Christian era, leaving open the question of the exact century.[1]

The language is a dialect of the Arabic, its earliest known form, and important philologically, for the Arabic of the Muslim civilization did not assume literary form until the publication of the Koran in the seventh Christian century. The script is in an alphabet of 29 letters, and represents an early forking from the primitive Semitic, so-called Phœnician, alphabet. In their name-formations the South Arabians closely resemble the Hebrews and the dominant class in the First Dynasty of Babylon of the third millennium, and this similarity opens a vista of an original unity of those widely dispersed stocks. There exist also some very remarkable correspondences in usage and terminology with the Hebrew religion.

I must content myself with giving a summary of the inscription. It consists of 23 long lines, and my English translation of it comprises some 725 words. It contains many obscure legal terms, and the syntax is often obscure, so that it cannot be presented in full to the layman or indeed to the scholar, without much detailed commentary. It is

[1] A large number of the inscriptions have appeared in the *Corpus inscriptionum semiticarum*, Part IV, *Inscriptiones himyariticæ;* others have been published by Glaser and by others in many scattered publications. A long desiderated introduction and corpus of the material has begun to appear in the *Handbuch der altarabischen Altertumskunde*, Vol. 1, 1927, edited by D. Nielsen, with the collaboration of F. Hommel and N. Rhodokanakis. For the dates cited above see this work, pp. 71, 74. For more popular introductions to the subject the reader may be referred to Professor Hommel's contribution on "Explorations in Arabia" in the late Professor Hilprecht's *Explorations in Bible Lands*, Philadelphia, 1903, and the same scholar's *Ancient Hebrew Tradition*, London, 1907. Over against Hommel's extreme position on the chronology J. Ktatsch's article "Saba" in the *Encyclopædia of Islam* should be consulted. It is to be said that apart from Hommel the students in the general field of ancient Oriental history do not accept the extreme chronology.

rhetorically interesting in that, apart from the official attestation at the end, it constitutes practically one long period.

Summary

Lines 1–3a. Declaration of the purpose of the edict, that everyone concerned should know what the law of the land is.

3b–7a. The edict refers to bodies of law already enacted: (1) in a parliamentary session held at a named time (year, month, day) and place (temple of a god) through delegates [?],

7b–9a. and (2) to a second parliamentary session, held similarly at a named time and place, which issued a further series of enactments through delegates [?].

9b–12a. Reference is then made to all enactments which may be issued in the future,

12b–14a. and especially [apparently] to all alleviations in the law which have been made or shall be made,

14b–19a. and then it is provided that all these past enactments and all that shall be enacted in the future shall be openly published as authoritative, and that they shall be binding upon the king and the kings [his successors] and the nation,

19b–20. so that the nation may be relieved in regard to all legal processes.

21–22a. And finally this edict shall be engraved on wood or stone, with like sanction as in the case of a royal edict.

22b–23. There follows the date of the promulgation of the decree, and a statement of its authentication by the prothonotaries ["witnesses"], at least eleven names of whom are given.

To sum up: The inscription refers to two previous parliaments, in which bodies of laws had been enacted; and the present inscription is the edict of a third parliament, in which action is taken for the promulgation of those laws and apparently provision made for future laws to be in accord with those previous acts of legislation; and this promulgation is made that all citizens may know their rights and duties.

The edict evidently, if indirectly, includes a Bill of Rights, as one section (19b–20) refers to the relief that the people should have from illegal exactions on their lands, homes, sons and daughters, and chattels. We learn negatively that there was a correction of former abuses. It is thus a promulgation of what may well be called a constitutional law.

Those concerned in the enactment represent several distinct estates:

The King (whose actual name in consonantal form is SHR YGL YHRGB): Towards him most respectful terms are used, *e.g.* the thrice repeated phrase, "in sincerity, submission, devotion to him." Publication is to be made in the name of the king; but the law is to be normative for the king as well as for the people; and with all the respect for royalty there is evident a constitutional limitation of his powers, for the edict is to have the same sanction as a royal decree. We may compare the loyal expressions used by the British Parliament in addressing the Crown.

The Nation or People: this is called by the national name, Kataban. It is composed primarily of two groups, whose designations generally occur paired together: the Nobles, and, evidently a contrasted estate, the TBN, who may be understood as the Commoners. At times the two estates are spoken of as together Kataban, at times, through negligence, one or the other is referred to as Kataban by itself.

Also two other estates are named, the designations for which cannot be interpreted: the PKDT and the BTLT, which are possibly racial or military castes.

And further a distinction is made between two geographical, and so economical, elements, those of the valleys, and those of the plains [uplands?].

Various synonymous terms are used for the parliaments, *e.g. kahal* (the same word in Hebrew, where it comes later to mean "church," like Greek *ekklesia*), "session," etc. And one word has been interpreted, but on slight grounds, as meaning representatives or delegates, sitting in the parliament.

Noticeable is the amount of legal terminology, which is always tediously repeated. I may note the scrupulous recurrence of the names of the estates; the repetition of five or six terms for various species of enactments, as "judgments, decisions, statutes, laws," etc. (compare the similar Biblical terminology). This repetitious verbiage is used with as much care as in a modern legal paper.

There is to be noticed the full and formal attestation of the edict at the end, with the signatures of at least eleven prothonotaries. It is to be assumed that the laws were kept in open archives for consultation by those concerned, for the edict is the official announcement of their existence and validity. This edict is ordered to be engraved on wood or stone; the laws were probably composed in books, of what material we do not know. There occur frequent references elsewhere to prothonotary records kept in "books" (as the Arabic word means).[2]

The writer knows of no other document coming down from high antiquity that displays such a parliamentary constitution of a state along with such a degree of legal and political development. We possess ancient law codes from Babylonia (that of Hammurabi *circa* 2100 B.C.) and Assyria; also the Hittite codes are now being deciphered. But the Mesopotamian codes are enactments of a despotic monarch, like Byzantine *corpora iuris*, while Hammurabi declares that he received his laws direct from his god. The Hebrew codes also claim to be promulgations of the Deity; these appear to be codifications of case-made law, or are ideal programmes of law. But we know nothing positive of the process of constitutional legislation in Israel, and hence cannot pair it with the South Arabic law.[3] Our inscription makes no

[2] See a note by the writer in the *Journal of the American Oriental Society*, 1917, p. 164.

[3] It will be recalled that the late Judge Sulzberger, an honored member of this Society, advanced the theory that the Hebrew state had a bicameral legislature;

reference to any divine source or sanction, although the parliaments are held in temples. However, there are many legal documents with the sanction expressed "in the name of the god X."

Such an advanced constitutional document as this, as also the many legal documents preserved in the South Arabian inscriptions, witness to a high stage of civilization in that land. The country is naturally very fertile, and this natural wealth was developed by great public works of reservoirs and irrigation, which presuppose a well organized and technically minded society. To the east of Yemen lay the frankincense lands of Hadramot, which produced the most valuable commodities of ancient trade. Yemen also was the nucleus of the trade with India and the east coast of Africa with their rare and highly prized products. All this merchandise was assembled in Yemen and then transported by caravan by the great highway following the west coast of the peninsula, the routes forking at the northern end into Egypt, Syria and Mesopotamia, and striking the Mediterranean outlet at Gaza. It was the development by the Greeks of the maritime route by way of the Red Sea and the Indian Ocean, followed more vigorously by the Roman Empire, that destroyed this monopoly of the South Arabian traders and brought about the economic downfall of their states.

Such a combination of natural resources and commercial advantages developed an elaborate economic condition. Moreover the population of the land was composed of various elements, with striking contrasts in polity: the roaming Arab, farmers and serfs, burghers and merchants, an upper aristocracy, probably one that had intruded itself by

see his *The Am ha-Aretz*, *The Ancient Hebrew Parliament*, Philadelphia, 1909, and *The Polity of the Ancient Hebrews*, Philadelphia, 1912. Such texts as ours from South Arabia strongly reinforce some of Judge Sulzberger's theories. The constitution of the Hebrew state was very much that of a constitutional monarchy, limited in practice by the several estates of the realm. And the king and the estates may have coöperated in legislation, in just such a way as our inscription testifies for South Arabia. While the Kataban inscription is archæologically unique, it is of extensive value in suggesting that similar constitutional processes existed elsewhere in the communities of the Semitic world, with their interplay of prince, aristocracy and commoners.

force, along with the inevitable military and religious castes. This social and economic condition was doubtless quite similar to that of the Italian cities in the Middle Ages. To the competition of class and class, to the rivalry for the control of the wealth which lay in their grasp, and to the adjustments of these clashing interests which the safety of the state required, may be attributed the remarkable legal and constitutional developments which the South Arabian inscriptions reveal.

TUNDRA VEGETATION OF CENTRAL ALASKA DIRECTLY UNDER THE ARCTIC CIRCLE

By JOHN W. HARSHBERGER

(*Read April 19, 1928*)

THE arctic islands and plains of vast extent stretching northward beyond the northern limits of the American and Eur-Asian coniferous forests, or taïgas, are covered with a vegetation which consists of dwarf shrubs, low trees, herbaceous plants, mosses and lichens. The Russian word tundra (toondra) originally signified a marshy plain which differs from steppe in that the ground is almost permanently frozen a few inches below the surface, even in midsummer, and with a climate more decidedly arctic than that of the plains farther south and with a corresponding increase in the number of small lakes and morasses.

Several diverse aspects of the tundra are noteworthy. The most desolate, or stony, tundra (rock tundra of Gates) is known as fjeld (fell-field).[1] The bare ground between the rocks is covered thinly with dwarf flowering plants with leathery leaves and strong, woody, perennial roots which establish themselves in the soil pockets or descend considerable distances into the clefts, or crannies of the rocks. The cushioned, or tufted growth forms of the seed plants are common. There are also extensive patches of lichens and mosses. The moss tundra has a less forbidding appearance. It is composed of dense mats of the larger straight-stemmed mosses. These generally grow on an acid soil layer of raw humus. On the peninsulas of Kanin and Kola projecting into the Arctic Ocean near the White Sea, according to Kihlman, the moss tundra is differentiated into peat hillocks (ricks) and puddles. The ricks are elevated cushions of mosses,

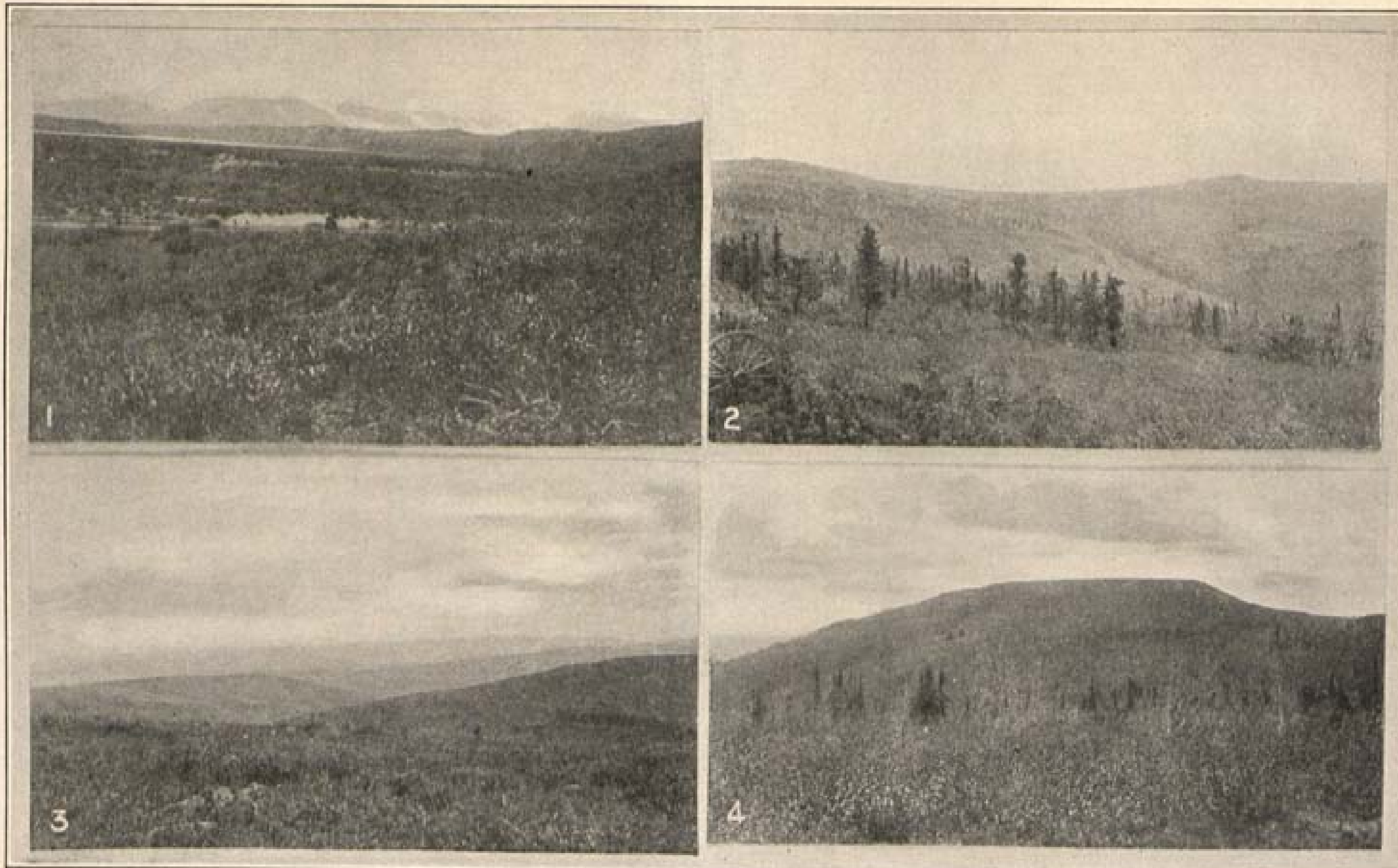
[1] Harshberger, John W., "Alpine Fell-Fields of Eastern North America," *The Geographical Review*, VII: 233–255, April 1919, Illustrated.

while the puddles are wet areas filled with sphagnum, and thus boggy conditions are created. A former student, Genji Nakahara, described to the writer the impounding of rivers by floating rafts of sphagnum, which, accumulating at the mouths of the rivers in Saghalin, formed dams back of which extensive lakes were formed. When the dam finally broke, the accumulated slush flowed out to sea. The lichen tundra is the third form recognized by plant ecologists. It is drier than the moss tundra and exists in several facies which depend upon the species of lichens which are dominant in these associations.

Tundra of Alaska in General

The tundra region of northern Alaska is low and gently undulating occupying the plain which stretches along the Arctic Ocean. Its flora is quite varied for besides lichens and mosses there are many small berry-bearing plants besides dwarf alders, birches and willows. The alders attain the greatest size, but grow in isolated clumps often 6 to 8 feet high. The ground is frozen a few inches below the surface and the heavy sponge-like covering of vegetation is usually saturated.[1] Occasionally high bluffs on the arctic coast in exposed situations are bare and bleak, but there is scarcely an area not covered by low, matted vegetation. Numerous small ponds are scattered over the Alaska tundra and around these bodies of water the vegetation is ranker than elsewhere. The district around Nome is an elevated coastal tundra the face of which in past ages was undermined probably by the encroachment of the sea. Reference to the colored map of the Vegetation of North America by the writer, published by Rand McNally & Company, Chicago, in 1920, will show the location of the barren grounds, or tundra of North America, especially that of Alaska, which is broken up into more or less detached, but extensive, areas by the river valleys, especially those of the Yukon River and its tributaries.

[1] Harshberger, John W., "Phytogeographic Survey of North America," 1911, p. 348.



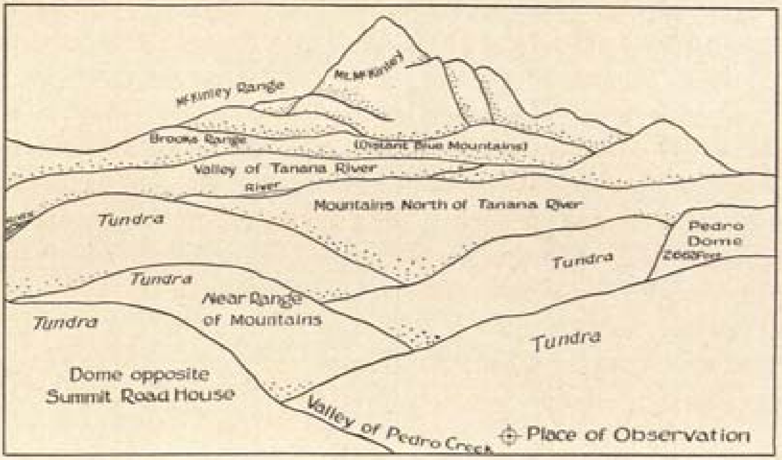
1. Tundra looking South toward Summit Lake and Snowy Alaska Range, August 6, 1926 2. Tundra from Road House toward Mountain Tops with Tors, July 30, 1926. 3. Tundra looking North from Pedro Dome. 4. Looking toward Pedro Dome with scattered White Spruces in the foreground, July 31, 1926.

Situated approximately 65° North, the tundra of the Tanana Hills in Central Alaska covers in general the domes and hill slopes above 2,000 feet altitude (Fig. 3). The forest at this altitude sending finger-like extensions up the gulches (Fig. 2) to 2,300 feet forms a tension line, where the trees gradually become dwarfed, wind-swept and detached as isolated crippled specimens (Fig. 5). We thus have on the tension lines between forest and tundra the same phenomena that we find at the absolute northern tree limit where the taïga is replaced by arctic plant associations. The distinguishing feature here between the tree limit on high mountains in more southern latitudes and in Central Alaska is that the tundra of the Tanana Hills is connected directly with the arctic tundra and may be considered to be its southern extension (Figs. 3 and 11). The tundra vegetation covers the plains along the Arctic Ocean and Bering Sea and in its southern extensions it covers the hills southward to the snowy Alaska Range (Fig 1). This fact of its geographic location is emphasized by the range of the caribou (reindeer), which in their migrations wander from the polar sea southward across the tundra to the Alaska Range.

The Tundra of Central Alaska

Between the Yukon and the Tanana rivers, there are low mountains which are designated on some maps as the Tanana Hills being located approximately in Latitude 65° North. The summits of these mountains are rounded domes that have received local names such as Coffee Dome, Mastodon Dome, Pedro Dome, Porcupine Dome and Summit Dome. Pedro Dome (Fig. 4), which was ascended by the writer, is approximately 2,662 feet and is the highest dome of the region. The character of the two hundred miles of country between Summit Dome on the one side and Pedro Dome on the other, and Mount McKinley, 20,464 feet, is shown in the accompanying sketch of the topography in profile.

The prevailing tundra may be classified as a fourth kind, namely, dwarf birch tundra, or the tundra of the upper hill slopes (Fig. 5) and dome summits.[1] Although the country is a succession of rolling hills with forest-filled valleys between and some objection might be raised to calling tundra the vegetation which covers the summits of the domes and the



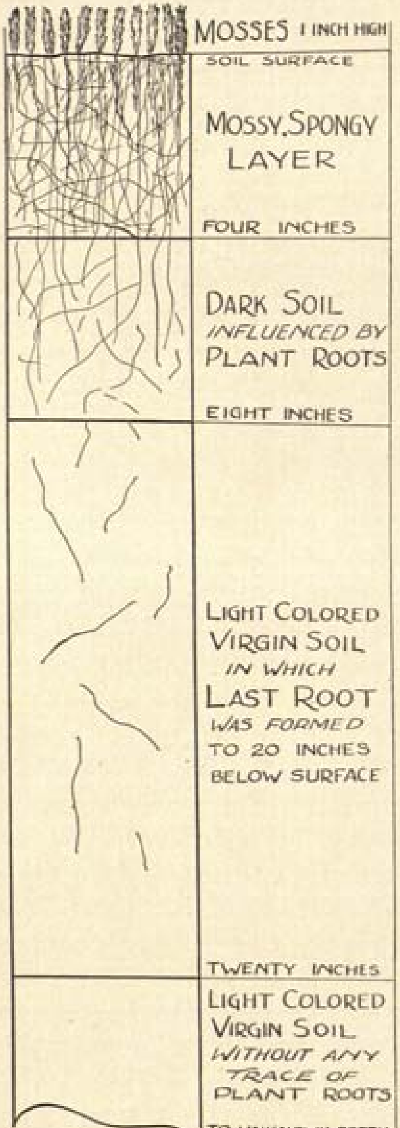
hills, yet the facies and the soil conditions are such as to convince the trained plant ecologist that true tundra prevails of another type.

Tundra Soil

Near the road house, where the writer stopped during the prosecution of his study of the tundra vegetation, a section of the soil was made to the depth of 60 inches (5 feet) to determine its character. The humus layer, or soil filled with the roots of shrubs and herbs and vegetable remains, occupied the first four inches of the section. Below this layer was one relatively dark in color occupying the next four inches of soil. Twelve inches below this layer, or 20 inches below the surface, the last roots were encountered in a lighter, gray soil. Below this layer virgin soil extended to an unknown depth. In digging the hole to determine the stratification of the soil, a shovel was used to remove the soil down to 30 inches, and then an iron poker was driven to a depth of 30 inches. No

[1] *Cf.* Gates, R. Ruggles, "Notes on the Tundra of Russian Lapland," *The Journal of Ecology*, XVI: 150–160, February 1928, especially the fourth paragraph on p. 156.

frost was found in the total 60 inches of soil penetrated. Miners, who were questioned, attributed this frost-free con-

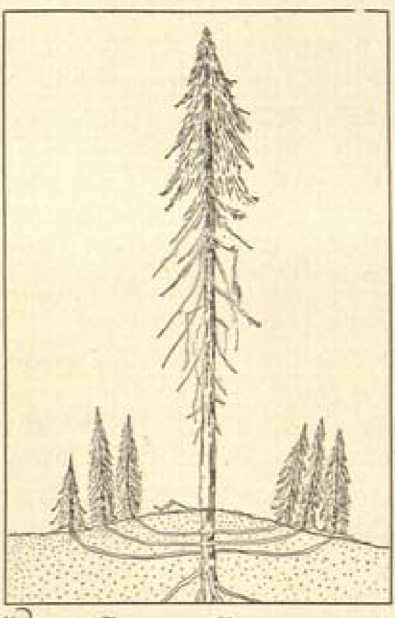


dition on the top of Summit Dome to the abundant rains which had thawed out the soil. According to John Murphy, an old miner, the soil is frozen on the benches at Smallwood, Alaska, to a depth of 275 feet, the frost disappearing from the upper six inches of surface soil during the summer months. Such frozen soils consist of muck, or a heavy moss covering which in a frozen condition may slide down (solifluction) on top of other frozen layers until by these increments the frozen soil may reach a depth of 200 feet.[1] In general, it may be said that on the benches, where muck (peat) is thick, and where the mossy covering is heavy, as at Chatanika, the soil may be frozen at a depth of 200 feet as determined in mining operations. On the hill slopes, where there is no deep muck, or a heavy mossy covering, the frost may leave the ground entirely. This renders possible the raising of grain crops, as along the ridge upon which the Agricultural Experiment Station near Fairbanks is located. Along the river valleys, where the river water percolates into the soil, the frost may leave the soil entirely with the advent of Spring. In other cases, subterranean water courses have been encountered in digging mine shafts and wells, and it has been found that the soil is frostless through which the underground stream flows. The observations of Mrs. Mary Lee Davis, who lived some years in Fairbanks are appropriate:[2] "Our own well is sixty-eight feet deep, and extends through frozen strata to a supply of excellent water that underlies much of our town in a mysteriously "thawed" area. The water is clear and pure and so cold that it frosts a glass immediately, as drawn from the tap, and if the well is not used daily, it freezes solidly especially in midsummer when the frost is being drawn upward through the soil to the sun." Mining companies with a knowledge of the effect of running water in removing frost from soils drive perforated pipes down through the frozen muck and by forcing water through these pipes slowly thaw out the soil. All through Central Alaska one is im-

[1] Harshberger, John W., "Unsolved Problems in Arctic Plant Geography," *Problems of Polar Research Special Publication*, No. 7. *American Geographical Society*, 1928: 148; Nordenskjöld, Otto, *The Geography of Polar Regions Special Publication* No. 8. *American Geographical Society*, 1928: 53-56.

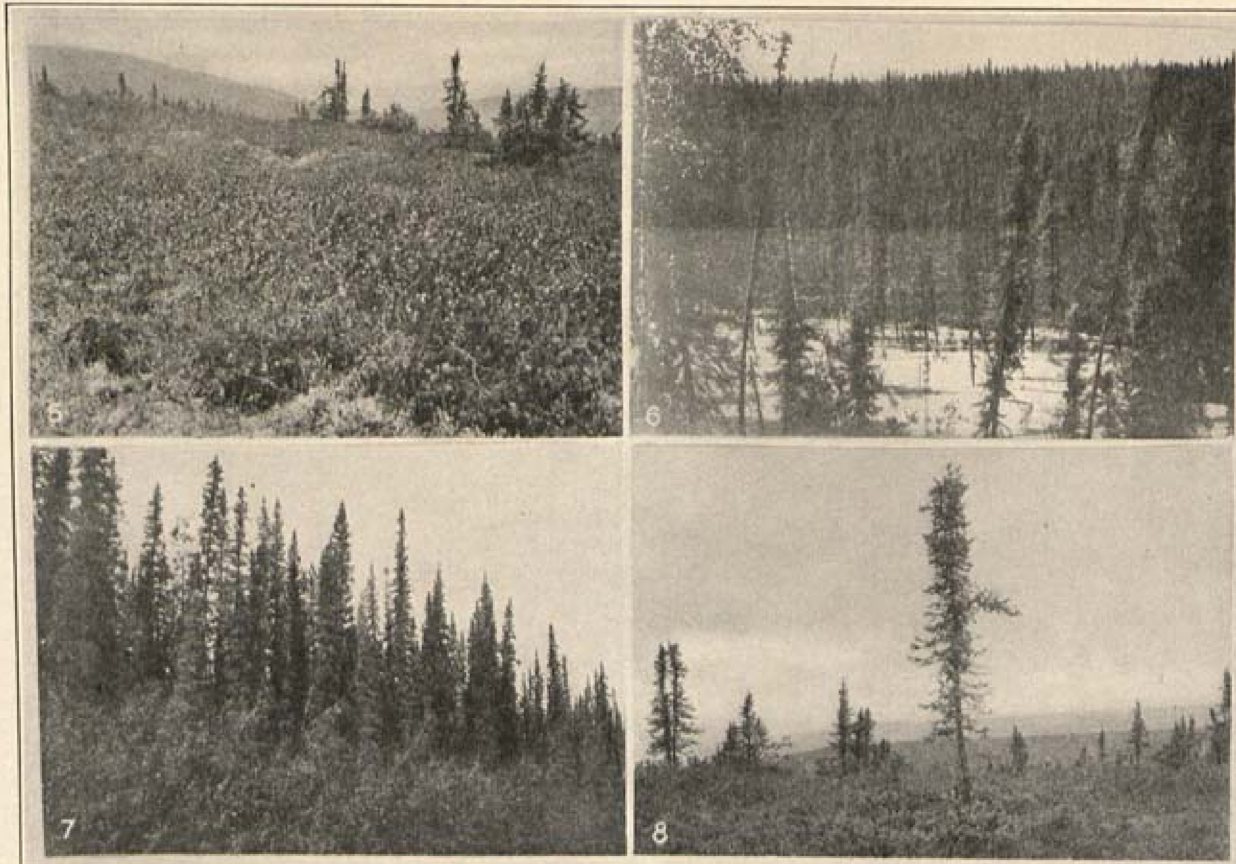
[2] Davis, Mary Lee, "The Social Arctic Circle," *Scribner's Magazine*, July, 1916, p. 73.

pressed by the fact that the character of the soils has a marked influence on the character of the vegetation. On muck soils, where the ground is frozen to considerable depths, the spruce trees are scattered, look scrawny and have a scattered growth (Fig. 6). On the hill slopes, that are better drained, where the frost leaves the soil in summer, the forest closes up and the trees look happy, being better conditioned (Fig. 7). This difference is also noticeable between the spruce trees well down on the slopes of the hills in the valleys and those that form the vanguard in their encroachment on the tundra, which covers uninterruptedly the tops of the domes and the hills. Here the trees become scattered, reduced in size and rely on reproduction



WHITE SPRUCE (*Picea Canadensis*)
Showing Reproduction by Layers

by layers to extend into unoccupied areas where the conditions permit their growth. The stronger winds on the hill tops contribute also in the determination of the growth forms of the trees. The accompanying photographs (Figs. 8, 9 and 10) and the text figure illustrate how the spruce trees gain new terrain by the process of layering.



5. *Betula glandulosa* Michx. backed by line of White Spruce trees. 6. Stunted White Spruce Trees on permanently frozen Soil, along Delta River, Alaska, August 5, 1926. 7. Tall Spruce Trees on well drained Soil fronted by Alders (*Alnus tenuifolia*). 8. First stage in layering of White Spruce.

Facies of the Vegetation

The vegetation, which covers the tops and upper slopes of the hills north of Fairbanks, Alaska, which vary in altitude from 2,200 feet to almost 2,700 feet, is that which characterizes the tundra. The dark-black soil with a covering of mosses and lichens seems to be prevalent on all of the domes. The lichens gray in color sometimes cover such large areas as to be distinguished some distance away. The Labrador tea (*Ledum groenlandicum*) forces its way upward through the spongy covering as also do the low shrubs the crowberry (*Empetrum nigrum*), mountain cranberry (*Vaccinium Vitis-Idaea*), the trailing azalea (*Loiseleuria procumbens*), several prostrate arctic willows (*Salix*), and the dwarf, scrub, birch (*Betula glandulosa*), which grows 2–3 feet tall. As the ecologist looks out across the hilly country, as from the Summit Road House, he notices on the top of the adjacent rounded dome projecting masses of rocks (Fig. 2) which recall the rocks known as tors which form such a marked feature of Dartmoor in southwestern England. Most of the heights of Dartmoor in Devonshire are masses of rocks standing up like old castles and they represent the stumps of higher mountains brought low by erosion. Just as in Devonshire,[1] where the broken down tors are locally termed "clitters" or "clatters" (Welsh *clechr*), so in Central Alaska there are great masses of broken rocks spreading down the slopes from the tors or mountain summits, as on the north slopes of Pedro Dome (Fig. 16). The varying colors of these slopes arrest the attention. Large gray areas of lichens blend with the brownish-green patches of hair mosses and shade off into the dark-green areas where the dwarf birches form low thickets. The outcropping granitic rocks aforementioned are tinted with black, gray and yellow crustaceous lichens. The spruce trees advancing up the V-shaped valleys in phalanxes are extended upward as fingers, or tongues, of the forest which deploy as thin lines into the tundra vegetation (Figs. 2 and 4). They ascend *in echelon* up the valley slopes. Scattered spruces may be

[1] Baring-Gould, S., "A Book of Dartmoor." London, 1923; 14–28.

seen far apart even in the tundra proper (Fig. 8). On protected slopes at 2,300 feet, the scattered spruces may be 12 feet tall, but where fully exposed they grow scarcely six feet in height. On the most exposed places, where spruces grow, they assume a basket-like form. That the spruces at the higher elevations are reproduced by layering is revealed when a fire has eaten away the covering of lichens and mosses, which grew about the bases of the trees, for now the connections between the parent tree and the smaller surrounding trees stand revealed (Text Figure).

The dwarf birch (*Betula glandulosa*) is the dominant plant and it grows usually about 2 feet tall (Fig. 5). The Labrador tea (*Ledum groenlandicum*) is secondary being about 1 foot tall. The blue berries (*Vaccinium ovalifolium* and *Vaccinium uliginosum*) rise about 6 inches above the lower plants such as the crowberry (*Empetrum nigrum*), trailing azalea (*Loiseleuria procumbens*), mountain cranberry (*Vaccinium Vitis-Idaea*) and clubmoss (*Lycopodium clavatum*) which are prostrate. The lichens and mosses over extensive areas form low cushions. Boleti and gill-bearing fungi grow out of the mossy cushions of *Polytrichum* and *Sphagnum*. A few of the arctic willows (*Salix*) rise in groups above the common level of the tundra vegetation. They are 6 to 8 feet tall.

Synecology

In the consideration of the sociology of the tundra plants, only the most general statement is advisable, for in such a vast country, as Alaska, with its great diversity of physiographic conditions what applies to one district may not be applicable to another, and yet in general the tundra in the different parts of Alaska, where the writer has seen it, appears to be superficially the same. The unitiated always remark on the monotony of the vegetation. It is in the floristic composition of the associations of tundra vegetation that the main differences are found and not in the general facies. Without attempting to delimit the various associations of tundra plants, they may be described in general terms.

Lichen Societies. Usually the lichens occupy the ground in societies composed of single species. The most important one both for its extent and its economic importance is the reindeer lichen (*Cladonia rangiferina*), which is food for the herds of caribou or reindeer. Wherever found on flat or sloping tundra, it covers considerable areas, which are distinguished by their light-gray color (Fig. 12). The other lichen societies are recognizable by their tints, which represent various combinations of browns, grays and greens. After, or during rains, the lichen societies are always more green than during dry periods, because the air is driven out of the spaces in their thalli and the green algal constituent becomes more conspicuous. In dry weather, the lichens become grayer in tone. This variable character of the color of lichen societies makes impossible an exact description of the color harmonies. The lichens enumerated below and collected in the tundra covering the Tanana Hills were determined by the late G. K. Merrill of Rockland, Maine, and they are representative of the lichen flora of Central Alaska.

Cetraria alpestris var. *portentosa* Mull.
Cetraria chrysantha Tuck.
Cetraria cucullata (Ball.) Ach.
Cetraria islandica (L.) Ach.
Cetraria nivalis (L.) Ach.
Cetraria pinastri (Scop.) Nyl.
Cetraria salpincola (Ehr.) Nyl.
Cladonia amaurocrea var. *celotea* Ach.
Cladonia coccifera var. *plumata* (Flk.) Schaer.
Cladonia coccifera var. *pleurota* (Flk.) Schaer.
Cladonia gracilis var. *elongata* (Jacq.) Flk.
Cladonia rangiferina (L.) Wats.
Cladonia sylvatica var. *sylvestris* Oed.
Nephroma arcticum (L.) Fr.?
Peltigera aphthosa (L.) Hoffm.
Peltigera scabrosa Th. Fr.
Stereocaulon tomentosum Fr.

The areas covered by these lichens varied in size from individual cushions, a few inches in diameter, to many square feet. The lichens either grew quite distinct from each other, or they were blended in various ways with other species and with the mosses. Crustaceous lichens covered the exposed rocks, but no collection was made of them (Fig. 16). The interspaces on the ground between the herbaceous flowering plants and the dwarf shrubs and trees were filled generally with lichens and mosses, so that in relatively few places (fell-fields) was the soil completely bare. As the writer walked across the tundra, he was impressed by the fact that he was walking on a spongy surface and this impression was heightened on the day of his arrival after a heavy rain, when the ground lichens and mosses were saturated with water.[1] Even with heavy, spiked shoes the feet got unavoidably wet. There are stretches of tundra where the tramper becomes fatigued, as he sinks into the lichens and mosses at every step, or staggers awkwardly over the tops of the low, shrubby growths, which bend and let the feet down between them. It was "mushing" in Alaska.

Moss Societies.—The mosses collected on the hill tundra of Central Alaska were identified by Edwin Bartram of Bushkill, Pa. They comprise *Marchantia polymorpha* Linn., *Pleurozium Schreberi* (Willd.) Mitt., *Polytrichum commune* Linn. and *Polytrichum juniperinum* Willd. The two hair mosses (*Polytrichum*) formed societies, which blended with those of the lichens aforementioned and were recognized by their dark-green color alternating with the gray areas of lichens. Two club mosses (*Lycopodium clavatum* Linn. and *L. complanatum* Linn.) were important constituents of the ground flora.

Flowering Plants as Constituents of the Ground Flora.—Several prostrate flowering plants were of such abundance as to emphasize their importance as constituents of the ground layer of the vegetation. Their presence was detectable at a considerable distance. Such plants, as the crowberry

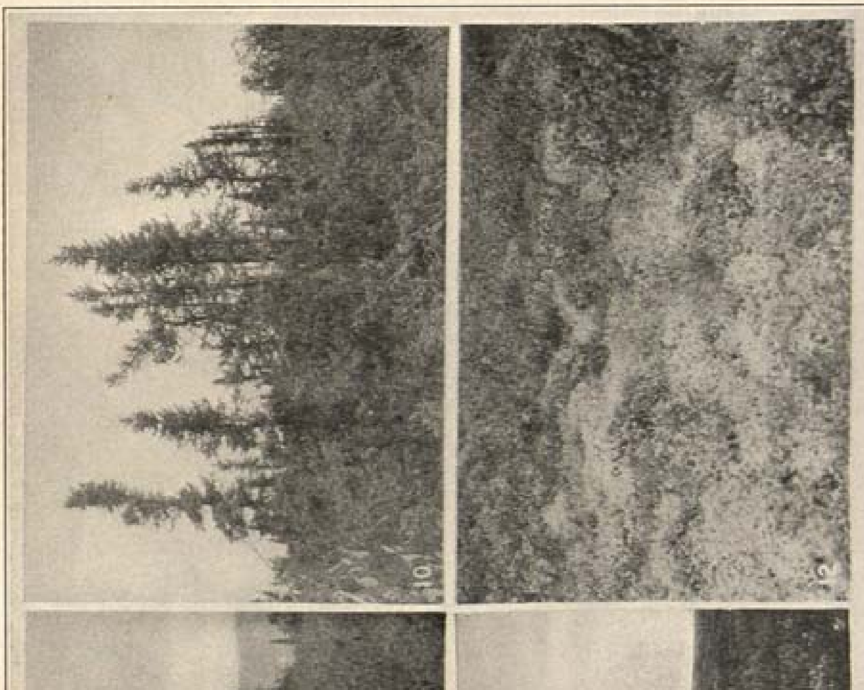
[1] *Cf.* Muir, John, "The Cruise of the Corwin," *Journal of the Expedition of 1881 in Search of De Long and the Jeannette.* Boston and New York, 1917, p. 116.

(*Empetum nigrum* Linn.) formed a very conspicuous element of the tundra vegetation. So also did the bunchberry (*Cornus canadensis* Linn.) in full red fruit on July 30, 1926, and the mountain cranberry (*Vaccinium Vitis-Idaea* Linn.) also with red fruits.

Shrubs and Trees of the Tundra.—The dwarf birch (*Betula glandulosa* Michx.) was the most abundant and generally distributed shrub of the tundra of the Tanana Hills (Fig. 5). It formed low thickets about 2 feet high. Occasionally where conditions were favorable it grew 3 feet tall. The two Labrador teas (*Ledum groenlandicum* Oeder and *L. palustre* Linn.) were found growing in dense matted growths about 1 foot tall. At this level grew also the blueberries (*Vaccinium ovalifolium* Smith and *V. uliginosum* Linn.). The alpine, or trailing Azalea (*Loiseleuria* (*Chamaecistus*) *procumbens* Desv.) with its pink flowers grew as a diffusely branched undershrub in its usual tufted, much-branched habit. A shrubby willow (*Salix pulchra* And.) was found on the dome opposite the Summit Road House, as a distinct element of the tundra vegetation.

Perhaps only three woody plants collected on the tundra are to be ranked as trees. They are a willow (*Salix glauca* Linn.), which grew 6 feet tall, the birch (*Betula kenaica* Evans) (Fig. 13), and the white spruce (*Picea canadensis* (Mill.) B.S.P.). The willow and the birch, wherever found, raised their leafy crowns well above the common level of the tundra vegetation, but never in great abundance. They formed scattered clumps of a few trees, usually not over 6 feet tall. The white spruce on the slopes grew to a respectable size, but on the wind-swept summits of the hills, it was always a sad spectacle in semi-erect, wind-swept, crippled and stag-headed specimens (Fig. 10). Its spread by means of layers has been described in a former section (Figs. 8, 9 and 10). On the tundra, if present, it was always as individual trees.

Herbs of the Tundra.—The herbaceous plants were determined in part by Paul C. Standley of the U. S. National Herbarium. As constituents of the hill tundra of Central



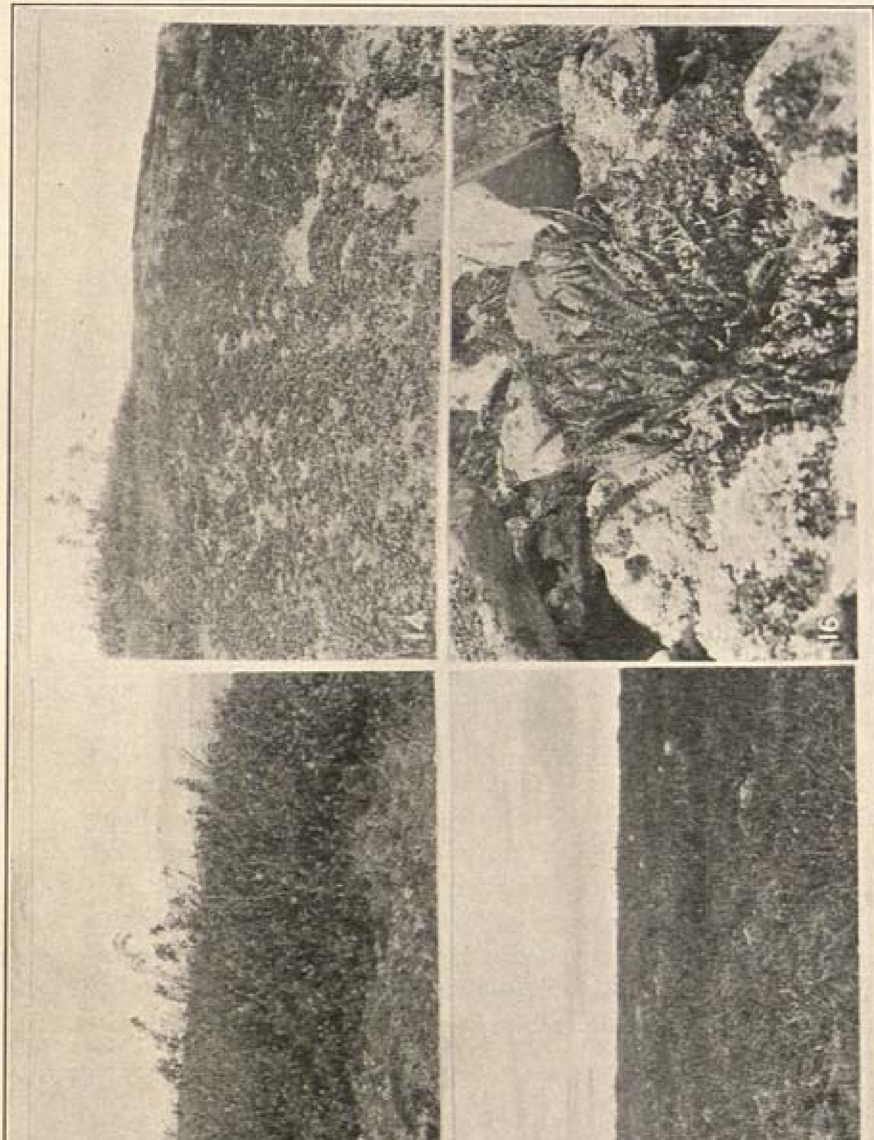
9. Second stage of layering of White Spruce. 10. Circular group of White Spruce Trees produced by layering. Parent tree has disappeared. 11. Tundra 116 miles South of Fairbanks looking toward Donnelly Dome. 12. Tundra Lichen Society. The Reindeer Lichen is here prominent.

Alaska they comprise *Juncoides campestre* (Linn.) Kuntze, *Calamagrostis canadensis* (Michx.) Beauv., *Polygonum bistorta* Linn., *Anemone narcissiflora*, *Arctous alpina* var. *rubra* (Fern) R. & W., *Linnaea borealis* Linn. var. *americana* (Forbes) Rehd., *Cornus canadensis* Linn., *Pedicularis euphrasioides* Stephan, *P. Oederi* Vahl,[1] *Campanula lasiocarpa* Cham. The grass (*Calamagrostis canadensis*) becomes the most abundant grass on the tundra after a fire has swept across the country. In the region investigated, it was common, but not general, as there was no evidence that the country had been burned over. Another evidence of this immunity from fire was the scarcity of the fireweed (*Epilobium angustifolium* Lam.), which in many parts of Alaska is the dominant and conspicuous plant where fires have burned. For miles and miles after fires have raged, the country is purple, when the fireweed is in flower. Around the mining towns and the Summit Road House, knotweed (*Polygonum alaskanum* (Small) Wright), pineapple weed (*Matricaria suaveolens* Pursh), shepherd's purse (*Capsella bursa-pastoris* Medic), chickweed (*Stellaria media* Vill), squirrel grass (*Hordeum jubatum* Linn.), white clover (*Trifolium repens* Linn.) were the most abundant weeds.

Fell-Field Associations

There were two places where the rocks were so plentiful and their tops exposed and where bare ground was visible that these areas might be called fell-field (fjeld), the rock tundra of Gates and other ecologists. On the summit of the Second Dome, back of the Summit Road House at 2,400 feet elevation and on the summit of Pedro Dome (Fig. 15), a flat, rocky tableland (2,662 feet), fell-field vegetation may be said to grow. There were cushions of the Arctic birch in fully exposed situations and the prostrate mats of *Arctous alpina* var. *rubra* (Fern.) R. & W. in autumn color growing close to the ground (Fig. 14). Here also grew in exposed rocky places *Diapensia lapponica* associated with other plants in

[1] The *Scrophulariaceae* were determined by Francis W. Pennell. Academy of Natural Sciences of Philadelphia.



13. *Betula kenaica* Evans, four feet tall. 14. Association of Alpina Bearberry *Arctous* (*Mairania*) *alpina* Niedenzu var. *rubra* (Fern) R. & W. 15. Fell-Field (fjeld) on top of Pedro Dome. 16. *Dryopteris fragrans* (L.) Schott as a clitter plant, North slope of Pedro Dome. Rocks covered with crustaceous and foliaceous lichens.

patches suggesting that we have here (Fig. 14) on a small scale patchy tundra (*Fleckentundra*) mentioned by Gates (see note below). The flora of Pedro Dome comprised *Equisetum sylvaticum* Linn., *Tofieldia coccinea* Richards, *Spiraea densiflora* Nutt., *Lupinus arcticus* Wats., *Saxifraga tricuspidata* Rottb., *Vaccinium uliginosum* Linn., *Vaccinium uliginosum* Linn. var. *pedris* Harshberger,[1] *Pedicularis euphrasioides* Stephan, *Campanula lasiocarpa* Cham., *Senecio frigidus* (Linn.) Fries and *Solidago multiradiata* Art.

The writer in finding *Vaccinium uliginosum* var. *pedris* feels that he has discovered a new variety of whortleberry. The fruit of this form is elongated and ellipsoidal, instead of spherical, and the berries are sweet instead of tart, as in *Vaccinium uliginosum*. In other characters, the presence of bloom on the fruit and in the vegetative characters, the new variety agrees with the species. This new variety, found on the fell-field on the summit of Pedro Dome at 2,662 feet on July 31, 1926, has been named variety *pedris* from the mountain on which it grew.

Clitter Plants [2]

The single fern collected on the tundra was *Dryopteris fragrans* (Linn.) Schott. It grew in great clumps among the loose rocks of the clitter on the north slope of Pedro Dome at 2,500 feet (Fig. 16) and in a similar habitat on the north side of the dome opposite the Summit Road House at 2,400 feet. Its rhizomes and roots grew down into the crevices of the loosely-piled rocks. On the flat tops of separate, large, angular boulders on the north slope of Pedro Dome grew mats of *Empetrum nigrum*, *Cladonia rangiferina* and *Vaccinium Vitis-*

[1] *Vaccinium uliginosum* Linn. var. *pedris* nova varietas. Bacca ovata; fructus dulcissimus. Culmen Montis Pedris, Alaska.

Since drafting this paper, the one by R. Ruggles Gates "Notes on the Tundra of Russian Lapland" has appeared (*The Journal of Ecology*, XVI: 150–160, Feb. 1928). On p. 152, Gates in a footnote speaks of *Vaccinium uliginosum*, as follows: "I observed a variety of *V. uliginosum* with quadrate fruits, another variety with pear-shaped, and a third with elongated cylindrical fruits." In the body of the article, he refers to each species of *Vaccinium* having its own flavor and range of shape.

[2] See ante.

Idaea, in abundant red fruit. Some of the rocks were covered with black, crustaceous lichens.

Conclusion

Stefansson[1] has emphasized the importance of the utilization of the "barren grounds," or tundra, in the raising of caribou for market, and the writer recently had a dinner of reindeer steak in Philadelphia. The caribou feeds on the reindeer lichen (*Cladonia rangiferina*) and other tundra plants, and in winter, if these plants are covered with snow, it paws away the snow to get the lichens (Fig. 12) and other tundra plants underneath. It probably uses also the spade-like, downward projecting, front prongs of its antlers to shovel the snow. The mistake of overcropping these arctic ranges should be avoided, for we do not want to repeat the economic errors, which have been made with the use of the prairie-plains of the United States proper. We should have learned by bitter experience that such deterioration of the natural pastures might have been prevented by a detailed scientific study of the plants of the open range and their best utilization in furnishing food for herds of cattle. The repetition of such economic waste can be prevented in the far north by a thorough investigation of the vegetation of the tundra and the feeding habits of the caribou, coupled with enlightened legislation based on the recommendations of the scientific experts, who have studied the interrelation of the caribou and the plants on which they feed. Detailed research work on the distribution, abundance, growth and reproduction of tundra plants should be made as well as the species of plants which are browsed by the caribou. The feeding habits of the caribou throughout the year should be studied exhaustively. This will furnish data for the control of the ranges and the number of reindeer that can be allowed to roam the tundra without seriously overcropping.

The method of approach has been suggested by Joseph Dixon in an illustrated article in *American Forests and Forest*

[1] Stefansson, Vilhjalmur, "Polar Pastures," *The Forum*, Vol. 75: 9–20, New York, 1926.

Life (March 1928, pages 143–145) entitled "What Deer Eat." The investigation of the food of deer was inaugurated in June 1926 in the Yosemite Valley by Professor A. W. Sampson of the Forest Service and this work was supplemented in 1927 by the studies of Joseph Dixon. With note book, pencil, binoculars, watch and camera ready for instant use the deer were followed, as quietly as possible, and the species of plants actually eaten were recorded. The identical plants upon which the deer fed were collected, dried and saved as herbarium specimens. This insured correct identification of any questionable plant and showed the extent and manner in which the plants were browsed. Examination of the stomach contents of deer shot in the open were used to check the results obtained in other ways. Dixon used the following methods to express the food preference of deer. The number of deer that browsed upon each species of plant was noted. The time or duration of each browsing was recorded. Then by multiplying the number of deer selecting any species of plant by the minutes spent in browsing he obtained what he designated as "deer minutes." Thus, if two deer browse on curly dock, each for a period of five minutes, the results would be totaled as, curly dock—10 deer minutes, when such observations were made daily for a period of two weeks, the investigators began to know what food was eaten by the deer in the Yosemite at that season of the year. To overcome the difficulties of observations made in thick vegetation a 17 inch f. 5.4 Ross telecentric lens and a reflecting camera were used, so that photographs of browsing deer were obtained of sufficient clearness and size to show what was being eaten. Dixon and Sampson found that the food of deer varied greatly with locality and season. Similar investigations might be used profitably in following the herds of caribou, as they migrate across the tundra at different seasons of the year, in order to collect data on which the management of such herds and the maintenance of the range plants might be based. This study of the Tundra Vegetation of Central Alaska is in part a contribution to that desirable end.

THE ATOM

By W. F. G. SWANN

IN ATTEMPTING a review of the story of modern atomic structure, it is appropriate that we first renew our acquaintance with a few of the principal actors in the play.

First we have the electron—the fundamental unit of negative charge—a thing usually thought of until quite recently as a small spherical shell of negative electricity of such minuteness that if you should magnify the diameter of that shell to the size of a piece of small lead shot, that piece of lead shot would, on the same scale of magnification, become larger than the sun. The mass of the electron is so small that if you should magnify all masses so that the electron attained a mass of two and a half grams, that two and a half grams would, on the same scale of magnification, become as heavy as the earth.

Then we have the "proton," the fundamental unit of positive charge—a thing 1800 times as heavy as the electron, but 1800 times smaller in size, so that if you should magnify it to the size of a pin's head, that pin's head would, on the same scale of magnification, attain a diameter equal to the diameter of the earth's orbit around the sun.

Out of these two bricks, the proton and the electron, the attempt has been made to explain all the architecture of nature, and all that nature does. What then are the properties of these bricks whereby they may assume so great a responsibility? The properties which have been imputed to them are comparatively simple. When all is at rest, protons repel protons, electrons repel electrons, while protons and electrons attract each other, the forces in all cases varying according to the inverse square of the distance between the particles concerned, and acting in the line joining them.

We speak of a space around a charge as containing an electromagnetic field. The lines of electric force due to a charge *A* are the lines along which a similar charge *B*, at rest, would be repelled by the charge on *A*. When *A* is at rest these lines travel out radially from it. When *A* is in motion with an unchanging velocity the directions of its lines of electric force are unaltered, and the intensity of the electric field is unaltered except for velocities comparable with that of light, velocities of the order 186,000 miles per second. The moving charge *A* is, however, accompanied by lines of magnetic force which, in the case of an unchanging velocity, form circles around its line of motion. These lines do nothing to another charge *B* so long as it is at rest. If it moves, however, they exert a force on it over and above that exerted by the electric field, a force perpendicular to the lines of magnetic force and to the direction of motion of the charge *B*. This force due to the magnetic field is the same force as we meet with in an electric motor where the wires carrying the current are set into motion by the magnetic field of the motor.

If an electric charge has its velocity changed suddenly, a sort of kink is produced in its lines of force, and this kink travels out along each line of force, its direction being practically perpendicular to the line of force. Theory also shows us that this transverse electric field as it is called is accompanied by a special magnetic field. If the charge moves backwards and forwards like a pendulum, these kinks are made first to one side and then to the other, and travel out into space after each other, in the form of a wave motion. A similar thing happens in the case of a charge moving in a circle. If a lot of elastic threads were tied to the walls of a room and their other ends were tied into a knot in the center of the room, and if the knot were then caused to describe a small circle, waves would travel out along each of the elastic cords and the situation would be closely analogous to what we should have in the case of the lines of force of an electron which was rotating in a circle.

The great electromagnetic theory invented by Maxwell on the basis of the experimental researches of Faraday, tells us the detailed story of the electromagnetic fields of moving charges, and among other things we know that if a charge sends out waves of this kind into the surrounding space, those waves carry electromagnetic energy, and can only be created at the expense of a continual loss of energy of the charge responsible for them.

Thus, to cite an example, an electron can move around a proton in a circle as a planet goes around the sun, the attraction of the proton for the electron being balanced by the centrifugal force of the latter. Were it not for the electromagnetic radiation, the electron could go on moving in the same circle forever. On account of this radiation, however, it must lose velocity and start to fall into the proton by spiralling around in circles of ever decreasing radius. The laws of dynamics teach us, moreover, that the nearer an electron gets to the center of attraction the greater the number of times it will go around per second, so that the more closely packed will be the waves which it sends out into the surrounding space. Such a system would not therefore emit radiation of a constant wave length, but radiation of a wave length which changed continually.

Such then are the official properties of the proton and electron—all they are entitled to on account of their heritage as the offspring of Maxwell's great electromagnetic scheme of philosophy. As we might expect, we shall find them in difficulties occasionally in their ambitious task of explaining the universe, and shall see them stealing here and there an extra property to which they have no inherent right, but which they must have to carry on their business.

The problem of the atom has three primary aspects, the chemical, the radioactive, and the spectroscopic. Since it is around the third aspect that the wars have waged most hotly during recent years, and since it is the spectroscopic phenomena which have led to those developments which in the last few years have bid fair to revolutionize our thought in philosophy, I shall concentrate upon them almost exclusively, and shall make only passing reference to the others.

A characteristic feature of the atom is that when caused to emit light in a manner more or less unimpeded by its neighbors, it does so in the form of perfectly definite vibrations of frequencies characteristic of the atom. We used to think, and for many purposes we may continue to think of these vibrations as passing out in the form of waves in an all-pervading æther. The greater the frequency of the vibrations, the smaller the lengths of the waves. The older theories had a most baffling task to account for the observed facts. Everything seemed to suggest that things should have been different from what they were.

The most alluring thought was of course to picture the atom as a little solar system, with the electrons rotating around a central nucleus of positive electricity as the planets rotate around the sun. We have already discussed the objection to this scheme. The emission of radiation by the atom would be accompanied by the spiralling of the electrons in towards the nucleus and the radiation would change its frequency continuously, so that instead of getting from the atom a number of discrete pure vibration frequencies, we should get a blurred composit of frequencies. So fundamental was this difficulty that the first attempts at a theory of the atom discarded completely the possibility of the planetary system and endeavored to think of the atom as a lot of electrons embedded in a relatively large sphere of positive electricity like plums in a jelly. The vibrations produced by electrons in this condition when disturbed from their positions of rest can be shown to be pure and non-blurred so that the difficulty in this respect was surmounted. It soon became evident, however, that the positive electricity could not exist in the atom in the form of a large sphere. For by measuring the deflections suffered by certain charged atoms in passing through thin sheets of matter it became clear that there existed in the atoms conditions which demanded that the positive electricity should be all collected into dimensions far smaller than those of the atoms themselves. It was necessary that the dimensions should be so small that it was possible to get near to all of the charge at once and so realize forces much larger than would be possible were the charge spread over the whole atom. This view was consistent moreover with requirements from another angle. For since experimental evidence led to the conclusion that the number of electrons in an atom was far too small to account for the atomic mass, it was necessary to look to the positive electricity for an explanation of that mass. Now, strange as it may seem, electrodynamic theory required that in order to get a large mass out of an assigned charge, it was necessary to concentrate it into exceedingly small dimensions, so from this consideration also we were driven to assume the positive charge in the atom to be a thing very small in size. But with the positive electricity concentrated into a minute thing or into a number of minute things, and the negative electricity concentrated into a number of minute things, there seemed no way in which to provide for a stable and permanent structure to be made out of these things unless they should be allowed to seek equilibrium against their attractions by revolving around each other; and so thought centered once more on the planetary idea of the atom with a determination to do the best that might be done with it in making it fit the optical results. And so there arose Bohr's great theory of atomic structure in which it is true the electrons are required to do things which they ought not to do according to their strict traditions but in which they pay for this privilege many fold in the achievements made.

Bohr's theory is founded upon the model of the atom suggested by Sir Ernest Rutherford. In this model we have for the center of the atom a heavy nucleus, containing all of the positive charge of the atom and in general some electrons as well. Around this nucleus we have electrons revolving in orbits as planets revolve around the sun. Suppose we take the lightest atom, the atom of hydrogen. This contains nothing but a single proton with an electron revolving around it. In order to picture to you the relative magnitudes it will suffice to say that the hydrogen atom would look like what the earth and sun would look like if, leaving them at their present distance apart, we should squeeze the sun until it was two miles in radius. You see we cannot draw a true diagram of the hydrogen atom; for, if we should draw the nucleus sufficiently large to be seen, we should have to draw the electron bigger than the blackboard and place it on a circle 300 miles away.

This characteristic of relative emptiness is shared by all the atoms. It explains why we can fire things like electrons through relatively thick pieces of metal. The metal is very compact as regards its resistance to the passage of a big thing but very empty to the passage of small things. The problem of walking through a brick wall is not so miraculous as it seems. I will tell you how to do it. If you should consider any plane section through your body you would find that in any area only about one millionth of the one thousand millionth of it would be covered with the sections of electrons, all the rest would be emptiness, and very much the same thing may be said about the brick wall. Therefore you see that there is plenty of room for you to walk through the brick wall. All you have to do is to make your electrons dodge those of the brick wall.

If we arrange the atoms in a row, in order of their atomic weight, then (with certain slight reservations which need not delay us here) the number which represents the position in the row is called the atomic number. On the Bohr-Sommerfeld theory of atomic structure, the net number of unbalanced protons in the nucleus of an atom is equal to the atomic number, and around the nucleus there revolve in minute planetary orbits as many electrons as there are unbalanced protons in the nucleus. Thus, helium, the atom next to hydrogen in weight, has a nucleus containing two unbalanced protons, and two electrons revolving in orbits around them. If there were nothing more than two protons and two electrons in the helium atom, we should expect the atom to be just twice as heavy as the hydrogen atom, whereas it is four times as heavy. This necessitates the supposition that, in addition to the two unbalanced protons in the nucleus, there shall be two more protons and two more electrons. This principle of half of the total number of protons in the nucleus being neutralized as regards their external action by electrons in the nucleus is one which holds for all the atoms, hydrogen being the only exception.

In order to avoid the difficulties of which I have spoken, and concerned with the fact that on a strict application of electromagnetic principles the electrons of an atom would eventually fall into the nucleus, and in order to provide for the known facts as regards the light which atoms may emit, Bohr introduced a few extra assumptions. These assumptions are rather drastic in type but they are simple in form and few in number. I can best illustrate them by considering the case of the hydrogen atom—the case of a single electron revolving around a single proton.

Now the first assumption made by Bohr is that, although we might be able to knock the electron out of the ring in which it found itself, by bombarding it with some other electron, or by some other means, we could not cause it to revolve in any ring we chose. There are only certain orbits in the atom which the electron may be permitted to have, and these are related to each other in a manner which is quite simple of expression, even though it may be rather artificial in statement. Suppose we draw a line from the nucleus to the electron, and measure the area which the line sweeps out per second. The area will, of course, increase with the size of the orbit. Now Bohr's first assumption amounts to supposing that the orbits must be such that these areas are integer multiples of the area for the smallest. We can have an orbit which makes this area twice, three times or four times the area for the smallest orbit, but we cannot have an orbit for which the area is two and a half times that unit. Moreover, the smallest orbit is such that the area described by the radius vector per second, when multiplied by 2π times the mass of the electron, gives a certain number h, which first made its appearance in the theory of heat radiation and has since invaded first one branch of physics and then another until it has finally assumed a status comparable with those of the electronic charge and mass.

In each of the possible orbits the electron possesses a perfectly definite energy, calculable in terms of the radius of the orbit according to exactly the same principles available for the calculation of the energy associated with the rotation of a planet around the sun. The greater the size of the orbit the greater the energy. We can only persuade an electron to leave the orbit in which it happens to be revolving at the time and go into some larger orbit by giving it energy; and, if it should go from a larger orbit to a smaller one, it would have to part with energy. Now the second assumption which Bohr makes is that, so long as the electron remains in any one orbit, it does not radiate any energy at all. The laws of classical electrodynamics would call for such radiation, but Bohr assumes that, for some reason or other, radiation does not take place. On the other hand he assumes that if one of these electrons, after having been thrown into one of the outer orbits, returns to the orbit from which it was thrown, or to some intermediate orbit, it gives out the whole of its surplus energy in the form of vibrations of definite wave length, determined entirely by the amount of energy which has been radiated. He assumes in fact that the number of vibrations emitted per second is just proportional to the energy change, the factor by which we must multiply that frequency (or number of vibrations per second) in order to obtain the energy change being the same mysterious constant, h, which had already been used in specifying the possible orbits, and which, as I have already pointed out, had previously made its appearance in other branches of physics.

The passage of an electron from any one orbit to a smaller orbit thus gives rise to a perfectly definite wave length radiated; and, in the ideal case, by properly stimulating the electron by throwing it out to the various orbits it should be possible to cause the atom to emit as many frequencies as there are possibilities of this kind of passing from one orbit to a smaller one. Thus we see how it may come about that this very simple structure, consisting of no more than a positive nucleus and an electron, may give rise to a large number of different frequencies.

Of course, it is to be distinctly understood that Bohr's theory makes no attempt to give what, in the ordinary use of words, we might call a reason for the performances which are postulated, and it does not describe any mechanism by which the radiation is emitted during the passage from one orbit to another.

Even a superficial examination of the light emitted by glowing hydrogen will show that there are orderly relations between the wave lengths of the various colors emitted. These relations have been studied very carefully by the spectroscopist; and, long before he had any theoretical reason for it, he knew that he could calculate the wave lengths actually found by substituting, successively, the numbers 3, 4, 5, etc., for n in the formula

$$\frac{1}{\lambda} = 109{,}678\left(\frac{1}{4} - \frac{1}{n^2}\right). \qquad (1)$$

Now the different wave lengths which Bohr's theory predicts as corresponding to the passage of an electron between the various orbits are just those which would be obtained by substituting the numbers 1, 2, 3, etc., for n_1 and n_2 in a formula of the type

$$\frac{1}{\lambda} = 109{,}678\left(\frac{1}{n_1^2} - \frac{1}{n_2^2}\right). \qquad (2)$$

A special case of this is the formula (1), where $n_1 = 2$. This is the Balmer series. But Bohr's theory predicted not only this possibility but also the possibilities to be obtained by putting $n_1 = 1$, and constructing a series on this basis, and the possibilities by putting $n_1 = 3$ and constructing a series on this basis, and so on. Some of the lines and series predicted in this way were known before the time of Bohr's theory, and others predicted by the theory have been found since. It is, moreover, a particularly cogent argument for the theory that the number 109,678, obtainable from spectroscopic data, is correctly predicted by the theory, at least to the degree of accuracy with which the quantities like the electronic charge and mass occurring in the expression of it are known.

Suppose now we pass to the next heaviest element, helium. What has the Bohr theory to say? According to the views we have already discussed, to the effect that the nuclear charge changes by one unit at a time as we proceed along the periodic table, the helium atom should contain a nucleus whose total charge is twice that of the hydrogen atom; and, around it we should have two electrons circulating in orbits. Here again, Bohr assumes that there are only certain definite orbits which the electron can describe, but the complexities of the motions for even this case are so great that the problem has not reached a final solution. If the helium is subjected to a strong electric discharge, however, we have reason to believe that an appreciable number of the atoms lose one of their electrons; and, under this condition, the system which remains should be just like the hydrogen atom, except that its nuclear charge would be twice as great. Bohr's theory becomes perfectly specific in its statements for this case. The corresponding orbits have exactly half the radii that they had in the case of the hydrogen atom, and the theory predicts for the wave lengths radiated a formula of the type (1) with a multiplying constant differing from 109,678 by a factor of four, a result in beautiful harmony with experiment. The beauty of this harmony is enhanced still more by the fact that it is not quite exact. Let me explain this apparent paradox. In the simple theory which leads to the factor four, it was assumed that in both the hydrogen and helium atom, the nucleus was so heavy in comparison with the electron that the former remained at rest while the latter revolved around it. Of course, when two bodies *A* and *B* are of equal mass, it is no more correct to say that A revolves about B than to say that B revolves about A. What happens is that they both revolve about a common point half way between them. When the masses are very unequal, as in the case of the sun and earth, or the nucleus and the electron, the point about which the rotation takes place is very near to the center of the heavier body. If we allow for the actual difference between the point of rotation and the center of the heavier mass, our formulæ are modified to an extent which is different for helium and hydrogen, since the helium nucleus is heavier than the hydrogen nucleus. The result of this is that the numerical factor which occurs in the formula for helium should not be just 4 times 109,678 but 4.001626 times this number, and this is just what it is.

Before leaving the hydrogen atom, I must refer to one or two other matters of very fundamental importance. You will observe that so far we have always spoken of the orbit of the electron about the nucleus as though it were a circle. We know, however, that both from theory and from observation, the orbit of a planet around the sun is, in general, an ellipse, with the sun at one focus; and, only in very special cases does this ellipse degenerate into a circle. Our atom would therefore seem much less artificial if we could permit the orbits to be ellipses. You will recall that the possible circular orbits were determined by the law that they were such that if the area swept out per second by the electron in the innermost orbit was $h/4\pi m$, in the next orbit it was twice this amount, and so on. Now the laws of nature have been very kind to the mathematician in providing for the well-known fact that, in the case of elliptic motion about a center of force, equal areas are swept out by the radius vector in equal intervals of time, so that if we speak of the area swept out per unit of time this has a definite meaning wherever the electron happens to be in its orbit. Unfortunately, however, while to fix the area swept out per second by the radius vector completely determines the orbit in the case of a circular orbit, it does not do so in the case of the elliptic orbit, since, for any given value of this quantity, there are an infinite number of different degrees of bulginess, or eccentricity as the mathematician calls it, which the orbit can have; and, in each of these orbits the electron possesses, in general, a different energy. If we should try to build a theory on the lines successful for the circular orbits, we should immediately land ourselves into trouble, because the passage of the electron from any one of the infinitely many orbits to any other would correspond to a line in the spectrum which was different from that corresponding to the passage between any other pair of orbits, so that we should predict spectra with infinitely many more lines than experiment requires. If we are to have elliptic orbits, we must find some further restriction to impose upon them in order to limit the number which are to be regarded as possible. Now Professor Sommerfeld has expressed this restriction in a very beautiful way. In its mathematical expression it is very closely bound up with the first restriction. In fact, both restrictions are separate aspects of a more general restriction which is expressed in Sommerfeld's mathematics. Unfortunately, the mathematical principles involved are too complicated for me to express them here in such a way as to bring out the harmony of this relationship. I can, however, tell you of this second condition in a manner which is very simple of expression but which, as a sort of penance for the simplicity of its expression, hides from you the beauty of its relationship to its fellow. The first condition is similar to that for circular orbits, and states that the area swept out per unit time by the line joining the nucleus to the electron must be $Th/2\pi m$, where T is an integer. The second condition, which serves to fix the eccentricity of the orbit, amounts to this. Suppose we consider the elliptic orbit in question, characterized by the integer T as regards the area swept out per second. Let us draw a circle about the ellipse in such a way as to just inclose the major axis. Then, the ellipse must be such that the ratio of QP to PO, Fig. 1, is the ratio of some integer number to the number T. With this restriction we limit ourselves to a finite number of possible orbits. There are many more than in the case of the circular orbits; but, for a curious reason, resulting from the fact that

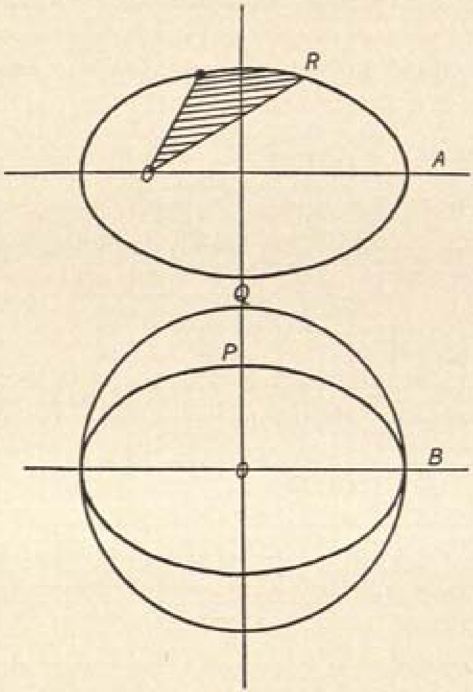


FIG. 1

they do not all correspond to different energies, the situation is not greatly complicated thereby. Fig. 2 represents the possible orbits for the case of hydrogen. It is customary to speak of each of these orbits as corresponding to a certain energy level, meaning thereby the energy of the electron when in the orbit. Now corresponding to the lowest energy level we find only one orbit, a circle, and the same circle as we encountered in the simple theory which confined itself to circular orbits entirely. Corresponding to the next energy level, which is again the same as in the case of purely circular orbits, we find two orbits, a circle and an ellipse. Corresponding to the next energy level, which is the same as the third level for purely circular orbits, we find one circle and two ellipses, and so on.

Carrying over the assumptions of the original Bohr theory we have to recognize that whenever an electron passes from one energy level to a lower one, it emits radiation of a frequency determined by the difference of the energies. Now

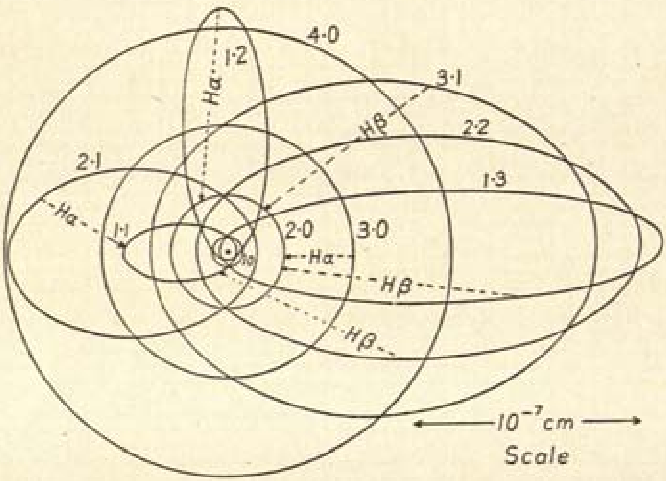


Fig. 2

the number of different energy levels is no more than it was on the simple theory of circular orbits. It is true that any given energy level may be realized in several ways, in an ellipse of one or of another kind, or in a circle; but, so long as we believe the Bohr hypothesis, any pair of orbits is equivalent to any other pair having the same corresponding energies, as regards the frequency of the light emitted as an electron passes from one member of the pair to the other. We thus see how this extension to elliptic orbits, while greatly enhancing the logical beauty of the scheme, is not attended by a destruction of the valuable features presented by the simpler model with circular orbits.

Once again, however, the foundations of the more complete theory receive a more complete justification from the fact that they are not quite correct. The matter of the relation between the numerical factors in the formula for the hydrogen and helium lines becomes satisfactorily taken account of, as in the simpler model for circular orbits, by recognition of the fact that the nucleus does not remain absolutely fixed during the revolution of the electron; but, there is something else. Electrodynamics and the theory of relativity alike support the conclusion that the mass of an electron experiences a very slight increase as the velocity of the electron increases. This increase mounts to an infinite extent for an electron which approaches the velocity of light; but, for velocities as great as 20 miles per second, it only amounts to one-half of one per cent. The velocities of the electrons in their orbits are sufficiently great to cause this variation of mass with velocity to play a small part in the dynamics of the system; and, the important thing which results is this: if we pick out all those elliptical orbits which, on the basis of neglect of change of mass with velocity, corresponded to the same energy level, these orbits will, with the change of mass with velocity taken into account, have very slightly different energy levels. Now what will be the result of this? Let us concentrate our attention on the two orbits, say *A* and *B*, which in the absence of this relativity correction had next to the lowest energy level, and the three orbits *C*, *D* and *E*, which under that condition had the energy level next but one to the lowest. In the absence of the relativity considerations, the frequency emitted by an electron is the same in a fall from *C* to *A* as from *D* to *A* or from *E* to *A*, or from *C*, *D* or *E* to *B*. With the relativity correction taken into account, however, the energies corresponding to *C*, *D* and *E* will be slightly different from one another, and the energies corresponding to *A* and *B* will be slightly different from each other, so that there will be a slightly different change in energy in falling from *C* to *A* from what there will be in falling from *C* to *B*, for example. Where, on the simple view, we got but one line in our spectrum corresponding to the fall of an electron between these two energy levels, we now get several, differing from each other by very small amounts. The theory thus leads us to believe that if we should examine our lines in the spectrum of hydrogen with sufficient care we should find that they consist of a number of separate lines very close together. This is exactly what experiment reveals; and, the measurement of the small separations of these lines, and their comparisons with the predicted amounts, were regarded, at any rate until a year or two ago, as the most beautiful verifications, not only of the Bohr-Sommerfeld theory, but also of the conclusions which the theory of relativity has suggested for the change of the mass of the electron with velocity.

While the Bohr theory tells us the frequency and the total amount of energy in the radiation emitted during the passage of an electron from one stationary orbit to another, it tells us nothing more about that radiation. It does not describe the mechanism of the process, neither does it give us any information as to the relative numbers of times the different transitions occur. In other words, it tells us nothing as to the measured intensities of radiation to be expected for the different colors. Bohr has endeavored to bridge this gap in his so-called correspondence principle.

If for simplicity we confine ourselves for a moment to two circular orbits between which an electron makes a jump, we observe that the electron has a certain frequency of revolution in the orbit which it leaves, and another frequency of revolution in the orbit to which it goes. Neither of these frequencies is the frequency which it emits in the process of the jump. The frequency emitted in the process of the jump falls between the other two. Now as we go out farther and farther from the nucleus the change in frequency of rotation from one orbit to the next gets less and less, so that for the orbits very far from the nucleus the difference in frequency between successive orbits is zero, and since the frequency of the light emitted in passage from one orbit to the next lies between the frequencies of rotation in those orbits themselves, it follows that for the orbits far out from the nucleus, the frequency emitted in a one quantum jump, that is a jump from one orbit to the next possible, is the same as the frequency of rotation in either the initial or the final orbit.

If we turn our attention to the elliptic orbits which are far removed from the nucleus we find a rather more general conclusion. The planetary motion of an electron in an ellipse is not of what is called a simple harmonic type. In other words, if I should imagine a piece of elastic fastened to the electron at one end, the other end going off to infinity in the plane of the electron's orbit, and if I should allow the electron by its elliptic motion to propagate waves along that piece of elastic, those waves would not be of the simple form associated with pure frequencies. They would correspond to what in acoustics would be a whole lot of different pure notes superimposed on each other. Now it is possible to analyze the elliptic motion in such a way as to find what those "notes" are, and moreover to find the intensities of each of them. There is one "note" as I may call it with a period corresponding to the complete period of the electron in its ellipse. We call it the fundamental. There is another note of half this period, we call it the first harmonic; another of a third the period, we call it the second harmonic; and so on. Now if we consider the elliptic motions in which the electron travels far away from the nucleus, the frequency of a one quantum jump is equal to the frequency of the fundamental in either of the ellipses concerned. The frequency in a two quantum jump is equal to the frequency of the first overtone in the elliptic motion. The frequency of a three quantum jump is equal to the frequency of the third overtone in the elliptic motion, and so on.

Now if it had only been a fact that the frequencies radiated were those of the elliptic motion, we could have calculated their relative intensities since we know all about the elliptic motion. What Bohr does is to postulate that, in the case of orbits far away from the nucleus, the intensities of the different frequencies actually radiated are proportional to what they would have been if they had been radiated from the elliptic motion itself. In particular this leads to the conclusion that if any of the harmonics, for example the *n*th harmonic, in an elliptic motion is absent, *i.e.*, if it has zero intensity, then the frequency radiated in the *n*th quantum jump will have zero intensity. It will not be there.

Bohr has extended this idea, with modifications, to orbits which are not so far removed from the nucleus in the foregoing sense. Here, however, he goes no farther than to assume that the frequency radiated, for example, in a three quantum jump is determined in some way by the amplitudes of the third harmonic in the orbit from which the electron comes and the third harmonic in the orbit to which it goes. Here the correspondence principle speaks with greatest definiteness when the intensity of either of the harmonics in question is zero, in which case it predicts zero intensity for the line as radiated on the quantum theory. It predicts that the line is absent. Bohr has also extended his correspondence principle in such a way as to provide information on the so-called polarization as well as the intensities.

Now physical theories are very like some people. Sturdy and brilliant in their youth, they develop ailments as they grow older. And so the Bohr-Sommerfeld theory has developed ailments.

The Bohr theory works very beautifully for that simplest of structures, the hydrogen atom; but when we come to more complicated atoms the trouble begins. The problem of what happens when two bodies are thrown into space and move about under their mutual attraction was solved by Newton, but the complete answer to what happens when even but three bodies are thrown into space and allowed to move under each other's influence has never been given. With the addition of more and more bodies the problem becomes complicated in rapidly increasing degree. You may imagine the problem which confronts the mathematician when he wants to work out the results to be expected in say an atom of iron which has 28 electrons to amuse him.

Now we should not be justified in condemning a theory as wrong because it is complicated and taxes too greatly the skill of the mathematician; but there is something more serious. For there are indications that even if we were endowed with infinite mathematical skill so that we could take into account all the mutual perturbations of the electrons, the results obtained by calculating these perturbations in the classical way would not be right. For the influence which the electrons ought to exert on each other is in part just the influence of that radiant energy which the quantum theory forbids them to exert while they are moving in their stationary orbits.

Then, apart from all this, the very meaning of the theory becomes ambiguous when applied to complicated atoms. For it turns out that the law which tells us how to choose from all the orbits which would be allowed by the astronomical law just those which are to be permitted—this very law ceases to speak with definiteness in the case of complicated systems. If there were some method of calculating and appropriately fixing the electronic orbits which were to be regarded as possible in the Bohr theory, the remainder of the story as to the frequencies emitted would be definite. We should simply have to consider all possible *states* of the atom corresponding to the different electronic orbits, and the difference between the energies in any two of these states divided by Planck's constant h would give us a possible frequency of vibration which could be emitted by the atom.

However, the complexity of affairs we should expect as a result of our calculations if we could carry them out along the lines of the Bohr theory seems to suggest that the spectrum we should predict for a substance like zinc, or copper, would be far more complicated than that which corresponds to nature herself. The human architects of the atom have built to enclose it a scaffolding far more complicated than the atom itself and one which will not hold itself together, let alone provide a support for the atom.

I have already referred to the fact that the theory in its simplest form says nothing about the relative intensities of the different frequencies emitted, and it even fails to predict why in certain cases frequencies which might have been expected to occur are absent. It is true that Bohr has attempted to bridge this difficulty in his correspondence principle. But this principle attains its end by the rather mysterious process of predicting what would happen in a certain case by setting up a correlation with what we should get in another case if something happened which does not happen, and as such it leaves us a little unsatisfied. I must, however, remark, even at this stage, that the newer theories would be in no position to criticize the correspondence principle on the ground of artificiality alone, for they live in houses of much thinner glass. It is only on the basis of lack of definiteness in its statements that the correspondence principle can be criticised in relation to the newer theories. I think it is worth while to point this out, for since the parents of the Bohr-Sommerfeld theory were classical and conservative, one is apt to regard radical tendencies in that theory as of much greater seriousness than one would attribute to them in such theories as the matrix theory or the theory of Schrödinger, which were born in radicalism.

The nature of these difficulties is such that even Bohr, the prime author of the theory, has felt the necessity for a complete revision of the whole matter, so that today we speak of the old Bohr theory as the classical quantum theory, and I have even heard the more recent developments referred to as the jazz quantum theory.

A few hundred years ago, the only thing which satisfied the average mind in the matter of an explanation of phenomena was an appeal to the supposed actions of some being or group of beings. The sun went around the earth because it was carried by angels. At this stage the mind was content. If we had raised the question of why the angels wanted to carry the sun around the earth, the probable answer would have been to the effect that it was not for us to inquire into that, the modern paraphrase of which statement is, "Well, we must assume something, we must take something for granted, we must start somewhere." Now it is in the difference in our conceptions of this "somewhere" from which we are willing to start that is to be found the root of most of those troubles in which one thinker will contend that the theory of some other is meaningless to him. Each individual, or class of individuals, has certain criteria (usually unconscious ones) which determine for him whether or not he will be satisfied with a certain type of theory. If the theory starts out from his own "somewhere" all is well. Possibly he is content if it proceeds to discuss nature in terms of masses interacting with each other through elastic media. If the elastic properties which it is necessary to attribute to the medium are very like those of water he will be more than content. He will be happy. You may convince him that, in the sense in which he uses the word "understand," he does not understand the reason why water acts as it does. You may rob him of his philosophy, but you cannot rob him of his happiness. He will shrug his shoulders and say, "You are talking Metaphysics. We must start somewhere." You may even get him to admit that he has no reason to be happy, but he will be happy all the same. Once he is happy he stays that way, regardless of any attempt on your part to show him that he ought to be morose.

If now you make some different starting point, a starting point which says that the orbits of the planets are a set of curves which make a certain integral minimum, or something equally abstruse, our friend will assert that your statement has no physical significance. "What," says he, "is the reason for starting there?" You may ask him what the significance was of starting where he started, and remind him of his confession that he could not justify it. As a last resort he will then probably make that ultimate personal appeal to his own anatomy in assuring you that he feels in his bones that such and such is the case, and beyond his bones he will not go.

Now he who would understand the point of view of the new mechanics of the quantum theory need not get to the point of feeling in his bones that it is true, but he must get to feel in those bones that there was nothing to the feeling which he may formerly have had in his bones as to the ultimate fundamentality of the older views. Those older points of view were built up by analogy with the behavior of the large scale phenomena of life which presented themselves to us in our youth and became ingrained in our consciousness as natural before we had reached the age at which we might question their necessity.

In considering such a matter as the theory of the atom there are two questions: (1) why it works, and (2) how it works. Now the first of these is the question which fascinates us most but is the more dangerous of the two, for its answer is entirely a function of our point of view. The problem is like the origin of man. To one mind the starting point of the Darwinian theory is more fundamental than the garden of Eden. To another, the garden of Eden is more fundamental than the Darwinian theory. One mind may feel that an atomic theory founded on something like a Newtonian law of gravitation is more in harmony with the fitness of things and with his feelings in particular than would be any other, although the more he thinks about the matter the less justification he will find for his choice, apart from the crucial test as to which fits best the observed facts. The tendency of the modern quantum theories is thus to concentrate attention upon how the atom works. And what is the problem before us in saying how the atom works? A large part of it is contained in the endeavor to state what light frequencies are emitted from the atom and what the relative intensities of the vibrations are under different conditions.

Now we might take a big book and write down in it all the measurements that had ever been taken by anybody on the spectra of atoms, and whenever anybody wanted any information about the atoms we could refer him to the book. To present such a set of data to a physicist would be something like presenting to a librarian a list of all the books in the library, arranged in no sort of order, or perhaps in the alphabetical order of the names of the authors. Our librarian would immediately want to make a card catalogue or something of the kind so that he could import some sort of order into his library and be able to give an idea of its scope and features without citing every book it contained.

A theory of the atom in the modern sense is crudely analogous to a card catalogue of the phenomena. To state the matter perhaps more exactly, we seek in the theory of the atom a way in which by saying only a few things we can deduce as their consequence all that that atom does. We make no profession of giving any reason for the few things—indeed we have advanced beyond the stage at which we take any particular comfort in a reason for anything. This aspect of physics has of course been in evidence from the time of Newton. In the pre-Newtonian days all sorts of reasons were sought to explain why the planets did what they did. Many liked the idea of the planet moving in a circle or, failing that, in a manner capable of being compounded out of circles. So complicated did the theory become—not so much in content, but in expression of what it sought to say, that there must have been many who felt as did the sovereign of Castille, when he remarked that "had the universe been constituted in that way he could have given the Deity good advice." The great feature of Newton's law of gravitation lies not so much in any *explanation* which it may be supposed to afford for the planetary motions, as in its power to give, in one simple statement, something from which the motions of all the planets may be calculated. The statement is that each planet moves in such a way that its motion at any and all subsequent instants may be calculated by supposing it to have an initial velocity in some direction at some point, and thereafter to depart continually from that motion in the sense and amount determined by an acceleration which at each instant is in the line joining it to the sun and is in amount proportional to the inverse square of the distance therefrom.

Now, it is not very easy to give any adequate idea of the sense in which these developments, which in the last two years have been associated with the names of Heisenberg, Born and Schrödinger, may be regarded as theories of the atom. Yet, I should like to try to give you some sort of an idea of the way in which these theories speak. By way of introduction let us revert for a moment to the now old and classical Bohr-Sommerfeld theory. Those familiar with the mathematical technique of the theory will recognize the following expressions:

$$H = H(p_1, p_2 \cdots q_1, q_2 \cdots), \tag{1}$$

$$-\frac{dp_r}{dt} = \frac{\partial H}{\partial q_r} \quad (2); \qquad \frac{dq_r}{dt} = \frac{\partial H}{\partial p_r}, \quad (3)$$

where H is the so-called Hamiltonian function appropriate to the system we are studying, in this case an atom. Equations (2) and (3) are the canonical equations, and the p's and q's are the momenta and coördinates of the system. Equations (2) and (3) are the differential equations of motion of the system. To be more explicit, we recall that for the hydrogen atom

$$H = \frac{1}{2\mu}(p_x^2 + p_y^2 + p_z^2) - \frac{e^2}{\sqrt{q_x^2 + q_y^2 + q_z^2}}, \tag{4}$$

where the q's are the x, y, and z coördinates of the electrons with respect to the nucleus, and where e, and μ are constants. Equations (2) and (3) give us the set of equations

$$\left.\begin{aligned} \frac{dp_x}{dt} &= \frac{-e^2 q_x}{r^3} \\ \frac{dp_y}{dt} &= \frac{-e^2 q_y}{r^3} \\ \frac{dp_z}{dt} &= \frac{-e^2 q_z}{r^3} \end{aligned}\right\} \quad (5) \qquad \left.\begin{aligned} \frac{dq_x}{dt} &= \frac{1}{\mu} p_x \\ \frac{dq_y}{dt} &= \frac{1}{\mu} p_y \\ \frac{dq_z}{dt} &= \frac{1}{\mu} p_z \end{aligned}\right\} \quad (6)$$

On solving these equations we can obtain the orbit of the electron provided that we specify the values of the p's and q's for some particular instant. Or, we may do another thing, the thing which is done in the quantum theory and, from all the infinite possible number of orbits which satisfy (5) and (6), we may pick out only those which satisfy some other conditions which our experts in the quantum theory will recall are taken as the conditions that, with $\int pdq$ taken in the usual sense, $\int pdq = nh$, the quantity n being an integer. For any one of the orbits we have picked out, we can find q_x, for example, as a function of the time. It will be a complicated function of the time, but it is capable of being analyzed into a number of simple harmonic functions—pure tones if you will. In other words we can write q_x, in the case of a simply periodic system, for example as

$$q_x = q_x' + q_x'' + q_x''' + \cdots, \text{ etc.,}$$

where

$$\begin{aligned} q_x' &= a_1 \operatorname{Cos} wt, \\ q_x'' &= a_2 \operatorname{Cos} 2wt, \\ q_x''' &= a_3 \operatorname{Cos} 3wt, \text{ etc.} \end{aligned}$$

If these tones had been appropriate to the frequencies emitted by the atom for any or all of the orbits, we should not have found it necessary to go further for our theory of the atom. For not only would the frequencies be determined, but the amplitudes would also be determined by our calculations. It is true that we should have had remaining the difficulty of how the electron could continue to move in its orbit if at the same time it were radiating; but this difficulty would have been one of a kind different from the main difficulty of determining the frequencies and amplitudes.

Now, I have been speaking about Hamiltonian functions, canonical equations, $\int pdq$, and the like, and the non-mathematical members of the audience do not know what I have been talking about. It may even have required a trial of their faith to believe that I know what I am talking about. I should like, for a moment, to put the two sections of the audience on the same level. I want to remove from the minds of our more learned friends all that is there concerned with what they will call the physical significance of the process we have sketched, and I want to stress the facts of what has actually been done—and here our non-mathematical friends will follow me provided they do not hold on to a veiled suspicion that I am trying to play a trick on them and imply something more than I say. What has actually been done is this:

We have, in (4), started with certain letters arranged in a certain way for this our hydrogen atom. It is almost as though we had started with the name of the atom. We have then done something with these letters by which, in (5) and (6), we have gotten other arrangements of the letters and symbols. Our learned friends will tell us that these are differential equations—that they mean something—that the dq_x/dt on the left-hand side of the first of equations (6) is the change per second in a certain quantity which represents the distance of the electron from the nucleus measured parallel to the axis of x. They will tell us that this equation shows that the x component velocity of the electron is a function of certain quantities which are on the right-hand side of the equation. Our non-mathematical friends will be impressed, but bewildered, by all this additional learning imparted into the significance of these expressions. I will ask them not to be bewildered, however, and not to listen to our mathematical friend who is raising all the trouble about the significance of his processes. I will ask them to take note of no more than this—that we have started with a certain arrangement of a set of letters and numbers, which we regard as characteristic of the hydrogen atom, a name form as I may call it, and that we have gotten out of this other arrangements of the letters and numbers by certain rules of procedure which are perfectly well defined; defined, I mean in the sense that even one who knew nothing about what our mathematical friend would call the physical significance of the steps could nevertheless be instructed as to how to get these new arrangements of the letters from what I have called the name-form. And so the procedure from the stage where we had reached these new arrangements of letters to the last stage, where we had

$$\left.\begin{aligned} q_x &= q_x' + q_x'' + \cdots, \text{ etc.} \\ \text{with}\quad q_x' &= a_1 \operatorname{Cos} wt, \\ q_x'' &= a_2 \operatorname{Cos} 2wt, \\ &\text{etc.} \end{aligned}\right\} \tag{7}$$

could be specifically defined in terms of a cold-blooded manipulation of the letters irrespective of any meaning to be attached to the various processes carried out. We are to understand that the processes of manipulation concerned are so specifically defined that we should know what to do had we started from any name-form other than (4) which we said was to be characteristic of the hydrogen atom. For each name-form, there is to result a set of relations like (7) which are obtained by carrying out these perfectly well-defined processes on the name-form.

The point where we are to tie up with nature is at the stage of the expressions (7).

The things which I have written as $a_1, a_2, \cdots, w, \cdots$, etc., will have in them certain letters like e, μ, n, h, which occurred in our original name-form, or which were introduced during the process of the manipulations of the letters. We must understand that we are to assign appropriate numbers to these symbols at the stage represented by (7) so that the a's and the w's will become numbers. We would then wish our theory to be to the effect that the numbers $w/2\pi$, $2w/2\pi$, $\cdots$ represent the various frequencies communicated by an atom to the surrounding medium and the a's represent the relative values of the amplitudes of vibration associated with these frequencies. As I have already remarked, the frequencies obtained in this way do not correspond to the frequencies yielded by nature, and our knowledge of the situation is such as to show us that we could not in any *reasonable* way choose a name-form which would, by the application of the process I have pictured, give rise to groups of frequencies such as we do find in nature.

Since then, starting with a certain name-form for the atom and going through certain manipulations of the letters, we arrive where we do not wish to be, we may ask ourselves whether there may be some other ways of manipulating the letters so as to take us where we really wish to go. There is a method which does this—it is the method of the theory of Born and Heisenberg, and the methods of manipulating the letters follow very closely the methods used in the theories of what we call matrices. In the more general sense, mathematics is the science which discusses the ways in which we may set up rules of procedure for the manipulations of letters —or anything else for that matter. These rules which are rich in relationship tickle our fancy, and get recognized as outstanding branches of the subject. That particular branch of mathematics which we first studied in our youth discusses only those manipulations of the letters which would be permitted if the letters represented numbers. In that particular branch of mathematics, *ab* denotes the multiplication of *b* by *a* and we are never permitted to say that *ab* is not the same thing as *ba*, because 3 times 2 is equal to 2 times 3.

But you may ask, "what do the letters mean if they are not numbers?" The answer is that they do not mean anything in particular. They are simply letters, that is all. The rules for manipulating the letters may be made perfectly definite without attaching any meaning to them, and we may define an interpretation to the results obtained at any stage.

But you will say, "all of this is very artificial and has but little meaning." Not at all. There appear to be some regularities in the behavior of the atom, and if we can discover the law of that regularity in a concise form we have done a great thing. For, if that law of regularity which we find fits the atom in all respects which we have observed, it will probably fit it in several things which we have not observed, and so suggest further search. In the whole realm of mathematics there are little oases rich in the expression of laws—little oases in which many harmonies find their expression. If we can find a region of regularity in which there is a one-to-one correspondence between certain things in the mathematics and certain phenomena of nature we have found a "theory" in the modern, or ultra-modern sense of the word.

You see how fundamental a thing is that expression which I have called the name-form of the atom. It is more fundamental than the actual name. We can almost think that, in the community of atoms, that atom which we have called hydrogen would be known rather as

$$\text{Mr. } \frac{1}{2\mu}(p_x^2 + p_y^2 + p_z^2) - \frac{e^2}{\sqrt{q_x^2 + q_y^2 + q_z^2}},$$

than Mr. Hydrogen.

One of the great achievements of the matrix theory of Born and Heisenberg lies in the fact that it leads to relations between the amplitudes of the various vibrations associated with the atom. Moreover, in a perfectly general way it leads to certain broad conclusions as to the occurrence or non-occurrence of certain frequencies. Whereas the correspondence principle of Bohr spoke only vaguely upon these matters, the matrix theory speaks with definiteness—provided that the mathematical processes necessary for the solution can be carried out.

There is in the matrix theory a difficulty analogous to that presented in the non-radiating orbits of Bohr. For all that the theory provides for is certain vibrations *in the atom.* It does not tell us anything about the radiations of these vibrations into the surrounding space, and the nature of the theory is such as to suggest that such radiations would be accompanied by changes in the atom itself which would interfere with the purity of the radiation emitted. We cannot readily escape these difficulties by claiming that the communication of what we ordinarily mean by energy to the surrounding ether does nothing to the atom. For since by such radiation an atom can affect another situated elsewhere, as in the photo-electric effect, for example, such an assumption would be the equivalent of postulating that an atom can affect others without itself suffering any change, and the denial of this possibility constitutes a postulate more fundamental than that of the conservation of energy.

Hard upon the heels of the matrix theory there followed the theory of Schrödinger. Here again, the endeavor is to provide for a procedure which will determine a set of vibrations with appropriate frequencies for the atom; the procedure in being thus made to provide for the frequencies provides also for the amplitudes, so that we arrive at the same stage as was reached on the matrix theory. The Schrödinger theory proceeds along lines which make in some respects a closer appeal to our physical intuitions, however.

If we should attach a prong to a tuning fork and cause it to set up ripples in a bowl of water, these ripples would be reflected from the walls of the bowl, and very quickly we should realize a steady state of what the physicists call standing waves in the water. The ordinary laws of dynamics would enable us to calculate the nature of these waves. Now, in the hydrogen atom and in the ether surrounding it, Schrödinger supposes that there exists a state of affairs crudely analogous to these standing waves. That which oscillates is not the light itself, but is the source which sends out the light emitted by the atom, and the standing waves are waves of electric density. If we should now endeavor to produce in the light waves the frequencies which we desire by simply assigning those frequencies of vibration in the standing waves, we should accomplish very little from a philosophical standpoint. What Schrödinger does is to start once more with that expression which I have called the name-form of the atom, and devise a system of laws by which one can specify the standing waves in the ether in such a manner that through the name-form as a starting point they shall receive the frequencies which they require. Once more we have the principle of starting with a certain mathematical form which is the thing characteristic of the atom, a form moreover of relatively simple structure and containing but few letters and numbers. We proceed to apply to it a process of mathematical manipulation, the same for all atoms, with a view to deducing the more detailed and complicated properties of the specific atom itself.

It turns out that while there is no simple electron in the older sense of the word there does exist a close relationship between the electric density distribution as defined in the Schrödinger theory and what was the electron in the old theory. If we should consider the case of a large electronic orbit of the old theory, the Schrödinger picture would correspond not remotely to it, but in place of the electron there would be a distribution of vibrating charge extending over an appreciably large region. The analogy with the older picture would become less and less the smaller the orbit until for such orbits as exist in the hydrogen atom in its unexcited state it would lose its significance completely. The situation is something like one where we feel perfectly definite in the meaning to be attached to the earth's inhabitants rotating around the sun which is at a distance large compared with their relative journeyings during the process, while it would be perfectly futile to form in our minds a picture of the people on Times Square, N. Y., as describing an orbit about the center thereof; for, in the latter case relative motions of different parts of the crowd would be large compared with any motion concerned in its complete journey around the center of the Square. Thus, while to the large-scaled eye the electron may be still regarded as an entity whose properties are confined to the region of a point, to the eye of an imaginary inhabitant of the atom itself it is merely a vague quivering nebulosity.[1]

The growth of the new theories during the last three years has taken place with such rapidity that one can hardly expect, as yet, consistency in all their parts. Even when the framework of the theory is consistent in its various funda-

[1] In developments which have only become elaborated since this address was presented it has become necessary, alas, to discard this concept of the electron, and to demote what is here called charge density to an abstract quantity whose average value in a certain region is merely a measure of the probability of the electron's presence in that region.

mentals, we shall be lucky if, as time goes on, it does not predict some consequences inconsistent with experiment. Or perhaps I had better modify that statement and say that we shall be unlucky unless it does this; for it is a fact which we may regard in the light of comfort, or in the opposite light according to our inclinations, that progress is only to be made in times of trouble. Let us beware of the day when difficulties vanish, when theories hang together and there are no facts that fail to fit them, for then indeed will science be dead and our labors ended.

THE DISTANCES OF THE STARS

By S. A. MITCHELL

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The last ten years has witnessed in astronomy a remarkable series of brilliant achievements that have thrilled and startled the whole scientific world. On the one hand, the astronomer in coöperation with the physicist and the chemist has been making an attack on the structure of the atom. The physicist and the chemist in their investigations have been confined to terrestrial laboratories and their researches on the practical side have been limited to the range of temperatures and pressures available by mechanical methods. No such limitations however have been placed on the work of the astronomer. He has had at his disposal, with no expense other than that of his astronomical equipment, the celestial laboratories of the sun and distant stars, where high temperatures and minute pressures are readily available to test and extend the physical-chemical theories. Remarkable discoveries have been made, into which subject on account of lack of time I cannot enter today.

The more brilliant of the recent astronomical investigations have been connected with the stars. It is no exaggeration to state that not a single one of these discoveries would have become possible were it not for our ability to measure with a high degree of precision the enormous distances that separate us from the stars.

The astronomers of the sixteenth century saw clearly that if the earth makes an annual journey about the sun, as stated by the Copernican theory, then the near-by stars must show an annual displacement back and forth with respect to the more distant stars. This annual parallax of the stars was too small to be discovered by the telescope invented by Galileo in 1609, and so one was forced to assume that the universe was made on a much larger scale than had hitherto been thought.

With each increase in telescopic power, with each instrumental improvement which added to the accuracy of stellar measurements, the problem of finding the distance of the stars was again attacked. Hooke, Flamsteed, Picard, Cassini, Horrebow and Halley, each in turn attempted to find the displacements of the stars. Each in turn failed in the attempt though Halley found that three of the brightest of the stars, Aldebaran, Sirius and Arcturus were not in reality *fixed*, since each star has a slight but unmistakable motion of its own, which we now call *proper motion*. Halley came to the conclusion that the stars were at least 20,000 or 30,000 times more distant than the sun, though the exact distance of the sun was then unknown.

The first of the world's gigantic telescopes was made by Sir William Herschel. He attempted an ingenious method of detecting the annual displacement of the stars by measuring accurately the relative positions of stars near each other in the sky, one being bright the other faint. Although Herschel did not succeed in detecting the parallaxes of any stars, he did find an entirely new type of stars, of which we now know many thousand, namely double and binary stars.

Following Herschel's time, continued attempts were made to measure the distances to the stars—but these attempts ended only in failure. It remained for the year 1838 to have the honor of measuring the first stellar distance. Singularly enough, the distance of not only one star was measured but three, by three different observers, using instruments of three different types and employing three different methods. The greatest honor probably belongs to Bessel in determining the distance of 61 Cygni. The instrument used was the heliometer which had the peculiarity that its object-glass was cut exactly in halves.

Once started, the work has gone steadily forward from that day to this. In the first fifty years, the total number of parallaxes measured was only about fifty, the difficulty lying in the fact that the stars are at such enormous distances that the annual parallaxes, or the angles to be measured are excessively minute. Judged by present day standards of accuracy, the results of these early measurements, acquired only after the expenditure of prodigious amounts of labor, are most of them fit only for the waste-basket, the errors of observation completely covering up the quantities sought.

In the Silliman lectures delivered at Yale in the year 1910, Dr. W. W. Campbell, the director of the Lick observatory summed up the situation in the following words: "Measures of stellar distances present difficulties so great that even today we possess reliable knowledge of the approximate distances of not over one hundred stars. At no point in astronomical science is fuller knowledge more desirable, more pressingly urgent, than in the subject of stellar distances; or speaking technically, of stellar parallaxes."

The year 1910 had witnessed the completion of two big pieces of splendid work. Lewis Boss had investigated the proper motions of 6,188 stars and had given the results in his *Preliminary General Catalogue.* This was a discussion of all the star catalogues that had been made since Bradley's time stretching over a period of one hundred and fifty years. On the other hand Campbell had just published the radial velocities of all the brighter stars in the sky within reach of the Lick telescope. He himself had constructed the spectrograph, had devised the methods of work and had seen his plan carried through to successful completion. The work of both Boss and Campbell are standards of excellence unsurpassed in quality even at the present day.

In spite of these two splendid achievements, each devoted to the motions of the stars, we knew comparatively little of the actual linear motions of the stars. Proper motions are derived in seconds of arc per year, measured at right angles to the line of sight and thus projected on the background of the stars. On the other hand, radial velocities, or motions in the line of sight, as measured by the spectrograph, give the motions towards or away from the observer but measured in kilometers per second. On account of the use of two different units of measurement (seconds of arc per year and kilometers per second), it was impossible to combine together proper motions and motions in the line of sight. However, as soon as the distance of the star is measured, then immediately the proper motions of seconds of arc per year can be transformed into kilometers per second; and as a result we at once ascertain the linear motion of the stars, both in magnitude and in direction. The weak link in the chain, as Campbell had stated, was the work on stellar distances. Substantial progress in our knowledge of stellar motions was at a stand-still until there was a decided improvement in parallax work, both in the quantity of stars measured but more particularly in the accuracy of the results attained.

As was to be expected, the improvements came through two agencies: first, through the application of photography instead of visual methods, and second, through the employment of larger and still larger telescopes. The largest instruments naturally give the highest precision, and any results less accurate than the very best are not good enough.

Pritchard at Oxford and Rutherfurd at New York did pioneer work in attempting parallaxes by photography. The noted Kapteyn, by using extra precaution in the work at the telescope greatly increased the accuracy of the results. Russell made a step forward in his work at Cambridge, England. It remained for Schlesinger, however, to show the pitfalls lying in wait for the parallax observer and to devise methods of counteracting or eliminating the defects that can creep into the measures, with the result that the work is now of the very highest quality. The few of us who are parallax observers are all of us followers after Schlesinger. We depart from his methods only in the details.

An astronomer can now finish as many parallaxes in a year as was formerly possible in a life time using visual methods, while the accuracy of photographic work has reached the next decimal place.

Schlesinger's measures became possible through his use, twenty years ago, of the largest telescope in the world, the Yerkes refractor of forty inches aperture. This however is a visual telescope and was not intended for photography. As every one knows, the ordinary photographic plate uses the blue and violet light, and makes little or no use of the green and yellow. The optician who ground the 40-inch objective made the yellow and green to come to a sharp focus. The blue and violet rays had to be left out of focus. If a photograph is taken with the ordinary plate and visual telescope, the star images are not sharp and clean-cut, with the result that measures of the highest quality cannot be made. By following the methods already used in commercial photography, G. W. Ritchey showed that with the employment of a yellow color-filter and isochromatic plates it was possible to secure stellar photographs with a visual telescope. Strange as it may seem, the images of the stars thus taken with a visual telescope are sharper and better defined than can be secured with a photographic telescope. Unfortunately, the visual telescope takes a much longer time to secure the photograph.

The only direct method of measuring the distances of the stars is known as the trigonometric method. This is the same method as is followed by an engineer who wishes to measure the distance across a river that can not be traversed, or it is the same way the astronomer uses to measure the distance to the moon. The base line used with the stars is the colossal one, judged by terrestrial standards, of the diameter of the earth's orbit about the sun of one hundred and eighty-six millions of miles. Compared with the distance of even the nearest of the fixed stars this enormous distance is pitifully small. Still it is the largest we have, and it becomes a question of either using that or nothing.

In modern photographic work we obtain an accuracy measured by a probable error of $\pm$ ″.01 or less. With the McCormick telescope, a hundredth of a second of arc means the two thousandth part of a millimeter, or the half of a micron. In the measurement of the relative positions of the stars on the photographic plate we cannot employ the high magnifying power in the measuring microscope used by the biologist, but must content ourselves with a moderate power of about ten.

At the McCormick observatory it takes about ten minutes to secure a suitably exposed plate. Instead of taking the plate out it is shifted about a tenth of an inch and another set of star images is impressed on the plate. On each star region for the determination of stellar distances we take about three or four plates, each with two images, in each of five successive seasons at six month intervals. The parallax depends on the measurement of fifteen to twenty plates. The average probable error of the McCormick parallaxes is $\pm 0''.009$. If more images are taken on each plate and more plates, it is possible to diminish the probable error. When we started parallax work about a dozen years ago it took a total of fifty hours altogether to secure a single parallax. By working out methods of efficiency we have been able to reduce the time required per parallax without in any way sacrificing the accuracy.

The modern work of determining the distance of the stars has been practically the combined research of six observatories, all of them in the United States except Greenwich. In the table below is given, in round numbers, the record of those observatories which have published 200 or more parallaxes.

Observatory	Published Parallaxes
Allegheny	800
McCormick	800
Yerkes	275
Mt. Wilson	275
Sproul	275
Greenwich	275
All others	275

It will be noticed that the observatories naturally fall into two classes, Allegheny and McCormick being in a class by themselves, each having measured about the same number of parallaxes and each having finished three times as many stars as any of the other participating observatories. The total number of individual parallaxes by photographic methods is now approaching 3,000 distributed over 2,000 separate stars. The six observatories are all in the northern hemisphere. Lately the Yale University observatory at Johannesburg and the Cape observatory have entered into this work.

The trigonometric method here described is the only direct method of measuring stellar distances. There are several very excellent indirect methods but their results must always be calibrated by means of the trigonometric parallaxes. There is no other way of standardizing them. In the last analysis, all indirect parallaxes, including those determined by spectroscopic methods, will stand or fall only in so far as the average of their results agree with the best of the modern trigonometric parallaxes. It is therefore of the utmost importance that these direct parallaxes be as reliable as possible since the standards for measuring the dimensions of the universe depend on the trigonometric parallaxes. The distances to the stars depend on the earth's distance to the sun, this in turn depends on the size of the earth, which in turn depends on the standard yard or meter. Since the Allegheny and McCormick observatories between them have measured the distances to most of the stars whose parallaxes are known by trigonometric methods, then it may almost be said that the dimensions of the universe depend on the Allegheny-McCormick standard.

Scientists all know that the exact size of or distance to an object is unknown and unknowable except through measured quantities. I can never know the exact width of this table. If I make the observation more and more refined I can increase the accuracy by diminishing the probable error. As has been said many times, the history of science is the continued attempt to reach the next decimal. Of course it is quite possible to apparently measure quite accurately the width of this table but with a scale that is either too long or too short, thus giving rise to systematic error. It is usually more difficult to investigate and eliminate the systematic errors in a series of measurements than to look after the accidental errors of measurements on which the probable error depends.

In measuring stellar distances there has been always the very closest kind of coöperation between the participating observatories. For instance, the McCormick observatory has a card catalogue which gives a complete record of every star that is on the observing program of Allegheny, Yerkes, Sproul and other observatories. In turn we have mapped out our program in order to duplicate in part the work of others in order to increase the accuracy of the combined work of all, but to avoid too much duplication which would limit the total output. We have put on our program those stars which in our opinion will give the maximum information to astronomy as a whole. We are always ready to measure the distance to any star in which any astronomer is particularly interested.

The duplication of results, to which I have just referred, gives a means of testing and comparing the measures of the different observatories, particularly in the investigation of any peculiar systematic errors influencing the results. In such an inter-comparison it is important that you yourself do not discuss your own measures. Of course you know that theoretically "figures never lie," but practically it is always possible to juggle figures in science as well as in business so as to make them tell almost any story you please. Up to the present, I have been able to resist the temptation of making the discussion myself. However, it has been done by many others using many different methods. I am here speaking about the systematic errors which influence all of the measures and not about accidental errors on which the probable error depends. On the whole the probable error of measurement is about the same for all of the observatories now participating in parallax work.

At Mt. Wilson observatory, a few years ago van Maanen and Stromberg, each independently and each using different methods, determined the systematic errors which are found in the parallax of four American observatories.

Observatory	Telescope	Stromberg	Van Maanen
McCormick.....	26-in. visual refractor	+″.0003	−″.0016
Allegheny......	30-in. photog. refractor	+.0026	+.0027
Yerkes.........	40-in. visual refractor	+.0034	+.0036
Mt. Wilson.....	60-in. photog. reflector	−.0030	−.0020

It will be seen that although the systematic errors are small enough to be almost negligible, yet it seems certain that such systematic errors do actually exist. If we can find the cause of these systematic differences then we can undoubtedly make our observations in such a way as to obviate or eliminate them.

The telescopes engaged in the work, all of great focal length and aperture, are of two different types, reflectors and refractors. For giving stellar positions with the highest degree of refinement, it is generally recognized that the reflector has some defects not found in the refractor. For instance, the reflector is very sensitive to changes of temperature that alter the focal length and hurt the definition. Moreover, spherical aberration limits the field of good definition in the reflector so that a 26-inch refractor has a much larger field of good definition than a 100-inch reflector. Then the colors of the stars may affect their positions on the photographic plate through atmospheric dispersion. When we get away from the zenith our atmosphere acts like a prism. A number of years ago Hertzsprung found that if a photograph is taken with a photographic telescope, then the light of a white star of A type is refracted more and is consequently found nearer the zenith than that of a reddish star of K type. At an altitude of 45°, the difference amounts to the very considerable angle of (0″.18) eighteen-hundredths of a second. This can be tested by a number of different methods, the simplest perhaps being in photographing at various altitudes a double star like β Cygni with pronounced differences of color. More recently Schlesinger reached the same conclusion in his splendid work of making a star catalogue by photography. At the same time that Hertzsprung found such a substantial difference in positions for white and for red stars when the photograph is taken by means of a photographic telescope using blue light, he found the effects of color when using a visual telescope and yellow light are so small as to be negligible.

It is readily seen in parallax work that if the star whose distance is to be determined is redder or bluer than the set of comparison stars, then the parallax may be affected by atmospheric dispersion and be in error if a photographic telescope is used unless special precautions are taken to obviate the effects. Recently, Slocum and Mitchell working independently have shown that parallaxes by visual telescopes are entirely free from these sources of error.

For those of us who are forced to use a visual telescope for photographic work, and are thus compelled to spend many more hours at night in the observatory, where it is generally cold and disagreeable, there is some small amount of satisfaction in knowing that our final results escape some of the snares lying in wait for the photographic telescope.

I hope you will pardon me if I have taken up in detail a few of the precautions that are necessary in securing trigonometric parallaxes of the very first quality. I know you will pardon me for not going into further details. Perhaps I may have persuaded you that it is no easy task to steer clear of all sources of systematic errors.

Granted, you will say, you know the distance of a star, then what further information is possible in addition to this bare fact. First, we can compare the intrinsic brightness of different stars by putting them all at a standard distance where the parallax is $0''.1$ or the distance ten parsecs. The brightness of the star is then called its *absolute magnitude* to distinguish from *apparent magnitude*. The absolute magnitude of our own important sun is about $+5$, or in other words, if placed off at the standard distance it would not be a brilliant star of first magnitude but would be of fifth magnitude and would give enough light only to be well visible to the naked eye.

Then as I have already told you, as soon as we know the parallax of a star, we can combine together the proper motion and radial velocity and thus can know the linear velocity both in magnitude and direction.

When this information is available from many stars we can find out whether there are any common or stream motions exhibited by the stars. As all motions are relative, we can also find how the motions of the stars reflect the motion of the sun, and thus can be derived the solar apex and the speed of the sun's motion.

If the star whose distance is known is a double star and the double star observer has found its orbit, then in obedience to the fact that Newton's law of gravitation is universal we can find the mass of the double star system in terms of the sun's mass as unit.

Let us apply these principles to a few of the more interesting stars. A few years ago we were all thrilled by Michelson's magnificent achievement of measuring the angular diameter of Betelgeuse with the 100-inch reflector at Mt. Wilson. Fortunately we have a very accurate determination of the distance to Betelgeuse from the four observatories, Allegheny, McCormick, Mt. Wilson and Yerkes. The absolute parallax is $0''.017$, the angular diameter is $0''.046$. Hence the linear diameter of Betelgeuse is $.046/.017 \times 93 \times 10^6$ miles, or 270,000,000 miles. This is more than three hundred times the diameter of the sun. Betelgeuse is so large that if we could put the sun at its center, then the surface of Betelgeuse would be found out in the vicinity of the orbit of Mars. Since the diameter of this star is 300 times that of the sun, its volume is $(300)^3$ or twenty seven million times that of the sun. Betelgeuse is not a double star and so we do not know its exact mass, but none the less we can make a very good guess at the mass. When we do this and work out the density of Betelgeuse we find that it has the minute density, about a millionth part of water or a thousand times less than the atmospheric air we now breathe. This is a star at a temperature about 3,000° C. It is not surprising that we call such a star a "giant."

Let us now have a look at Sirius, the brightest of the fixed stars. Its parallax is also known accurately. It is one of the comparatively near stars. Its light reaches us in 8.8 years while that from Betelgeuse takes 190 years.

Sirius is a double star with an accurately known orbit, the period being about fifty years. On account of the large parallax and accurate orbit we know that Sirius and its companion together have a mass that is 3.4 times that of the sun. The most interesting thing about Sirius, however, is its companion. With a moderate sized telescope we can see the companion when near elongation (as is the case now) shining much more feebly than the brilliant Sirius. Although the primary star gives 10,000 times as much light as the companion still its mass is only 2½ times as much. On account of the intense brilliancy of Sirius it has been excessively difficult to obtain any reliable information about the spectrum of the companion. You can readily see that it is well nigh impossible to photograph the spectrum of the companion and at the same time to keep the light from the primary star from passing through the slit and thereby masking the fainter spectrum. It was necessary to wait until Adams with his genius could attack the problem with the 100-inch at Mt. Wilson.

Before tracing this subject further I wish to stop a moment to touch on a few points of Eddington's remarkably brilliant work in investigating the temperatures and internal structures of the stars. In his theoretical work he took all of the stars, including our sun, for which the masses are accurately known. He plotted the absolute magnitudes against the logarithm of the mass, and he found to his amazement that all of the stars investigated fell very nearly on a curved line.

Now let us return to the companion of Sirius. Its spectrum is about Fo, its temperature is 8,000° C., its mass is ninety-five per cent that of the sun. In brightness however it gives only 1/360th part that of the sun. Since the amount of light sent out by Sirius and the companion depend on the areas of the two bodies, then the area of this companion is 1/360th that of the sun. We are able with a little simple arithmetic to find that in diameter the companion is only 1/19th that of the sun and is therefore a little larger than the earth but yet smaller than Uranus; from which is readily derived, and with little chance of error, that the density of the companion of Sirius is at least 50,000 times that of water. The heaviest solid known to us is platinum and yet here is a star two thousand times heavier, bulk for bulk than our heaviest solid.

The strangest thing of all found by Eddington's researches was that in spite of the enormous density the star is in a gaseous state and obeys the laws of perfect gases.

Those of you who are not physicists and astronomers will say, "Stuff and nonsense, I do not believe a word of it. It is impossible for a gas to have a density two thousand times greater than platinum." If you say this I shall have to reply, "You do not have to believe it unless you want to, but I do not see how we can alter the facts even for your benefit."

On account of the very high density, the relativity shift at the companion of Sirius is very large. As Eddington has stated, Adams killed two birds with one stone. He first of all made a remarkable confirmation of the Einstein theory. Important as this is, it is not as epoch-making as the other result of the observations of Adams, namely, that a body can exist in a gaseous state and be thousands of times heavier than the densest solid found under terrestrial conditions. With the comparatively feeble temperatures available in our terrestrial laboratories we have very little opportunity of seeing what happens to the structure of an atom when conditions of observation are changed. While on the contrary under the colossal temperatures found in the celestial laboratories in the interiors of the stars with temperatures of fifty millions of degrees or more, the external electrons have such violent motions that they are jostled hither and thither and are torn away from the nucleus to which under ordinary conditions they would belong.

Again to borrow from Eddington, the crinolines that surround the atoms in their dance under terrestrial conditions become torn from them and their bodies become almost naked under the superheated atmosphere at the center of a star.

Here then are two of our bright stars, Betelgeuse and Sirius. The former has a density one thousand times rarer than the air we breathe, while the companion of Sirius is two thousand times denser than our heaviest solid; and yet both obey the laws of gases.

How can we bring all of the stars of the sky into one theory of evolution, for this is what we wish to do? Henry Norris Russell was the first to show that if absolute magnitudes are plotted against spectral types, that then all the stars investigated fall into two groups, one lying near the giant-branch of the diagram, and the other stars lying near the dwarf or main-sequence branch.

Among the red stars of M type that so far have had their distances measured there is not a single star, in more than five hundred, that falls intermediate between the giant and dwarf branches. For the other types we cannot say that the stars can always be divided into giants and dwarfs. In fact there are three or four "white dwarfs" (the companion of Sirius being one of them) that lie entirely outside the scheme of things and do not permit themselves to be yet brought into any theory of evolution.

It has been very interesting work, forging the last link that has made possible these interesting scientific developments. Those of you who are engaged in other sciences may think that the work of the astronomer might become very tiresome, working at night under disagreeable conditions (my worst have been thirteen hours in a temperature of

26° F. below zero) and following out the same methods on star after star. Some of my friends facetiously say that if you meet an astronomer late in the afternoon you can always tell what the weather predictions are without looking at the sky, the dour face of the astronomer showing that it is clear and he will have to work. Be that as it may, I can say that after having myself measured nearly 300 parallaxes I still take just as much interest in getting another star completed as I did in the early stages of the work. In a way it is an interesting game of demonstrating that in spite of all the possible sources of error you can get a parallax of high accuracy agreeing in value with the average of the best work of others.

As a by-product of parallax work there is obtained the proper motion of the star in right ascension. We ordinarily measure the plate in one coördinate only. If we turn the plate through 90° we can determine the proper motion in declination. The precision of the photographic measurements is so high that if we measure two pairs of plates separated by an interval of six to eight years the accuracy of the annual proper motion is equal to that of the Boss catalogue resulting from observations from many different observatories spread over an interval of 150 years. As our early plates are now more than a dozen years old we can obtain proper motions with a smaller probable error than those of Boss. The catalogue proper motions, however, are absolute, those from the photographic plates are relative to the particular set of stars chosen for reference. A comparison of the results by the two methods has led to interesting conclusions.

At the request of Kapteyn, the photographic measures have been carried out at the McCormick observatory on more than 300 Boss stars. We found as Kapteyn had suspected, a systematic error underlying the Boss proper motions, a conclusion that has been completely verified by other researches. We found also the velocity and the direction of the sun's motion in space. As a further investigation we have found from our plates that the stars in the plane of the Milky Way are on the average about two and a half times more distant than those at the pole of the Galaxy.

Now we are about to reverse the process. By assuming the motion of the Boss stars corrected for systematic error we can find the motions of the faint stars on our plates reduced to the Boss system. As the Boss stars on our program are fairly well distributed over the sky, the sample sections that we can secure with our photographs should be fairly representative of the whole sky. We get well measurable images on our plates in the exposures that we can give, down to the twelfth visual magnitude. If now we can know the spectra of these faint stars, which we are now securing from the extension of the work at Harvard observatory, we shall then be in a position to discuss the motions of the faint stars at different Galactic latitudes to see how these motions are correlated to spectral type. It is a big project to carry through successfully, but when we do get through with it we believe that the game will be worth the candle.

The probable error by the trigonometric method is the same whether we measure a near star with large parallax or a far-off star with small parallax. Obviously as we get to more and more distant stars the percentage error becomes greater and greater, and consequently the distance of the star becomes less and less accurately known. As a matter of fact we know comparatively little of the actual distance of a star when its measured parallax is less than $0''.03$, or when it is at a distance of more than a hundred light years. It is only by measuring a number of stars of the same type that we can learn more precisely concerning their distances when these distances are great. For instance, it has been known for a long time that the stars of B type are very far off. The small parallaxes that were to be expected did not deter us at the McCormick observatory from putting these stars on our program. We have now finished more than sixty stars of B type with the result that we know the average distance and absolute magnitude of these early type stars with a high degree of precision.

There are a number of different ways of deriving the distances of the stars by indirect methods. For lack of time I can only touch upon these briefly here. We would naturally expect that the brighter stars on the average would be nearer to us than the faint stars, or in other words, that stars are bright because they are close. The brightest star, Sirius, is not the closest by any means, while Canopus and Rigel are very distant stars. Brilliancy is no certain indication of proximity. We might then turn to proper motion, for surely the fastest moving stars are the closest. While in general this is true there are many exceptions to the rule. After all the proper motion is only one component of the total motion. Kapteyn, van Rhijn, Luyten and others have derived formulæ for deriving mean parallaxes from a knowledge of the proper motions, magnitudes and spectral types. These formulæ are good as far as they go, but at best they can give only average results.

By investigating the orbital motions of double stars, Jackson and Furner have derived hypothetical or dynamical parallaxes. Russell has done much along the same line. Again these results can represent average conditions only.

The spectroscopic method of deriving parallaxes as developed by Adams and his co-workers at the Mt. Wilson observatory has given results of the very highest value. Victoria, Norman Lockyer, Arcetri and Harvard have followed the methods of Adams, differing from him mainly in the lines of the spectrum chosen for investigation and in the manner of estimating or measuring the relative intensities of the individual lines. There are more spectroscopic parallaxes now completed than trigonometric. The distances of about three thousand stars are known by the former method, while out of this number only about two thousand have trigonometric measures. The spectroscopic method was first applied by Adams to the late type stars only. Afterwards the investigation was extended to the A type stars and even to the more distant stars of B type.

When the first spectroscopic parallaxes were published there was no adequate explanation available, but later the reason for the peculiar differences in the intensities of the lines of the stellar spectra became understood through the theory of ionization developed by Saha. As already stated, the spectroscopic parallaxes must be standardized and calibrated by the trigonometric parallaxes. At best the spectroscopic results likewise can represent mean or average conditions only. The dark lines of the stellar spectrum are caused by absorption in the atmosphere of the star near its surface. If the individual star being investigated differs from the average conditions found in the type of star for which the empirical curves have been drawn connecting difference of line intensities with absolute magnitude, then the spectroscopic parallax may differ systematically from the trigonometric value. In general there is a surprising agreement between the spectroscopic and trigonometric parallaxes and yet on the other hand there are a number of surprising disagreements. The parallax of the brilliant star Arcturus has been determined from the photographic measures carried out at Allegheny, McCormick and Yerkes observatories. The absolute parallax from trigonometric methods is $+ 0''.091 \pm 0''.005$. The spectroscopic parallaxes of this star differ among themselves greatly while all of them have the peculiarity of having a larger parallax than the trigonometric. The spectroscopic values are: Victoria $+ 0''.100$, Norman Lockyer $+ 0''.145$, Mt. Wilson $+ 0''.158$ and Harvard $+ 0''.209$. The cause of the systematic difference is probably due to the fact that Arcturus is much more massive than the average star of Ko type. Pannekoek and others have suggested that these systematic differences may give a method of ascertaining the mass of the star.

The probable error of determining the spectroscopic parallax is about twenty per cent. The distances for the nearby stars are derived with less accuracy than those more distant. For the more distant stars, those for instance, where the parallax is less than $0''.03$, the spectroscopic method gives a higher accuracy than the trigonometric parallax. If the parallax of a star is greater than $0''.05$, then the trigonometric parallax is the more accurate one. The trigonometric parallax has the added advantage that it is entirely independent of all other conditions or assumptions.

These two methods of determining stellar distances have been developed side by side, the one supplementing the other, and together they have made come to pass the enormous increase in knowledge of distances of the past decade.

At best, however, we can hardly ascertain the distance of a heavenly object by these two methods combined if the object is farther off than a thousand light years. To Mt. Wilson and Harvard observatories we owe the development of a remarkable series of researches whereby we can speak with confidence of objects so far off in the sky that the light that now reaches our eyes started from its source one million years ago. This information comes namely as the result of brilliant work by Shapley. As to the details I cannot enter now. Briefly, by observing the period of variation of the light of stars in distant clusters, and by the application of the "period-luminosity" law, the absolute magnitudes of these cluster variables can be ascertained. Since their apparent magnitudes are known from the photographs, a simple formula gives directly the parallax and the distance.

In this way Shapley has found that the globular cluster in Hercules, Messier 13, consisting as it does of at least 60,000 stars, is at a distance of 35,000 light years. Messier 5 and Messier 3 are at distances of 40,000 and 45,000 light years respectively, while N.G.C. 7006 is at the great distance of 230,000 light years.

Shapley estimates that in the Small Magellanic Cloud there are 500,000 stars brighter than the eighteenth magnitude while it is at a distance of 100,000 light years. The distance to the Large Magellanic Cloud is 112,000 light years or 34,500 parsecs while its diameter is 4,300 parsecs which is greater in size than was commonly assigned to the whole sidereal universe at the beginning of the present century. One of the variable stars in the Large Cloud, S Doradus, at its maximum brightness emits 500,000 times as much light as the sun.

At the Mt. Wilson observatory, Hubble finds that the nebulosity of the Andromeda nebula, at least in the outer portions, resolves itself into separate stars. By the extension of Shapley's methods he has found that this nebula and also Messier 33 in Triangulum are each at the colossal distance from us of 850,000 light years.

There has been much difference of opinion among astronomers as to whether these distances represent the actual truth. One of the weakest links in the whole chain of evidence is that the entire fabric of distance is based on eleven Cepheids only. It is impossible to determine the parallaxes of these fundamental stars by trigonometric methods for the reason that the Cepheids are very distant stars with very small parallaxes. They are therefore beyond the range of the direct method unless we could succeed in measuring great numbers of them. Moreover the peculiarities of their spectra put them almost beyond the spectroscopic test. The only method of securing the parallaxes of the Cepheids with a higher accuracy than is now available seems to lie in the indirect method of observing accurately their radial velocities and proper motions. The Cepheids are nearly all of them faint stars and beyond the reach of meridian circles. The only means available of deriving accurate proper motions is by photography with large telescopes. A start on this has already been made at Mt. Wilson and McCormick observatories and already the first series of plates have been secured on about 150 Cepheid stars. It will now be necessary to wait for another eight or ten years before taking the second series, until the lapse of a sufficient time interval to permit the determination of the proper motions with sufficient precision.

The mind of the astronomer that contemplates with serenity the time of a million years does not fret in being forced to wait a paltry decade to complete his observations.

METEOROLOGY FOR AVIATION

By WILLIAM R. BLAIR

Signal Corps, U. S. A.

(*Read April 20, 1928*)

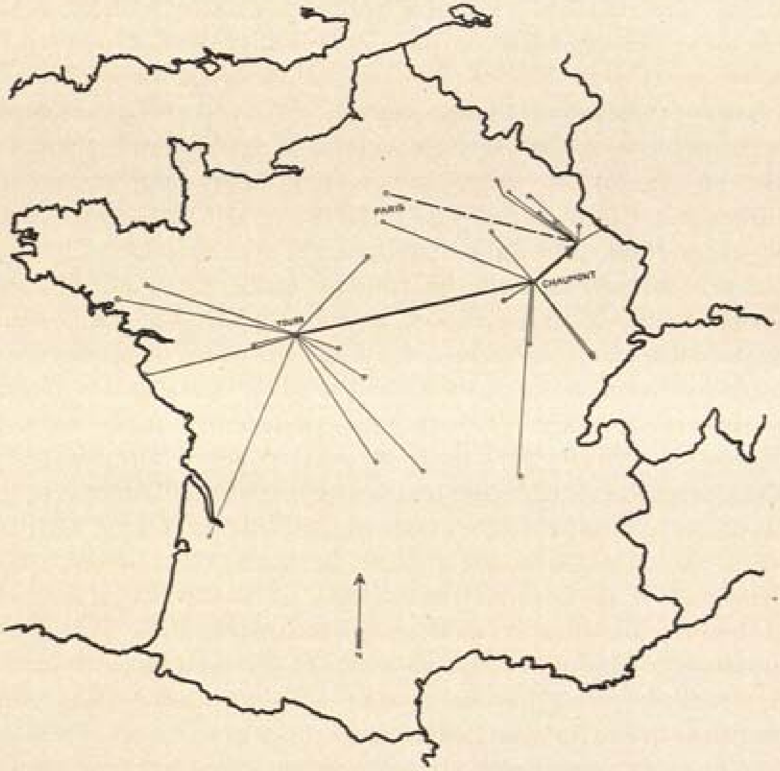
THE interest of your Society in Aviation that we appreciate so highly is by no means a new one. I recall a very pleasant occasion nineteen years ago last month on which I spoke to the Society on the subject of Meteorology for Aviation. At that time the subject of greatest interest to those concerned with aviation was the way in which the atmosphere sustains the plane in flight and the effect of variations in atmospheric conditions on the safety of flight. I recall the paper considered such matters as the variations in support that a plane would experience in flying either with or against a normally gusty current of air, as well as the effect of the high winds, fog, rain and such conditions on the location and arrangement of flying fields and hangars and the handling of planes on the airdrome and in the air under these conditions.

Some years later, at the beginning of the war, I prepared a pamphlet on this subject in which the weather conditions affecting airplanes in flight were discussed in considerable detail, especial attention being given to atmospheric equilibrium. In addition to this pamphlet, a chart was prepared of the United States, at that time considered confidential, in which all weather phenomena that were considered to affect planes in flight adversely were weighted, their occurrence in different sections of the country studied and so charted as to show the number of good flying days by months and by years that might be experienced in any part of the United States. This chart was employed as a guide to the location of flying fields.

These two papers are mentioned to indicate the points of view on the subject in hand twenty years ago and eleven years ago, in comparison with the point of view of today. Up to the beginning of the World War there was little thought of airways as such or the meteorological service that would be needed for them. Airplane flights up to that time were local in character and of comparatively short radius. The mention of meteorology for aëronautics today presents quite a different picture. We now think of a net-work of airways covering the country and of airplane flights that may be of any length from flights between neighboring airports up to transcontinental or transoceanic distances. The weather hazard in flying has to a considerable extent been overcome both by the improvement in the flying equipment and by the increased skill of the pilots. There are still, however, many weather conditions dangerous to flight, the existence of which on any airway should be forecast and warnings of them issued to pilots flying those airways. There are many other meteorological conditions of which the pilot may take advantage provided he is made aware of their existence, present or future, or both. The thought uppermost in the mind of the meteorologist now is how best to serve aircraft flying this extensive and daily increasing network of airways. It is more a question of personnel, organization and method, than one of meteorological phenomena and their aërodynamic bearing. The administrative side of the work is for the moment more important than the technical side. It is not so much what the pilot needs to know as how best to get essential information into his possession at the proper time, that requires immediate consideration.

The problem is not altogether a new one. On a smaller scale, but with added complications, this is the problem that I found in the A. E. F. in the late summer of 1917 when it became my duty to organize a meteorological service for the American Forces in France. I venture to consider briefly this meteorological service of the A. E. F. because it was a service organized and operated under almost ideal conditions. While its purpose was to serve the American Expeditionary Forces in France, it must be kept in mind that the activities of those forces covered a very wide range, including industrial and commercial, as well as military operations, the latter including both local and airway flying.

The personnel for this service was very largely drawn from those men in the draft who had completed, or nearly completed, their college or engineering school courses. The



fundamental education of this personnel was such that it was a matter of weeks, rather than of months, to train them in the making of meteorological observations and many among them were found thoroughly qualified to receive instruction in the more technical work of forecasting the weather. It remained to organize this personnel into an effective unit that would properly serve activities of the American Expeditionary Forces and at the same time be complementary to that part of the general European meteorological service that was available to the A. E. F. The training area occupied by the American Expeditionary forces, extending as it did from Brest to Bordeaux and from thence across France to the Swiss border on the South and to Paris on the North, was well located with reference to that section of the front occupied by the American Forces, from the point of view of rendering to the forces at the front the best possible meteorological service. The diagram shows the general arrangement of the more important American stations throughout this training area and the communication channels through which the data were collected from the stations and delivered to the headquarters of the meteorological service near Colombey les Belles, also to intermediate points. The broken line extending from Le Bourget to Colombey les Belles represents an all-American communication channel over which meteorological data from the neutral and friendly countries of Western Europe were transmitted four times daily. These data were collected at Le Bourget and from there distributed to other parts of the front as well as to that occupied by the American Forces. This distribution so taxed the communication facilities established by the French at Le Bourget that a Signal Corps telegraph terminal with an operator was installed to handle the transmission of the data to Colombey les Belles. It happened that this installation so speeded up our receipt of the data that we were able to communicate it to certain British air units located near Vezelize by telephone approximately an hour earlier than they could have received it through other channels. These data together with regular reports of observations made at the same hours by our own stations constituted the basis of the regular forecast work at Colombey les Belles for the American Expeditionary Forces wherever located. In addition to these regular reports a system of special reports from our own meteorological stations was organized. These special reports were to be made promptly at the time that the meteorological instruments on the station indicated certain disturbed conditions. It will be noted that the stations are located in three different distinct arcs of circles in a general way centering on that part of the front occupied by the American Army. In order to complete this system, arrangement was made for the French meteorological stations at Angerville, Dijon and Lyon to report into this special net. A number of A. E. F. stations in their turn reported into a specially reporting French net. A fundamental principle with the Meteorological Service A. E. F. was that meteorological observing stations should be located in such a manner as to cover the area concerned from a meteorological point of view. Centers of flying or of other activity within an area so covered, were best served by the organization of a special reporting net of these observing stations located to the weatherward of the center. This may result in any one observing station reporting into one, or into several such nets. Squalls and sudden storms of a local nature usually approached our part of the front from the Southwest quarter. It never happened, therefore, that we did not have at least two observations of the same squall before it reached the front. Often we had three such observations. These observations enabled us to determine the direction and speed of movement of the local storm and to predict its arrival at the front as well as its character upon arrival with a time accuracy of fifteen to twenty minutes.

It was of the utmost importance, especially to air units, both planes and observation balloons, that the forecast of storms of this type should be definite and of individual storms rather than of stormy conditions. An observation balloon, for example, could not be brought out of the air for an afternoon because local storms were probable. The observations being made by the balloon were of the utmost importance in connection with effective artillery fire and other operations, and it was essential that the balloon be kept in the air up to the last minute that it was safe for it to stay there, also that it go up again just as soon as the disturbance had passed. The same statement applies to observation planes. It would apply to planes carrying mail or flying any regular schedules today.

In addition to the activities on the front and with especial reference to aviation, it should be explained that all airplanes being used in the A. E. F. were assembled and put in commission at Orly Field, just south of Paris. From this point planes were ferried to the flying fields at the front and to the aviation training centers at Issoudun, Clermont Ferrand, Tours and other points. Well travelled airways were thus established between Orly Field and the points named. Meteorological service for these airways and for the flying centers involved, was provided by a responsible local meteorological organization. In order to effect this service a forecast for each flying center was made and telegraphed from headquarters station of the meteorological service at Colombey les Belles. This forecast was subject to modification before issue by the local meteorological organization. Observational data from all stations in the net making special reports and located to the weatherward of important flying centers, were made immediately available to those centers. It will be apparent that this organization placed a responsible meteorologist with each important command and at the same time provided for the fullest use of the available observational data. You may be interested in this connection in the detail with which it was possible to give meteorological advice, both as to present and future weather conditions, under this arrangement. On more than one occasion groups of planes were brought from Orly Field to the First Air Depot at Colombey les Belles in clearings of considerably less extent than the distance between these two points, but since the clearing was moving in the same direction as the planes, the planes were flying in clear weather throughout their journey. No squall or local storm arrived at that part of the front occupied by the American Forces, the arrival of which had not been individually announced to all units affected. Such details as this were of course taken care of by special forecasts.

An idea of the detailed information contained in the regular forecasts distributed each morning and evening may be obtained from consideration of the following forecast taken at random from the regular issue:

"Forecast A1. ARGONNE until 6 p.m. 1 Nov.

A. Weather fair for all arms except for aviation and observation in the forenoon.

B. Surface winds, 1–5 m/s East.
At 2,000 meters, 3–8 m/s East to Southeast.
At 5,000 meters, 4–8 m/s Southwest to West.

C. Overcast, with occasional large clear spaces in forenoon.
Clear to one-half cloudy in afternoon.
Fog until afternoon, Haze all day.

D. Cloud height, 3,000 m. with occasional lower masses 1,200 m.

E. Visibility, very poor, 1–2 km. until noon, improving to 3 to 5 km. in afternoon.

F. No rain.

G. Maximum temperature, 10° C.

K. Odds in favor of forecast, 6–1.

Issued at 6:10 a.m."

It will be noted that this forecast is for that part of the front between the Argonne Forest and the Meuse River. The detail contained in it was such that it was necessary to issue simultaneously with it a similar but somewhat different forecast for that part of the front between St. Mihiel and the Moselle River, although the entire length of the front involved was less than 100 Km. Attention is invited to the language of the forecast. A direct statement is made of the conditions expected under each heading of the forecast, while in paragraph *K* the forecaster gives his own opinion of the accuracy of the preceding statements. The Commander in the field, or the pilot in his plane, must take chances with other things, as well as with the weather. It may happen in the case of the forecast that all observational data is not available to the forecaster at the time of the regular forecast. Such a condition would result from the partial breakdown of the communication facilities, or delay might be occasioned by the necessity for transmitting more urgent communications.

The meteorological situations as indicated by charted observational data are not as definite in their indications of future weather conditions at some times as at others. The forecaster is, therefore, in the best position to place an estimate on the accuracy of his forecast and should transmit this estimate of accuracy with the forecast. It was a rule in the A. E. F. that unless odds in favor of the forecast were at least 3 to 1, the forecaster would issue a better forecast just as soon as conditions warranted.

An accuracy for the month of October 1918, of between 5 and 6 to 1 in the forecasts was found to check almost exactly with the forecaster's estimates of his forecasts for the month.

Meteorological information and forecasts should be issued with reference to the activity served and since they are for use by non-meteorologists, they should contain the essential element of decision, so far as meteorological conditions are concerned, that the meteorologist is, or should be, best qualified to supply.

Returning now to the network of airways that is rapidly coming into being over our country, we find them radiating from the great centers of population that have grown up around business, industrial, commercial, agricultural, or other activities. The center of operations of the American Forces on the western front in France is comparable to any one of these centers of civil activity, both as to aviation and other pursuits, and the meteorological system that served so well overseas is recommended as the ideal one for the effective and economical service of these centers. Three essential elements enter into this system. The first of these is personnel. While boys with high school education may be trained to make fairly reliable meteorological observations, it is essential that the meteorologist attempting to make a useful local forecast have thorough preparation in the physics involved in the weather processes. His preparatory work in physics should include thoroughly practical courses covering thermodynamics, kinetic theory of gases, neucleation, electron theory and certain phases of spectroscopy. These studies should be carried well beyond the work usually prescribed for undergraduate students. The progress of personnel coming into the meteorological service with less preparation than this is seriously limited. Their experience as observers would, however, be an extremely valuable asset, if the more promising of them could be furloughed or in some way provided with the opportunity of completing the preparation in physics above outlined.

The second essential element entering into this meteorological system is a suitable number and distribution of observation stations so that every meteorologically distinct area of the country will have its station. The frequency and hours of observations at these stations should be such as to take care of the larger weather changes with provision for special observational reports as local conditions demand. Ideal hours from the point of view of our own country based on the 75th meridian, are 4, 10, 16 and 22, on the 24-hour dial.

The third essential element is the organization of these stations into suitable nets with communication facilities for the purpose of collecting the weather data observed. These nets should include:

(*a*) A country-wide net for the collection of regular observations. These regular observations once collected could, in whole or in part, be suitably distributed to centers of aviation and other activities and, properly interpreted there, would provide meteorologists in charge with essential information concerning the general weather conditions prevailing to the weatherward of the center served by them.

(*b*) Local nets about the various centers served for the collection of special reports required by the location and by the activities of the centers. These local nets will often overlap and it will frequently happen that an observation station will report into more than one local net.

As between a general and a local forecast, that is a forecast applicable to a large area and a forecast applicable to a small area, the latter is much the more difficult to make. It is proportionally the more useful, since it supplies information in greater detail and with greater definiteness. It follows that the key men of this ideal meteorological system will be located at the centers of flying and other activities, for which the meteorological service is provided. It will be the duty of each of these key men to become familiar with the activities of his center requiring meteorological service and to provide service with reference to these activities.

THE DEVELOPMENT OF THE HEAVIER-THAN-AIR MACHINE

By C. H. BIDDLECOMBE

Formerly Major in the Royal Air Force

(*Read April 20, 1928*)

THAT phase of Aëronautics which is possibly the most interesting and certainly the most valuable to humanity is the creation of the commercial fleets of the air. From the day that the first voyager set out for the unknown—possibly by accident—astride a floating tree trunk, transportation has been the indispensable forerunner of civilization. On a continent where the civilizing influences of even the crudest forms of transport—canoes, horses and dogs—are still exercised in reclaiming the hinterlands of Canada and South America, the airplane as a vehicle of commerce has, I believe, an arresting interest for the philosophic mind. The history of this newest agent of the God of Speed—the wing-footed Mercury—is extremely brief insofar as practical use is concerned, but, in the legendary song and story of the human race, winged flight has provided a theme for the poets of twenty-five centuries.

The earliest surviving myth is, of course, the story of Daedalus and Icarus, who may be said to have been the earliest proponents of the ornithopter—a wing-flapping type of airplane. The ornithopter was very naturally the first type of aërial vehicle to engage the thought of mankind, and for many centuries the flapping of wings seemed to be the logical means of obtaining flight. The earliest mention of a wing-flapping model which carried a suggestion of authenticity is the story related by Aulus Gellius, the Roman author of "Attic Nights," who refers to the flying dove of Archytas in terms that indicate successful short flights by a wooden model of a bird. A previous mention of attempted wing-flapping flight is the British legend of King Bladud, supposedly the tenth king of Great Britain, who died in 852 B.C. The story runs that Bladud made feather wings for himself and practiced gliding from high places; his enthusiasm presumably outstripped his skill one day, as he fell in flight onto the temple of his God Appolyn in the City of Trinoraitein—more widely known as London. Incidentally, King Bladud was the father of the equally mythical King Lear, and I sometimes wonder if Shakespeare had this in mind when he gave us King Lear as a madman.

The next outstanding figure in the legend of flight is Oliver of Malmesbury who flourished about 1020 A.D.; he is reported to have fitted wings to his hands and feet and jumped from a high tower, maintaining flight for about one eighth of a mile, when a gust of wind caused him to crash. Milton remarks of Oliver of Malmesbury that, "He got so conceited of his art that he attributed the cause of his fall solely to want of a tail, as birds have, and which he forgot to make to his hinder parts." Later on John Damian, a favorite courtier of James IV of Scotland, attempted a similar feat from the tower of Stirling Castle, and fell into the kitchen midden; a contemporary writer and critic remarked of this flight that hens' feathers would naturally return to the manure heap, so why did not Damian use eagles' wings?"

The legends of Greece and Rome, of India and China, the Sagas of Scandinavia—all have their quota of tales of flight by man, and the urge to conquer the Empire of the Air has been confined to no one period of human history, but it has been left to this present era of science and invention to realize the dreams of some thousands of years.

Passing over the notes on the mechanics of flight left us by Roger Bacon in 1250, the extremely interesting experiments of Leonardo da Vinci in 1450 and the balloon flights made in the eighteenth century, we come to the birth of the heavier-than-air machine in the first years of the nineteenth century.

Sir George Cayley, born in 1774, is perhaps the real pioneer of this type of flying vehicle; his writings are still regarded as some of the finest essays on flying, and his practical work with man-carrying gliders places him in the front rank of contributors to a great cause. Sir George notes that his coachman gave notice to quit in 1808, as he was hired to drive, and not to fly. It appears that the coachman resigned immediately after crashing-up at the end of a three hundred yard glide across a valley—we can scarcely blame him.

Cayley's progress was, of course, hampered by the absence of a power unit to drive his gliders, and the same handicap applied to the efforts of the other gliding pioneers—Wenham, Stringfellow, Pilcher, the Lilienthals and a host of others who gave time, money and their lives in the pursuit of knowledge.

It was not until 1890 that Clement Ader succeeded in producing a machine capable of power driven flight; on October 9th of this year Ader sent the first power driven airplane into the air at Chateau d' Armain, in France. This machine had a span of 54 feet, weighed 1,100 pounds, was propelled by two four-bladed tractor airscrews driven by a 30-horse-power steam engine, and made a flight of 164 feet.

Six years later an American pioneer, Professor Samuel Langley, duplicated this success by a model which flew from the Potomac River on May 6, 1896; the flight is described in the issue of *Nature* dated May 28, 1896, by Doctor Alexander Graham Bell, with whose name you are doubtless familiar, and who was an eye-witness of the event.

In the following year Clement Ader's model *Avion* made a circular flight of nearly 1,000 feet at Satory in France, on October 14; this flight ended in the same manner as the two previous flights by power-driven models—the machine crashed out of control.

The question of control was for the following six years a baffling problem to the numerous brilliant minds devoted to the science of aëronautics, and it was not until December 17, 1903, that Orville Wright flew 120 feet on the sands at Kitty Hawk, North Carolina, in an airplane that was really capable of being in some measure controlled. This flight was the start of heavier-than-air aviation, as we know it today, and it is unnecessary for me to refer to the immortality conferred upon the Wright brothers by virtue of their having opened the road to the solution of what was at that date the abstruse problem of controllability. Subsequent flights by these famous pioneers demonstrated that mankind's dream of the centuries had at last come true—at last had come the triumphant vindication of the gallant band of scientists, engineers and amateur experimenters whose apparently crazy efforts had been a subject for amusement for more than a century. It is, I think, a peculiarly happy circumstance that this first success should have come to Americans—we have in the United States a field for air transportation unparalleled in any other quarter of the globe and we have a country where the impossible is being accomplished every day.

From 1903 to 1908 progress was perceptibly slow—I may remark that the Wright success seemed incredible to Europeans three thousand miles away and a certain element of doubt pervaded the minds of the multitude. This was to a degree dispelled by the flights of Santos—Dumont and Ellenhammer in France in 1906, on power-driven box-kites, but it again remained for an American, largely, to settle beyond dispute the feasibility of heavier-than-air flying. Orville Wright's demonstrations in France in 1908, together with flights by Bleriot and Farman, aroused a real and lasting interest in the art, and from this time onward progress became rapid and consistent. It may be noted that this year—1908—saw the first death arising from the operation of a power-driven airplane, and to the United States again fell the honor of giving the first life in the cause of modern airplane development; Lieutenant Selfridge being killed on September 12. In this year another famous American aviator, the late S. F. Cody, made many successful flights in England, and one of the great English pioneers, A. V. Roe, also flew the ancestor of the world famous Avro machines.

1909 witnessed another event of far-reaching importance, the echoes of which are still reverberating through the War Ministries of Europe; this was the crossing of the English Channel by air, and on that sunlit Sunday morning of July 25, the Island history of England ended. The chapter of a thousand years in the book of England's story closed when Bleriot landed on the cliffs of Dover in a biplane fitted with a 25-horse-power Anzani engine, having made the flight at an average speed of 35 miles per hour.

It is interesting to note that in this year the certified pilots of heavier-than-air machines numbered 541, of which 353 were French and 26 American, the two countries being at the top and bottom of the list respectively. The United States is, of course, at the top of the list today with over 4,000 recorded pilots.

In 1909 another name famous in American aëronautics came to the fore in Europe when Glenn Curtiss won the first Gordon Bennett Cup at the Aviation Meet at Rheims in August.

The next year—1910—was notable as producing the first serious development of aviation in England; the Brooklands Airdrome by the end of the year had 18 hangars filled with airplanes bearing the famous names of Handley Page, Sopwith, Avro, Martinsyde, Bristol and Short Brothers. The first British heavier-than-air fatality also occurred in this year, the Hon. C. S. Rolls, of Rolls-Royce car fame, being killed at Bournemouth through the collapse of the tail elevator which he had added to his Wright bi-plane. Claude Graham-White also won the Gordon-Bennett Cup in this year, at New York, with a Bleriot machine.

The truly international aspect of aviation is well illustrated by the Gordon-Bennett race of 1911, which was won for America, in England, by Charles Weymann, flying a French Nieuport machine; Weyman being a West Indian of partly native and partly French extraction, bearing a German name, and having been brought up in Paris.

In 1911 was built the first all-steel tubing machine, which had wooden spars and was the forerunner of the most modern airplanes of today; it was unfortunately about fourteen years ahead of its time and was regarded as too revolutionary in principle to be widely adopted.

From 1911 onward the development of the heavier-than-air machine was fostered by the interest displayed by the various governments of the world. Prizes of varying amounts were offered for successful machines and flying branches of the naval and military forces were brought into being. Incidentally, an American, S. F. Cody, won the first prize offered by the British War Office for a satisfactory airplane, in 1912, and in September of that year the first Air Mail route was operated, from Hendon Airdrome to Windsor Castle, running for a month as an experiment.

Then came the tremendous stimulus of war to forward progress at breakneck speed; overnight almost, the airplane ceased to be a new agent of transportation—a new toy for the master players of the war game to play with in their leisure hours. It became instead an instrument of destruction or defense against destruction. Successful flight was no longer merely the ambition of a few enthusiastic scientists and engineers—the interesting experiment of a few far-sighted Chiefs of Staff; it became a vital factor in the continued existence of great nations and far-flung empires.

Expense meant nothing—lives were of small account, and great fleets of aircraft were borne upwards on the wings of a mighty storm, until it seemed that the skies of Europe were shadowed by battling squadrons. The storm died and the airplane was left to fly by itself—given again to men anxious to fly on into the unknown territory of Commercial Aviation. But in the years between, much had been learned regarding the mechanics of flight—four years of the intensively imperative demands of war had produced results that would have required probably fifteen years of peace. Designing and constructing staffs had been collected and trained in every important country—a vast fund of experience had been gathered in respect to engines and airplanes, metals and woods, stresses and strains—experience which formed the basis of the airplane industry of today.

The first efforts to commercialize the post-war airplane naturally tended toward the modification of purely war machines—the conversion of military craft to meet the needs of peace. We thus had an extremely uneconomic condition which resulted in the so-called commercial operation of airplanes which carried as low as one pound of paying load for each horse power developed—a condition obviously unsound when compared to any other form of transport. The financial results of this type of operation were so impossible that in Europe the governments of those countries that were endeavoring to develop air transport were forced to subsidize the operators or see them disappear under economic pressure. Both results obtained at various times—some operators were subsidized, others became bankrupt. In the United States, the policy of providing subsidies has always been regarded unfavorably in principle, and as a result there was no air transport in this country until the passage of the Kelly Bill in 1925. The operation of the Trans-Continental Air Mail Route by the Post Office Department cannot of course be regarded as commercial air transport; at the same time, however, this operation, together with the work of a large number of fixed-base airplane operators, served to keep alive the public interest in aviation and to maintain a continuity of aëronautical activity. In 1926—before the Contract Routes were really in full operation—the Post Office Department and fixed-base operators covered a flying mileage of about 19,000,000 miles. The greater part of this flying was carried out with airplanes either of war-time vintage, or, if new, designed around and fitted with engines dating back to 1918 and earlier. Today, I never cease to wonder at the courage and pertinacious determination of the men who kept flying alive during the years 1919–1925, handicapped as they were in almost every regard—with airplane and engines more suitable for the economic scrap-heap than for operation, with no subsidy and with comparatively scant public support.

However, the vision of these men is nearing fulfillment more and more as each year passes; 1925 brought the Air Mail Act, giving a definite solid incentive to the production and operation of a more economically correct airplane than the past had ever justified. Owing largely to the interest of the United States Navy, a really revolutionary change in the whole field of airplane design and use was made possible by the production of the air-cooled engine, and really sound and profitable air transport came within general reach for the first time in history.

It is of interest to note that the great American name of Wright was again the leader in this development, the Wright "Whirlwind" engine being the first air-cooled engine which proved sufficiently light and reliable to permit of the economically sound operation of aircraft. From this time onwards the pay load per horse power of the heavier-than-air machine became a factor around which serious budgets could be made, instead of somewhat of a joke to those versed in the business aspect of transportation.

From a figure of one pound per horse power, we were at once enabled to consider three, to even six pounds, and this latter figure does not represent the optimum—it is perfectly feasible today to build a comparatively slow freight-carrying airplane with a pay load of eight pounds per horse power, but the volume of business regularly obtainable for this type is not yet sufficient to create the demand for the vehicle. In this connection, there is still much discussion as to the economic value of this relatively high pay load, obtained as it is by the sacrifice of high speeds, in the light of our present knowledge. In general, the basic rule of all transportation activity is to carry the maximum load for the power used, but there are exceptions to this rule even in the older forms of transport-extra-fare trains, high speed motor busses and crack Atlantic passenger liners are examples. We are thus in the air transport industry today in the throes of argument as to whether really high speed is required by the business community at a correspondingly high price, or whether we must carry larger loads at the lower speeds. Until very recently, there was little serious divergence of opinion as to the greater value of the increased pay load, but the developments of the last few months seem to indicate that higher speeds are more profitable ultimately, and that the really large load carrying capacity is the function of the lighter-than-air dirigible.

If this is eventually proved true, it is fairly safe to prophesy that the future of the airplane is in high-speed transportation at upwards of two hundred miles per hour over comparatively short stages, and that long journeys, such as the Atlantic and Pacific crossings, New York to South America, England to Australia, etc., will be done by the dirigible.

This conclusion is, I believe, the most logical that can be drawn from our past experience, and we can with confidence await the day when the earth will be encircled by giant dirigibles traveling at a little over one hundred miles per hour, stopping only at twenty-five hundred mile intervals, and being supplied with passengers from short airplane feeder lines with a two hundred mile per hour schedule over distances of some fifteen hundred miles.

TISSUE RESPIRATION AS THE FUNCTION OF THE INTERNAL SECRETIONS WHICH SCIENCE HAS SANCTIONED

By CHARLES E. de M. SAJOUS

(*Read April 21, 1928*)

An all-important feature of general biology—one very costly in human lives in medicine—is well expressed by W. H. Howell, professor of physiology in John Hopkins [1] when he states that "the respiratory history of oxygen ceases after this element has reached the tissues." This means that although oxygen was discovered by Lavoisier in 1771–80 and has since been traced to all tissues, its rôle therein has remained unknown. Personal labors, started in 1888 and continued ever since, have imposed the conclusion that both pulmonary and tissue respiration are carried on by the ductless glands, and that they are the only fundamental functions of these organs that science has sanctioned since I first formulated them in 1903.[2]

The internal secretions are now being developed in three main directions: the physiologic, initiated by Claude Bernard in 1848, who introduced the term "internal secretions"; the clinical, inaugurated by Addison in 1849; and the therapeutic, which, though traceable back over three thousand years to India, was placed on its modern footing by Brown-Séquard in 1889.

In general practice, the trend has been to follow the lead of Brown-Séquard. The remarkable effects of testicular fluids upon his own organism, which effects I had occasion

[1] Howell, W. H.: "Text-Book of Physiology," 10th ed., 1927, p. 915.

[2] Sajous, C. E. de M.: "Internal Secretions and the Principles of Medicine," 1903, 1st ed., pp. 120–128 and 801–850. Same in 2d ed., 1907, and 3d ed., 1909. In 4th ed., 1911; 5th ed., 1912; 6th ed., 1914; 7th ed., 1916; 8th ed., 1919; and 9th ed., 1921, pp. 56–70 and 801–850. In 10th ed., 1922, pp. 2–8, 59–71 and 801–850. Also, Gaz. d. hôp. (Par.), 1907, 80, p. 1407; Practitioner (Lond.), 1915, 94, p. 179; Endocrinology, 1917, 1, p. 4; ibid., 1922, 6, p. 197.

to witness, constituted a legitimate foundation for the use of extracts of the various ductless glands in practice and they were tried in all classes of disease with excellent effects in many, and either moderate or no beneficial results in others. Moreover, I [3] have called attention to various conditions in which their use in practice might prove harmful. The rational or scientific use of these powerful agents could only be placed on a sound foundation by adequate knowledge of their fundamental mode of action. The same need is quite as applicable, however, to every phase of clinical medicine, pathogenesis, pathology, symptomatology, etc.

Unfortunately, and despite a large aggregate of invaluable contributions to our knowledge on the subject, the tendency is persistently to assert that the functions of these organs are unknown.

The purpose of this paper is to show that this discouraging attitude is groundless and that it is due mainly to the fact that no single branch of Medicine, pure or applied, can solve so far-reaching a problem. It required, to do so, the broader knowledge that co-ordination, analysis and synthesis of all branches at all relevant to the subject could furnish—a mode of research now termed "synthesis of sciences."

It was adopted over thirty years ago as the basis of my own researches on the ductless glands—studded, here and there, I may add, with experimental investigations needed to elucidate any mooted question which the research brought to light. A few details are necessary here to illustrate its bearing upon Medicine as a whole.

In 1902, after editing twenty-five volumes of my Annual of the Universal Medical Sciences, I became convinced that although some departments of Medicine were advancing at giant's strides, gaps existed on all sides which prevented the development of clinical medicine on rational lines. Symptoms, etiology, pathology and treatment were described with exceeding care, but the relations of cause to effect, that is to say the manner in which the lesions were produced or the symptoms were evoked by them, or the processes through

[3] Sajous, C. E. de M.: *Virginia Med. Semi-Monthly*, 1910, 14, p. 561.

which the remedial measures produced their effects, good or bad, were left to the uncertainties of pure conjecture. Evidently some great fundamental factor was missing which hampered progress on all sides.

It was from this direction, after fruitless inquiries in other fields, that light seemed to appear, owing to the concomitance of several related facts. Metabolism, according to the leading British physiologist of the late nineties, Sir Michael Foster, was "made up of guesses and gaps." Others equally eminent on the continent, including particularly Professor Bohr, of Copenhagen, had pointed out several discrepancies in the diffusion doctrine of respiration, the latter physiologist suggesting—a view sustained by Haldane, Vaughan, Lorrain Smith, the Oxford School and others—that the pulmonary epithelium might secrete oxygen into the blood while secreting CO_2 into the alveolar air. This hypothesis, dating back to 1891 however, is still *sub judice*. These shortcomings were but normal consequences of the third missing factor: knowledge of the role of oxygen in the tissue cells—the very foundation of all biological phenomena.

My own investigations which, besides human tissues, included organisms throughout the entire phylogenetic scale and plant life, then led me to the definite conclusion that the secretion of the medullary portion of the adrenals in man possessed all the properties required to fill this function. Briefly, it showed a marked affinity for oxygen; its course after leaving the adrenals and passing to the vena cava led it directly to the pulmonary alveoli; it acted as catalyzer in the Traube sense, being able to take up oxygen and to deal it out, activated, to the tissue cells through the intermediary of the red corpuscles.

This interpretation has been sustained with suggestive unanimity by a large number of investigators in the pure and applied branches of Medicine. They[4] found inde-

[4] Series of articles: Byelaventz: Rousskii Vratch, 1903, 2, p. 247; Bernstein and Falta: Verhandl. d. deutsch. Kong. f. innere Med. (Wiesb.), 1912, 39, p. 536; Menten: Amer. Jour. Physiol., 1917, 44, p. 176; Tompkins, Sturgis and Wearn: Arch. Int. Med., 1919, 24, p. 269; Sandiford: Amer. Jour. Physiol., 1920, 51, p. 407; Januschke

pendently that the medullary principle as represented by adrenalin increased not only the intake of oxygen, but also the output of carbon dioxide and the volume of air breathed. They established, moreover, that this adrenal medullary principle, epinephrin, generally known as adrenalin, was secreted by the adrenals in quantities sufficient to produce these respiratory phenomena and also to raise the temperature and the respiratory quotient. My conclusion that it was the constituent of hemoglobin in the red corpuscles which on exposure to the air in the pulmonary cells became oxyhemoglobin, was likewise confirmed spectroscopically. In fact, it was found that adrenalin could itself act as hemoglobin. What might be termed a crucial test at the University of Chicago, also favored strikingly my earlier deductions. Venous blood from the veins which carry the adrenal products to the general blood stream, when diluted with salt solution, became red and showed spectroscopically an increased formation of hemoglobin while the adrenal medullary principle, adrenalin, when added to venous blood was found to do likewise. What this means is that the adrenal medullary principle was able to convert venous blood into arterial blood. Referring to the latter experiments, which may be said to cap the whole series of the few recited, Professor E. Sharpey-Schäfer,[5] of Edinburgh University recalled that it was "a view which has been strenuously advocated by Sajous."

Further sustaining my interpretation is the fact that physiologists themselves have introduced terms which clearly betoken its solidity. Thus, R. G. Hoskins[6] now refers to adrenalin as a "respiratory catalyst," while Boothby and Sandiford[7] employ the term "calorigenic action" of adrenalin,

and Pollak: Arch. f. exper. Path. u. Pharmakol., 1911, 66, 205; Nice, Rock and Courtright: Amer. Jour. Physiol., 1914, 34, pp. 326, 331; Kariya and Tanaka: Jour. Tokyo Med. Assoc., 1913, 26, no. 20; abst., Sei-i-Kwai Med. Jour., 1913, 32, p. 10; Menten and Crile: Amer. Jour. Physiol., 1915, 38, p. 225; Menten: Ibid., 1917, 44, p. 176; Jackson, D. E.: Jour. Pharmacol. and Exper. Therap., 1913, 4, p. 291; Dixon, W. E.: Brit. Med. Jour., 1902, 2, p. 242; Januschke and Pollak: Arch. f. exper. Path. u. Pharmacol., 1911, 66, p. 205.

[5] Sharpey-Schäfer, E.: "The Endocrine Organs," 2d ed., 1924, p. 151.

[6] Hoskins, R. G.: *Science Monthly*, 1924, 18, p. 257.

[7] Boothby and Sandiford: *Amer. Jour. Physiol.*, 1923, 66, p. 93.

suggested to them by our fellow-member Professor Graham Lusk.

Doubtless the greatest problem in the realm of medical thought seems to me to be brought within the possibility of a solution by the deductions I have reached, that of tissue respiration, which ever since the discovery of oxygen has remained unsolved. Indeed, in accord with Howell's previously quoted statement to the same effect the Journal of the American Medical Association, as recently as 1919,[8] stated editorially that "an answer to question as to how the all-important oxidations in the body are brought about is almost as obscure today as it was a hundred years ago."

To this day, the entire field of biology has been burdened with makeshifts to compensate for this lack of knowledge concerning the very foundation of the process of tissue life.

In 1903 I wrote what proved to be the first book on the Internal Secretions, a work of over eighteen hundred pages. It contained, besides an analysis of the physiological rôle of the various ductless glands, a study of seventy drugs and other remedial measures and forty-three diseases *in extenso* and sixty-one *in parvo*. All were found to accommodate the newer principles of tissue respiration with the adrenals as fundamental organs. I deemed it warranted, therefore, to state in the Preface,[9] that, quoting my own words, "*the adrenals could be considered as the key to tissue respiration.*"

While, as we have seen, the rôle of adrenalin in tissue respiration has been sustained by a large number of observers, and successfully antagonized by none, their researches have not explained the manner in which this adrenal active principle provokes and sustains thermogenesis or heat production in its relations to the metabolic process.

At our Bicentenary Meeting last year[10] I summarized personal labors in this connection in which biochemistry, histology, physiology, pathology and clinical medicine played

[8] Editorial: *Jour. of the Am. Medical Assoc.*, 1919, *72*, 1697.

[9] Sajous, C. E. de M.: "Internal Secretions and the Principles of Medicine," 1903, Preface, p. vii.

[10] Sajous, C. E. de M.: *Amer. Philosoph. Society Bicen. Celebration*, 1927, p. 47.

the leading parts. I attributed therein to the phospholipoid *lecithin*, the active rôle in heat production, the 3.96 per cent. of phosphorus this substance contains being oxidized by what I termed *adrenoxidase* because of its identity as a catalytic oxidizing enzyme of adrenal origin, and represented in the foregoing remarks by adrenalin. This lecithin, which is known to be present in all living tissues, I regarded as the basis of tissue life in so far as heat production is concerned. To another substance, the monatomic alcohol *cholesterol* I attributed the property of acting as inhibitor of the thermogenic activity of lecithin, in order to prevent excessive heat production.

You will recall that on the following day, at the same meeting, our fellow member Dr. D. T. MacDougal, Director of the Carnegie Institution laboratory for plant physiology, reported that he had found lecithin and cholesterol to be dominant agents in the life process of plants, thus concording with my own findings in animal tissues. Concerning the third agent adrenoxidase, which I found to be the oxidizing enzyme in the process, Dr. MacDougal [11] remarks in a recent article: "The lipoids have been characterized as autooxidizable, but it is highly probable that such action is due to the presence of oxidases as proposed by Dr. Sajous in connection with his studies of the suprarenals and of the blood."

In accord with Dr. MacDougal's opinion, I have found [12] an oxidase in plants which presents all of the properties of the adrenoxidase derived from the adrenals, which adrenalin represents in our laboratories. Eleven hundred personal tests in over one hundred different foods, including vegetables, fruits and fungi, showed that while their content varied greatly colorimetrically, as well as biologically, all contained the homologue of the animal adrenoxidase. This homologue I found to be tyrosinase, also a catalyst, which enables the oxygen absorbed by the plant to be ozonized or activated,

[11] MacDougal, D. T.: "Personal Communication." Article to be published.

[12] Sajous, C. E. de M.: *New York Med. Jour.*, 1923, 117, p. 325; and 1924, 119, p. 1.

as shown by the Swiss biochemist Schoenbein in 1857, and which in 1897 I found to apply to the animal cell as well.

This not only adds a new link to the many which now attest to the homogeneity of the thermogenic process in the plant and animal cell, but it renders practically invulnerable the views concerning the nature of tissue respiration I have been urging since January, 1903.

As to the practical bearing of the foregoing remarks, I submitted last year a few of the many directions in which our knowledge of practical medicine would be enhanced. *Fever*, a process which pathologists had not yet explained, was shown to be but an exacerbation of the thermogenic process sustained by the adrenals, the heat energy liberated having for its purpose to increase the activity of the bactericidal and antitoxic enzymes.[13] This explained the beneficial influence of fever, but also the harm done through hemolysis and autolysis, likewise obscure functions at the present time. Examples were submitted of the life-saving value of this knowledge in diphtheria and other diseases of children, in the senile form of pneumonia and other diseases of the lungs, in disorders of the liver and nervous system, and in mental diseases. A reduction of the incidence of insanity of sixty per cent. was estimated as possible judging from results in practice on the new lines over present possibilities which ignore these organs in the causation, pathology and treatment of these diseases.

To-day I will briefly refer to the endocrine factors in the series of diseases which head our mortality lists—diseases of the heart and blood vessels, especially those due to valvular myocardial lesions, and to the disorders in which arteriosclerosis and high blood pressure give rise to cerebral hemorrhage, or apoplexy, and angina pectoris. It may seem anomalous to add to the series the various forms of paralysis due to encephalitis and spinal disorders. This is because all owe their pathological states in many cases to one underlying factor in all, muscular tissue, of major importance from the

[13] Sajous, C. E. de M.: *Amer. Philosoph. Society Bicen. Celebration*, 1927, p. 47.

endocrinological standpoint. Thus, the heart is a muscular organ; the blood vessels owe their contractility to an external muscular coat, while the paralyses, the choreas, tremors and convulsive disorders. all involve pathologically muscular elements.

Muscles also regulate, in great part, the general function of nutrition. The mastication and swallowing of foods is carried out by the maxillary, lingual and esophageal muscles. The stomach owes its peristaltic propulsive power to its muscular coat. The whole intestinal canal is similarly supplied. Our great lymphatic system which covers our body and follows all our blood vessels owes the progression of its lymph to its muscular elements. Respiration, which utilizes the bronchial and thoracic muscles, locomotion and the innumerable uses of the arms and hands, all depend on muscular contractions. The very expression of our face, the movements of our eyes, the adjustment of the ossicles of our ears and many other functions I could name, owe them to their muscular supply. Briefly, our muscles, big and small, which constitute two fifths of our body, may be said to dominate practically all functions.

Now, physiology, despite much arduous effort, has been unable so far to furnish an explanation of the manner in which muscular tissue carries on its functions, the nature of which we physicians should know in order to work out satisfactorily both pathology and treatment. Professor Meyerhof, of Kiel,[14] a recent Nobel Prize winner, answered this question in 1924 when he wrote that the work on muscular dynamics so far "had only resulted in experimental stagnation, going round in a circle, and in theories of muscular function condemned to fruitless speculation." At present the labors which hold sway are due to the brilliant investigations of Fletcher and Hopkins,[15] A V. Hill,[16] Meyerhof,[17] and Fiske

[14] Meyerhof: "Chemical Dynamics of Life Phenomena," 1924, p. 62.

[15] Fletcher and Hopkins: *Jour. of Physiol.*, 1907, 35, p. 247; 1911, 43, p. 281; 1913, 47, p. 361; and *Proc. Roy. Soc.*, 1917, B. 89, 444.

[16] Hill, A. V.: *Jour. of Physiol.*, 1911, 43, p. 1; 1912, 44, p. 466; 1913, 46, p. 28; 1914, 48, p. x; *Ergeb. Physiol.*, 1916, 15, p. 340.

[17] Meyerhof: *Loc. cit., supra.*

and Subbarow.[18] It may easily be predicted, however, that their investigations will never reap their reward until they include the ductless glands in their program.

My own labors in this connection have shown that while lecithin and its congeners in the brain, cephalin, and in the heart muscle, cuorin, have been studied chemically and therapeutically, their participation in muscular dynamics have not been taken into account, merely being referred to as having been found in these muscular tissues. Indeed, as stated by our leading authorities on the subject H. and J. S. MacLean, of London,[19] only last year (1927) in reference to lecithins in general: "Their great importance in the living organism is indirectly proved by their general occurrence in every cell; but little or no experimental proof indicating their specific function has as yet been obtained."

We have here convincing evidence of the mutual aid afforded in solving a problem, by the concomitant use of all branches of medical science. Indeed, it is only through histology that the presence of both lipoids, lecithin and cholesterol, may be discerned in muscle tissue. What has been termed the "fat droplet" in the muscle fiber gives characteristic reactions of lecithin, while what is known in histology as the "interstitial granules" in muscle tissue contain the cholesterol.

Permeating the whole muscular fiber also is the adrenoxidase, the catalytic oxidizing enzyme secreted by the adrenals as adrenoxin before it reaches the pulmonary alveoli, as shown by its characteristic staining and chemical properties, which also are those of adrenalin. All three agents, the oxidizing enzyme and two lipoids, are brought to muscle tissues by red corpuscles, which are known to contain all of them, and by certain leucocytes, the eosinophiles in particular. All three take part in muscular contraction when the arterial supply is increased through nervous impulses to the muscular arteries.

[18] Fiske and Subbarow: "Science," 1927, 65, p. 401.
[19] MacLean, H. and J. S.: "Lecithins and Allied Substances," 1927.

The heat energy liberated by the action of the adrenoxidase upon the phosphorus of the lecithin (controlled by the cholesterol) also serves muscular contraction by increasing the hydrolytic activity of the muscular enzymes on the carbohydrates which reach the muscle as glycogen to convert it into muscle sugar or glucose.

This constitutes from my viewpoint the thermogenic mechanism of muscle tissue which so far has never been described. In view of the presence of muscular tissue in practically all organs, and also of the fact that all impulses to the muscles are derived from the cerebrospinal nervous system which itself owes its functional activity to the same trio of thermogenic bodies, lecithin, cholesterol and adrenoxidase, the importance of this newer foundation for the study of the diseases of nervous and muscular systems cannot but suggest itself.

In closing, reference may be made as an index of the trend of things, to results recorded in 1923, by Boothby and Sandiford [20] who through their connection with the Mayo Clinic, had all opportunities for careful research. These authors, after quoting the contributions of other physiologists, beginning with Barcroft and Dixon [21] in 1906, found experimentally that "adrenalin, when injected into dogs in doses that seem probably within the power of the adrenal glands to secrete, does actually increase the rate of heat production." . . . "The increased metabolism is accompanied by an elevation of the respiratory quotient."

This makes it evident that physiology working independently, reached in 1923 a point in the functions of the adrenals which I had reached though far more comprehensively in 1903, and have developed since, and that my views have all along been poised on a strong foundation. It affords proof also that my labors were the first to point out since the discovery of oxygen, a century and a half ago, the rôle of this element in tissue respiration.

[20] Boothby and Sandiford: *Amer. Jour. Physiol.*, 1923, 66, p. 93.
[21] Barcroft and Dixon: *Jour. of Physiol.*, 1906, 35, p. 1906.

To indicate what this means as regards the development of our knowledge of disease and treatment may be surmised from the statement of a distinguished physiologist, Professor Halliburton,[22] of London, as recently as 1921, that "our knowledge of tissue respiration is so scant that we can say but little of its pathological bearing," which means in the present connection, the whole clinical field.

Summary and Deductions.—Analysis of the whole field of Medicine showed that the prevailing obscurity concerning pulmonary and tissue respiration was the cause of the defects noted.

I submitted, in 1903, the results of my labors showing that it was the *secretion of the medullary portion of the adrenals* (of which epinephrin and adrenalin are active principles) which *carried on both* previously obscure functions *pulmonary and tissue respiration.* Laboratory investigations in various departments, including physiology, have fully sustained this conclusion while it has been successfully opposed by none.[23]

This conclusion did not, however, tell us *how* these respiratory processes were carried out. It pointed only to the dominant active agent.

Investigations also based on the synthesis of sciences method, while editing nine additional editions of my Analytic Cyclopedia of Practical Medicine (sixty-four volumes) with, besides my own archives, those of the extensive library of the Philadelphia College of Physicians as sources of literature,* then showed conclusively—inasmuch as this deduction was tested in all the main diseases and found to fill the gaps that had been discerned in them—that the terms "*internal secretion,*" "*ductless glands*" and "*endocrine organs*" *designated merely a mode of transmission of certain secretions, but meant nothing as to function.*

[22] Halliburton: "Text-Book of Physiology," 15th ed., 1921.

[23] A two years' study by Dr. F. S. Hammett, physiologist and biochemist of the Wistar Institute, has verified the fact that the adrenals occupy this leading position, and so announced at the 1925 meeting of the Association for the Study of Internal Secretions.

* I wish in this connection to express my gratitude to Mr. C. P. Fisher, our librarian, and his staff, for their generous assistance at all times during the last thirty years.

The true functions of these organs I found to be far greater than had been generally supposed. Briefly, *the endocrine organs collectively constitute the mechanism through which the life process itself is sustained, and by means of which it is protected against disease and death. They do so by constituting what I have termed the "thermogenic system" in which the adrenals are dominant executives, and the thyroid and parathyroids as accelerators of heat production. The heat energy liberated has for its purpose to activate more or less various enzymes in all forms, which enzymes are the direct factors in sustaining cellular metabolism and tissue life.*

LIGHTER-THAN-AIR MACHINES

By C. E. ROSENDAHL

Lieutenant Commander, U. S. Navy

(*Read April 20, 1928*)

Were the designers and promoters of that great distance annihilator, the magic carpet of the Arabian Nights, on hand today, they would no doubt be seriously concerned over the problems of furnishing bigger, better and faster magic carpets, for mankind continues to demand greater speed in transportation. High speed trains and swift steamers with the aid of extra fare continue to flourish; further increases in everyday railroad and steamer speeds are much more expensive and furthermore very difficult to obtain. Fortunately, however, scientific progress has, in comparatively recent years, opened up the most promising transportation medium of all—that by air—and we are now well on the way to enjoy its practical benefits increasingly day by day.

Aircraft are easily classified into two types: the heavier-than-air type represented by the airplane, and the lighter-than-air type represented by the airship. Although the latter term is often used indiscriminately for all types of aircraft, we in the lighter-than-air branch feel that the designation "airship" should properly be applied only to dirigible balloons as they are fundamentally ships that float in air; as a matter of fact, the airship and the submarine are analogous to a large extent both in construction and in operation. The airplane is, of course, an aërodynamic instrument depending entirely on the effort of its engine and propeller to keep it aloft as well as to drive it to its destination. The airship is primarily an aërostatic form of carrier and even though it actually enjoys aërodynamic control over fluctuations of buoyancy and of load, practically the entire effort of its propellers is devoted to its propulsion through the air, and the airship is entirely capable of remaining aloft even should there occur the extremely unlikely failure of all its propulsion plants.

The airplane naturally has the greater appeal to the average individual who wishes to fly his own craft. He pictures himself as the possessor and crew of a plane rather than of a huge airship, just as the average man who wishes to obtain his pleasure on the water gets a sailboat or a small powerboat and not a sea-going steamer. The airship in the past has not been as well understood as its smaller cousin but the realization is gradually dawning that airships as well as airplanes are essential both to commercial transport and to the national defense. The editor of *The Aëroplane*, a British aëronautical publication, has aptly said: "Airships breed like elephants and airplanes like rabbits. Consequently, the airship is many generations in the process of evolution behind the airplane."

Among the less commonly understood facts in connection with aircraft is the effect on efficiency of increasing the size of such craft. We frequently read of the design and pending construction of "huge airliners" of both lighter-than-air and heavier-than-air types, but actually both types do not lend themselves to the application of the time-worn slogan "bigger and better" as far as efficiency goes. In the case of the airplane, it is known that the limit of efficient increase in size is about to be reached. At a point roughly some two tons heavier than Commander Byrd's plane, the *America*, the larger plane demands a greater proportional amount of "dead" weight and it becomes a less efficient load carrier. We may say with certainty that in the light of present knowledge, the future of the airplane, both in peace and in war, lies in pursuits other than transoceanic transportation and other long-range operations. On the other hand, as we increase the size of an airship, its lifting capacity increases much more rapidly than do its structural and power plant weights. Designers of extensive practical experience have found that the efficiency of rigid airships increases with increased size up to ships of 15,000,000 cubic feet lifting gas capacity, or six times the size of the *Los Angeles*.

It is primarily for these reasons, then, that we maintain that the field of long range aërial transport belongs to the airship. Other reasons, such as the greater facility and ease of navigation and the more abundant comforts of the airships, are also of importance in sustaining this contention. But despite this fundamental difference, there is an abundance of legitimate uses for each type of aircraft and, properly employed, there can be no conflict or competition. The airplane can provide higher speed travel over moderate non-stop distances whereas the airship provides rapid, more comfortable travel, over great distances, and both types are indispensable. A large seagoing steamer might be employed on a short coastal run but that would be an extravagant and inefficient employment; likewise an airship might inefficiently do some of the functions of an airplane, but the plane cannot perform the legitimate functions of the airship.

The history of lighter-than-air craft dates back to 1783 when the first balloon flight was made. Men had observed that hot air would rise; therefore by inflating a bag or container of light material with heated air, the container could be made to rise and take with it a basket or car in which to carry passengers or other loads. Soon man was able to produce hydrogen gas in sufficient quantity to inflate a balloon and since hydrogen is so much lighter than air, it has always been the most efficient of lifting gases. In November 1783 a Frenchman, made the first free balloon flight by a human being. The essentials of free-ballooning were all developed in the eighteenth century. But man soon became desirous of providing balloons with motive power so that he might fly independent of the wind. Early effort consisted of rowing in the balloon with silken oars. In 1852 the first power driven or dirigible balloon was built. It derived its motive power from a three-horse-power steam engine and attained a speed of five miles an hour. The modern airship had to wait for the development of the gasoline engine and a light yet strong material with which to build the structure.

Santos Dumont, a rich and romantic young Brazilian with a strong mechanical bent, in the years immediately following 1898 built fourteen airships of various types and did much to arouse interest in flying. Various other efforts along the non-rigid and semi-rigid principles were continued by many others.

It was in 1900 that Count Zeppelin completed and flew his first rigid airship in Germany. In 1908 our first American airship, a small non-rigid type, was built. In 1916 the American Navy began to use small airships and continued until in 1919 their employment in the Navy had dwindled to almost nothing. Later the Navy became interested in rigid airships and constructed the ZR-1, named the *Shenandoah*, in 1923. The ZR-2 contracted for in England, was wrecked and lost there. The ZR-3, later christened the *Los Angeles*, was built in Germany by Allied permission, to replace the two destroyed German Zeppelins which should have been delivered to us. It is thus seen that the United States has operated actually only two rigid airships.

At this point, let me briefly classify lighter-than-air craft. We have: free balloons, which possess no motive power and drift with the wind, the pilot having only up and down control; the captive or kite balloon which floats aloft and is made fast to the earth by a wire cable; and then come the dirigible balloons which are of three classes—non-rigid, semi-rigid and rigid.

The non-rigid airship is one whose gas bag contains no internal structure and owes its shape solely to the outward pressures of the gas and air contained within it. The semi-rigid has a partial internal structure which it relies upon together with the internal gas and air pressures to maintain its shape. The rigid airship is one whose form is maintained entirely by a rigid skeleton structure. There are a number of non-rigids in this country—our Navy now operates the J-3 and the J-4 for training purposes and our Army has several similar ships called the TC class. Non-rigid airships are of necessity small but can cruise at 55 or more miles per hour for between 12 and 24 hours. They are useful for convoy work, coastal patrol, anti-submarine work, photography and mapping and a number of other purposes.

Semi-rigid airships have a somewhat greater range and greater carrying capacity than the non-rigid and form the intermediate step to the rigid airship. The Navy has no semi-rigids at the present time but the Army operates one, the RS-1, from Scott Field, Illinois. The Russians, Japanese and Italians favor the semi-rigid and it was a ship of this type, the *Norge*, which carried the Amundsen-Ellsworth-Nobile Expedition over the North Pole in 1926. The rigid airship, of which the *Los Angeles* is an example, has a much greater cruising radius and carrying ability than the other two classes. It is the rigid type that we consider holds such high potentialities for aërial transportation.

In order that you may better understand the usefulness of the rigid airship, let me point out a few of its outstanding flights:

(*a*) Small commercial rigid airships operated in Germany both before and after the war and carried 37,000 passengers without accident or mishap. Most of this was before the war, as the post-war commercial ships had to be delivered to the Allies after only brief German operation. The proof of this commercial venture was that the ships practically always carried capacity loads. The Bodensee, one of these commercial airships, was turned over to the Italians and is still operated by the Italian air force today.

(*b*) The German L-59, in November 1917, took off from her base in Bulgaria, carrying a cargo of fourteen tons of medical supplies and small arms' ammunition to the besieged German East African Colonies. Just as the destination was about reached, a radio message was received by the airship stating that the German Colony had surrendered. She therefore returned to her base without landing. Although she had been in the air for almost 100 hours and had traveled about 4,500 miles with her fourteen-ton cargo, upon landing she still had sufficient fuel for an additional 48 hours' flight.

(*c*) The round trip of the British R-34 between England and the United States in July, 1919, was a noteworthy achievement, as that type and size vessel was then already obsolete.

(*d*) In October, 1924, our American built *Shenandoah*, modeled after the German 1916 war type but not completed until 1923, cruised across the continent, up the Pacific Coast and returned to Lakehurst, having covered over 9,000 miles in many kinds of weather, basing entirely on mooring masts for over 19 days.

(*e*) The Zeppelin *Dixmude*, while operated by the French, stayed aloft for 118 hours or nearly five days, making the world's record for aircraft.

(*f*) The *Los Angeles*, then designated as the ZR-3, on her delivery flight from Germany in October 1924, covered 5,060 miles in eighty-one hours, spanning the actual ocean expanse in sixty-one hours, her average speed being over 62 miles per hour. This more modern ship thus showed a much better performance than the earlier R-34.

(*g*) Time does not permit recounting to you here the varied wartime uses to which airships were put, but they were many and important.

(*h*) It is significant to note that until the recent crossing by the Junkers plane *Bremen*, the only successful westbound flights across the Atlantic have been made by airships and the first of these as far back as 1919.

All these flights were made with ships that do not compare in size or otherwise with the airships we shall soon see in operation.

And here are some of the recent developments in the airship situation. In England nearing completion are two huge commercial airships of 5,000,000 cubic feet capacity—each twice as large as the *Los Angeles* and each capable of carrying one hundred passengers in comfort for 4,000 miles. Commander Burney is in the United States today arranging for a probable visit of the first of these two ships to the United States this fall. The second of these ships will be finished only a little later. Great Britain has built these two large airships to unite her Empire more closely and accordingly has laid out an airship route from England to India via Egypt; servicing stations and terminal facilities are nearly complete and mooring masts are to be built in Canada as well. By airship from England to Canada will take two and one half days, whereas steamers now require six days; from England to Egypt will require two and one half days by airship as opposed to six days by steamer; from England to Singapore will require eight days by airship whereas twenty-four days are required by steamer—making a possible saving of sixteen days by airship.

At this very moment in Friedrichshafen, Germany, the birthplace of the *Los Angeles*, the airship LZ-127 (to be called the *Count Zeppelin*) is being rushed to completion and her maiden voyages across the Atlantic to the United States are expected to be made this early fall. This ship, one and one half times the size of the *Los Angeles*, built largely by popular subscriptions by the German people, embodies a number of novel features. This ship was intended to be operated under a subsidy from the Spanish Government for a commercial run between Spain and the Argentine. Unfortunately, recent despatches indicate that the Argentine terminal will not be ready at the scheduled time so that the LZ-127 may not be enabled to undertake the South American run, but there are many other fields open to her.

And what about new construction in the United States? At present our only large airship is the *Los Angeles*. The last Congress authorized as a part of the five-year aircraft building program, two large naval rigid airships of about six million cubic feet gas capacity—larger than even the new British ships about to be completed. Unfortunately their construction has not yet been begun—we hope and expect it will be in the immediate future. These ships will embody a number of novel features of great importance.

It is easy to realize the commercial possibilities of aircraft and particularly of airships. In order to present clearly and briefly what the *Naval* functions of airships are, I can do no better than to quote from a Congressional report which was rendered only last year. After an exhaustive investigation the report rendered reads partly as follows:

"The Committee finds that airships of adequate size hold unquestionable possibilities as adjuncts to the Fleet. Large airships are peculiarly naval as their sphere of greatest usefulness lies over the water; they are essentially long-distance, weight-carrying machines, having long radii of action, ability to keep in the air for long periods, superior habitability, the ability to operate at night successfully without the necessity for elaborate lighted airways, and wide range of speed variation to the extent of being able to stop all engines and still remain aloft.

"Their principal naval mission will be scouting and reconnaissance, augmented by such uses as anti-submarine operations, convoy work, carrying airplanes, transportation of and communication with detached units, and under certain conditions, bombing.

"In the case of a large airship of proved type of construction, built so that interior parts are accessible for repair during flight; filled with noninflammable helium gas; equipped with machine guns for defense or limited offense; and carrying two or more airplanes for self-protection, vulnerability will be reduced to a point where it will not militate against the airship playing an influential rôle in military operations.

"So decided are the possibilities of lighter-than-air craft, it is felt that we cannot afford to do otherwise than to follow up its present advantage and determine the utility and limitations of rigid airships when employed in active operations with our other naval forces; we have all the necessary facilities and are prepared to go ahead vigorously with the further development of this type of craft. The Committee has found that rigid airship development in this country lags far behind airplane development. The expenditures on airships have been only about 2 per cent of the total expenditures for aviation. It is believed that the construction of the two rigid airships included in the bill will go far toward building up in this country an airship industry, which, when it is established on a sound basis, will be in a position to carry forward the commercial development of airships.

"The Committee feels that the least that should be done in this field is to provide for two rigid airships of approximately 6,000,000 cubic feet volume each, to be used as adjuncts to the fleet." This ends the quotation.

It is interesting to note that the German naval commanders on at least several occasions, postponed movements of their naval forces awaiting the availability of their airship scouting forces. British wartime naval commanders saw the benefits of airship scouts but although a large airship construction program was begun, British rigid airships could not be finished in time for their use in the World War. Whatever may be the actual relative value of modern surface scouts and of modern airships remains to be determined, but it is certain that each, and particularly the airship, has improved and any well rounded Navy requires both types.

By joint agreement with the Army and to prevent duplication, our Navy is charged with the development of rigid airships in the United States. While airships do hold great potentialities, we who work with them know also that airships are not yet wholly perfected instruments. The problems of both military and commercial airships have so far coincided to a large extent. As our contribution to airship progress, we have conducted many experiments that are about to produce their results within the near future. The greatest problems of airship operation in the past have been those due to inherited undeveloped methods and equipment for handling airships on the ground—in other words, terminal facilities for airships have been inefficient and inadequate. It is remarkable indeed that airships, handled only by man power on the ground, have been able to do as much as they have. However, we are never content to do with man power what we can develop mechanical power to do and this substitution of machines for most of the men now used on the ground is the major problem we in the United States hope to have solved in the near future. We hope eventually to make the airship just as available as steamers now are; the airship will not have to be berthed in a shed regularly but will moor outside between flights and go into a hanger only for "dry-docking." I do not imagine that the pioneers in the early development of railroads and steamships could possibly have visualized the vast extent and scope of the auxiliaries that remained to be developed to make their new means of transportation sufficiently flexible. I do not believe they even dreamed of the vast amount of dredging, tunneling, elaborate docks, fleets of tugs, drydocks, etc., that were found to be necessary. Similarly in the pioneering stage of aircraft, analogous facilities are just being developed and some of the problems involved are quite complicated. Seaports grew up where nature had already provided certain natural features such as deep and broad expanses of water. Man would not attempt to make a seaport along a shelving sandy beach nor would he expect to use for that purpose a location where tides of sixty feet prevail. Yet we attempt to put airports anywhere and everywhere and it is only recently that the realization is becoming more general, that for airships at least, some geographical locations are very much better endowed fundamentally by nature as airport sites than are others. However, in his wildest dreams, no enthusiast has ever proposed for airships such an expensive auxiliary as that of numerous artificial islands in the sea—airships do not require them. Aircraft may always be dependent to some extent—a continuously decreasing one fortunately—on weather but this is no reason for abandoning flying. Even stolid surface craft after hundreds of years of development are not immune to the caprices of the weather. There will, of course, always be hazards in flying—there still are in railroads, automobiles and steamers, but their use goes on.

In the matter of comforts in travel, airships can provide the best. In modern airships you ride in a sheltered structure, there is no noise, vibration, dirt, smoke, and the motion, when there is any, is usually only a very mild gradual pitching. I have never seen any seasickness in an airship. There are ample comforts for sitting, sleeping, reading, writing, card playing, walking about and exercising and the new passenger airships contemplate even ball rooms. But, of most importance, the airship provides an electric kitchen which can furnish as satisfactory a menu as can be desired. Perhaps airships will never provide swimming pools as huge steamers do, but when you are crossing the Atlantic in two days instead of six you can probably dispense with your daily swim for that period. Fogs, muddy or snow covered fields present no insurmountable difficulties for airships and airship flight at night and in darkness is generally even easier than in the daytime. It is in the field of transoceanic rather than in transcontinental transport that the airship will soon be a competitor.

Airships are capable of many improvements as they become larger—"bigger and better" is a correct slogan for airships up to at least six times the size of the *Los Angeles*. And what is more important, as earlier indicated, the efficiency of the airship or the amount of its useful load compared to its "dead" load increases with larger ships. With this increase it becomes possible to add structural strength, more speed and greater performance—all factors of great importance. All this is possible in the light of present principles of design and construction and with the materials now available; the future may and probably does hold new variations in construction and design and also lighter materials which will add to increased airship efficiency.

In passing let us take a brief glance at some of the greater airship problems that have confronted us in the United States.

First of all, there has been a lack of airships with which to carry on. At no time have we ever had more than one rigid airship in operating condition. With numerous projects demanding attention, their trial has necessarily been slow. We have not been able to benefit by simultaneous and competitive trials of experimental installations such as has been practicable in airplane and surface ship endeavors because of their greater numbers. It must be borne in mind also that our actual rigid airship operating experience did not begin until late in 1923 and was unavoidably subjected to numerous interruptions in flying.

The lack of airship bases has handicapped development of airships in this country. There is only one base in the United States—Lakehurst—properly fully fitted for handling rigid airships. A few secondary bases such as are afforded by high mooring masts are widely scattered but these are not wholly perfect nor all that we could desire. Consequently most flights have had to be planned to begin at Lakehurst and to finish there. Of course it is true that we now, through actual experience, know better what requirements should be fulfilled at operating bases, and any airship ports to be built in the future will prove more efficient and even less expensive than those of the past. Where training and experimental flights are of primary importance, the selection of the site for an airship base must receive serious consideration. We do not learn to swim by being thrown overboard in a rough sea and, in the past, operations have been materially influenced largely by the unfavorable meteorological location of our one airship base.

Undeveloped handling methods have already been pointed out as one of the most urgent airship operating problems demanding solution.

It must be borne in mind also, that in the compromise between weight and strength, all rigid airships at present in existence, as well as those of the past, were designed and constructed with flying qualities and considerations predominant, whereas handling was left a secondary matter. We are certain that future design can and must yield more to handling considerations.

The present tall mooring mast has served as an important link in the transition period of airships; but, unfortunately, it has limitations. The most serious drawback to the high mast is the danger to the moored ship from vertical air currents. The common conception of wind is that of a mere horizontal flow of air and were this idea always true, flying would be comparatively simple. It so happens, however, that the atmosphere abounds in waves and in vertical currents of air as well as in the common horizontal flow. Squalls and thunderstorms we know are accompanied by vertical currents and, although airships have ridden out storms while moored to a mast, it becomes a matter of serious consideration when one thinks of what might be the result in extreme cases when the ship, riding normally at a high mast with only her nose secured, has the stern lifted violently upward or perhaps forced violently downward together with rapid change in azimuth. Lightning and precipitation are not so much to be feared. It is my opinion that the proper place for mooring out a ship is at or very near the ground in the currents of lower velocity, where the nose may be held in an arrangement which will give perfect freedom to answer the wind and where the stern may be controlled vertically, yet given freedom horizontally to travel over the ground. But before this idea can be executed, it has been necessary to obtain a great deal of data on the strength and directional variations of gusts in order to determine whether the loads imposed would be within the safe limitations of the ship's structure. This meteorological data was not already available and required specially designed instruments for obtaining it. Although the structure and other characteristics of gusts have not yet been accurately analyzed, there is sufficiently great promise already indicated to warrant proceeding with the application of this mooring scheme.

I believe that when landing, mooring out and handling problems are correctly solved and the large ground crews accordingly reduced, the moment will have arrived when commercial enterprise can safely step in to the operation of rigid airships. There will always be other features that require development and refinement, but they are of such a nature that I believe their overhead can be readily absorbed. During the immediate future there will be demonstrated at Lakehurst the use of equipment, largely mechanical, which is the result of our experience. It is believed that this equipment, when refined, will prove the basis for the successful solution of terminal problems.

The helium situation has not always been satisfactory in the past. On several occasions our reserve operating supply of helium has dwindled to nothing and, coupled with certain other avoidable material conditions, has at times forced our airships out of operation. Born under war time conditions and necessarily of a pioneering nature, our original helium project proved of entirely insufficient scope for even our peace time needs; not because of any major difficulties but rather because of actually small ones easily capable of remedy.

The United States is committed to a helium inflation policy and it is interesting to note the high regard of other nations for this safer buoyant medium.

The last Congress provided an appropriation of $1,063,000 for expansion and development of the helium project. The results of this are yet in the future but it will allow continuation of the work necessary to produce cheap helium. Also, recently private capital entered the field of helium production and the real issue of producing helium cheaply should very soon be achieved. Private enterprise in the helium field is certainly necessary before commercial operation of airships becomes feasible. Present indications point to an abundant supply of helium for generations for all the airships we shall probably operate.

Heretofore, man's ordinary routine of life has found two daily weather maps sufficient. For years, the Weather Bureau, twice daily, has collected meteorological readings from a large number of stations scattered over the United States and certain possessions, and parts of Canada. Utilizing these readings, synoptic charts are constructed from which the forecasts are made. With the means and funds available, the Weather Bureau has done wonders, but the speeding up of life by the present and promised prevalence of flying and aërial transportation makes the present weather service inadequate, at least for flying needs.

In flying, one changes his location so rapidly that he may be continuously running into new weather conditions, and aircraft in flight thus require frequent weather data. Not only is it necessary to have knowledge of dangerous weather conditions so that they may be avoided, but it is equally important to be able to take the maximum advantage of favorable and beneficial conditions.

To make trans-oceanic flying common and safe, as it will be in a few years, we must duplicate for the ocean expanse what we have been doing in the way of weather information for terra firma. For special flights, the Weather Bureau has obtained readings from the numerous ships at sea and drawn the necessary maps. However, although this service would be, even under ordinary conditions, of inestimable value even for surface craft, lack of funds has made it impossible to render this great public aid. Some day we shall come to the realization of this world weather service as a means towards securing greater friendliness and coöperation amongst nations as well as being of greater benefit to the air and surface sailors of all nations. With the coming, in a few months, of trans-oceanic airships, this service becomes mandatory.

There are other interesting problems also but perhaps you may be interested in visualizing a few of the new features of our new airships. As I have indicated earlier, the larger the airship, the greater is the proportion of useful lift provided. We may expend this increase by providing stronger ships, greater speed, and greater performance. Actually we intend to gain in all three of these lines. Instead of long slender ships of the *Shenandoah* design, the tendency is to the comparatively shorter and fatter type. Instead of only one longitudinal corridor as in ships of the past, there will be

3 or 4 such passageways in our new ships, thereby providing much greater longitudinal strength. The *Shenandoah* and all rigids up to this time of course had only one such corridor. In our ships inflated with the safe helium gas, we shall put all the engines inside the hull and thereby eliminate the present high resistance of external power cars. Provision will also be made for obtaining thrust from the propellers not only ahead and astern but also up and down vertically —a great advantage for landing and taking off. And perhaps a little more startling, our new naval ships will each have an interior stowage for four service type airplanes and a means of attaching them and detaching them in flight. Commercial airships also will find this airplane adjunct of indispensable value.

The building of the two new naval rigid airships in this country should be the first and most important step in the establishment of an American airship industry. We are now behind Europe in airship construction and will be for several years. The National Advisory Committee last year had to report briefly: "The World leadership in the design and construction of rigid airships has passed from the United States to Europe." This situation need be only a temporary one—our fortunate monopoly of the supply of the entire safe helium gas, although not as well appreciated as it possibly should be, alone gives us a world wide advantage in safety for airships. Other problems are equally capable of solution.

We can regain our supremacy in airships by going ahead at once with new ships of modern size and characteristics. The *Los Angeles*, now hailed as a "giant airship," will very soon be considered a small ship. It is my opinion that huge airships as well as airplanes will prove to be of indispensable commercial value and an auxiliary of high naval value that will add strength to our American Navy and the American nation. But, what is more, by providing intimate and rapid contact of the peoples of the earth, the airship will soon be recognized as an instrument of the highest order for helping reach that elusive goal of world peace.

TWO ADDITIONAL SPECIES OF TRIANAEOPIPER

By WILLIAM TRELEASE

SINCE the publication of the genus *Trianaeopiper*,[1] I have had the privilege of examining the numerous Piperaceae contained in the very large collections of plants made in Colombia by Pennell, Killip and Hazen in 1922 and by Killip and Smith in 1926. Among the former are two numbers referable to this interesting segregate from *Piper* which it seems advisable to characterize in the volume containing the description of the genus; both are from the general region of the Andes from which the others come, so that the genus still remains highly localized, though as yet known only by a single collection for each species.

URBANA, ILLINOIS.
November 28, 1928.

TRIANAEOPIPER KILLIPI n. sp.

Scarcely lignescent; flowering internodes comparatively thick and short, obscurely and evanescently crisp-pubescent at least near the nodes; leaves orbicular, bluntly short-acuminate, cordate, about 18 cm. in diameter, multiple-nerved from the lower half, the nerves about 8 × 2 and crisp-pubescent beneath; petiole some 10 cm. long, somewhat crisp-pubescent, narrowly winged; spikes solitary in the axils, 7 × 60 mm., obtuse; peduncle 2 × 45–50 mm., glabrate.

Type locality: La Gallera, Micay valley, at 1400–1500 m. (Killip 7762, as sheet 1142407 in the U. S. National Herbarium).

Distribution: Western cordillera of Columbia.

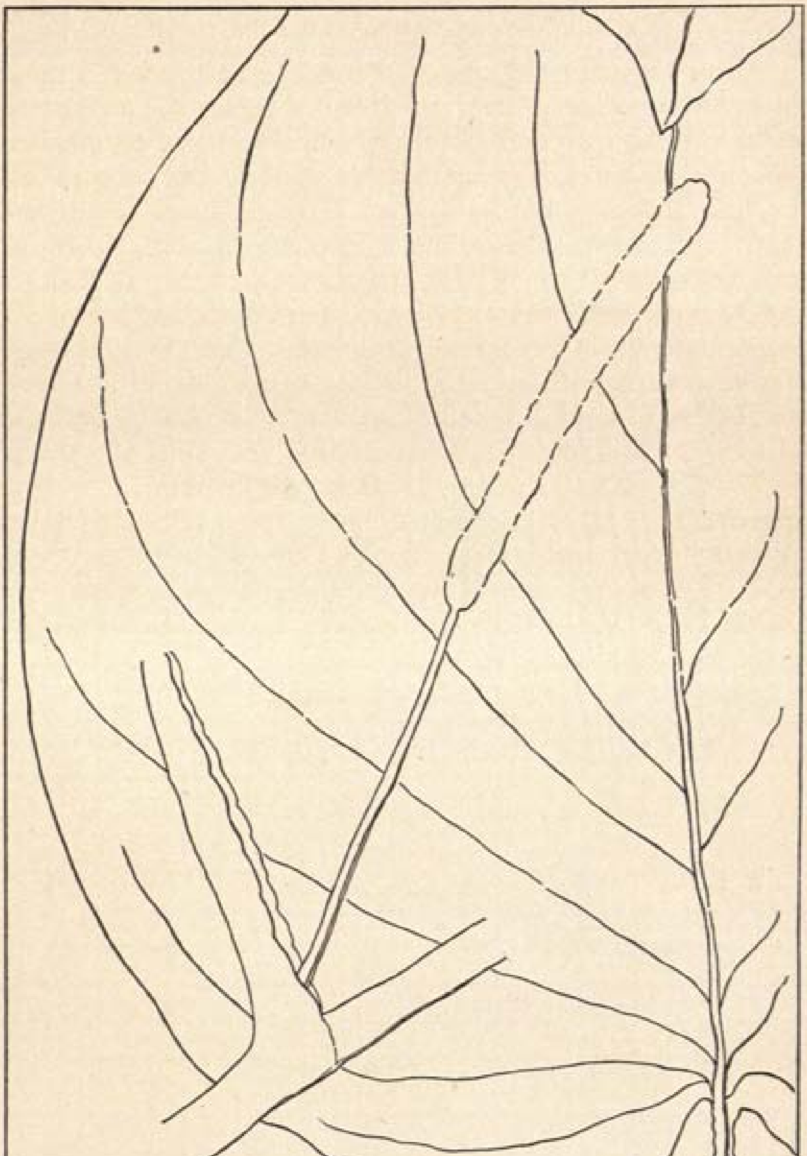
[1] *Proc. Amer. Phil. Soc.*, 67:47. 1928.

Trianaeopiper santa-rosanum n. sp.

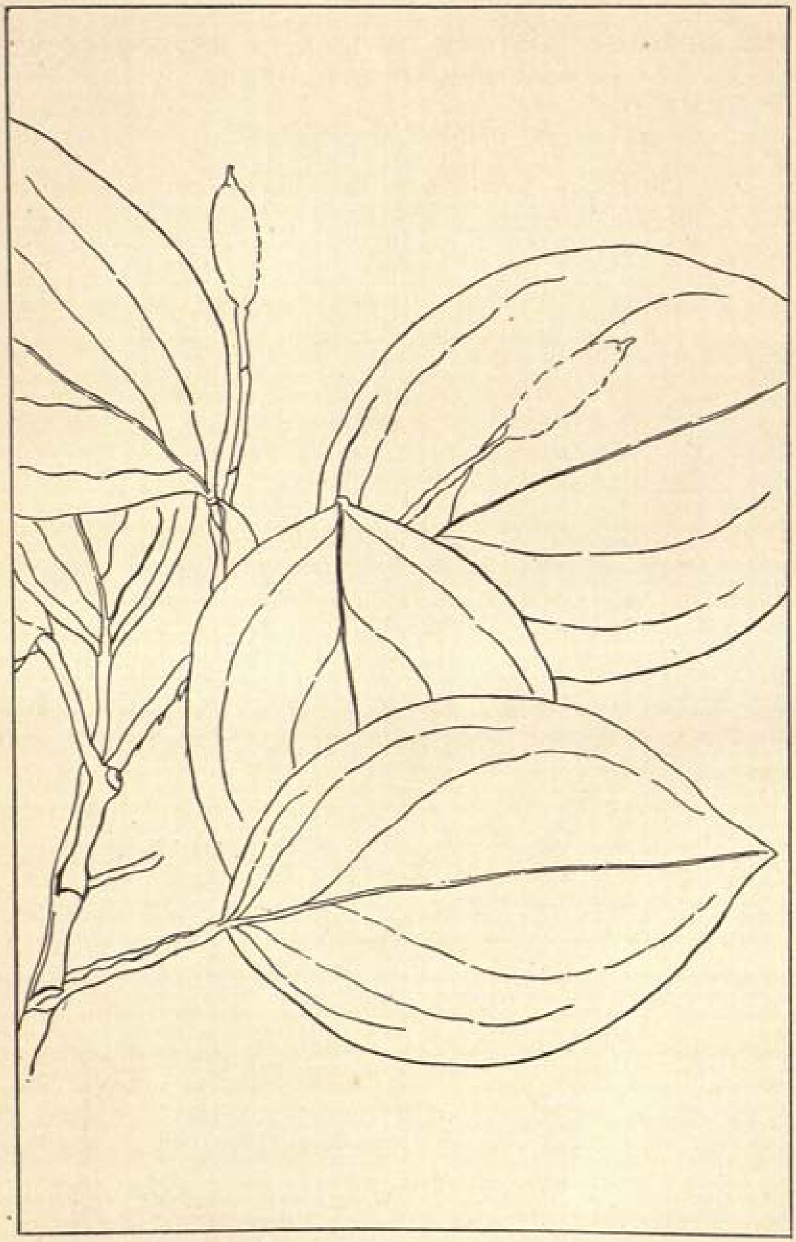
A low (15 cm.) suffruticose *Piper*-like herb; stem 3 mm. thick, brown-tomentulose, the swollen nodes 2 cm. apart; leaves alternate, round-elliptic, obscurely short-accuminate, subacute at base, moderate (4 × 6–6 × 9 cm.), somewhat revolute, glabrous but somewhat granular above, paler beneath with nerves and veinlets crisp-hairy, multiple-nerved from below about the middle, the branches of the midrib 3 to 5 × 2, impressed above and, like their anastomosing branches, prominently raised beneath; petiole 2–3.5 cm. long, brown-tomentulose, striate-nerved, broadly winged or with a submembranous margin; spikes axillary (or also terminal), solitary on 2-bracted stalks scarcely 2 cm. long, short and thick (5 × 15 mm.), densely flowered; peduncle brown-tomentulose, 3.5 cm. long; bracts crescentic-subpeltate, ciliolate; ovary turbinate; stigma apical.

Type locality: Santa Rosa, El Valle, at 200–300 m. (Killip 11548 at the Gray Herbarium, and as sheet 1143242 in the U. S. National Herbarium).

Distribution: Dagua valley, Colombia.



Trianaeopiper Killipi.
Type. Natural size.



Trianaeopiper santa-rosanum.
Type. Natural size.

THE UPRIGHT POSTURE OF MAN: A REVIEW OF ITS ORIGIN AND EVOLUTION

By WILLIAM K. GREGORY

THE upright posture of man has long been considered one of his god-like attributes. Milton in *Paradise Lost* says that in the Garden of Eden there were

> " . . . . all kind
> Of living creatures new to sight and strange."

But:

> "Two of far nobler shape, erect and tall,
> Godlike erect, with native honor clad
> In naked majesty seemed lords of all,
> And worthy seemed;"

Thus the erect posture which has enabled man to look down on the world of quadrupeds may well be one of the bases for man's colossal and impregnable superiority complex.

The contrary notion, that man is an uptilted and still only partly refashioned four-footed animal, is to this day deemed impious and even blasphemous by many accredited spokesmen of millions of people in Boston, Dayton, and points west and south.

Benjamin Franklin, the illustrious founder of this Society, who labored so successfully for the diffusion of *useful* knowledge, might very well be imagined as approving the suggestion that the Society should devote one or more of its stated meetings to a consideration of the problems relating to man's posture; for it will be obvious to everyone that the subject has more than an academic interest, especially if it can be shown that the erect posture is not entirely an unmixed blessing, since it has made civilized man liable to fallen arches, to assorted hernias and to all the unpleasant visceral ptoses, prolapses and similar ills that flesh is heir to. This aspect of the subject has been very fully and ably dealt with by others, especially Sir Arthur Keith in his lectures before the British Medical Association, in 1923, and can here only be referred to in passing.

The object of the present address is rather to call your attention to the remarkable and as yet not widely known fact that Nature has preserved for our attentive examination a long sequence of fossil and recent vertebrate animals which has every appearance of exhibiting the main stages of evolution,—though not the direct line of ancestry,—as our ancestors gradually mastered the increasingly difficult feats, first of swimming with their fish-like bodies, next of crawling and running on land, then of climbing in the trees and finally of balancing the body on the hind limbs.

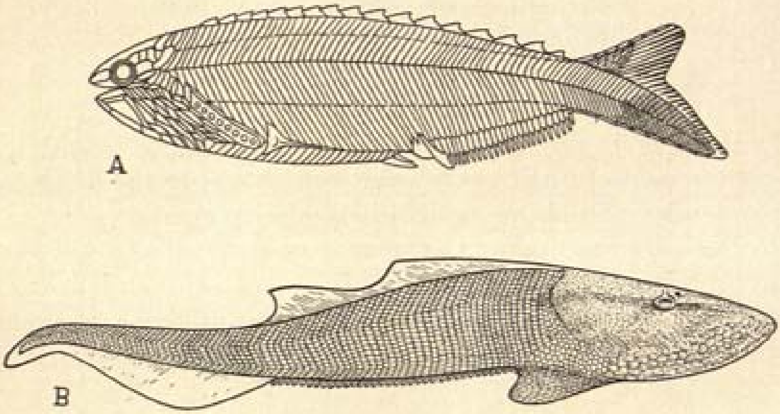


FIG. 1.—Restoration of two primitive Silurian chordates.
A. *Rhyncholepis parvulus*. After Kiær.
B. *Aceraspis robustus*. Data from Kiær.

The Basic Patent of Our Locomotor System

It has been well said that evolution bears no evidence of a beginning and no prospect of an end. But we may arbitrarily begin at that vastly distant point when at least the ground-plan of the vertebrate type of organization had already been achieved. The inconspicuous fossils here figured (Fig. 1) represent two of the very oldest and most primitive known forerunners of the vertebrates, from the Upper Silurian of Denmark. From these and similar specimens recently described in the memoirs of Stensiö and Kiær it is evident that

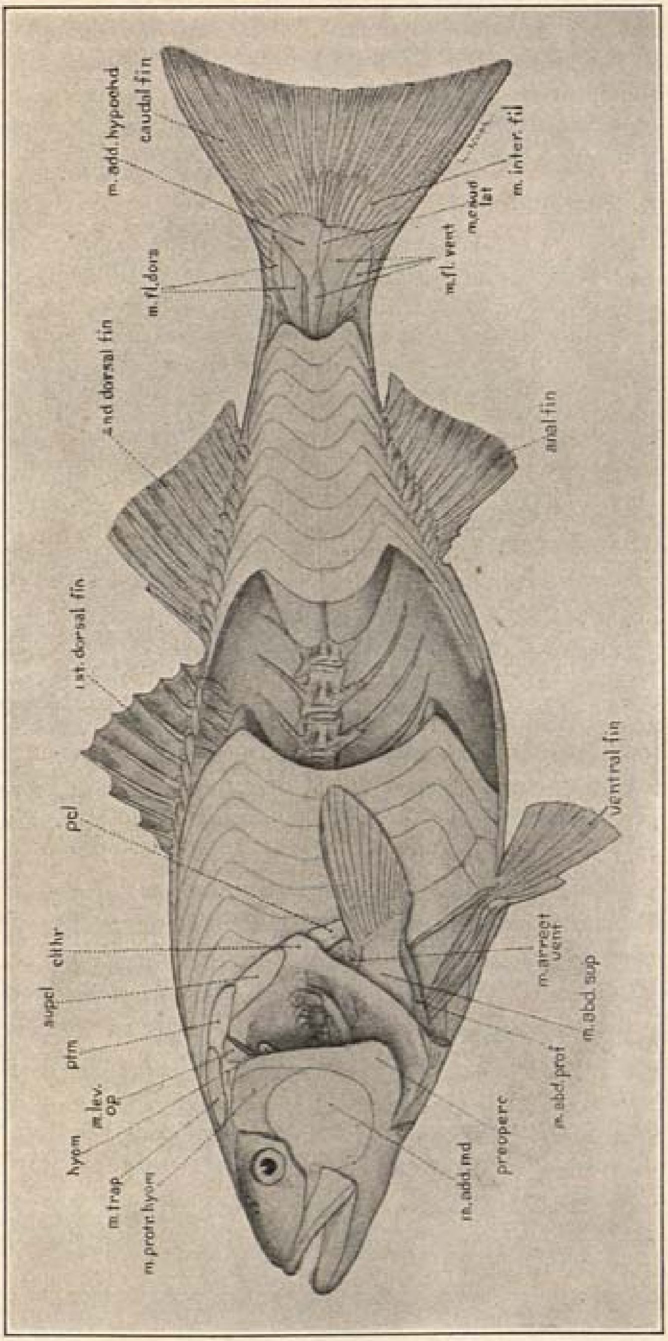


FIG. 2.—The locomotor apparatus of a typical fish (*Roccus lineatus*). From specimen.

at least as far back as Silurian times the predecessors of the vertebrates had already learned to move through the water in a forward direction, by propagating a series of waves in their own elongate bodies, the waves passing backward from the head toward the tail after the manner illustrated in the modern eel.

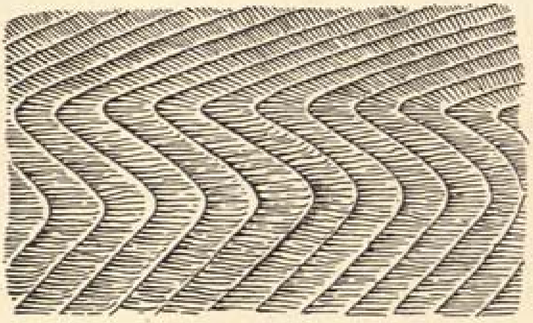


FIG. 3.—Arrangement of zigzag muscle segments, connective tissue, septa and red muscle fibres in a shark.

It will be observed that on each side of the body there is a series of oblique zigzag strips, which apparently represent the muscular zigzags or myomeres of modern fish (Fig. 2). Each zigzag is separated from its neighbors by connective tissue partitions. The myomere may be regarded as the gross or macroscopic unit of the locomotor apparatus of all vertebrates, since, as we shall presently see, it is from combinations of these zigzag muscle strips that the complex musculature of the body and limbs of vertebrates is built up. In a modern amphibian, for example, we see very clearly the myomeres on the side of the body. They differ from those of fish chiefly in being more hoop-like. In the early embryos of the higher vertebrates, including man, the primitive spinal segments, now more block-like than zigzag, can be very clearly recognized as the bases from which the segments of the backbone, as well as the highly complex musculature of the trunk and limbs are built up.

The contractility of each myomere is due to the red muscle fibers that are stretched horizontally between the septa that bound the myomeres (Fig. 3). These red muscle fibers may be regarded as the microscopic units of the locomotor system. Sir Arthur Keith has compared each red muscle fiber to a tiny gas-engine in which the energizing substance utilizes the oxygen of the blood after performing the work of shortening the muscle fiber in spite of the resistance or load of the muscle.

The Origin of Fins and Limbs

These simple zigzag muscles are still the only locomotor units needed for the propulsion of the lancelet *Amphioxus*, and the same is true in the existing lampreys and hagfishes or cyclostomes, a group which the researches of Stensiö have now definitely shown to be derived from the grade of the old Silurian prevertebrates or ostracoderms. Undulatory movements of the body are all that is necessary for the propulsion of these types. Their fins, which are simply raised folds of stiffened skin, mainly serve as keels and rudders, with a minimum of independent movement of their own. In the sharks, which represent the first main advance toward the higher vertebrate type, the entire musculature of the sides of the body is still formed by the myomeres, but the median and paired fins show many stages in the elaboration of more or less independent musculature and movements. At first the myomeres in the body merely extend into the bases of the fins. Gradually these extended parts of the myomeres begin to combine with each other in opposing groups, which serve to bend the fin to one side or the other or to impart to it an undulatory movement of its own. Usually the simple dorsal and anal fins (Fig. 4, *E*) retain the more primitive conditions, in which the muscles of the fins are most obviously merely the extensions of the outer layers of the myomeres, while, as noted by Tate Regan, the pelvic fins show an intermediate condition in which the fins begin to become paddle-like. The pectoral fins finally acquire a narrow wrist-like base capable of elaborate movements of abduction, adduction, elevation, depression, rotation and undulation.

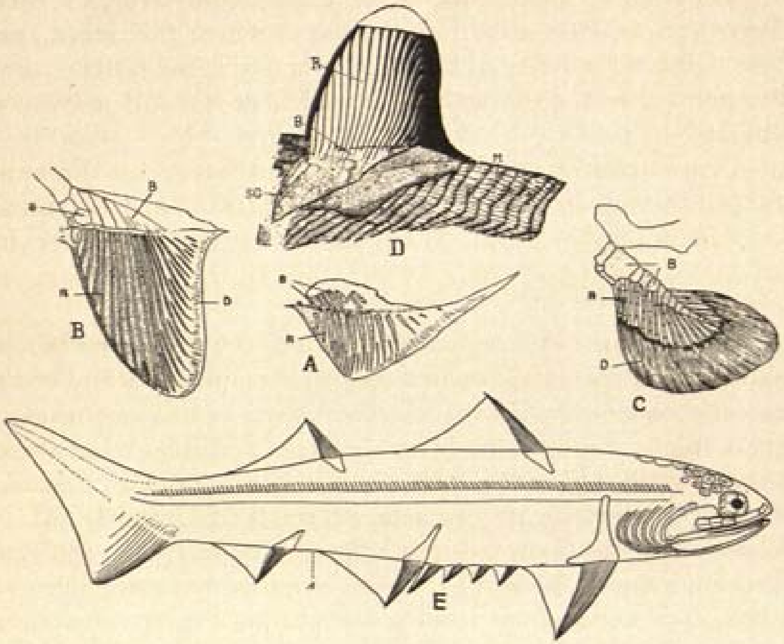


FIG. 4.—Progressive structural stages in the fins of Palæozoic sharks. After Dean

A. Ventral fins of Devonian shark (*Cladoselache*), showing separate rod-like supports of "fin-fold" fin.

B. Pectoral fin of *Cladoselache*, showing pectoral girdle and basal pieces presumably derived from fusion of separate rods.

C. Pectoral fin of Permian *Pleuracanthus*, showing fully developed paddle-like fin with jointed axis.

D. Pectoral fin of *Cladoselache*, partly covered by preserved myomeres.

E. Restoration of generalized acanthodian by Dean.

Meanwhile the supporting rods or skeleton of the fins show a parallel series of changes. At first they are separate rods, merely capable of slight bending to one side or another. Next they become freely movable as the trackers of a piano keyboard; next the segments of the rods nearest the body become squeezed together into several articulated pieces; finally there results a fan-shaped paddle with a wrist-like base. This "fin-fold theory" of the origin of the paired fins is here adopted from the researches of Thacher, Balfour, Dean, R. C. Osburn, Tate Regan, Goodrich, Schmalhausen and others, who seem to have overthrown completely the older hypothesis of Gegenbaur, according to which the leaf-shaped fore and hind paddles of the lung-fishes were nearer the starting-point than the simple fin-folds of the dorsal and anal fins of sharks.

On the contrary, the leaf-like paired paddles of the lung-fishes, as well as the lobe-finned paddles of the Devonian rhipidistian fishes (Fig. 5, *A*), all appear to be rather far advanced toward the four-footed stage of the earliest amphibians.

The gap in our knowledge between the common stem of the dipnoan and lobe-finned fishes on the one hand, and of the oldest known amphibians on the other, is still far greater than any that occurs later in the series of four-footed vertebrates leading to man. Nevertheless, the researches of certain authors during the last decade, especially those of D. M. S. Watson on the Lower Carboniferous amphibians of Great Britain, leave little doubt that the amphibians, and indeed all the higher vertebrates leading to man and other mammals, were derived from some generalized family of air-breathing, lobe-finned fishes that was also related to the stem of the dipnoan series. For in these oldest known amphibians (Fig. 5, *B*) the patterns of the skull as seen from the side, top and under side, bear the most detailed and unmistakable marks of remote kinship with the corresponding views of the skulls of the lobe-finned fishes. The same is true of the labyrinthodont construction of their teeth and of the basic composition of the backbone, shoulder-girdle and of their pectoral and pelvic paddles, which correspond to our arms and legs.

In the ancient mail-clad fishes, which had already solved the problem of breathing by gills when the water was pure and by lungs when the swamps dried up, the paired paddles (Fig. 5, *A*) also foreshadowed our fore limbs and legs in the fact that there was only a single bone corresponding respectively to our humerus and femur at the proximal end of each paddle. The distal parts of the paddles however, were still

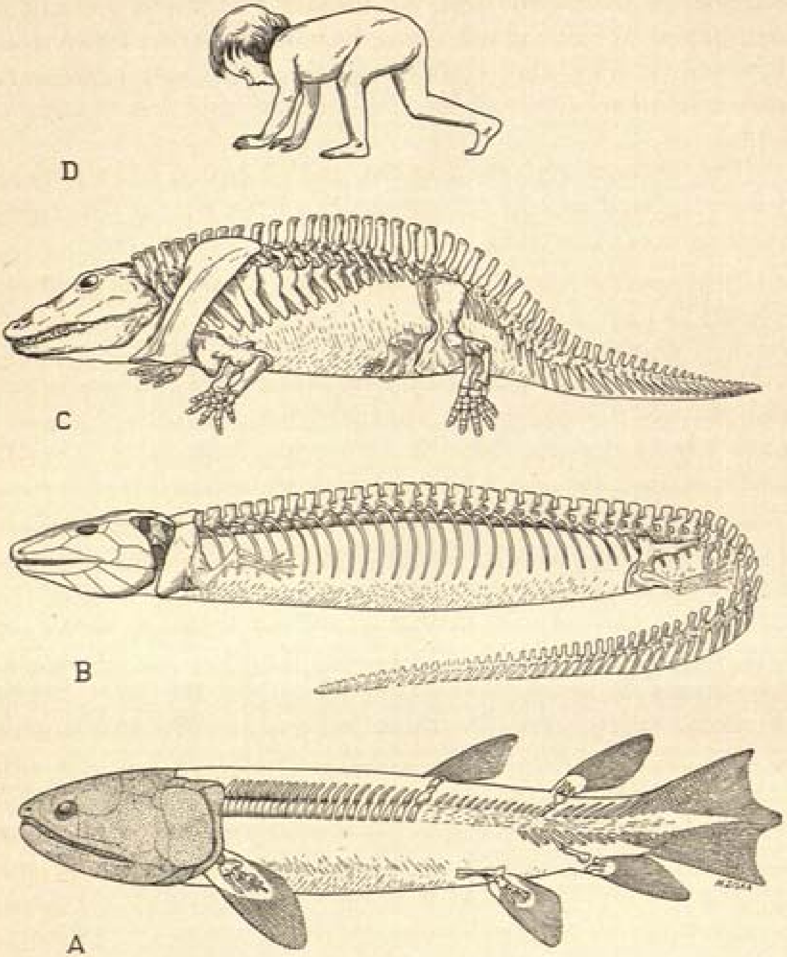


Fig. 5.—Comparison of (A) Devonian lobe-fin, (B) Carboniferous amphibian, (C, Permian amphibian, and (D) modern child, running on all fours.

A. Reconstructed skeleton of *Eusthenopteron foordi*, based on the data of Bryant) Hussakof, Goodrich.

B. *Eogyrinus*. Reconstruction slightly modified from Watson.

C. *Eryops megacephalus*. Based chiefly on the mounted skeleton in the American Museum of Natural History. Details of pectoral girdle and limb after Miner.

D. Modern child running on all fours. After Hrdlička.

adapted primarily for aquatic life and had not yet begun to manifest the profound rearrangement and twisting which must have supervened when the full weight of the body came to be rested upon them.

The Origin of the Carpal and Tarsal Elements

The most difficult part of the problem of the evolution of the fore limbs relates to the origin of the bones of the wrists and fingers (Fig. 6). The ray-like rods in the paddles of the lobe-finned fishes lie more or less in line with the long axis of the humerus or proximal arm-bone. In the oldest amphibians a new bend, that at the elbow, has appeared and the ray-like elements of the hand are more or less sharply bent upon the long axis of the fore arm (Fig. 6). It is difficult to identify with exactness the carpals, metacarpals and phalanges of the amphibians in the ray-like paddles of the lobe-finned fishes. In the first place the bones of the digits proper were not very well known in the paddles of the lobe-finned fishes. In *Sauripterus* of this group, however, they appear to be represented by bony rods in the distal portion of the flesh-covered lobe lying beneath the dermal rays.

There is one important fact which may furnish a clue to the homologies of the carpal elements in lobe-finned fish and earliest tetrapod: namely, in both these types all the skeletal rays converge or slant toward one of the two elements that articulate with the single piece corresponding to the human humerus. In both cases the element toward which the digital rays converge appears to correspond to our ulna. A similar arrangement is found in the pelvic appendages, in which all the digits converge toward the representative of the fibula. This being the case, we may perhaps infer that when the ends of the pectoral and pelvic paddles were turned down to the ground and began to support the body, the rods that gave rise to the carpal elements were shortened and broadened in the manner suggested in the accompanying diagram (Fig. 6).

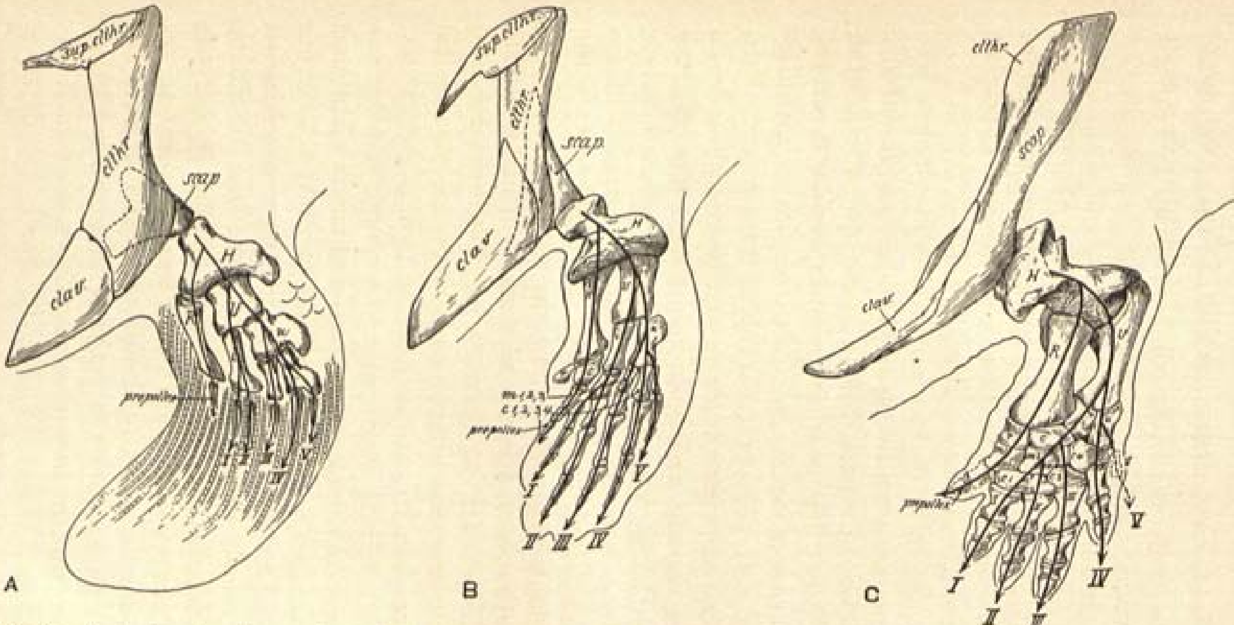


Fig. 6.—Pectoral arch and appendage of (A) Devonian lobe-fin, (B) hypothetical intermediate and (C) primitive amphibian.

A. Generalized rhipidistian, essentially *Eusthenopteron*, but with digital rods restored from *Sauripterus*. Data from Bryant, Patten, Hussakof, Gregory.

B. Hypothetical intermediate stage, showing tentative identification of the carpal elements of the amphibian with certain of the rod-like bony radials of the lobe-fin. Based on studies of many recent and fossil fishes and amphibia, and conformable with the conclusions of Schmalhausen (1912), Watson (1917), Gregory, Miner and Noble (1923).

C. Permian amphibian (*Eryops*), based on Cope's specimen in the American Museum of Natural History. Identification of elements after Gregory, Miner and Noble.

Origin and Early Evolution of the Shoulder-girdle

The shoulder-girdle of these air-breathing, lobe-finned fishes deserves particular attention because it undoubtedly had in it the potentiality of giving rise to the shoulder-girdle of all land-living vertebrates, which, however varied in detail and however modified in various directions by degeneration, may always be traced indubitably back to a central type that is preserved in the oldest known amphibians and reptiles.

The shoulder-girdle of the ancient lobe-finned fishes, like that of the tetrapods or land-living vertebrates, was essentially duplex in character, consisting of an inner, or endoskeletal, base preformed in cartilage, and an outer or dermal sheathing of over-lapping bony plates. The inner or primary shoulder-girdle, including only the coraco-scapular bars of each side, was probably derived originally from the fusion of the basal segments of the serially arranged rods that supported the primitive fin-fold. By the time of the lobe-finned fishes, however, it had lost all trace of its compound origin and appeared as a single piece, the coraco-scapular element of the right and left sides. Hence we owe the major part of our own shoulder-girdle, namely the scapula or shoulder-blade with its coracoid process, to these far-off lobe-finned fishes. In the latter, however, the ventral or coracoid portion of the bar was the principal part, the scapula being of slight extent.

The outer or dermal layer of the shoulder-girdle was originally attached to the upper back part of the skull by two successive plates, the post-temporal and the supracleithrum (Fig. 5, *A*, *B*). The main part of the great crescent-shaped shoulder-plate, called the cleithrum, formed the boundary between the muscular sides of the body behind it and the capacious gill-chamber in front of it. The cleithrum, like all other parts of the dermal girdle, was originally of complex microscopic structure, exactly like the body-scales behind it. Below and in front of the bow-shaped cleithrum on each side came another dermal plate, the clavicle, which was destined to give rise eventually to the human collarbone. The ventral or connecting piece, or interclavicle, is represented in the ganoid fishes by a rhomboid median scale on the mid-ventral surface between the right and left halves of the girdle.

In the typical tetrapod, or primitive four-footed animal, both the outer and the inner plates of the shoulder-girdle had already suffered profound modifications. When the fore part of the body began to be supported on the tips of the pectoral paddles the strains transmitted to the scapulo-coracoid plates through the enlarged humeri became relatively great, especially since the elbows were at first widely everted, so that the center of gravity of the body was relatively far from the points of support. To meet this mechanical disadvantage of the sprawling posture the scapulo-coracoid blade became greatly enlarged (Fig. 5, *B*, *C*) and so eventually did the muscles arising from it. Very soon also the pieces that formerly connected the shoulder-plates with the skull dwindled away and disappeared, so that the muscles immediately beneath them were free to pull the upper ends of the U-shaped shoulder-girdle forward, alternating on the right and left sides, in the first crude attempts at walking (Fig. 9). *Pari passu* with the increasing size of the scapula, the cleithrum, which was the dermal plate immediately above it, gradually lost its predominant importance and gave place to the enlarging clavicle and interclavicle. The main point to keep in mind at this stage is that the shoulder-girdle of fishes arose at the dividing line between the gill-chamber in front and the muscular body behind and that its principal office was to prevent the thrusts from the body and pectoral paddles from interfering with the branchial apparatus. In the tetrapods, on the contrary, the shoulder-girdle is early set free from its connection with the skull and branchial chamber and delegated to the difficult feat of supporting the whole fore part of the body, including the head.

Until a few years ago the foregoing account of the transformation of the pectoral girdle of the lobe-finned fish into that of the primitive tetrapod still lacked the direct confirmation afforded by palæontological evidence of intermediate

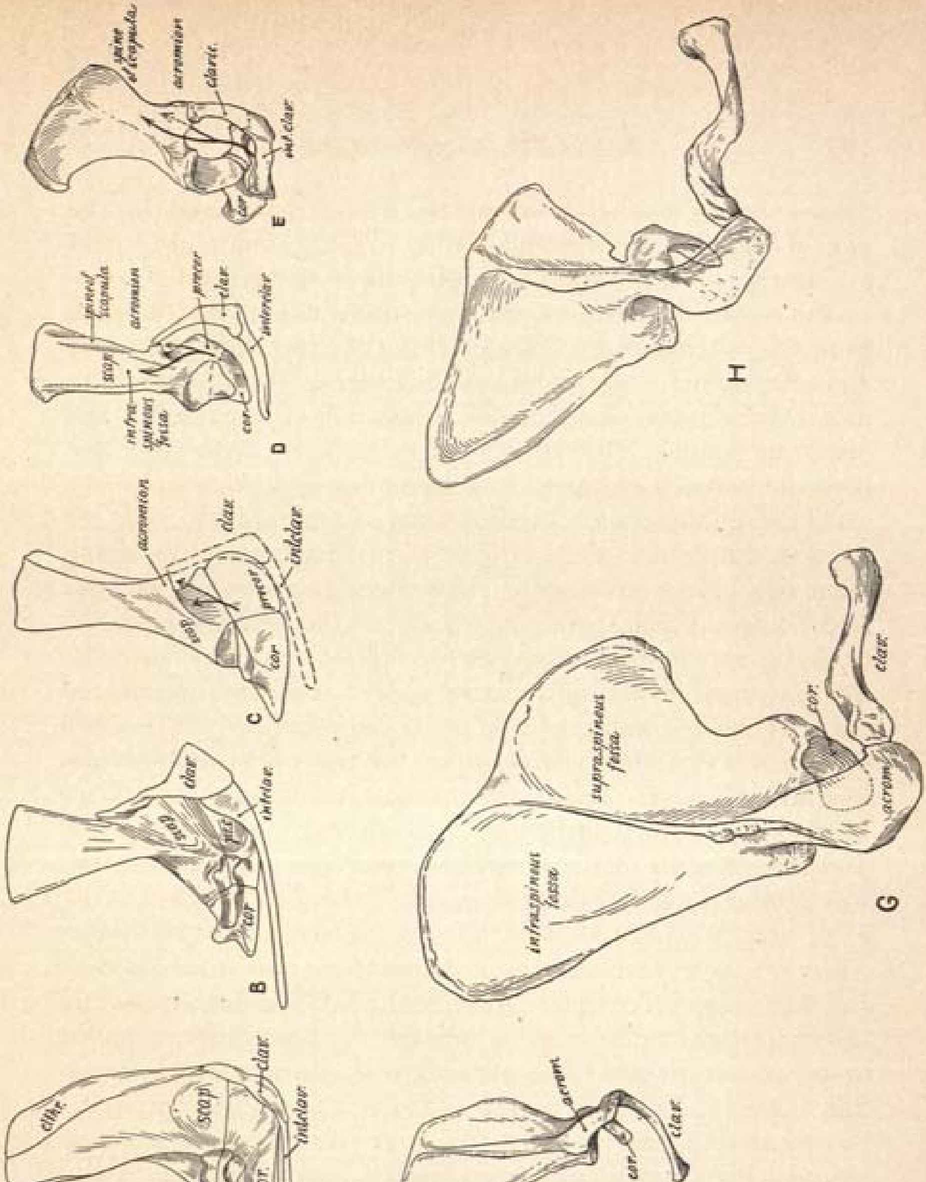


Fig. 7.—Evolution of the shoulder-girdle from primitive tetrapod to man.

A. Primitive cotylosaurian reptile (*Diadectes*). Data from Romer and specimen.

B. Primitive theromorph reptile (*Dimetrodon*). After Romer; data from Williston.

C. Progressive mammal-like reptile (*Dicynodon*). After Romer.

D. Advanced mammal-like reptile (*Cynognathus*). Data from Gregory and Camp, Romer.

E. Primitive egg-laying mammal (*Echidna*). From specimen.

The arrows in C, D, and E, represent the position of the supracoracoid muscle mass, the upper part of which gives rise to the supra- and infra-spinatus of mammals.

F. Primitive primate (Lemur).

Note the great reduction of the coracoid blade, loss of the interclavicle, retention of the acromian process and clavicle. Supra- and infra-spinus fossæ fully developed. G. Gorilla. H. Man.

stages, for up to that time the gap between the lobe-fin and the oldest known amphibian was still profound. But now we owe to Professor D. M. S. Watson of London the demonstration of the existence of an almost ideally intermediate type of shoulder-girdle (Fig. 5, *B*) in the fossil form named by him *Eogyrinus*, from the Carboniferous of Great Britain, in which the dermal shoulder-girdle was still strongly connected with the skull, the cleithrum was still the dominant element and the scapula had not attained its full size.

The subsequent evolution of the shoulder-girdle from the earliest amphibian to man is now well understood and may be reviewed in a few words. The cleithrum, which as we have seen, forms the largest part of the shoulder-girdle of fishes, suffered progressive reduction in the series of mammal-like reptiles until in the early mammals it has either disappeared entirely or, according to Broom, become reduced to the condition of a vestigial dermal cap on the acromial process of the scapula. The lowest of the existing mammals, the monotremes of Australia, still retain a well developed interclavicle but in mammals above the monotremes this element becomes vestigial or entirely disappears.

From the arrangement of the muscle-bearing surfaces (Fig. 7) on the scapulo-coracoid plate in the mammal-like reptiles and monotremes we infer that the future supra- and infra-spinatus muscles grew upward from the supracoracoid mass and invaded the outer surface of the scapula (Romer), also that the anterior border of the scapula became reflected outward to form the spine of the scapula, the supraspinatus fossa being a new development in the mammals (Wilson and Hill, Watson, Gregory and Camp, Romer).

Origin of the Pelvis and Hind Limbs

Turning now to the origin of the hind limbs, we observe first that in the early stages of evolution these lag behind the fore limbs; their important role is to serve mainly as "bilge-keels" or lateral stabilizers (Fig. 4, *E*). Later the pelvis becomes intimately associated also with the function of reproduction and so remains to the present day.

The pelvic fins, at least in the more primitive fishes, at first strongly resemble the simple dorsal and anal fins in having a widely extended base, but in later stages, as already stated, they become more paddle-like, through the shortening of their bases, the growing together of the rod-like cartilages of the inner row, and the fusion of their muscle slips, derived from the myomeres of the body, into compound muscles for performing various movements. This parallelism with the pectoral fins finally develops to such an extent that both in the lobe-finned and dipnoan fishes and in the earliest tetrapods or amphibians (Fig. 5) the fore and hind limbs have become much alike in general appearance.

Even in man the anatomy of the upper extremities reveals so many striking analogies with that of the lower extremities that several ingenious but mutually contradictory schemes have been worked out for homologizing each muscle of the fore limb with some one of the hind limb. Geddes (1912) pointed out that in some respects the arrangement of the chief muscles of the fore limb may be visualized as in essentials a mirror image of the musculature of the hind limbs. Some anatomists indeed have not hesitated to homologize the thumb of the human fore limb with the fifth digit of the hind limb and the radius of the fore limb with the fibula of the hind limb. The anatomists who suggested these comparisons did not, however, take into consideration the construction of the skeleton both in the oldest known fossil amphibians and reptiles and in their predecessors, the lobe-finned and dipnoan fishes, all of which are far older and more generalized vertebrates than man. For all these forms show that the digits of the hands do not appear to be arranged (Fig. 5) in the opposite order as compared with the digits of the hind feet; that is to say, the number of phalanges in the fore feet rises from two in the thumb to four or five in the fourth digit and three in the fifth digit, and the same, or nearly the same, arrangement holds good in the hind foot. So too, the radius of the fore limb of the older tetrapods does not appear to correspond in form and function with the fibula of the hind foot, as it should do if the correspondence between the digits of the hands and feet were in the reversed order, but it does appear to correspond with the tibia, just as the human radius appears to correspond with the human tibia, at least in its relations to the digits.

Although the extremities themselves resemble each other in these oldest tetrapods, yet as a whole the pectoral girdle is profoundly different from the pelvic girdle. The pectoral girdle, as we have seen, is functionally related with the throat, neck and head. Even in man the fish-like muscular connections of the throat with the collarbone, of the collarbone with the back of the skull, still persist. The pelvic girdle, on the other hand, arose as a bony base for the pelvic fins, lying between the lateral and ventral muscles of the body and those of the tail. It never had special bony plates overlying it, as did the shoulder-girdle, and it was from the first intimately associated with the common exit of the digestive, excretory and reproductive systems. Hence much of the musculature of the pelvic limbs was essentially different from that of the pectoral limbs and the attempt to establish precise homologies between the fore and hind limbs has led to much unnecessary mystification. While there are undoubted similarities between the hands and feet, due to the general similarity of their functions, yet not only the pectoral and pelvic girdles but also the humerus and femur of the earlier vertebrates were profoundly dissimilar, as they still are in man.

The Twisting and Bending of the Limbs

There is one important aspect of the problem of the origin of the hands and feet of land-living animals that requires further consideration. Exactly in what directions did the primitive pectoral and pelvic paddles twist and turn when they were turned downward to support the body? Did the pectoral paddles, originally directed backward, curl first outward and then downward, so that the palmar surface of our thumb may represent the upper outer border of the primitive fan-shaped paddle (Fig. 8)? Or did the pectoral paddle first

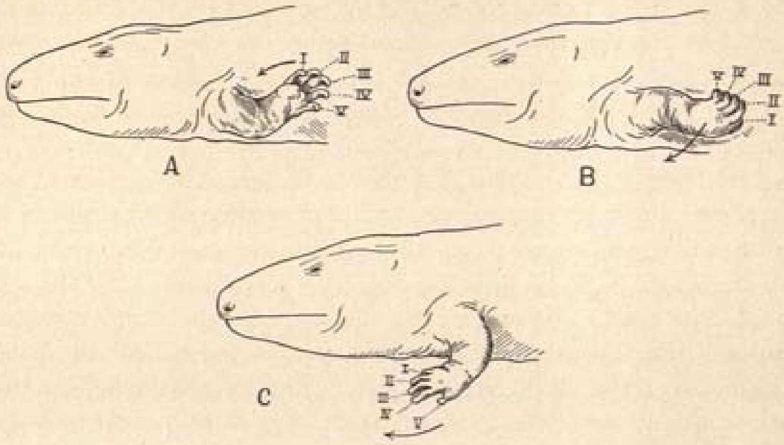


FIG. 8.—Two possible modes of twisting of the pectoral and pelvic limbs.

A. Palmar surface of the future hind limb turned outward, first digit uppermost. Subsequent rotation of limb in direction of arrow.

B. Palmar surface turned inward, first digit on ventral border. Subsequent movement of limb in direction of arrow.

C. Primitive tetrapod position, with first digit on inner side and short fifth digit on outer side.

bend downward and then outward, so that the back of the hand may represent the original outer surface and the little finger its upper border? The strong twisting of the shaft of the humerus of primitive amphibians and reptiles lends weight to the former concept, namely that the fore paddle was twisted at the elbow, as the elbow was everted, in such a manner that the future palm was rotated through nearly 180°. On the other hand, direct comparison of the pectoral and pelvic paddles of the lobe-finned fishes with those of the oldest known tetrapods supports the contrary opinion that there was not a 180° rotation of the future palmar surface but only a twisting at the elbow and a downward turning of the future hand. Investigations of the mode of development and innervation of the muscle masses in amphibia may shed further light on this still obscure matter.

So too in the hind limb there is need of light as to the exact way in which the pelvic paddle was turned downward so as to make the knee joint point forward and outward, while the elbow joint points backward and outward, an arrangement of such remarkable constancy that it persists even in the crawling human infant (Fig. 5, *D*). Does the sole of the foot correspond with the palm of the hand and thus do the flexor muscles of the hand correspond with the extensors of the foot? Again, the marked similarity of the fore and hind feet in all the oldest known tetrapods weighs heavily against this view. A more detailed and comprehensive review of the ontogeny of the hind limb in primitive recent vertebrates may be expected to throw further light on this phase of the subject. At any rate, whatever the differences in the orientation of the original borders of the pectoral and pelvic fins may eventually be shown to have been, it seems highly probable that such differences were already foreshadowed in the pectoral and pelvic paddles of lobe-finned fishes before ever these paddles were used as limbs for the support of the body on land.

Early Methods of Locomotion on Land

Once the primitive tetrapod or land-living type of organization had been achieved, the subsequent changes in the skeleton from the lowest amphibian to man introduced no major changes in the basic plan, however great were the advances in the ways of living and in the mental life.

The primitive tetrapod was essentially a creature in which, as seen from in front, the fore part of the body was slung by muscular straps between the U-shaped shoulder-girdle, while the hind part of the body was slung by muscular straps attached to the V-shaped pelvis. As seen from the side (Fig. 9), the scapular blade of the shoulder-girdle constituted the first tower of a suspension bridge, while the iliac blade of the pelvic girdle formed the second tower. The pathway of the bridge would be represented by the backbone, while the cantilever trestlework supporting the pathway would be represented by the ribs and by such muscular springs as the serratus muscles of the pectoral and the oblique abdominal muscles of the pelvic girdle. This elaborate bridge in turn rests on the limbs, which in the early stages are widely bowed outward in the transverse plane, ready to check any undue

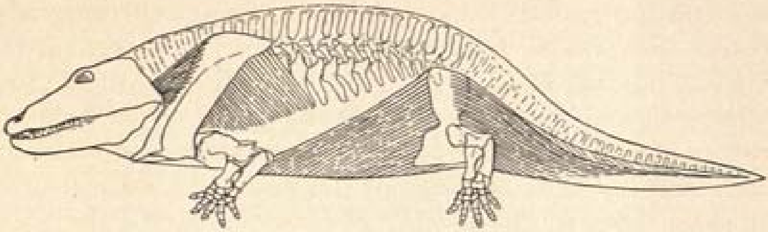


FIG. 9.—Primitive amphibian (*Eryops*), showing bridge-like construction.

tendency for the bridge to fall over on its side. Nor was there in the earlier vertebrates any such close functional integration of the pelvis with the backbone as was later achieved through the shortening and expansion of the sacral ribs.

In the earliest attempts at locomotion the wriggling movement of the body brought about by the zigzag muscle segments of the flanks was still the primary source of forward locomotion, the hands and feet serving primarily as temporary braces for the alternating transmission of this wriggling thrust to the ground as the body swayed and bent first to one side, then to the other. Presently the ventral surface was lifted completely off the ground (Fig. 10) and henceforward the creature relied solely for propulsion upon the lengthening or extension of the limbs. From the very first the limbs acted as jointed compound levers which alternately folded up and extended, on the very same principle which is still found operating in the legs of man.

From the first also there was a criss-cross alternation of flexion and extension of the fore and hind limbs, according to which, for example, the right fore limb would be moving backward while the left hind limb was moving forward, just as our arms swing alternately with the movements of our legs in walking.

In the typical earlier vertebrates the arms and knees sprawled widely from the side (Fig. 10), the track-way was wide, the stride comparatively short and the belly was not raised much above the ground. This method of locomotion, which may still be seen in the existing tailed amphibians, was

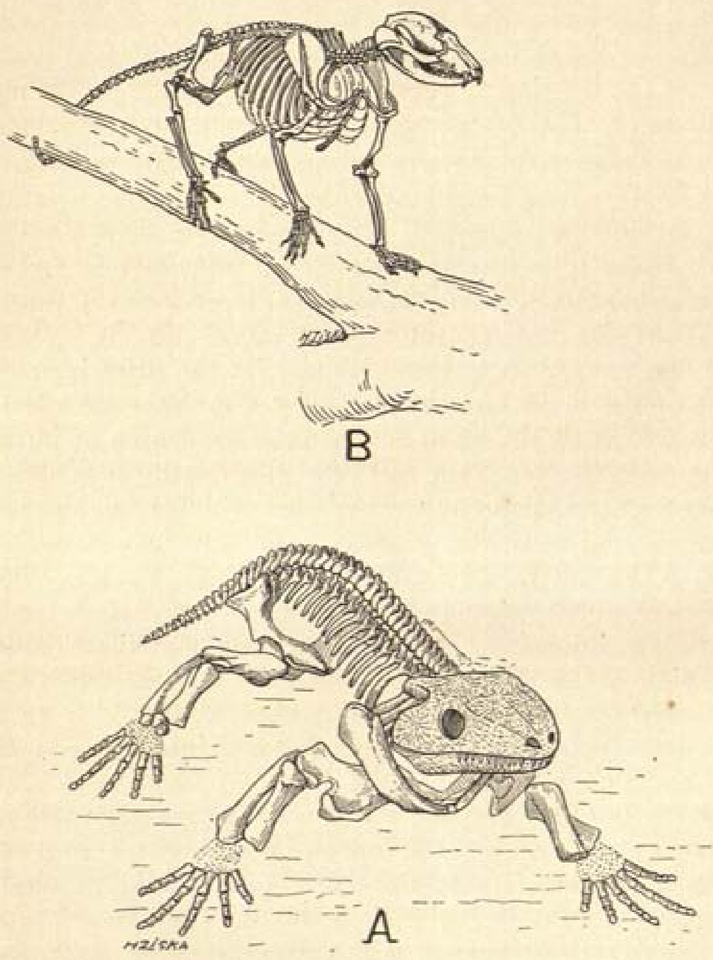


FIG. 10.—Comparison of primitive reptile (Seymouria) and primitive mammal (Opossum).

appropriate for creatures of relatively low vitality and unstable body temperature. But in that branch of the reptiles known as the theromorph or mammal-like reptiles which finally led to the mammals, the construction of the skeleton indicates greater bodily activity, with methods of locomotion that in the later members of the group became almost mammal-like (Pl. I–IV). Meanwhile certain features of the anatomy of the ribs and skeleton of the higher mammal-like reptiles show that these creatures were also beginning to approach the mammalian grade in their improved adaptations for breathing, which in turn suggests a higher and more stable body temperature. At any rate, all the subsequent advances in the locomotor arrangements, as well as in the brain and reproductive methods of mammals, appear to be associated with the far higher vitality and activity of the mammals as compared with the lower and older vertebrates. Man has, in fact, been able to attain the upright posture precisely because he has inherited from the earliest tetrapods the highly adaptable ground-plan of their locomotor machinery and from the earlier mammals the ability to capture and utilize a relatively great amount of energy.

In the earlier tetrapods, as we have seen, the limbs sprawled widely at the side but in the mammal-like reptiles and still more in the mammals themselves the body was raised off the ground, the feet were brought under the body and the elbows and knees were drawn in at the sides. While we know but little of the limbs of the earliest mammals during the millions of years of the domination of the dinosaurs, it is a highly significant fact that the only three Jurassic mammal limb bones hitherto known, a humerus and two incomplete femora from the Jurassic of England, as recently studied in great detail by Dr. George Gaylord Simpson, are found to be intermediate in construction between the corresponding limb bones of the mammal-like reptiles and those of the existing mammals, including man.

Effects of Life in the Trees

During these long Dark Ages of mammalian history the evidence indicates that these earlier mammals which stood near the main line of ascent to modern mammals were small insect-eating forms with low mammalian types of brain and a primitive type of skeleton not unlike that of the smaller existing opossums. During the latter part of this period certain small mammals began the habit of climbing trees, for early in the next period, the Eocene epoch, the tree-climbing habit had already impressed itself deeply upon the skeleton of the earliest known representatives of the Primates, the order to which man belongs.

The priceless fossil skeletons of *Notharctus* (Plate II–VI) in the American Museum of Natural History, show that early in Eocene times the degree of adaptation to tree-climbing habits was already well advanced in this early family of Primates. This is obvious from certain features of the skeleton, especially the construction of the hind feet which were of the grasping type, like those of modern lemurs. The known foot and limb bones of fossil lemurs, monkeys and apes from various horizons of the Age of Mammals are very few in comparison with the known number of fragmentary jaws of the same species. Hence it is highly significant that all the foot and limb bones of these forms that are known are of unquestionably arboreal type. Also, marked traces of present or past arboreal adaptations are conspicuous in the limbs of all the varied recent tree-shrews, lemurs, tarsiers, American monkeys, Old World monkeys and apes. Hence the inference seems unavoidable that the order of Primates as a whole was of arboreal ancestry, a fact of great bearing on the question of man's origin.

So long as progression was limited to the ground the task of propelling the complex suspension bridge with movable piers was comparatively simple. But in proportion as the early mammals succeeded in climbing trees and in leaping and running among the branches, the problem became more and more complex, especially in the nervous devices necessary for more accurate and speedy adjustments in balancing.

When certain of these tree-living primates, namely the ancestors of the gibbons, took to sitting upright and leaping upright in the trees and running upright on the ground, the complexities became further compounded. It was as if the living and swaying compound suspension bridge already described had succeeded in turning itself partly into a drawbridge, and as if when the drawbridge was raised the rear piers began to walk off, balancing the whole thing on their shifting bases.

Sir Arthur Keith, who for many years past has made profound and far-reaching investigations on the anatomy of the anthropoids, was astonished to find that even the gibbons (Plate I–VII), the lowest of the anthropoids, had, so to speak, already solved the problem of bipedal upright progression, to such an extent indeed that in a great many ways their visceral arrangements were fundamentally identical with those seen in man; in brief, that from an anatomical viewpoint the gibbon stands far nearer to man than it does to the lowest of the primates and the same is true as to the crown patterns of its upper and lower molar teeth. On the other hand, the gibbon retains the ischial callosities and many other souvenirs of its pronograde relatives the tailed monkeys of the Old World, which still run and leap on all fours with the backbone in a horizontal position, while the erect gibbon progresses with its backbone at right angles to the line of its advance.

Even closer to man than the gibbon are the chimpanzee (Plate I–VIII) and the gorilla, not only in general appearance but in the truly astounding array of human characters in their brains, visceral anatomy, skeleton, teeth, as well as in the unfolding of their sexual life, embryonic development, reaction to blood tests and drugs, susceptibility to various diseases and so forth. In addition to all this, the fragmentary fossil jaws and teeth of primates already known show characters of successively higher grades, the oldest and lowest teeth recalling those of the still older tree-shrews, while the highest, those of *Dryopithecus rhenanus*, are difficult to distinguish from those of the Piltdown early human stage.

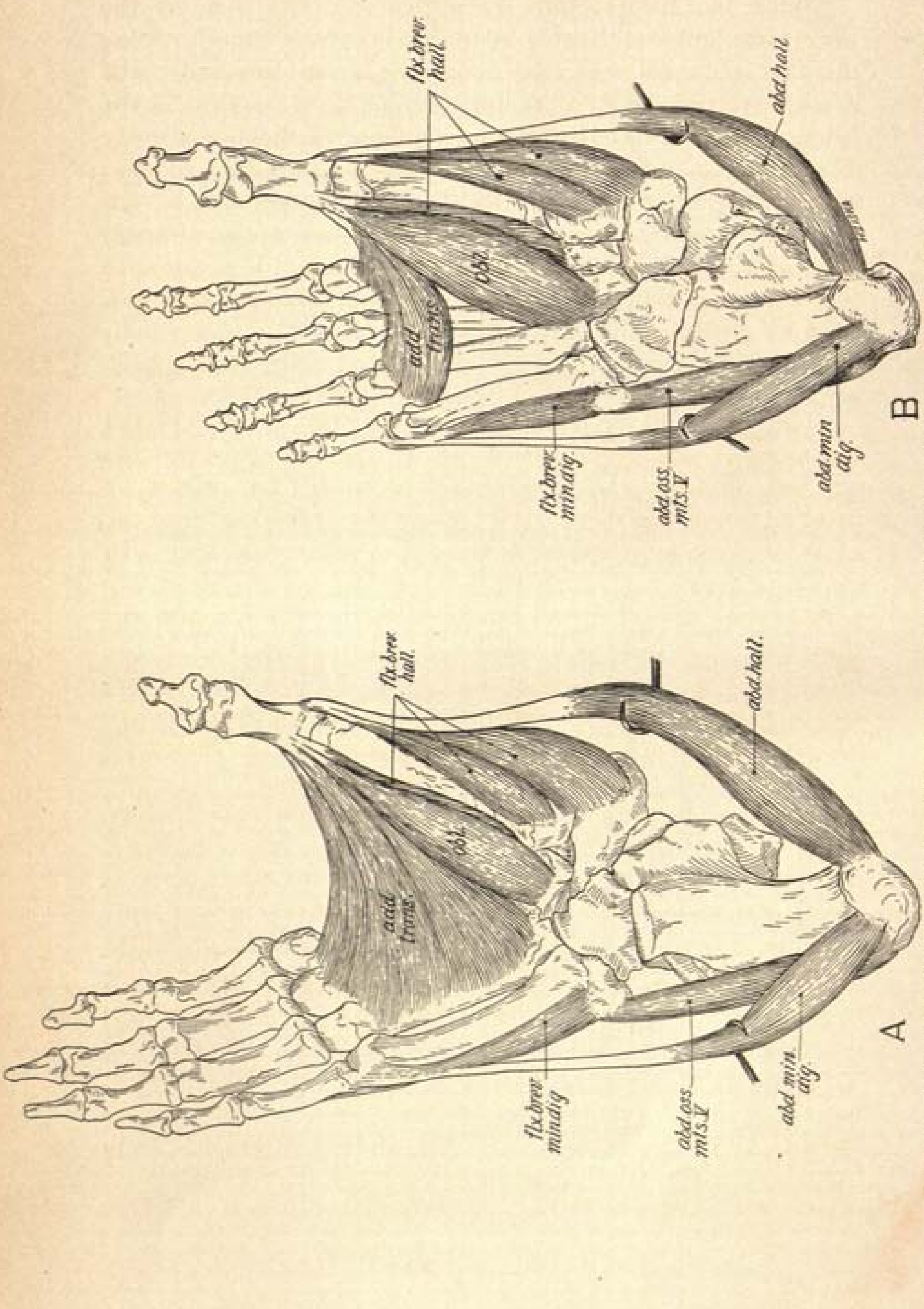


FIG. 11.—Grasping muscles of the great toe and their opponents in (A) gorilla and (B) man. Dissections prepared for the author by Professor D. J. Morton.

Since the hitherto known types of fossil men of the Pleistocene age had already acquired definitely human status, the first separation of man from the primitive anthropoid stock doubtless began at a much earlier time, perhaps in the Miocene epoch. The enormous amount of evidence already available sufficiently establishes the common origin of man with the forerunners of the gorilla and the chimpanzee as well as the arboreal character of the common ancestral stock. We may therefore without further delay attempt to summarize the later evolution of the human foot, backbone and pelvis, pectoral girdle and limb.

Later Evolution of the Human Foot

The hind foot of the oldest known primates was of the biramous or pincer-like type in which the enlarged great toe was set off from the remaining four toes. This biramous construction is equally conspicuous in the arrangement of the muscles of the foot in modern lemurs, monkeys and apes. In man the great toe has been brought around to be nearly parallel with the other toes but inspection of the skeleton will show that the foot as a whole is still anatomically of the biramous type, namely that the great toe has gone on increasing in size while the outer four toes have greatly diminished. The superficially hand-like appearance of the feet of the chimpanzee and gorilla was viewed in earlier times as an important and fundamental point of difference from the foot of man; but Huxley and his successors have repeatedly shown that the foot of chimpanzee and gorilla is operated by muscles which correspond not to the muscles of the human hand, but to those of the human foot (Fig. 11). In brief, the musculature as well as the skeleton of the human foot is fundamentally of the biramous anthropoid type but it has been greatly modified in detail by long ages of running on the ground; to such an extent that these later modifications, like the later writing on a palimpsest, have obscured the older record beneath. However, as the evolution of the human foot has frequently been discussed in recent years, I will here content myself with the statement that this very organ which was formerly supposed to offer the greatest objection to the theory of the remote arboreal origin of man, has upon closer inspection proved to offer the most convincing evidence in favor of such an origin. In brief, the available anatomical evidence indicates that when certain already upright-moving anthropoids came to spend more time on open ground, they intensified the habit of running upright already practised by the gibbon, and that possibly through the slow operation of Natural Selection, the hind limbs became longer, the great toe was drawn partly forward and the outer toes partly inward, all the toes being gradually turned more toward the ground. The detailed changes in the musculature, as the sole of the foot gave up its grasping power and became important in the forward thrust in walking, have been discussed by Keith, Weidenreich, Morton and others.

Later Evolution of the Backbone and Pelvis

In the lower pronograde primates the bodies of the centra of the vertebral column, especially in the lumbar region, are relatively elongate. With the advent of the more erectly climbing posture of the great apes, these centra tend to shorten and widen until in the gorilla they begin to approach the human type. Meanwhile the thorax has gradually acquired a wide, more or less circular section, the ribs being arched in such a way that they tend to be flush with the tips of the vertebræ. All this has been ingeniously explained by Sir Arthur Keith as a result of the habit of brachiation, or climbing with the arms. The S-shaped curve of the backbone of man, as seen in the side view, has the effect of bringing the center of gravity of the body in line with the fulcrum or center of support at the head of the femur. The beginnings of the lumbar curve are seen in the gorilla.

The ilium, or upper rod of the pelvis, in the running and leaping pronograde primates is narrow. As the habit of sitting and walking upright is established, the gluteus and iliacus muscles, on opposite sides of the ilium, widen out in a transverse direction and the blade of the ilium widens with them. The relatively longer ilia of the lower primates give a

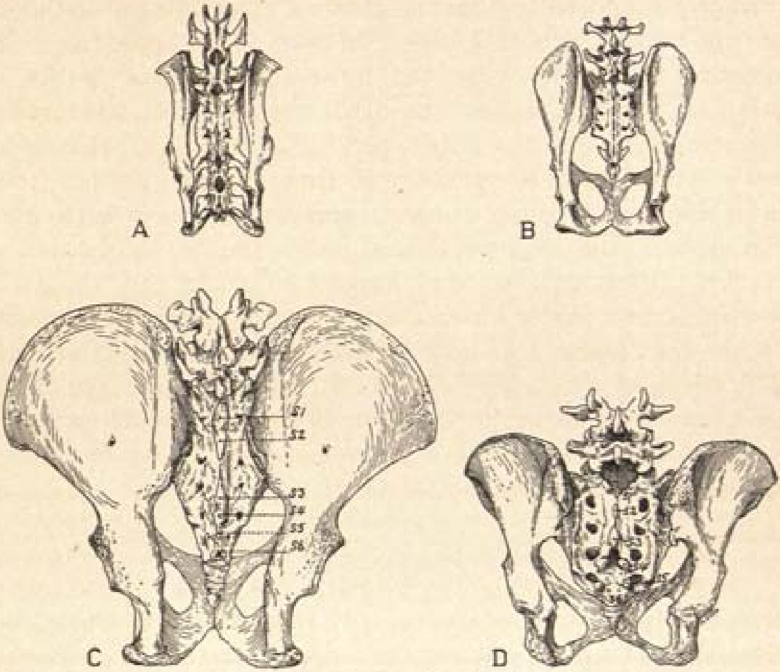


FIG. 12.—Transverse widening of the ilia in primates.
A. Lemur, with long narrow ilia adapted for leaping.
B. Gibbon. Incipient widening of the iliac blade.
C. Gorilla.
D. Man.

longer contraction for these muscles, which permits an extremely wide range of movement of the femur. On the other hand, the widening of the ilium, which reaches an extreme in man, greatly increases the force of these muscles, which are of great importance in holding the body erect, especially when the opposite limb is lifted clear of the ground. The transverse widening of the iliac crests (Fig. 12) has also been conditioned by a widening of the middle space between them; this gives room for the transverse expansion of the erector spinæ muscles, which are very broad and powerful in erectly walking man. The great rearward expansion of the posterior border of the ilium tends to shift the line of the vertebral column backward so as to bring the center of gravity of the torso and head

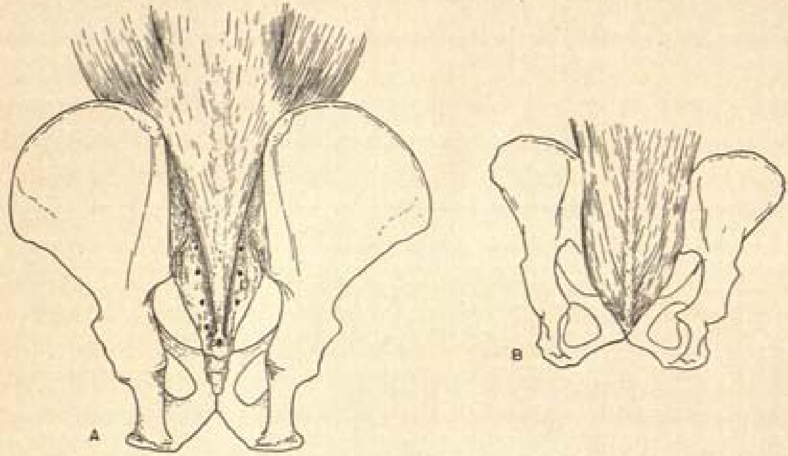


Fig. 13.—Vertical shortening of the ilia in man, with increasing space for erector spinæ muscles.
A. Gorilla. From specimen.
B. Man.

directly above the points of support, which are the heads of the femora. Again, the straightening of the knees in standing tends to shift part of the weight through the vertical column of bone and thus to relieve the strain on the muscles of the knee and hip.

Later Evolution of the Hand

As already noted, the lower primates, including the lemurs and apes, run and leap in the trees like quadrupeds, but the anthropoid apes tend on the whole to progress by swinging from branch to branch, with the weight of the body suspended from the arms. The extreme importance of this mode of locomotion in the problem of man's origin was apparently first adequately appreciated by Sir Arthur Keith, who invented for it the name *brachiation*, or progression by means of the arms. In his classic studies on man's posture already so often referred to, he has shown how deep-seated is the anatomical correspondence of the human chest, abdomen, loins, groin and lower limb with the corresponding regions of the brachiating anthropoids, how the various peculiar characters of man are explicable as later adaptations to special human habits on the part of a formerly brachiating group; he has also shown that many of the hernias, ptoses and prolapses suffered by humanity are likewise explainable as the results of failure of this formerly brachiating type of organization under the severe strains of modern life.

Since it is impossible to review adequately this evening the great field of the evidence of brachiating habits in our anthropoid ancestors, we may select for special notice a single division of this manifold subject, namely the evidence afforded by the construction of the human hand. To deal summarily even with this fraction of the subject, we may affirm without fear of successful contradiction that nowhere else in the whole class of mammals is to be found a hand which is so strikingly man-like in character as are those of the gorilla (Figs. 14, 15) and of the chimpanzee. The correspondence in the musculature, as recorded by many anatomists and recently reviewed by Sonntag, is extraordinarily close. The basic correspondence in the skeleton of the hand is most readily grasped at the first view and it becomes the more impressive as we study it more closely.

It should be noted that these resemblances persist in spite of the fact that in man the hand is chiefly used for carrying and manipulating objects, while in the apes it is still very largely used as an organ of locomotion. The principal differences between the hands of man, the chimpanzee and the gorilla are to be found in the different relative lengths of certain parts: thus the thumb of man is relatively longer than that of the chimpanzee, while his fingers are relatively shorter. This inferiority of the chimpanzee's thumb might at first sight be thought to be an objection to the derivation of man from brachiating ancestors; for, it might be argued, the habit of brachiation leads to the use of the hands as hooks and thus involves the thumb in a course of degeneration that is the opposite to the highly progressive development of the human thumb. But to this objection it may be replied that during the millions of years since the divergence of man from the

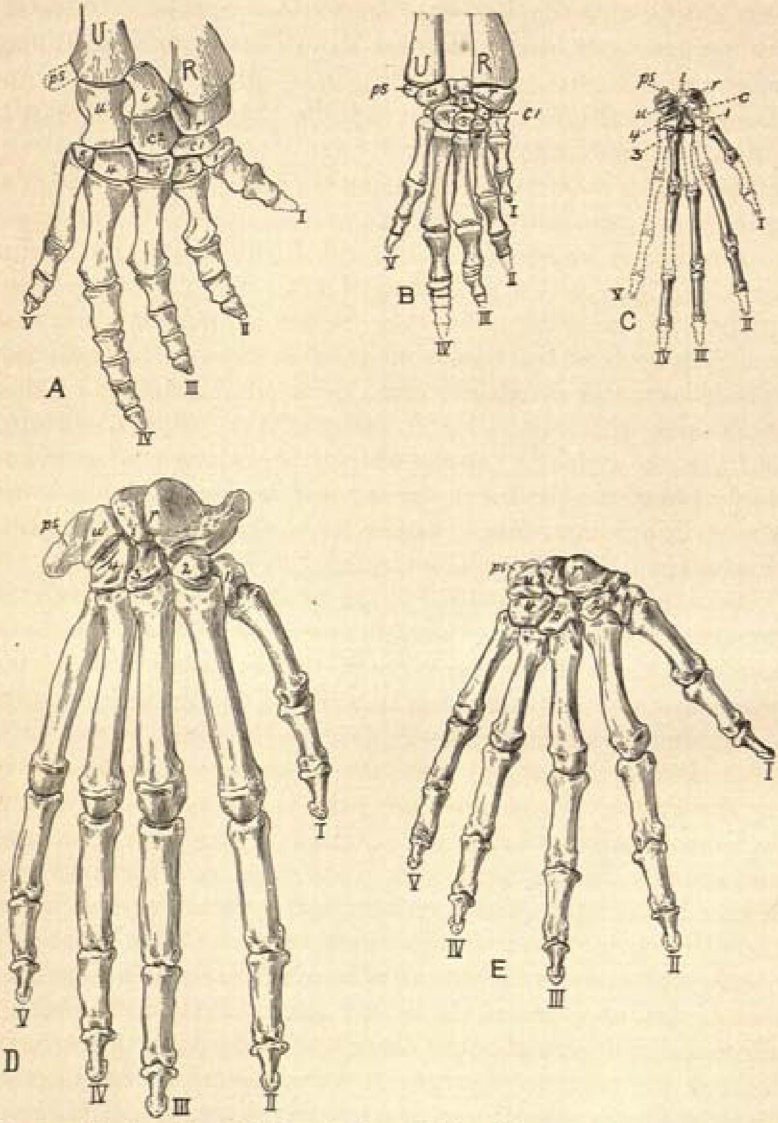


FIG. 14.—Structural stages in the evolution of the hand from primitive reptile to man.
A. Primitive theromorph reptile (*Ophiacodon*). After Williston.
B. Progressive mammal-like reptile (*Theriodesmus*). From Williston, modified from Seeley.
C. Primitive primate (*Notharctus*). D. Gorilla. E. Man.

primitive anthropoid stock, there has been plenty of time for the reduction of the thumb in the persistently brachiating chimpanzee, as well as for the lengthening of the thumb in the formerly brachiating man, exactly as the comparative anatomical evidence indicates.

The thumb of the adult male mountain gorilla, which is now giving up his former brachiating habits, has become very powerful and robust, although it is not as long in proportion to the entire hand-length as is the thumb of man. In short the anatomical evidence indicates that the common brachiating ancestral stock of man, gorilla and chimpanzee (whose former existence is sufficiently established by a host of characters common to its now diversified descendants) had not yet attained the extreme of brachiating adaptations seen in the modern orang-utan, but was still in the early brachiating stage, with hands and thumbs of moderate proportions.

On the other hand, we apparently should not construe the extreme shortness of the hands of very early fœtal stages of man as at all reminiscent of the adult conditions in the relatively near ancestral anthropoid stock. Schultz (1927) has shown that even in the long-handed gibbon, as well as in other monkeys, apes and man, the hands are much shorter in the early fœtal stages than they are in the adult and that in these features the hands of early fœtal monkeys and apes presumably do not hark back to their immediate adult ancestors but exhibit conditions that are peculiar to early fœtal life. So too in the early fœtal stages of the horse, figured by Ewart, the main or third digit is relatively very short and broad, quite unlike the long narrow third digit of its three-toed ancestors. Nor do the forefeet of the modern horse ever pass through the successive stages of reduction at all comparable with the known fossil succession from the four-toed *Eohippus* to the later three-toed and one-toed forms. Again, Deniker, Selenka, Keith, Schultz and other embryologists have shown that in many cases the generic and even the specific characters of the adult begin to be visible in the fœtal stages, so that the fœtal gibbon early begins to be recognizable as a gibbon and

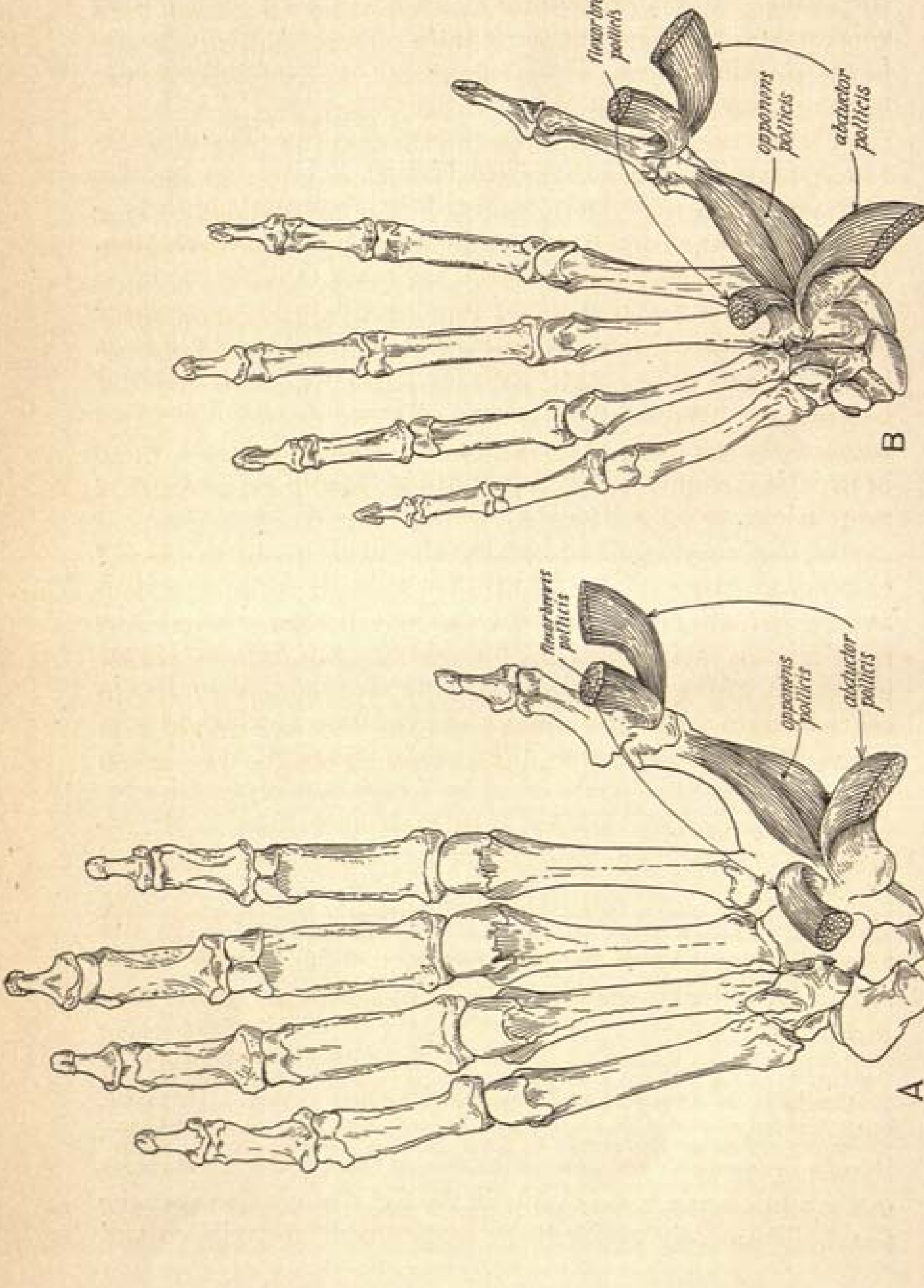


FIG. 15.—Opponens and related muscles of the hand of (A) gorilla and (B) man. Dissections prepared for the author by Professor D. J. Morton.

would never be mistaken for a fœtal gorilla. Thus it is not surprising that the early fœtal hands of man, while retaining a general family resemblance to those of the gibbon, already partly foreshadow the adult human characters just as do the germs of the teeth. In other words, many, but not all, the fœtal characters anticipate or foreshadow the present adult conditions rather than recapitulate ancient adult conditions, so that the shortness of the hands in the fœtal stages of man does not seem to constitute a valid objection to the derivation of man from earlier adult stages with longer hands.

In brief, the hands, as indeed the whole organization of the gorilla and the chimpanzee are thoroughly adapted for more or less upright progression in the trees, that is, for the habit of brachiation. Again, the hands, as well as the fore limb as a whole, the chest, abdomen and viscera of man, to say nothing of his feet, teeth, brain and physiologic reactions, prove that he is far more nearly related to the brachiating anthropoid stock than to their predecessors the pronograde monkeys.[1] In other words, it was solely through the habit of brachiation, at least in its earlier stages, that beneficent Nature rescued us from monkeyhood. By turning the backbone of our pronograde ancestor up on end, she literally set him on his feet and not only raised his face toward the sky but encouraged him to use his hands and his brains in working out his own salvation.

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[1] A vast accumulation of published evidence is on record in support of this statement.

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PLATE I.—Structural Series of Skeletons from Fish to Man.

I. Devonian lobe-fin (*Eusthenopteron*). Data from Bryant and Hussakof.
II. Carboniferous amphibian (*Eogyrinus*). Data from Watson.
III. Primitive Permo-Carboniferous reptile (*Seymouria*). Data from Broili, Williston, Watson, Romer.
IV. Progressive Triassic pro-mammal (*Cynognathus*). Data from Seeley, Gregory and Camp.
V. Primitive mammal (Opossum).
VI. Primitive lemuroid Eocene primate (*Notharctus*), Data from Gregory.
VII. Proto-anthropoid (Gibbon).
VIII. Typical anthropoid (Chimpanzee).
IX. Man.

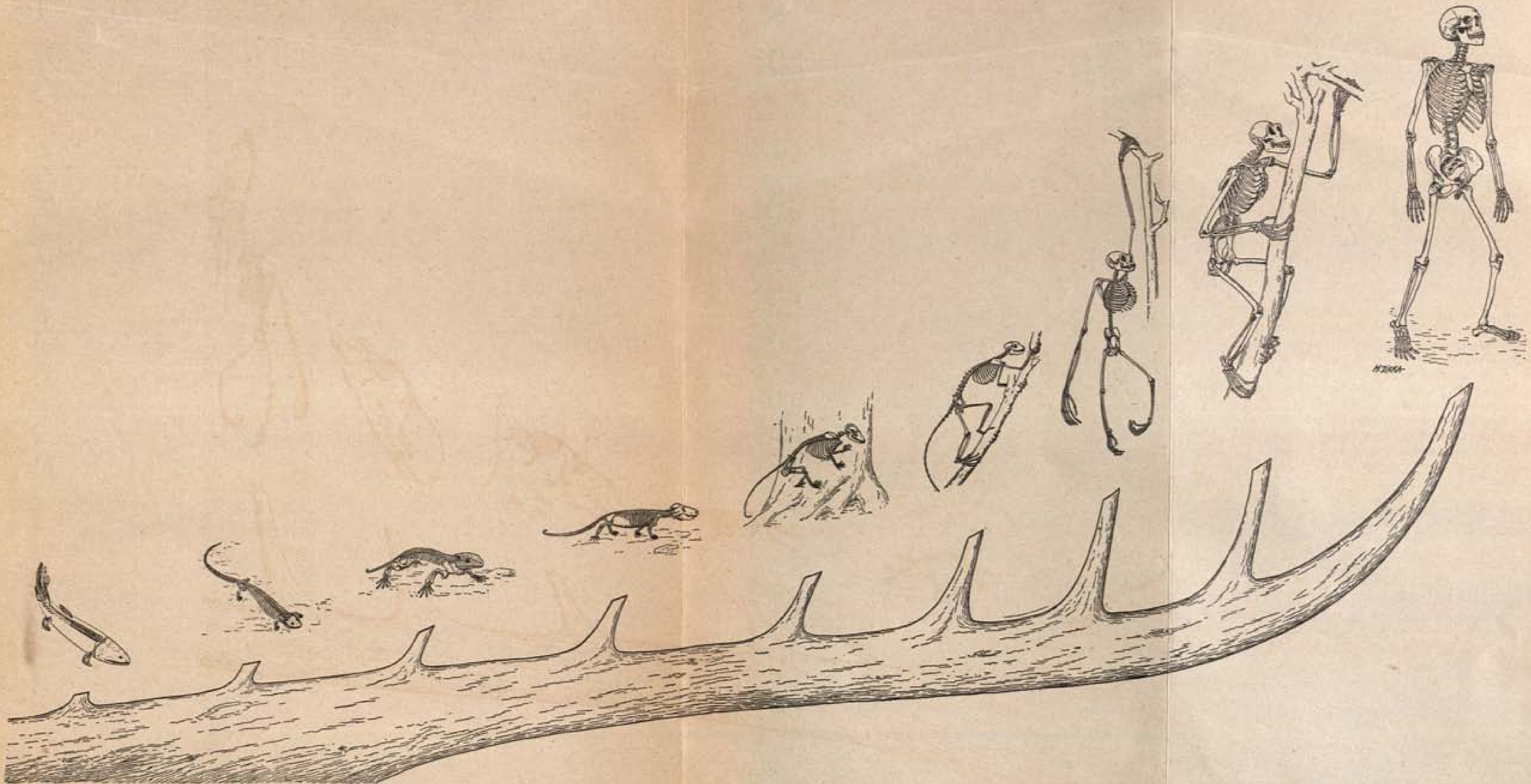


PLATE II.—Man's Debt to the Lower Vertebrates.

I. Devonian lobe-finned fish (*Eusthenopteron*). Data from Bryant, Hussakof, and specimens.

II. Lower Carboniferous amphibian (*Microbrachium*). Slightly modified from Watson.

III. Primitive Permo-Carboniferous reptile (*Seymouria*). Data from Williston, Watson, Romer.

IV. Progressive mammal-like reptile (*Cynognathus*). Data from Seeley, Gregory and Camp, and specimen.

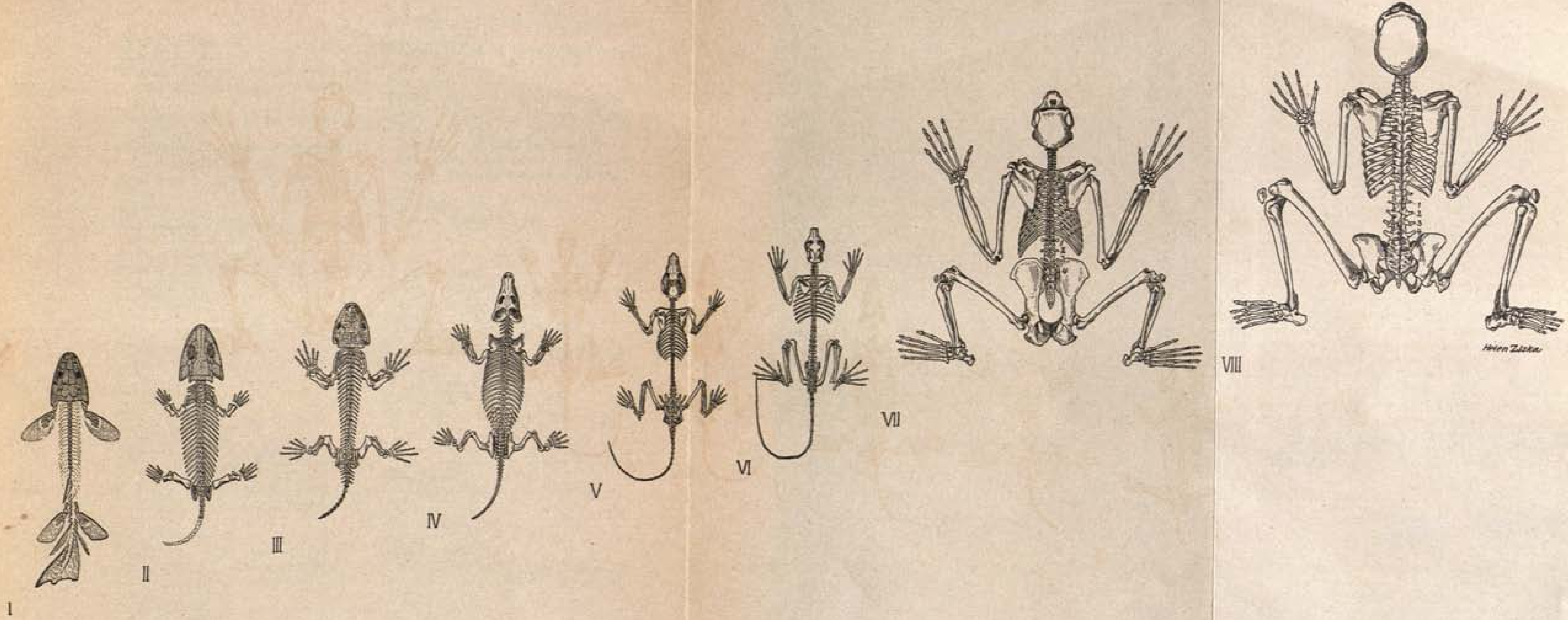
V. Primitive marsupial mammal (Opossum). From specimen.

VI. Primitive Eocene primate (*Notharctus*). Data from Gregory and specimen.

VII. Chimpanzee.

VIII. Man.

Specimens posed in primitive tetrapod position in order to show man's inheritance from the lower vertebrates of every bone in the skeleton.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